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Abstract

Diachronic morphosyntacticians of all theoretical persuasions agree that there is a tendency for "more lexical" linguistic material to develop "more functional" characteristics over time, a process generally known as grammaticalization. While most previous work on grammaticalization has been conducted in surface-oriented functionalist frameworks, this dissertation aims to illuminate the deeper structural properties of a sub-set of these phenomena, diachronic affixation, as well as its much rarer opposite, de-affixation, a phenomenon in which previously bound material becomes a syntactically independent form. This approach differs from previous generative approaches to this problem in utilising a non-lexicalist, piece-based, syntactic approach to morphology, Distributed Morphology (DM), according to which both words and phrases are built by the same generative system. Besides providing a schematic typology for the structural properties of affix-genesis and highlighting the theoretical advantage of DM, this dissertation has four main theoretical points. First, it makes explicit predictions about the locus of newly affixed material. Second, it argues, that affix-exodus is no less natural a change than affix-genesis. Third, it explores the similarities between affix-exodus and two other varieties of linguistic change: morphological re-cuttings and the disintegration of complex heads. Finally, it demonstrates that similar phenomena can also occur within a word. This is predicted by a theoretical framework with the properties of DM specified above. In addition to its specific contribution to work on diachronic morphosyntax, this dissertation has implications for morphology, morphosyntax, and historical linguistics more broadly, and argues that no additional diachronic-specific component is needed in the grammar.

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Anthony Kroch

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Julie Anne Legate

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MORPHEME BOUNDARIES AND STRUCTURAL
CHANGE: AFFIXES RUNNING AMOK

Chaya Eliana Ariel Diertani

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Supervisor of Dissertation:

David Embick, Associate Professor of Linguistics

Chair of Graduate Group:

Eugene Buckley, Associate Professor of Linguistics

Dissertation Committee:

Anthony Kroch, Kahn Term Professor in the Cognitive Sciences

Julie Anne Legate, Associate Professor of Linguistics

Donald Ringe, Kahn Term Professor in Linguistics

MORPHEME BOUNDARIES AND STRUCTURAL CHANGE:
AFFIXES RUNNING AMOK

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Chaya Eliana Ariel Diertani

Dedication

To those who left my life too soon –

Sandra Burton, Georgine Leiner Delfs, John Ross Fisher, Brian Hammon

To unseen sources of inspiration –

Michael Faraday, Veronica Franco, Edward Gibbon

And to my heroes –

Alan Delfs, Robert A. Delfs, Irim Sarwar

With love.

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Graduate school is frequently an exercise in surviving psychological torture, and as a result, perhaps the most significant debt one owes upon one's emergence is the least tangible: to all of the people who have prevented one from throwing oneself off the Ponte Vecchio

whilst singing “O Mio Babbino Caro” (at times an all-too-tempting prospect, as any survivor of graduate school will no doubt concur). I owe more than I can say to the various people who, knowingly or unknowingly, provided moral support, hugs, chocolate, books, games of Dominion, commiseration, encouragement, and various other sources of relaxation, diversion, and entertainment: Lydia Nassima Abdo, Emily Anna Bridges, Robin Clark, Elisabeth Cohen, J. Michael DeAngelis, Alan Delfs, Aaron Dinkin, Aviad Eilam, Ben George, Kaitlin Heller, Kristen Hendricks, Garincha Hilaire, Ariela Housman, Chris Johnson, Yaron Karl, David Kowarsky, Maura Krause, Catherine Lai, Cassia Martin, Rebecca McKeown, Jean-François Mondon, the Pennsylvania Ballet Company, Philomen Probert, Beatrice Santorini, Irim Sarwar, David and Lark-Aeryn Speyer, Leslie Sudock, Warren Tusk, Donald X. Vaccarino, Josh and Lucy Von Korff, Robin Ward and the mysterious person who gave me a chupacabra. Thanks, you guys.

ABSTRACT

MORPHEME BOUNDARIES AND STRUCTURAL CHANGE:

AFFIXES RUNNING AMOK

Chaya Eliana Ariel Diertani

Supervisor: David S. Embick

Diachronic morphosyntacticians of all theoretical persuasions agree that there is a tendency for "more lexical" linguistic material to develop "more functional" characteristics over time, a process generally known as grammaticalization. While most previous work on grammaticalization has been conducted in surface-oriented functionalist frameworks, this dissertation aims to illuminate the deeper structural properties of a sub-set of these phenomena, diachronic affixation, as well as its much rarer opposite, de-affixation, a phenomenon in which previously bound material becomes a syntactically independent form. This approach differs from previous generative approaches to this problem in utilising a non-lexicalist, piece-based, syntactic approach to morphology, Distributed Morphology (DM), according to which both words and phrases are built by the same generative system. Besides providing a schematic typology for the structural properties of affix-genesis and highlighting the theoretical advantage of DM, this dissertation has four main theoretical points. First, it makes explicit predictions about the locus of newly affixed material. Second, it argues, on the basis of detailed case studies, that affix-exodus is no less natural a change than affix-genesis, and that both are types of common morphosyntactic changes involving errors on the part of learners in their analysis of the placement or nature of morpheme boundaries. Third, it explores the similarities between affix-exodus and two other varieties of linguistic change: morphological re-cuttings (e.g. English "a nadder" > "an adder") and the disintegration of complex heads (e.g. the rise of English do-support). Finally, it demonstrates that very similar phenomena can also occur within a word, most often string-vacuously, but sometimes not.

This is predicted by a theoretical framework with the properties of DM specified above. In addition to its specific contribution to work on diachronic morphosyntax, this dissertation has implications for morphology, morphosyntax, and historical linguistics more broadly. In particular, it touches on the relationship between synchronic research and diachronic research, indicating that the traditional rigid separation of the two, while methodologically useful, is somewhat misleading, in that most of the same principles applicable to the synchronic domain are also implicated diachronically.

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List of Abbreviations

Below is the list of abbreviations used in the glosses of this dissertation. Some of the more specialised glosses (e.g. the Persian *ezafe*) are explained in the relevant chapter.

A	=	absolute	INST	=	instrumental
ABES	=	abessive	INTR	=	intransitive
ABL	=	ablative	INV	=	inversion
ABSL	=	absolutive	ITER	=	iterative
ACC	=	accusative	JUS	=	jussive
ACT	=	active	LOC	=	locative
ADV	=	adverbial	LV	=	light verb
ALL	=	allative	MASC	=	masculine
ANAL	=	analytic	MP	=	mediopassive
AND	=	andative	NARR	=	narrative
AOR	=	aorist	NEG	=	negation
ASP	=	aspect	NEUT	=	neuter
AUG	=	augment	NOM	=	nominative
AUX	=	auxiliary	OBJ	=	object
BEN	=	benefactive	OBL	=	oblique
C	=	conjunct	OPT	=	optative
CAUS	=	causative	ORD	=	ordinal
CD	=	deuterotonic	PASS	=	passive
CLIT	=	clitic	PAST	=	past
CM	=	class marker	PERF	=	perfect
COLL	=	collective	PL	=	plural
COMIT	=	comitative	POSS	=	possessive
COMP	=	complementizer	PREP	=	preposition
COMPL	=	completive	PRES	=	present
COND	=	conditional	PRET	=	preterit
CONJ	=	conjunction	PROG	=	progressive
CONTEMP	=	contemporative	PRV	=	preverb
CONTIN	=	continuous	PTCL	=	particle
COP	=	copula	PTCP	=	participle
CP	=	prototonic	PURP	=	purposive
DAT	=	dative	Q	=	interrogative
DEF	=	definite	REDUP	=	reduplicant
DEM	=	demonstrative	REFL	=	reflexive
DEP	=	deponent	REL	=	relative
DIM	=	diminutive	REP	=	repetitive
EMPH	=	emphatic	REV	=	reversive
ERG	=	ergative	RPT	=	reported
EV	=	epenthetic vowel	SG	=	singular
EVID	=	evidential	SGLT	=	singulative
EX	=	exclusive	STAT	=	stative
EZF	=	<i>ezafe</i>	SUBJ	=	subjunctive
FM	=	focus marker	SUPRL	=	superlative
FOC	=	focus	TH	=	theme
FUT	=	future	TR	=	transitive
GEN	=	genitive	VN	=	verbal-noun
GER	=	gerund			
ILL	=	illative			
IMPF	=	imperfect			
IMPV	=	imperative			
IND	=	indicative			
INDEF	=	indefinite			
INF	=	infinitive			

Preface

One of the very first classes I ever took as a freshman at the University of Pennsylvania was a freshman seminar taught by Beatrice Santorini called “The Birth and Life of Words,” and it amuses me to think that, almost twelve years later, I am now submitting a dissertation which could very well have the same title, although of course it doesn’t. I figured out what interested me fairly early in my undergraduate career, although it took me rather longer to figure out the best way of pursuing it.

My initial training in linguistics was actually in traditional Indo-European philology. I was fascinated by the diversity that a group of related languages could display, and I wanted to know what made them the way they were. Sound changes were well and good, but it was always the morphology that interested me the most, in part because it was less regular, and in part because of the structure behind it. But as the years progressed, it slowly dawned on me that the questions that were becoming of greatest interest to me were not questions that traditional historical linguistics was equipped to answer. I was interested in Indo-European morphology, but I was also interested in morphological change, and language change in general, and I knew that I didn’t have the methodological background to do what I really wanted to do. There were too many holes, too little formal sophistication, and not enough attention paid to languages outside the family and the possible insights they could provide.

I spent a year or two working in diachronic syntax, an area in which generative linguists have made tremendous progress. Diachronic morphosyntax, however, remained my primary interest, and it is, sadly, an area that few generative linguists have ventured into. Much of the work done in diachronic morphosyntax seemed to me to be asking the wrong questions and concentrating on the wrong details. In particular, far too little attention has been paid to what to me is the most interesting aspect of the problem: the structural consequences of change – a situation not very different from that in traditional historical linguistics.

This dissertation, then, is my attempt to advocate a different approach to morphosyntactic change, an approach that is very much the product of my slightly schizophrenic linguistic background, the traditional and the generative. It is also my attempt to demonstrate that there is no need for traditional and generative linguists to consider themselves at odds. It is my firm belief that this dissertation has benefitted from both halves of my training; in fact, I could not have written it had my background been more homogeneous. And despite my feeling that traditional linguistics has too little respect for structure, I find no major incompatibilities between the traditional and generative sides of my intellectual pedigree; to me they blend together seamlessly.

My career as a graduate student has not been smooth; all of us spend far too much time grappling with identity crises, depression, and various assorted inner demons, and I am certainly no exception. There have been far too many times over the last few years when I have been unable to remember why I was bothering to write the dissertation at all, periods lasting many months when I was convinced I hated linguistics and everything associated with it. As I finish this now, I find that I am starting to remember – that, in fact, I love this work, with all of its challenges. I love the complexity, the diversity of the data, the way I can sense the answer to how the pieces of the puzzle fit together hovering just outside my grasp. This dissertation has been a labour of love. Not the faery-tale love of the silver screen, with starry-eyed princesses and princes on white horses and happy-ever-afters; but the kind of love one finds in real life: the kind that takes work and tears and determination; the kind that gives you flights of rapture and moments of utter despair; the kind that brings sleepless nights, both good and bad; the kind that one can never abandon, even through the absurdity of it all, because of the underlying conviction that somehow, somewhere, it is all worth it. The last six years – and the last twelve – have been both pain and passion, agony and ecstasy, and I have been privileged, more than I can express, to have experienced them.

Chapter One

Introduction

From the Neogrammarians to Antoine Meillet to more recent linguists like Halle and Hale, numerous linguists have recognised the need to treat linguistic change as a discontinuous process rooted in individual speakers, particularly children acquiring their native language. Yet surprisingly little work has been done from this viewpoint in the last few decades; the prominent approaches to morphosyntactic change in particular require that one view language change as a process completely independent of speakers. Moreover, surprisingly little work has been done on morphological change from the perspective of generative grammar. This dissertation aims to remedy both of these gaps by providing a rigorous, precise account of morphosyntactic change with explicit reference to the role played by the learner.

The subjects that will be dealt with within this dissertation are topics typically dealt with under the heading *grammaticalization* or *grammaticalization studies*. Although there are a number of scholars who research in this area, the terms themselves have no standard definition. There is a fundamental divide between those researchers who believe in grammaticalization as a serious theoretical concept (1.1a; e.g. Haspelmath, Heine, Hopper, Traugott) and sceptics who regard it as a convenient descriptive label without theoretical status (1.1b; e.g. Campbell, Janda, Joseph, Newmeyer, Norde).

(1.1a) ‘We define grammaticalization as the process whereby lexical items and constructions come in certain linguistic contexts to serve grammatical functions, and, once grammaticalized, continue to develop new grammatical functions.’

(Hopper and Traugott 1993:xv)

(1.1b) ‘[G]rammaticalization appears to be no more than a cover term for a conjunction of familiar developments from different spheres of language, none of which require or entail any of the others.’

(Newmeyer 2001:225)

This dissertation follows Newmeyer in assuming that “grammaticalization” is essentially a convenient descriptive label rather than an important theoretical concept. In fact, besides being convenient, the term is incredibly vague, as it may refer to a very broad class of phenomena which clearly represent very difficult structures. My aim here is to account for a very specific, well-defined sub-part of the phenomena traditionally huddled under the grammaticalization umbrella, namely changes affecting what we might call the “wordhood” status of individual grammatical elements: affixation, a diachronic event whereby erstwhile words become affixes, and de-affixation, a diachronic event whereby erstwhile affixes become words. Along the way, I will introduce other types of diachronic phenomena which tend not to be discussed under the heading of “grammaticalization”¹ but which nevertheless have clear properties in common with affixation and de-affixation.

1.1 Key Themes

Four key themes recur throughout the chapters that follow. To wit:

¹ Sadly, this often means that they are not discussed at all; cf. Joseph 2005.

- (1.2a) Modelling morphosyntactic change within a non-lexicalist, piece-based syntactic theory of morphology has a number of advantages.
- (1.2b) Morphosyntactic change is best treated as a succession of different synchronic grammars.
- (1.2c) De-affixation is in no way unnatural, but rather fits comfortably within a typology of morphosyntactic changes with similar structural properties.
- (1.2d) Morphosyntactic changes of the type discussed here arise in circumstances of analytical ambiguity, frequently implicating the location and/or nature of various morpheme boundaries.

As mentioned above, the theoretical framework used in this dissertation is Distributed Morphology (DM; Halle and Marantz 1993). As stated in (1.2a), DM is a piece-based, syntactic theory of morphology, according to which all word-building is done within the syntax; therefore, there is no generative lexicon in this system. The choice of DM is motivated by the ease with which some of the best insights of functionalist work in this area can be translated into a more formal, precise idiom that makes clear predictions about what kinds of structural changes are possible; this will be spelled out at some length in Chapter Three. This advantage is a consequence of having morphology and syntax generated by a single system, as such a model predicts that syntactic changes should have a direct impact on morphology, and vice versa. A further advantage of DM is that no diachronic-specific mechanisms or properties need to be postulated to account for change; the same operations used to account for synchronic grammars are all that is necessary.

This leads immediately to the second theme in (1.2b), which concerns the non-teleological, learner-centric model of language change assumed here and elaborated in Chapter Two. My view is that language change, in the usual case, is connected to innovative analyses of the data made by native-speaker learners. It is not the case that the language is changing independently of its speakers, nor that language transmission is seamless and continuous. Instead, each new speaker must recreate the grammar entirely on his own, and if he should fail to replicate exactly the grammar of the speakers who acquired the language ahead of him, an innovative grammar is the result. The speaker himself may remain entirely unaware that he has erred. This is why no independent diachronic mechanisms exist: there is only the conservative grammar, the innovative grammar, and the difference between them. As detailed in Chapter Two, this approach to change is far from novel, but it has become alarmingly rare in recent work on morphosyntactic change. One of my purposes in writing this dissertation, therefore, is to demonstrate how morphosyntactic change is modelled when concerns about directionality are irrelevant.

The third theme, elaborated in (1.2c), concerns directionality itself, specifically de-affixation. In the grammaticalization literature, so-called counterdirectional changes – i.e., examples of “more functional” material becoming “more lexical” – are a perpetual problem; as I will discuss in Chapter Two, some researchers try to discount them or dismiss them, while others even try to argue that they do not truly exist. Changes of this nature are, in some circles, regarded as “unnatural”. The view developed here is different: I argue that de-affixation is not at all unnatural, particularly when it is considered in the context of other kinds of morphosyntactic change. In fact, a typology of morphosyntactic changes with similar structural properties can be easily delineated; each of these kinds of change shares features with de-affixation and affixation. Furthermore, issues of inherent directionality play no role in the model of change developed in this dissertation, since there is no separate diachronic component in my theory; thus, though it may be rarer than affixation,² de-affixation cannot be “unnatural” in the sense of violating universal properties of language.

² But cf. Chapters Two and Five for discussion as to why the relative rarity of de-affixation may be less certain than is commonly acknowledged.

(1.2d) is perhaps less an independent theme than a natural product of the others. According to the theoretical position taken by this dissertation, morphosyntactic change results when learners fail to exactly replicate the grammars of their forebears (1.2b), and there is a typology of changes with similar properties (1.2c). Taken together, the implication is that we ought to be able to examine the recurring properties and thereby identify specific errors that learners are known to make. It will emerge that very often morphosyntactic changes arise in contexts where the “correct” analysis is only one of multiple possible analyses because the data are ambiguous. As we shall see, most of these concern morpheme boundaries.

- (1.3a) Learners may put morpheme boundaries in an innovative place.
- (1.3b) Learners may interpret a morpheme boundary as being of a different type than it was in previous generations (M-word to Sub-word or vice versa).
- (1.3c) Learners may interpret a morpheme as an exponent of a different structural position than it was previously – particularly if there are null exponents involved.
- (1.3d) Learners “like” to interpret surface affixation as structural affixation, possibly because they have a bias towards equating morphosyntactic words with phonological words.

As I will discuss later, (1.3c) may be the single most general type of error resulting in morphosyntactic change.

1.2 Prospectus

The primary chapters of this dissertation are divided into two principal parts, the first (Chapters Three through Five) dealing with affixation and de-affixation and the second (Chapters Six and Seven) with other types of change. These two parts are bookended by shorter chapters that concern the theoretical background and secondary literature.

Chapter Two is essentially a preface to the remainder of the dissertation. Its primary goal is to lay out explicitly the theoretical assumptions that underlie the rest of the dissertation, including both the view of linguistic change and the morphosyntactic framework adopted here. Much of the work on grammaticalization and related topics conducted over the last few decades has taken a heavily teleological perspective with respect to diachrony, often combined with the notion that language exists as a separate object beyond its speakers and changes independently of them; however, this position is far from universal, and in fact, it is not even traditional. The view taken here is consistent with the work of such diverse linguistics as Brugmann, Osthoff, and Paul on the one hand, and Halle and Kroch on the other – namely, that linguistic change is fundamentally non-deterministic and cannot be separated from its speakers. Linguistic change is linked to novel reanalyses of the data on the part of language learners; in other words, learners fail to perfectly replicate the grammar of their immediate linguistic forebears. All discussions of reanalysis and innovation within this dissertation will be centred in this understanding of the cause of change.

In addition to laying out the requisite diachronic background, Chapter Two also provides a brief introduction to the key terminology and concepts of DM, as these are necessary for complete understanding of the analyses that follow in later chapters.

The next three chapters are the central chapters of the dissertation, as they concentrate specifically on affixation and de-affixation. Chapter Three works out in detail the basic structural properties of diachronic affixation, building on five detailed case studies, carefully chosen for their relative simplicity. While the standard literature on this topic gives the impression that new affixes are added to the grammar in a straightforward, almost trivial fashion, it will emerge almost immediately that the actual examples are considerably more

complex, and each depends a great deal on the linguistic context in which it is situated. Nevertheless, despite their complexity and diversity, the actual structures involved are fairly clear, and it is possible to articulate a typology of possible structural preconditions for affixation without undue difficulty.³ The chapter also pays attention to potential issues of learnability and transmission that may arise, as well as necessary preconditions in areas like phonology and semantics; the importance of surface linear order is maintained throughout. The chapter concludes with a discussion of some implications specific to SOV languages.

Chapter Four is ultimately a continuation of Chapter Three: while the case studies in Chapter Three were chosen for their relative simplicity, those in Chapter Four were selected to illustrate some of the issues that arise when more complex examples are considered. Two broader issues are addressed: the problem of the locus of new affixes inside a morphological word, and the problem of redundant new affixes. DM allows for a specific prediction about the location of a new affix inside its new host: the affix should appear on the periphery, since this was its position prior to its incorporation and change of status. By and large, this prediction is borne out; however, there are exceptions in the form of infixes of various categories, which pose potential challenges to the theory articulated in this dissertation. I argue that these apparent exceptions can receive plausible explanations once other linguistic factors are considered; for example, morphological infixes often reflect the “trapping” of a clitic when two independent M-words became a single M-word, while phonological infixes are in fact underlyingly peripheral, and presumably were so when first affixed. I also provide a novel analysis for the perpetual puzzle of the Old Irish verbal system along the way.

The second half of Chapter Four deals with multiple exponence in various guises. On a naïve view, multiple exponence poses a serious challenge for this theory: if the locus of an affix is taken to reflect its permitted position prior to its affixation, then redundant affixes should be ruled out, since it would imply that, in its previous incarnation as an independent word, the affix could occupy multiple structural positions simultaneously. Closer examination reveals that none of the various types of multiple exponence arose under these circumstances. Three sub-types of multiple exponence are identified and discussed: redetermination, by which the exponents expressing the same features reflect different diachronic tiers of affixation rather than a single event of redundancy; fusion, whereby two M-words, each comprised of multiple Sub-words and marked for the same features, are joined into a single M-word; and sporadic pleonasm, the spontaneous emergence of redundant marking. While these cases are certainly complex (and the last remains particularly mysterious), they are in no way incompatible with the views of morphosyntactic change adopted here.

Chapter Five concentrates on de-affixation, and it aims to correct two common misperceptions about this phenomenon by focusing on six detailed case studies. First, I demonstrate that de-affixation can be accounted for using exactly the same analytical tools used for affixation in Chapter Three. Second, I argue that de-affixation does not require exceptional circumstances in order to occur. Previous work in this area, including my own, has held the position that de-affixation is rare because it can only arise when the grammar of its language is undergoing significant changes in other areas, particularly phonology and syntax. The actual case studies reveal that de-affixation *can* arise in a context of unrelated grammatical changes, but need not; in fact, the changes that produce de-affixation may be very local. Having established these points, the chapter turns to a more general discussion of the structural properties and learnability issues associated with de-affixation. Though more data is needed, preliminary results suggest that de-affixation is structurally more permissive than affixation.

³ Compounds (e.g. “bear-shark”) will largely be excluded from discussion for reasons of time and space, although the questions they raise are both interesting and relevant, and should be examined at some later date.

Chapters Six and Seven move the discussion to other varieties of morphosyntactic change which share similar properties with affixation and de-affixation; in particular, all changes involve word or morpheme boundaries in some way or other. Extending the discussion to other kinds of change has several advantages. First, when situated in this context, de-affixation begins to seem much more natural and less exotic, as it is simply part of a constellation of possible morphosyntactic changes. Second, with the focus of so much of the sub-field on the definition of grammaticalization, many other interesting phenomena are overlooked. Paying attention to a broader range of possible morphosyntactic changes gives us a better sense of the kinds of errors language learners may make, and the types of structures and surface configurations that they find ambiguous or difficult to acquire.

The primary focus of Chapter Six is specifically those types of change which resemble de-affixation in some respect. Two such phenomena are discussed: morphological re-cutting and complex head disintegration. Morphological re-cutting is well-known in the historical literature. This is the phenomenon whereby learners incorrectly position a word or morpheme boundary with respect to the linear string of segments; in English, for instance, there are many examples where the [n] of the indefinite article has been reinterpreted as belonging to a vowel-initial noun, or vice versa. Some cases of morphological re-cutting have more interesting grammatical consequences, resulting, for instance, in new phonological shapes for affixes; several such case studies are discussed in the chapter. Morphological re-cutting is similar to de-affixation; both involve learners misinterpreting the location of an M-word boundary. The difference is that de-affixation has consequences on a structural level, while morphological re-cutting is a strictly surface phenomenon.

Complex head disintegration (CHD) has not, to my knowledge, been previously identified as a discrete phenomenon, unlike morphological re-cutting, although at least one example of it – the rise of *do*-support in English – has been the subject of a great deal of research. CHD is the replacement of a synthetic construction by an analytic construction in particular grammatical contexts (which may be either phonological or syntactic or both). While synthetic and analytic constructions frequently exist side by side – cf. e.g. complex tenses in German or French – in ordinary examples, the two are essentially interchangeable. CHD differs in that the synthetic construction is actually *ungrammatical*, or at least strongly dispreferred, in certain contexts. This is similar to de-affixation, in that a two-word construction comes into being where previously there was only a one-word construction; the difference is that in de-affixation, one of the two pieces is created by the historical change, whereas in CHD, the analytic construction is an option already present in the language, which learners are simply exploiting.

Chapter Seven deals with affix migration: instances of affixes moving to different structural positions within an M-word, without ceasing to be part of this same M-word. Three different such phenomena are explored. The first cases involve complex pronominals (or demonstratives) originally consisting of a simpler pronominal with a cliticised particle; it is very common, in a number of genetically unrelated languages, for the particle and case suffix to be seen to “invert”, descriptively. These examples have been argued to be evidence in favour of “externalisation of inflection” (Haspelmath 1993); I will argue instead that they reflect changes in the learner’s structural analysis and often involve morphological re-cutting. The second type of example involves the relocation of an affix following a change in its function. On the basis of the understanding that changes in the location of an affix can be correlated with changes in its function, we can infer that changes in an affix’s function alone can reflect underlying, string-vacuous changes in its structural position; the remainder of the chapter concentrates on changes of this type.

In addition to sharing some key properties with affixation and de-affixation, affix migration is theoretically important because it substantiates a prediction that falls out from the choice of DM as an analytical framework. Because, in DM, the same generative system is responsible for building both words and phrases, we ought to be able to find evidence that

similar diachronic phenomena occur both above and below the level of the M-word. The examples discussed in Chapter Seven provide evidence that this prediction is correct.

This dissertation is not the first attempt to account for the phenomena discussed herein within a generative framework. Chapter Eight briefly describes two other accounts: the Optimality Theory account of Kiparsky (2011) and the Minimalist-couched analysis of Roberts and Roussou (2003). Of the two, Kiparsky is dealing more directly with the same phenomena as this dissertation; Roberts and Roussou are more concerned with grammaticalization involving M-words, with no changes in morphological status. Both accounts suffer from some empirical problems.

By and large, the scope of this dissertation is limited to considerations of structural configurations and the relationship between surface linear ordering and deeper structures. However, it is equally important to take into account matters of learnability and general cognition, and therefore, at various junctures throughout the next eight chapters, I discuss the relationship of the learner to what she is acquiring. In my concluding chapter, Chapter Nine, I will develop a bit more fully some of the concepts invoked in previous chapters, and suggest that future research be conducted along these lines.

Chapter Two

Theoretical Preliminaries

2.1 Overview

In this chapter, I set out the theoretical background on which the following chapters will draw, in the context of discussion about some of the general theoretical issues concerning historical linguistics. Most of the issues discussed in this chapter are discussed more fully by Janda (2001), who is particularly concerned with linguistic discontinuity; Janda (2005), who delivers a highly critical review of the grammaticalization field in general; and Newmeyer (1998, 2001), who aims to demonstrate the epiphenomenal status of grammaticalization. I particularly encourage the reader to seek out Newmeyer (2001), as that paper is both essentially correct on all counts and also extremely well-written. The second and fourth sections of this chapter include a large number of direct quotations, in order to give as accurate an impression as possible of the kinds of arguments employed by authors on both sides of the issues concerned.

Following a discussion of the controversy over continuity in historical linguistics, I set out my own assumptions about morphosyntactic change in section 2.2. I argue that language change is best viewed as a discontinuous process rather than a steady progression of gradual changes, and that there are no diachronic “rules” so much as tendencies. Rather, the focus of diachronic work should be on such topics as the transmission of different synchronic grammars, the biases of language learners towards one analysis over another, and the possible influence of one linguistic system on another (e.g., the influence of phonology on morphology, or the influence of morphology on syntax).

Section 2.3 provides a brief overview of the Distributed Morphology framework, in which the remainder of the dissertation will be couched. The definitions and concepts discussed in this section will be essential for the analyses presented in later chapters.

The chapter concludes with a brief discussion of the problems with the prevailing methodology for the treatment of the phenomena dealt with in this dissertation, arguing that “grammaticalization theorists” such as Haspelmath, Heine, Hopper, and Traugott place too much emphasis on the reification of what is essentially a descriptive observation of diachronic phenomena, with the result that many more interesting issues are neglected. I conclude section 2.4 by agreeing with Newmeyer and others that grammaticalization is best treated as an epiphenomenon of changes occurring elsewhere in the grammar, particularly at the level of structure. Unlike the rest of this chapter, the concepts discussed in 2.4 are not essential for the remainder of the dissertation; this discussion is included for the sake of demonstrating what this dissertation is *not* about and for providing a brief justification of this decision.

As for what this dissertation *is* about, it is worth stressing it here. Language change is a complex topic that can be approached from several directions, each trying to explain a different phenomenon. One might, for instance, try to give an account of the habitual co-occurrence of a cluster of particular diachronic phenomena; alternatively, one might aim for an account of the mechanisms underlying certain types of change. The choice of explanation determines, to a great extent, the direction of the investigation. My goal in this dissertation is to illuminate the underlying mechanisms involved in changes concerning the morphosyntactic interface of the grammar.

2.2 Theoretical Assumptions about Linguistic Change

The trends currently prevalent in morphosyntactic research have resulted in an environment in which the superficial consequences of potentially interesting structural

changes (e.g. grammaticalization) are considered the problems in need of solution; this will be addressed more thoroughly in section 2.4. In contrast, this dissertation focuses on the nature of the deeper grammatical changes signalled by the surface-level changes.

Under the “grammaticalization studies” programme, many different kinds of linguistic changes have been lumped together under the heading “grammaticalization”; some of these, such as the assignment of grammatical, semantic, or pragmatic functions to language-specific prosodic contours, are not even specifically morphosyntactic in nature. As it is typically used, therefore, the term “grammaticalization” refers more to the direction of change than to any explicit kind of change; therefore, the grammatical changes underlying different varieties of “grammaticalization” may be very divergent. In this way, the term is largely uninformative for the present purposes, since the focus here is on a very specific sub-type of grammaticalization, and on the structural level at that.

With this narrower focus comes a need for more precise terminology. For this reason, I will not be using the terms *grammaticalization* and *de-grammaticalization* in this dissertation unless I am discussing the work of another researcher who does employ the term. When I do allude to grammaticalization or de-grammaticalization, I will be using the terms in their most superficial sense, as defined in (2.1-2).

- (2.1) *Grammaticalization*: convenient descriptive label for a diachronic process whereby formerly “more lexical” items become increasingly “more functional”
- (2.2) *De-grammaticalization*: convenient descriptive label for a diachronic process whereby formerly “more functional” items become “more lexical”

The material presented in the following chapters of this dissertation is predicated on the view that language change occurs in language transmission; cf. Yang (2002), with references. The primary proponents of change are children learning their native language; the secondary proponents are adults learning a second (or third, and so on) language and adults engaging in dialect contact (cf. Labov 2007). Change occurs when the child attempting to analyse the adult system makes an error which is not later corrected. When the same errors are made by multiple children, or by children with some status among their peer group, they propagate; the innovation spreads within a speech community and eventually may gradually take over.

Several important points must be made in this context. First, language changes as the result of behaviour by the speakers; it does not change on its own or as the result of influences other than those of the speakers. This is hardly a novel opinion, as it has been repeated in the work of many noteworthy scholars from the Neogrammarians forward, many of whom are otherwise quite diverse in their opinions. This can be clearly demonstrated from the following collection of passages from authors as diverse as Paul, Meillet, Kiparsky, and Lightfoot, arranged in chronological order.⁴

...[L]anguage is not a thing which leads a life of its own outside of and above human beings, but ... [one which] has its true existence only in the individual, and hence ... all changes in the life of a language can only proceed from the individual speaker.

Osthoff and Brugmann (1878:xii/1967:204)

All ... [mental] processes are executed in individual minds and nowhere else.

Paul (1880/1920/1970:11, translated after Sampson 1980:27)

⁴ For a more comprehensive assortment of opinions on both sides, with extensive critical discussion, cf. Janda (2001)

The principal condition that allows both the rapidity and the intensity of language change is the fact that the transmission of language is discontinuous... The language is not given to him as a block, all in one piece... For each individual, his language is thus a total recreation achieved under the influence of the milieu that surrounds him. There could not be a more absolute discontinuity.... When language change takes place, things happen in an analogous manner... the discontinuity is of course greater than in the case of the regular transmission of older speakers' language to younger speakers.

Meillet (1929/1936:74-5; translated after Janda 2001:273-4)

Since every child constructs his own optimal grammar by induction from the utterances to which he has been exposed, it is not necessary that the child and his parents have identical grammars, for ... a given set of utterances can be generated by more than one grammar... It is clear that such discontinuities in the grammars of successive generations must exercise a profound influence on the further evolution of the language.

Halle (1962:65)

The point is simply that a language is not some gradually and imperceptibly changing object which smoothly floats through time and space, as historical linguistics based on philological material all too easily suggests. Rather, the transmission of language is discontinuous, and a language is recreated by each child on the basis of the speech data it hears.

Kiparsky (1968:175)

...[T]he child constructs a grammar whose rules are maximally general, consistent with the data available to him. Since the child constructs the optimal grammar consistent with these data, his grammar may differ from that of his elders in having fewer rules or more general rules.

K. Hale (1973:403)

...[G]rammars are essentially discontinuous and have to be "created" afresh by each individual child.

Lightfoot (1981:212)

This position is far from universal; in fact, there is a great deal of controversy on this point. One finds, particularly amongst grammaticalization theorists, a tendency to consider language change a force unto itself, divorced from speakers and subject to its own laws.

Thus our view of grammaticalization is much more mechanistic than functional: the relation between grammar and function is indirect and mediated by diachronic processes. The processes that lead to grammaticalization occur in language use for their own sakes.

Bybee et al (1994:298)

Some even view speakers as passive objects in the experience of an agentive force:

[G]rammaticalization [is a] ... psychological process... that... speakers undergo during the course of the history of the language.

Croft (1990:257)

Proponents of this view often use lifecycle metaphors to illustrate the continuity of linguistic change, or speak of well-ordered "pathways" of change. The following passage from Pagliuca (1994) is often quoted to illustrate this way of thinking.⁵

⁵ Cf. e.g. Janda (2001), Newmeyer (2001).

As a lexical construction enters and continues along a grammaticalization pathway, ... it undergoes successive changes ... broadly interpretable as representing a unidirectional movement away from its original specific and concrete reference and to increasingly abstract reference. Moreover, ... the most advanced grammatical forms, in their travel along developmental pathways, may ... undergo ... continuous reduction from originally free, unbound items ... to affixes...

Pagliuca (1994:ix)

It is striking that these sentiments should be so widespread among functionalists, who ordinarily stress the human-centric nature of language; note the explicit non-functional position of Bybee et al (1994) expressed by the quotation above. However, the pathway-metaphor accords well with the bias of many functionally-oriented grammaticalization theorists who seek to emphasise what they see as universal trends in language change. A pathway has a predetermined direction; the idea, then, is that once a particular lexical item sets foot on it, it has no choice but to travel in the preconceived direction, which it will proceed to do, regardless of how speakers of the language feel about it.⁶ In other words, language changes independently of its speakers because it is a deterministic process.

“Pathway” metaphors are insidious because they invite, perhaps sub-consciously, a particular teleological view of change: that once a linguistic entity has undergone a particular change, it has no choice but to follow out the rest of some trajectory. In fact, this is simply not true, and it must be emphasised that there is no evidence indicating that there is a final stage of completion that all grammaticalising elements must eventually attain. In Chapter Three, I will discuss the Classical Armenian definite prefix, a rare example of a documented change that appeared briefly and then vanished. There may be little data available about such cases, but this is almost certainly due to their ephemeral nature rather than some teleological “pathway” of change.

In his discussion of the discontinuity controversy, Janda (2001:269) points out that one source of the confusion is the tendency to view historical changes from a macro-perspective, from its initial outset to its “culmination”. In the case of a noun becoming a case marker, for instance, the entire trajectory is viewed as a single change, even though it may take a millennia for the noun to become a case suffix and even though the noun is quite likely to have spent centuries, if not longer, as an adposition. This is viewed as one large change. Bynon (1977:6), for example, assumes that only large-scale changes are worthy of study, on the grounds that changes on the smaller scale are too similar to each other to be revealing. Klein (1999:92) even expresses the view that the study of long-term changes is preferable because there is too much “intermediate ‘noise’” in smaller-scale changes; he argues that focussing on long-term change ensures that ‘we can in fact make macrodiachronic sense ... of the often overdetailed historical record.’ Haspelmath also agrees that details can get into the way:

In the writings of some of these linguists, one senses a frustration with theoreticians who make broad sweeping claims but do not back them up with solid and careful historical linguistic work. Clearly, once one starts asking larger questions, there is the danger that one pays less attention to the data and more attention to the ideas, but there is also the opposite danger of missing the generalizations and the big picture because one sees too many details..

Haspelmath (2004:15)

⁶ Janda (2001:287) relates this position with colourful contempt: ‘[M]uch work on grammaticalization... relies heavily on a particular “path(way)” metaphor in which the walkway at issue leads gently but firmly downhill (as if gravity as well as narrowly-spaced locking turnstiles prevented any retrograde movement) and is plastered with signs forbidding any wandering off the path to pick flowers or picnic on the grass.’

It is certainly true that simplicity is an advantage when one is devising a new conceptual framework; the complexities are built in later, when tested on the data. But this should not mean that data are flat out ignored as too messy. Despite the general functionalist habit of sneering at generative work for ignoring data, many of them are just as guilty, and some of the consequences are not small. When actual data are taken into consideration, it becomes clear very quickly that the large-scale “pathway” types of change, the favoured object of study for many scholars like Haspelmath, do *not* follow a well-behaved cline. In fact, it is very difficult to find *any* examples of “textbook grammaticalization” that behave exactly in the predicted fashion.

A case in point is the Romance adverbial suffix *-mente*, which developed from the ablative case form of Latin *mens* ‘mind’. Although *-mente* has been one of the field’s favourite examples for quite some time, even it does not display the kind of pathway expected when its case is examined at a sufficient level of detail; rather, as demonstrated in Bauer (2003:447), the development of the Romance *-mente* suffix ‘does not always follow a straight line’. The problems include a lack of detailed textual documentation for the crucial stages of development; the continued issue of the morphological status of *-mente* (phonologically it resembles a compound element rather than a suffix in some languages); the fact that the suffix developed at different rates in different languages, and not at all in Romanian (a problem, considering that many scholars consider it pan-Romance and therefore postulate an early, uniform development); and, finally, the fact that there *is* clear textual evidence that the Spanish *-mente* must be a borrowing from another dialect, probably Aragonese or Catalan, because the actual inherited Spanish form was *-mient(r)e*. Despite these difficulties, *-mente* continues to be regarded as a “well-behaved” example. Norde (2009:41) begins her excellent summary of the problems by observing,

Some stock examples of grammaticalization are so commonly quoted in grammaticalization studies that they appear to have become part of the linguistic collected memory. More often than not, they are quoted without references, since everybody has at least a vague notion of their development. Thus they start to lead a life of their own, figuring prominently in grammaticalization studies, and few people seem to be bothered about the details of development, let alone the question of whether these really are such straightforward examples as tradition has it... what may seem a clear-cut case of grammaticalization may in fact be far more complex once the historical details are considered, which has serious implications for too sweeping universalistic claims.

Norde (2009:41)

The upshot is that when the actual details of supposed cases of grammaticalization are reviewed in detail, the conception of these changes of occurring at a leisurely, smooth, hardly perceptible rate is severely challenged. While it is possible to observe the changes in a specific item from lexical status to affixal status over the span of several centuries, it is not the case that the item in question will necessarily be gradually changing over the entire period. It is far more likely that change will occur in fits and spurts, with language learners successfully replicating the grammar of their elders for centuries until a change occurs in one generation.⁷ Many researchers⁸ have recently argued that linguistic change is better viewed as a “punctuated equilibrium”, as proposed for biological evolution by Eldredge and Gould (1972).⁹

⁷ That is, with respect to deeper structural changes. Changes in e.g. the precise phonetic property of vowels in a particular speech community have been observed to change gradually from generation to generation; but these are surface-level changes, not structural changes.

⁸ Cf. e.g. Lightfoot (1991:173, 1998), Dixon (1997), Lass (1997).

⁹ In other words, evolution of language bears some resemblance to evolution of species, e.g. bear-sharks.

This is, in fact, exactly what we should expect, given that as historical linguists, we are supposed to be guided by the Uniformitarian Principle. The entire tenet of historical linguistics is that we must assume that the same types of changes we observe in the present also underlie the past; without this, historical work becomes impossible. This is also the entire point of sociolinguistic research such as that of Labov and his students: by studying ongoing changes in progress, inferences about historical changes may be drawn. Yet grammaticalization theorists typically pay very little attention to sociolinguistic research (again, a curious oversight, for a sub-discipline which prides itself on its focus on “speakers” and “usage”, in contrast to abstract generative approaches), on the grounds that the changes they are interested in happen so slowly as to be immeasurable.

The notion that we should *only* study changes over several centuries therefore runs counter to the crucial assumptions of both historical linguistics and sociolinguistics, since it implies that what happens in a single generation is, in a sense, irrelevant: it is only the long-term changes that “matter”. It requires that one entirely ignore the role of the grammar of speakers themselves.

An additional problem with only examining long-term changes is that we are required to assume that speakers somehow have access to etymological stages of their language. They must know where along the pathway each of the items they encounter are located at the time when they come to learn them. Some authors seem to suppose this knowledge on the part of speakers; consider, for instance, the definition of “auxiliary” in Heine (1993:131):

- (2.3) *Auxiliaries*: linguistic items located along the grammaticalization chain extending from full verb to grammatical inflection of tense, aspect, and modality

But children learning a language are dealing with notoriously sparse data. They do not have access even to the grammatical knowledge of their elders; they have only what their elders produce in front of them to guide them towards learning their language. Put another way, their eventual linguistic competence depends entirely on the linguistic performance of the speech community in which they find themselves. Children can learn only what they have evidence for, and the evidence they have comes in the form of linearised speech. They have no way of knowing where components of their grammar fall on any hypothetical diachronic trajectory. In fact, language learners are not bound by etymology in any way. They are not bound to continue any diachronic “trends” or further any movement down a grammaticalization cline, and they are also under no impetus to analyse a section of the data in the same way their parents did.

All of this brings us to another important point: the relationship between synchronic and diachronic linguistics. The position I will take here is that the two are inseparable; however, various researchers have made similar statements motivated by assumptions and hypotheses which are essentially the reverse of mine, and therefore before I explain what I mean by this statement, I will first clarify what I do *not* mean.

Some scholars who hold the position of continuity in language change take a further step in reasoning to posit a so-called “panchronic” grammar encompassing both synchronic and diachronic grammar. This step is taken, for instance, by Heine et al. (1991:258), who argue that ‘it is both unjustified and impractical to maintain a distinction between synchrony and diachrony’ because ‘[grammaticalization] phenomena exhibit simultaneously a synchronic-psychological and a diachronic relation’. One also encounters declarations such as Hopper (1987:148): ‘Because grammar is always emergent but never present, it could be said that it never exists as such... There is, in other words, no “grammar” but only “grammaticalization” – movements towards structure’, or a report made by Lass (1987:155) upon a remark of Traugott’s that ‘synchronic linguistic structure, far from being the central

concern of linguistic theory, ought rather to be treated as “a kind of way-station along the path of history.”¹⁰

Again, sentiments like the preceding seem to presuppose that language change is a sort of primal, glacial force, moving gradually but inexorably forward with little participation from its speakers. On this assumption, speakers must have access to the etymology of particular constructions, at least enough to know where on a particular cline of development each lexical or grammatical item falls. In this way, synchrony and diachrony blur together, and therefore a panchronic grammar is necessary, because speakers must know both where their language has been and where it is going.

This is *not* the view taken here. There is a close relationship between synchrony and diachrony, but it is of a very different nature.

Children arrive at the grammar of their native language on the basis of the speech they hear around them. They may or may not arrive at precisely the same grammar as their target, but their grammar will be close enough that only relatively close scrutiny is likely to reveal any differences. It may be the case, then, that a particular generation *P* has a grammar *Q*, while generation *P+I* has a grammar *Q'*, which is different in some respect. *Q* and *Q'* may be contemporaneous, but they still reflect two different synchronic stages of the same language. In order to determine what has happened diachronically, it is necessary to first analyse both *Q* and *Q'* synchronically, and then to compare the results. There is no need to posit additional diachronic rules or mechanisms for mapping between *Q* and *Q'*, and in fact this is quite the wrong way to think about the matter.

There *is* no “diachronic grammar”, or at least, not in the sense usually intended. It is more accurate to say that there is a succession of synchronic grammars¹¹, which will be identical in most respects, but not necessarily in all respects; the rules of generation *P*'s grammar may be expanded, deleted, or otherwise modified by generation *P+I*. Thus, a language does not change incrementally or imperceptibly. Rather, languages change in the transmission between one generation and the next; the changes involved may be very small, as we will see in the following chapters, but they are, in their own way, both sharp and discrete. Kroch (1989c:200) explains, ‘When a language changes, it simply acquires a different grammar. The change from one grammar to another is necessarily instantaneous and its causes are necessarily external.’

When I say that I am assuming that synchrony and diachrony are inseparable, then, I mean several things. First, diachrony is the link between grammars of different generations of speakers, not a type of grammar itself. In fact, it is misleading to speak of synchronic grammars and diachronic grammars or synchrony and diachrony as though comparing objects of the same type. A grammar is primarily a synchronic object. The term “diachronic grammar” can only be used in reference to a sequence of well-articulated synchronic grammars.

- (2.4) *Diachrony*: a succession of synchronic grammars from different generations of speakers
- (2.5) *Diachronic grammar*: a chronicle of a succession of articulated (synchronic) grammars

¹⁰ Lass (1997), quoting a comment made by Traugott in ‘a lecture at the Deutsche Gesellschaft für Sprachwissenschaft summer school, Göttingen 1992.’

¹¹ Even “succession” is misleading, since it implies direct lineage and there can be no such thing: each person’s grammar is formulated individually on the basis of input from (usually) multiple sources of data (each of whom has *also* formulated his grammar individually), and therefore there is a great deal of both overlap and discontinuity. I will continue to use the term for the sake of simplicity, but it should be understood that by using the term *succession*, I do not intend to imply any linearity.

The crucial point is that in a diachronic study, the object of analysis is not gradually evolving principles, but rather a series of discontinuous snapshots. When analysing diachronic data, then, the point is to explicate how the grammar of generation *P* might be reanalysed by speakers of generation *P+1*. Since there are no independent diachronic principles, this is done by using the same set of grammatical rules, principles, or mechanisms available for synchronic analysis. Diachronic changes are thus constrained in much the same way synchronic grammars are. Nothing illegitimate in a synchronic grammar should be permitted in the diachronic domain. Furthermore, in order to understand a diachronic development, it is necessary to first understand the “before” and “after” synchronic stages of the grammar; otherwise, there is no way to identify the nature of the change in any other than purely descriptive terms.

This is not to say that there are no diachronic *tendencies*, for there certainly are, and the bulk of this dissertation will be concerned with illuminating some of them, on both superficial and structural levels. But these tendencies are only tendencies constrained by principles of synchronic grammar; they are not deterministic and do not have the status of rules.

All of what I have said thus far effectively “reduces” diachronic linguistics to a secondary role; the emphasis is placed on the synchronic, and rightfully so, since language is always synchronic to its speakers. However, historical data can be relevant for synchronic analysis as well. Synchronic stages of a language are rarely devoid of traces of historical developments. Speakers learn what they have evidence for, and sometimes the vagaries of history bequeath them something of a mess: massive allomorphy, for instance, as in the case of the early Polynesian passive suffix or the complex Old and Middle Irish verbal system.

Lass (1990) has discussed various examples of speakers finding new uses for what he terms “junk” morphology: morphology which had a function in earlier stages of the grammar but has since become synchronically functionless. He gives an example from Afrikaans. Modern standard Dutch makes a distinction between predicative and attributive adjectives: attributive adjectives have the suffix *-e* unless they modify a neuter singular noun not preceded by a determiner; predicative adjectives lack *-e*. The gender system has been lost in Afrikaans, with the result that the conditioning of *-e* in attributive adjectives has become morphophonological: derived adjectives, compound adjectives, and adjectives with stem alternants appear with the *-e* suffix; adjectives which fall into none of the previous categories do not.

In this way, speakers are often “stuck” with a certain amount of diachronic junk, and they generally prefer to preserve the system in any way they can understand it. The task of the language learner, first and foremost, is to make sense out of the jumbled data in front of him. If a particular morpheme appears in the speaker’s input, the learner will try to find a purpose for it. Thus, learners are quite willing to carry around their elders’ extra baggage.¹² This tendency may have consequences for synchronic analysis, just as it does for language learners.

Historical evidence can also provide evidence for a better understanding of particular synchronic data. Kroch (1989c) argues,

...from an understanding of the process by which [languages] change, [we may hope] to learn more about their principles of organization. After all, perturbing a complex system and observing its subsequent evolution is often an excellent way of inferring internal structure. In addition... knowledge of the historical process by which a

¹² Labov (2007) and Preston (2008) argue that much of the eventual smoothing out of synchronic peculiarities may be due to second-language learning and contact, rather than native acquisition; children are content to learn irregularities while adults tend to simplify and streamline the grammar. With respect to morphology and syntax in particular, Tony Kroch (p.c.) points to the fact that the Irish language, which was more or less isolated and had few second-language speakers, was able to tolerate massive and opaque allomorphy, as well as a great deal of redundancy, for many generations.

language has reached a particular state may be important to the proper assignment of responsibility to historical and general factors.

Kroch (1989c:199–200)

Different stages of language may be subject to different synchronic rules, but they are all constrained by the same underlying linguistic principles, and they change in ways which reflect these principles. Therefore, by observing the kinds of change that occur cross-linguistically, it is possible to draw conclusions about the kinds of analyses available to speakers. Similarly, the comparison of a grammar Q' with an earlier grammar Q reveals something about the nature of the evidence available to the speakers of Q', and thus also about the inferences speakers of Q' may have drawn in constructing their grammar. When multiple analyses are available for a particular phenomenon within a specific language, a comparison of an earlier stage of that language may provide evidence that leads the analyst to favour one of the alternatives over others.

The preceding discussion can essentially be boiled down to the following five points:

- (2.6a) Language does not change independently of its speakers.
- (2.6b) Language change is not deterministic.
- (2.6c) Language learners are not bound by etymology.
- (2.6d) Language learners are faced with incomplete data, which they will endeavour to make sense of in whatever way the evidence seems to favour.
- (2.6e) Linguistic diachrony and linguistic synchrony are closely intertwined.

These five points form the basis of the analyses and discussion in the core chapters of this dissertation. Although many of the case studies I will examine do involve multiple stages, I will be considering each of these stages individually, so that rather than viewing the changes as a single long trajectory, I will be looking at a series of small changes, each of which is assumed to be a reanalysis occurring in a specific generation of speakers rather than the result of long-term drift. The focus will be on the respective grammars of generations P and P+1, as well as the nature of the grammatical differences between them.

As I consider grammaticalization no more than an epiphenomenon, I am concerned primarily with underlying structural changes, rather than surface-level phenomena. Rather than attempt to handle every kind of phenomenon ever labelled “grammaticalization”, the investigation here will be limited to a single sub-category, namely, changes involving shifts in morpheme boundaries – the “independent word” > “affix” portion of the “cline”, so to speak. As I will show in Chapter Three, even “independent word” > “affix” is really only a descriptive label for several kinds of structural changes.

One consequence of viewing affixation as a structural change is that the concept of “paradigm” becomes irrelevant. No new paradigmatic dimensions are created as a result of affixation, as might be expected, because the same combination of meaning was already present in the language beforehand; the difference was that earlier, it was present as two words rather than one. Morphosyntactic changes are structural and form-based, not changes in semantic space; formal affixation does not give a language a “new tense” so much as a new formal expression of a previously analytic tense category.

This is not to say that there may not be additional semantic changes concurrent with the syntactic changes; however, the two are independent, at least to some extent. Evidence of this independence can be seen in the coexistence of synthetic and analytic forms encoding, in parallel, the same functional distinctions: note for instance the semantic equivalence of the English synthetic and analytic comparative and superlative forms (*X-er* versus *more X*), or of the Latin synthetic and analytic perfect forms (the latter usually but not always a feature of the passive).

Two additional theoretical assumptions about historical linguistics, which have not been featured in the discussion thus far, should probably be mentioned here. The first

concerns the phonological primacy in language acquisition. As mentioned above, children learning their native language are dealing with notoriously sparse data; all they have to go on is the speech produced around them. This means that their access to the data is limited to linearised strings of phonological material, which they must then make sense of and analyse into a hierarchical structure. Because of this limitation, learners by necessity must take all phonological evidence very seriously. They are likely, therefore, to be greatly influenced by vowel harmony, vowel reduction, and prosodic cues such as stress when they are making decisions about the placement of various linguistic boundaries or the status of terminals.

I also assume here the competing-grammar model of Kroch (e.g. 1994, 2001). Both sociolinguistic studies of change in progress (Labov 1994, 2001, 2010) and corpus-based diachronic syntactic studies conducted by Kroch and others have provided ample evidence for variation in the context of linguistic change. In the case of syntactic change, the variants are often grammatically incompatible. This variation could be taken as indicating a heterogeneous speech community in which some speakers have acquired the innovative variant while others retain the older one; the dynamical systems model of Niyogi and Berwick (1997) assumes this situation. However, empirical studies have shown that the variation is actually *intraspeaker*, with speakers themselves producing both variants; Kroch (2001:30) notes that ‘texts from the same time period generally seem more similar than different in their frequencies of the competing variants’. The frequency with which speakers favour one grammar over the other is not itself a feature of the grammar; rather, speakers have simultaneous access to both grammars, and the relative percentage of their usage falls under the domain of performance.¹³

There is no reason to assume *a priori* that morphosyntactic variation and change should differ from syntactic change in this respect, and therefore I will assume here that the competing-grammar model is equally applicable to the kinds of linguistic changes that fall under the general “grammaticalization” heading. As will become clear in later chapters, there are obvious cases in which the syntax may give evidence in favour of an affixal analysis over a clitic analysis or vice versa: for instance, if an item of questionable analysis is mandatory on all conjunctions, speakers have evidence favouring an affix analysis, whereas if there are circumstances under which the item and its host-or-stem can be separated, then speakers have evidence that the item may be a clitic. These are clearly cases in which one would expect to find variation, and thus evidence of competing grammars.

Before concluding this section, it is worth saying a few words about the widespread belief that grammaticalization-type topics cannot be accounted for within a generativist framework. As mentioned above, some functionalists believe that the very existence of grammaticalization phenomena is sufficient to negate the entire generative enterprise. Here I will discuss specifically the much more constructive treatment of Norde (2009:94–100). Her three primary objections to generative approaches are listed in (2.7).

- (2.7a) Language change cannot be explained ‘on the basis of an abstraction (i.e. innate universal grammar), when there is no theory-independent evidence for how such a grammar may be structured.’
- (2.7b) The generative approach does not account for *why* change is triggered.
- (2.7c) The generative approach cannot explain “(diachronic) gradualness and (synchronic) gradience”.

I confess to not really understanding the point of (2.7a), because functionalist approaches come with their own assumptions about the structure of language, and while these assumptions do have a very different colour from generativist assumptions, in particular in being considerably less formally precise, they are assumptions about the object “language” all

¹³ For further discussion, particularly in connection with the Constant Rate Effect, cf. Kroch (2001:26-31).

the same, and therefore no more intrinsically suited to making claims about linguistic diachrony than any generative approach.

(2.7b) essentially describes the notorious actuation problem, which is indeed a legitimate problem, perhaps the most difficult problem facing historical linguistics – including those working in the grammaticalization tradition. It is not, therefore, a generative-specific issue. However, as Norde herself points out, it is true that generative approaches and functionalist approaches differ in their formulation of the problem, creating something of a translation issue. Functionalist approaches assume that grammatical changes happen in the language of adults, that grammars change when *usage* changes. Generative approaches assume that changes in usage do not necessarily reflect changes in competence, and that the change occurs when a new generation of speakers interprets the output of their parents in a novel way. That is, the difference is that functionalists are trying to account for changes in adult usage, not necessarily changes in underlying grammars, whereas the latter is of more theoretical interest to generativists.

Arguably, this difference in perspective does not necessarily indicate a *bona fide* problem: it is simply the case that two different groups of people are studying two different objects. This is not an inherently problematic situation; it becomes problematic either if one group is simply studying an object that doesn't exist or if the two groups refuse to talk to each other. Admittedly, one or both problems seem to obtain, in that many functionalists profess not to believe in deep syntax and most functionalists and formalists refuse to talk to each other (cf. Newmeyer 1998). But these are problems *of the field's making*, an artifice of ideological views that have become largely politicised. It is my view that functionalists have indeed identified an array of very interesting data deserving of further scrutiny (including both specific cases and statistical generalisations), and that formalists can contribute to a deeper understanding of some of the issues.

The point, therefore, is that while it is true that thus far generative historical linguists have not solved the actuation problem, this is insufficient reason to bar them from diachronic work altogether. Generativists have been concentrating on different kinds of issues. Furthermore, *no one* can claim to have solved the actuation problem, including grammaticalization theorists. Thus, this objection cannot be fairly levelled specifically at generativists.

(2.7c) returns us to the larger topic of this section, namely, the dispute over the nature of the character of linguistic change. As I have already argued in this section that the “pathway” and “macrophenomenon” position (which Norde subscribes to) is untenable, I will add here only the observation that Norde overlooks the difference between changes at the individual level and changes at the level of speech community. She also suggests that the generative movement in general cannot handle variation, but this is simply untrue; cf. Kroch (1994).

Norde (2009:96) also discusses a more general concern, namely that grammaticalization reveals directional tendencies which cannot be easily accounted for within a generative approach; in other words, the fact that e.g. nouns tend to become adpositions and then case markers cannot be explained under a generative approach. I agree that she is correct in one respect: such issues are not part of the grammar *per se*. However, generativists are hardly restricted to examining only issues strictly contained by a grammar. We can, and do, also examine issues general to acquisition or cognition; and in fact, on the idea that language changes when speakers acquire a grammar different from that of their parents, this is precisely where we *should* be looking for an explanation of directional tendencies.

Ultimately, Norde adopts the moderate position that generative grammar is not, in fact, orthogonal to the diachronic discussion, but rather that it is complementary to functionalist work. In this, she differs dramatically from many of her more polemic colleagues. She suggests,

The forte of generative linguistics lies in its potential to formalise changes in the grammar, especially on the level of syntax, whereas usage-based approaches provide an adequate means to capture the rise of variation (or trigger change, in generativist terms)¹⁴.

Norde (2009:100)

Norde is entirely correct that one of the greatest advantages to using generative methodology is the powerful precision of its formalism, although I would argue that the generative movement has more to offer than this alone. This dissertation aims to demonstrate how a specific generative formalism can successfully describe the grammatical differences between generations of speakers, but also how taking deeper syntactic structures seriously can illuminate why speakers come to different conclusions about the data than their forebears and common properties of different surface changes.

Fischer (2008:348) argues that an important difference between functional grammaticalization theorists on the one hand and generativists with what she calls “rule-based” frameworks on the other is that the latter are forced to make artificial distinctions which make diachronic changes harder to capture. She writes,

The disadvantage of a [generative] system, however, is that... next to the rule system, one needs a different type of lexical system to account for all the exceptions and rules. In an analogically based system, there is only one system, grammar and lexicon are one. This is also the position taken by most grammaticalization linguists.

Fischer (2008:348)

Invoking two different systems to capture grammaticalization-type effects *is* a serious disadvantage in many generative frameworks; Fischer is quite correct on that score. However, she is quite wrong that this is a necessary property of generative approaches: rather, it is a property of *many* generative approaches, including (unfortunately) many of those currently most popular. In the following section I briefly describe a generative framework which does not have this problem, and which I will be using in the remainder of this dissertation.

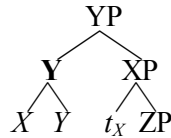
2.3 Distributed Morphology

In the chapters that follow, I will be assuming the non-lexicalist, syntactic framework of Distributed Morphology (henceforth DM), according to which morphology and syntax are part of a single system, with word-building taking place in the syntax rather than in a separate lexicon. Under this approach, there is no generative lexicon; “words” are not syntactically privileged objects (nor even really relevant for the syntax), but rather syntactic objects as complex as phrases. This contrasts with the conception of “wordhood” in lexicalist theories, where “words” are assembled in a separate lexicon prior to their interaction with syntax, and therefore are syntactically privileged, since the syntax considers them atomistic.

While wordhood is not particularly important for the syntax under DM, it *is* very important for the phonology. As Embick and Halle (in progress) point out, phonological words tend to correlate with particular syntactic configurations. Consider the relationship between the nodes X and Y in (2.8).

¹⁴ This latter claim may be debatable, however, as the “usage-based” accounts Norde argues for are not easily reconciled with empirical sociolinguistic findings. I thank David Embick (p.c.) for this observation.

(2.8)



X and Y form a complex head due to movement of X; phrased differently, X has been syntactically affixed to Y. In addition to this close syntactic relationship, X and Y will also have a close phonological connection. This does not mean that the morphology and phonology are inseparable, as they are in globalist/parallelist theories (e.g. most articulations of Optimality Theory); DM takes a localist, serialist approach, whereby the syntactic derivation is computed first, and then fed into the phonology at PF.¹⁵

The morphological word comprises the domain over which word-level phonological processes are calculated, e.g. vowel harmony and stress placement. There may be sub-domains within the morphological word, which are taken to reflect specific cyclic phases in the derivation. Completion of a phase triggers Spell-Out, an operation that sends the structure as currently assembled to PF and LF. In the view of Marantz (2007) and Embick and Marantz (2008), Spell-Out is triggered by category-defining heads (i.e. *a*, *n*, or *v*); other heads (e.g. Tense) are not cyclic and do not trigger Spell-Out. These domains are relevant for allomorphy, as discussed in Embick (2010); they are also relevant for phonological processes, in the sense that members of a complex head which are “outside” a particular phase may share a looser phonological dependency with the “inner” elements which have gone through earlier cycles together. The M-word boundary itself also marks the end of a phase; this is reflected by the fact that certain phonological processes (such as word-final devoicing in languages like German) target the M-word boundary itself.

There are, however, some complications. For instance, although elements in a compound form a complex head, compound elements often do not show the same close phonological effects. Similarly, some classes of affixes may behave differently than others, which could reflect either differences in underlying structure or the effects of diachrony. For instance, as discussed above, the Romance suffix *-mente* behaves phonologically as though it were a compound element rather than an affix in some languages. Clitics create other complications: they are discrete morphological words that do not constitute a separate phonological word.

All of these complications are highly relevant to the issues dealt with in this dissertation, because they represent a deviation from the usual correspondence of phonological words with morphological words. If learners tend to use phonology to guide themselves in assigning M-word boundaries correctly, deviations of this type create a potential locus for linguistic change, since the learner may re-interpret the data.

Returning now to the schematisation in (2.8), it is specifically this structural relationship between X and Y that will be referred to via the term *affixation* in this dissertation.

(2.9) *Affixation*: movement-derived relationship of syntactic terminals which together comprise both a complex syntactic head and a phonological word

A final point of terminology relating to (2.8) refers to the typing of the terminal nodes, which is relevant both in general terms and more specifically for this proposal. There are two types: the Y node in bold is an M-word, while the italicised X and Y nodes are Sub-words. These terms are defined as follows, based on Embick and Noyer (2001):

¹⁵ For discussion on the differences between the globalist/parallelist approach and the localist/serialist approach, cf. Embick (2010).

(2.10) *M-word*: (potentially complex) head not dominated by further head-projection

(2.11) *Sub-word*: terminal node within an M-word (i.e. a Root or feature bundle)

This structural sense of *affixation* is to be distinguished from post-syntactic cases of “affixation under adjacency”, or Local Dislocation (henceforth LD), although this, too, will be very important in the chapters that follow. For a discussion of the mechanics of LD, cf. Chapter 3.8.

(2.12) *Local Dislocation*: structure-blind affixation of linearly adjacent elements at PF

Since it is assumed that morphology and syntax manipulate discrete pieces within a single generative system, it is fairly easy to apply the same mechanisms and terminology to diachronic processes, by treating generations of speakers as analogous to stages of derivation. In synchronic affixation, there is a stage of derivation prior to affixation, and a stage of derivation subsequent to it. The diachronic equivalent is a generation P which does not have a particular process of affixation Q and a generation $P+1$ which does have Q . Therefore, the term *affixation* is also used with a diachronic sense in this dissertation: diachronic affixation is a development whereby a complex head is created containing two syntactic terminals which did not formerly bear this relationship to each other. This results in a former M-word becoming a Sub-word.

(2.13) *Affixation, diachronic*: the state of affairs whereby a terminal analysed as an M-word in generation P is analysed as a Sub-word by generation $P+1$

The opposite of affixation is *de-affixation*, which refers to the severing of a complex head relationship. In this case, generation P has a movement rule Q , but generation $P+1$ does not have Q . Strictly speaking, the term *de-affixation* is not used as precisely as *affixation*, since Q may be either a syntactic operation or a post-syntactic operation; the term refers more to the result – a novel M-word – than the process.

(2.14) *De-affixation*: the state of affairs whereby a Sub-word Q in generation P becomes an M-word in generation $P+1$

The astute reader will have noticed a significant terminological difficulty here, owing to the many different senses of the term *affixation*. In order to avoid confusion as much as possible, I will typically refer to an instance of diachronic affixation, in the above sense, as *affix-genesis*. A desire for terminological parallelism compels me to refer to de-affixation as *affix-exodus*.

Crucially, in both kinds of diachronic event, the identity of the pieces manipulated by the syntax does not change. Rather, what changes is the strength of the phonological dependency between the pieces, the nature of their morphosyntactic relationship, the degree of syntactic independence enjoyed by the pieces, and the speaker’s intuitions about wordhood. Chapters Three and Five will demonstrate that this can be modelled easily in a framework with a single generative system.

This approach accords well with the intuitions underlying some of the more traditional approaches to morphosyntactic change. Even among staunch functionalists, it is not controversial to propose a connection between the morphology and syntax; consider the famous slogan of Givón (1971): ‘Today’s morphology is yesterday’s syntax.’ In DM terms, today’s morphology is *today’s* syntax. Therefore, applying DM to morphosyntactic change

allows for the exploration of structural parameters together with the preservation of many of the best insights previously made by grammaticalization theorists.

While a syntactic piece-based framework offers considerable explanatory power for morphosyntactic change, it also imposes constraints on the kinds of changes which can occur. The prediction is that changes may occur only if they can be expressed by pre-existing syntactic mechanisms. For instance, two morphosyntactic pieces may come to be linearly adjacent via ordinary syntactic movement, and then become fused into a single piece via LD. Under the assumptions adopted here, it is predicted that many grammatical changes can be described in similar terms; for instance, generation P might have a grammar whereby two pieces are linearly adjacent owing to head movement, while the innovating generation P+1 has a grammar which adds an additional operation of LD to the end of the derivation. Both generations are working with the same inventory of pieces and with very similar syntactic processes, but differ in that the innovating generation has an additional step which results in a closer syntactic relationship between the pieces. This explains the facility with which diachronic affixation occurs cross-linguistically. However, processes of diachronic affixation that do not obey the synchronic restrictions on e.g. LD are not allowed.

DM also makes specific predictions regarding the locus of new affixes: a new affix will appear on the periphery of the word it has become attached to, exactly where it was located in its pre-affixal days, because the syntactic change that makes it an affix rather than a clitic is minimal. Discussion of the cases in which affixes are *not* present on the periphery, including cases of mesocclisis, will be dealt with in Chapter Four.

I have chosen to use DM not because it is my synchronic framework of choice,¹⁶ but rather because it is the only theory of syntax currently active which possesses the necessary properties to handle diachronic phenomena in a clear, intuitive manner. It may later prove to be the case that DM as currently articulated cannot quite account for a particular diachronic phenomenon, but a theory like DM in its essential properties is exactly what is needed. The crucial assumption is the unification of morphology and syntax within a single generative system. When word structure and syntactic structure are connected in this way, no theoretical apparatus is necessary for diachronic analysis beyond that which is already required for synchronic analysis. Therefore, the means by which syntactic terminals may experience a change in their status or structural position between grammars of different generations can be modelled simply and clearly by a succession of often minuscule derivational changes.

2.4 The Myth(s) of Grammaticalization

As alluded to in section 2.2, this dissertation aims to counter two related problematic tendencies in traditional work on grammaticalization: the near-total prevalence of surface-oriented accounts of morphosyntactic change¹⁷ and the tendency to elevate descriptive tendencies to the status of explanatory theories. When combined, these tendencies render the majority of the literature completely uninformative as to the structural properties involved in such change. Even worse, many researchers (cf. Heine 1994) have been misled into thinking that labelling a particular historical change “grammaticalization” constitutes the solution of a problem rather than its identification. In this section, I will briefly discuss some of the tenets of grammaticalization studies that I find particularly problematic.

As mentioned in the previous chapter, there is a sharp divide in the grammaticalization literature between those who regard it as a driving force, subject to its own laws, and those who regard it as an epiphenomenon of deeper levels of change. The crux

¹⁶ DM *is* my synchronic framework of choice, but the causality runs in the other direction: I became interested in DM more generally after realising its potential for analysing diachronic phenomena.

¹⁷ For some notable exceptions, cf. Kroch (1994), Roberts and Roussou (1999, 2003), Kiparsky (2011), and Faarlund (2009).

of the disagreement involves the status of what are generally considered the four hallmarks of grammaticalization: (1) phonetic reduction, (2) semantic bleaching, (3) structural reanalysis, and (4) increased “grammaticality”. Grammaticalization “theorists” view these as diagnostic criteria with theoretical significance, while sceptics, most eloquently Newmeyer (1998, 2002), have argued that all of the usual criteria are epiphenomenal, and that there is no distinct process of grammaticalization.

It is not difficult to find examples to demonstrate the independence of the first three factors mentioned above. Phonetic reduction is a known feature of fast, unstressed speech, and it can lead to what would otherwise be labelled “exceptional” sound changes, but it need not necessarily involve any structural reanalysis, and need not co-occur with semantic changes, although it may herald incipient changes on deeper structural levels; cf. e.g. the reduction of unstressed vowels to schwa. Semantic bleaching befalls many lexical items with no accompanying structural reanalysis or phonetic reduction; consider for example the American English use of *rock*, formerly restricted to large masses of stone and now generalised to refer to stones in general without any changes in either pronunciation or lexical status. Finally, structural reanalyses need not involve any other changes; no obvious semantic or phonological erosion was involved in e.g. the loss of V2 in English (cf. e.g. Kroch 1989a) or other typical changes in word order.

The independence of these three features is not unknown to grammaticalization theorists, who consider the fourth feature, increased “grammaticality”, to be the crucial element, so that the strict definition of grammaticalization is defined as the process whereby one linguistic item adopts “more grammatical” functions. Norde (2009:31) argues that the fact that the other features occur independently is irrelevant, and that the relevant factor is that *in grammaticalization*, they do *not* appear independently of each other; similarly, Haspelmath (2004:26), calls grammaticalization the macrophenomenon uniting the microphenomena of phonological reduction, semantic bleaching, and reanalysis; and Dahl (1996) argues:

This to me seems like saying that since love and sex can occur without each other, they are totally different phenomena. For [this argument to go through], he would have to show not only that the processes can occur independently but also that they are unrelated even in the well-documented cases when they show up together. What some of us have claimed is that the things that happen in grammaticalization do so in an orderly fashion which not only predicts what changes can occur but also puts constraints on what synchronic grammatical systems are found.

Dahl (1996)

This is a strange argument. The logic appears to run as follows:

- (2.15a) We observe four phenomena A, B, C, and D.
- (2.15b) A, B, and C occur independently.
- (2.15c) D does not occur independently of (some subset of) A, B, and C.
- (2.15d) The appearance of D is clearly related to the factors A, B, and C that it occurs with.
- (2.15e) The expression of D depends in some way upon the factors A, B, and C that it occurs with.
- (2.15f) Therefore, D constitutes a separate and significant process in which A, B, and C are involved.

As Janda (2005:60–1) points out, most macrophenomena¹⁸ are not simply the sum of their parts, but have properties unique to themselves as a whole; the argument sketched above

¹⁸ His examples include “things like heat, liquidity, solidity, photosynthesis, and consciousness”.

does not describe such a scenario. It would seem to me, therefore, that the logical conclusion of the argument above is not (2.15f) but something to the effect of (2.15f').

(2.15f') Therefore, D is a *product* of (some subset of) the features A, B, and C, not an independent process.

Most work in grammaticalization studies, however, does adhere to the logic of (2.15f), and therefore assumes that grammaticalization constitutes an independent linguistic process. The result is that rather than exploring *how* the interaction of the three independent processes results in what is labelled grammaticalization, researchers take the independent processes as symptoms that grammaticalization has occurred, and look no further for an explanation. This is surely the wrong approach, because the very interaction it ignores is the most crucial part of the entire change.

Skewed logic is only the beginning of the problem, however. If one follows the above logic, grammaticalization has occurred when some linguistic entity becomes “more grammatical”. It should be noted that the range of phenomena which qualify as “grammaticalization” under this definition is extremely broad, in that potentially all of the following could be included:

- (2.16a) Clitics becoming affixes;
- (2.16b) Independent words reducing to clitics;
- (2.16c) Verbs acquiring modal functions;
- (2.16d) Lexical verbs developing into light verbs;
- (2.16e) Inflectional affixes shifting to derivational affixes;
- (2.16f) Nouns being used as adpositions;
- (2.16g) Former lexical items developing uses as complementizers;
- (2.16h) The attachment of semantic or pragmatic functions to prosody¹⁹;
- (2.16i) Fixing of specific word order patterns;
- (2.16j) The emergence of “formal idioms”²⁰ (e.g. English *let's*).

Not all grammaticalization theorists would include each of the above in their definition of “grammaticalization”. Given the diversity of the list, this is not problematic in and of itself; there is what one might call a family resemblance between the items, but clearly they are different enough that the structural changes involved are likely to be very different. The problem, rather, is that there is no real consensus on which of the above ought to be included under the heading “grammaticalization” – and, worse, whatever is considered to fall “outside” the definition is likely to be completely ignored in the literature: if it isn't grammaticalization, then it isn't interesting.²¹

Most scholars, therefore, introduce additional criteria for separating “grammaticalization” from other changes. These additional criteria rarely capture all possible phenomena, however, thus forcing some category out of the definition; meanwhile, one can often find the exact opposite of the suggested criterion suggested as a criterion in the writings of another author. For instance, “scope” is often invoked as a “parameter” of grammaticalization, following Lehmann (1995[1982]:143), who argued that “[t]he structural scope of a sign decreases with increasing grammaticalization”; for instance, case suffixes take scope over a single bare noun, whereas adpositions take scope over a full DP. However, Traugott (1997a), Tabor and Traugott (1998), and Visconti (2004:177) have all argued that

¹⁹ Catherine Lai, p.c.

²⁰ In the sense of Fillmore, Kay, and O'Connor (1988).

²¹ Fortunately, the overwhelming diversity of opinions as to what constitutes “grammaticalization” almost guarantees that if one researcher excludes X by his definition, *someone* will nevertheless have included it. Nevertheless, this is clearly not an ideal state of affairs.

scope *expansion* is involved in grammaticalization. In particular, there is a general consensus that the change from deontic modals to epistemic modals is both scope-expansion and grammaticalization.

The upshot is that no two people seem to share exactly the same definition of grammaticalization, even though there is widespread agreement (in certain circles) that it exists as a significant phenomenon. For a reasonable survey of this diversity of opinion, cf. Norde (2009, ch. 1–3).

Despite the lack of agreement on the precise definition of “grammaticalization”, there is general agreement that the entire category, however it is defined, is evidence for the cline shown in (2.17) (from Hopper and Traugott 1993:7):

(2.17) content item > grammatical word > clitic > inflectional affix

Many grammaticalization theorists claim (2.17) as a universal rule of language change (cf. e.g. Haspelmath 1999). The resulting “unidirectionality” hypothesis can be summarised as follows:

(2.18) *Unidirectionality Hypothesis*: elements may become “more grammatical” but never “more lexical”.

The strong unidirectionality hypothesis has been clearly shown to be false, in the face of numerous counterexamples involving affixes becoming lexical items, or clitics becoming full words, or affixes becoming clitics or pronouns. A good example is English *-ish*, which has an impeccable Proto-Indo-European pedigree as an adjectival suffix, but has recently begun to spread as an adverb. This usage is established enough to make it into the *Oxford English Dictionary*, which has several examples; two of the clearest are given below.²²

(2.19a) ‘Trust Davie Morrow.’ ‘You know him?’ ‘Ish. He’s a regular across the road.’

(2.19b) Mr Langmead, speaking by telephone from London, hesitated. ‘Ish,’ he said, employing the international shorthand for slight hedge.

The OED’s earliest attestation of adverbial *-ish* is 1986, and the phenomenon is recent enough that there has been very little mention of it in the literature; for exceptions, cf. Morris (1998)²³, Norde (2009:223–5), and Kuzmack (in prep).

It is trivially true that one never encounters e.g. case suffixes detaching themselves and becoming the polysyllabic postpositions they had been etymologically, complete with richer semantics and phonology; but this is really a straw man, since no one has ever seriously suggested the possibility (cf. Dahl 1996, Norde 2009:111–112). Nevertheless, grammatical items undeniably can and do become more lexical than they were before, and this requires an explanation regardless of the terminology employed. Some of these counterexamples, and the issues surrounding them, will be discussed in Chapter Five.

Researchers who wish to maintain (2.18) anyhow customarily do so either by holding to a weakened version (making (2.18) a tendency rather than a law), or by saying that the counterexamples are not true counterexamples because they are examples of something else entirely. The latter approach effectively builds unidirectionality into the definition of grammaticalization, thereby creating a tautology; Janda (2005:59) wryly comments that this

²² (2.19a) from C. Bateman, *Cycle of Violence*, 1995, vi. 94; (2.19b) from the *New York Times*, 5 September 2002, D8/5.

²³ In her discussion of the semantics of the adjectival suffix, Morris (1998) gives a few examples of some intermediate forms where *-ish* acts like a clitic, e.g. *a man-in-the-street-ish sort of opinion*, but she downplays their importance and makes no mention of the adverbial usage.

reformulation is “as unrevealing as saying that walking due north is necessarily unidirectional”. The former approach is taken by e.g. Haspelmath (2004) and Hopper and Traugott (1993), who argue that the generalisations expressed by the unidirectionality hypothesis are so robust as to render the sporadic counterexamples unimportant. Three particularly radical expressions of this position are quoted here:

Although ... [examples of] degrammaticalization ... have been observed to occur, they are statistically insignificant and will be ignored in the remainder of this work. ... [M]any cases of alleged de-grammaticalization found in the literature on this subject can be shown to be the result of an inadequate analysis.

Heine et al (1991:4–5)

[As regards, e.g.,] case affixes becoming adpositions ... [or]... agreement affixes turning into independent pronouns, ... It is an undeniable empirical fact that such changes do not occur...

Haspelmath (1998:319)

There are at most a few cases of affixes turning into phrasal clitics... but ... I see no reason to regard these isolated cases as threats to the robust empirical generalization that grammaticalization is overwhelmingly unidirectional.

Haspelmath (1998:347n2)

Hopper and Traugott themselves prefer to express a more moderate position.

Some counterexamples do exist. Their existence, and their relative infrequency, in fact help define our notion of what prototypical grammaticalization is ... [They] should caution us against making uncritical inferences about directions of grammaticalization where historical data is not available. Usually such inferences are justified, however, and the rare counterexamples should not be allowed to deprive us of a useful descriptive method and an important source of data.

Hopper and Traugott (1993:126–8)

That even Hopper and Traugott are slightly less sanguine about purported counterexamples than one might conclude from their quote is revealed by the fact that they leap at the chance to explain any purported counterexamples as irrelevant to the discussion because they are in fact examples of something else, thereby biasing the already skewed asymmetry of the data further in favour of grammaticalization. For instance, the use of the nominal suffixes *-ism* and *-ology* as nouns runs counter to the predicted direction of less grammatical to more grammatical, but Hopper and Traugott do not consider them exceptions because they are examples of “lexicalization” and therefore quite orthogonal to the discussion.

There is a more important point to be made, however, and it concerns the phrase “a useful descriptive method and an important source of data”. Despite their apparent words to the contrary, Hopper and Traugott do not consider grammaticalization a useful descriptive method; rather, they have awarded it predictive explanatory power.

One of the tenets of this way of thinking is the idea that the data is strongly biased in one direction, that of grammaticalization. Although well-established in the literature, the apparent bias of the data is not as solid as researchers in the Hopper and Traugott tradition would have one believe. There are several problems with the claims made about the data.

The first issue is the pervasive circularity surrounding the case studies used to prove the asymmetry claim: all too often, researchers use as evidence reconstructions they have made *under the assumption that grammaticalization has occurred*. That is to say, claims of the following type are often made, albeit somewhat less directly:

- (2.20a) We assume the following proposition: lexical material becomes functional material through a process called *grammaticalization*.
- (2.20b) There is a suggestive phonological and semantic relationship between an affix X and a lexical item Y in language Z.
- (2.20c) Since we have assumed (a), we can legitimately hypothesise that X developed out of Y.
- (2.20d) Therefore, we can reconstruct this process as having occurred in the prehistory of language Z.
- (2.20e) Therefore, X is evidence in favour of the proposition that lexical material becomes functional material through a process called *grammaticalization*.

The logic here is clearly flawed. X is *not* evidence in favour of the theorem, because it was derived from the initial assumption of the theorem. This is a reconstructed example, and therefore there is no direct evidence for the development of X from Y; thus, it is specifically the theorem which allows us to reconstruct X as developing from Y. Since the theorem is the sole basis of this conclusion, it is completely illegitimate to then turn around and claim the conclusion as evidence for the theorem. The only direct evidence for the theorem are those examples which have been documented as occurring within the historical record of a language, although comparative evidence can be valid as well.

Many reconstructions have been made on the basis of this assumption, and many of these are entirely reasonable; some are even quite brilliant. This does nothing to change the fact that they are only reconstructions, and therefore only hypotheses. They are totally inadmissible as evidence favouring the preponderance of morphosyntactic changes in the desired direction.

Newmeyer (2001:217) gives as an example of this circular reasoning a reconstruction in Heine (1994). Heine is examining the progressive construction in Ewe, such as (2.21).

- (2.21) Kofi le xɔ tu- m̄.
 PROG house build-PROG
 ‘Kofi is building a house.’

Observing that *le* bears a resemblance to a locative auxiliary verb (*le*) and *-m̄* to a locative adposition (*me*), Heine suggests that the progressive markers have evolved from the locatives as a result of the metaphorical extension of spatial terms to temporal. This is a perfectly reasonable hypothesis; however, he then proceeds to use this reconstruction as evidence in favour of grammaticalization theory, arguing that it explains the sequence “locative > progressive”. As Newmeyer points out, this reasoning is entirely circular and therefore inadmissible: ‘There is no known sense of “explanation” in which the assumption of X to demonstrate Y can legitimately allow one to conclude that X has been confirmed’ (Newmeyer 2001:217). Similar sentiments have been expressed by Janda (2001:371): ‘To put matters bluntly: a reconstruction initially justified by invoking a certain principle (of grammaticalization) cannot later be argued to provide independent confirmation for that same principle.’

Argumentation of this nature is all too common in work of this kind. Heine and Kuteva (2002) offer a purported “world lexicon” of grammaticalization; the book is intended as a useful reference guide with an exhaustive listing of known, attested case studies, with references. However, the authors admit that “[m]ost of the over 400 grammaticalization processes discussed in this book are based on ... reconstruction work”, further admitting that “in some cases the evidence available is not yet satisfactory”. Thus, Heine and Kuteva (2002) is almost utterly useless as a reference guide; it is only a catalogue of reconstructions. Even more egregiously, they do not adhere to the time-honoured tradition of the historical linguist: the explicit notation of unattested or reconstructed forms. Their reconstructions are not marked with asterisks, as is standard; nor do they make any other attempt to differentiate the

reconstructed forms from the attested forms. Therefore, a casual reader who has not read their introduction and is, perhaps, skimming for a specific case study may not even notice that the forms are insecure. Moreover, this disingenuity is not limited specifically to this work alone; it can be found elsewhere in the literature.

Another problem with the sampling is that it is not entirely clear what exactly is being counted. Joseph (2005:4) concludes that the issue of statistical balance between directional and counter-directional examples is “a nonissue in the absence of any meaningful way of counting tokens”. His summary of the problem is succinct:

[I]t is simply not clear what ought to count as a token of change to greater or lesser grammatical status: each acquisition by a given element of some feature indicating greater grammatical status or only a particular accumulation of such features, developments with a single element or with set of related elements, or just what?
(Joseph 2005:4)

No two people seem to have exactly the same definition of “more grammatical”, and therefore each person who performs the count is likely to reach a different total. In particular, researchers with a very strong unidirectionality bias are likely to overlook potential counterexamples, or (as discussed above) label them irrelevant to the discussion. This means that the very tokens potentially most crucial to the question of statistical preponderance might not even be counted at all! Moreover, since the sample size of the data set is completely unknown to begin with (we have no idea how many relevant case studies have simply never come to the attention to researchers), the bias will have a huge impact on the apparent relevant statistics. Lass writes,

Say in the course of your work you have found 542 changes that confirm a direction, and none that don't. Question is, 542 out of *what*? Does a UD-believer's inability to find the counterexamples, and/or the observed frequency of the confirming instances, reflect a “real” property of the domain or merely the accidental tendentiousness of a chosen database? Note that not finding things is an *argumentum ex silentio*, which is not at the top of anybody's hierarchy of epistemic goodness.

(Lass 2000:214)

Another issue that researchers have failed to notice that the data are necessarily biased in favour of examples which have left traces in the modern language; as Allen (1995:2) writes, ‘it is often easier to observe the presence of a phenomenon such as grammaticalization than to record change or loss.’ It is quite true that phonological/semantic resemblances between grammatical markers and lexical forms are often attested, and that reconstructing a historical relationship between the two is sometimes plausible. However, it is important not to overlook the fact that de-grammaticalization is much less likely to leave observable traces.

Parallels to sample biasing involving sound changes may be drawn in this context. It is well known that, while phonemic splits can often be reconstructed on the basis of comparative reconstruction, mergers are much harder to identify, particularly mergers which occurred in all of the surviving branches of the family tree. Attempts to reconstruct Latin based on extant Romance languages, for instance, produce something quite similar to attested Latin, but not identical to it, because certain mergers have occurred in all of the surviving Latin daughters. Thus, while Latin had ten vowel phonemes, Hall (1955) demonstrated that only nine can be reconstructed for Proto-Romance, owing to a merger between long and short [a].²⁴ The Romance languages also have no traces of Latin [h] (Sturtevant 1920:§130).²⁵

²⁴ In fact, the Romance languages themselves give no direct evidence for contrasts in vowel length rather than vowel quality; cf. Hall (1976:178).

The fact that mergers are harder to spot historically has absolutely no bearing on their relative frequency compared to splits. Sociolinguistic research has demonstrated that mergers are in fact not only quite common, but prone to spreading over fairly large dialect communities; Labov (1994:313) speaks of Herzog's Principle:

(2.22) Herzog's Principle: Mergers spread at the expense of distinctions.

In fact, while splits are easier to spot diachronically, mergers are easier to spot synchronically. In his discussion of the synchronic frequencies of splits and mergers, Labov (1994:331) notes that the apparent observed asymmetry says little about asymmetry on a larger diachronic scale:

Most reports of phonemic change involve mergers: the reduction in phonemic inventory. This simple fact would lead to the odd conclusion that most languages are steadily reducing their vowel inventory. Since any overview of language history shows that this is not so, it stands to reason that just as many phonemic splits must take place as mergers. For reasons that are not entirely clear, it is not easy for students of the speech community to locate the ongoing creation of phonemic distinctions.

(Labov 1994:331)

The same may well be true of grammaticalization-type changes. Our understanding of the relative occurrence of grammaticalization and de-grammaticalization is limited to what we observe in progress and to changes within the historical record, and the latter are likely to be biased in favour of grammaticalization simply because these changes are more likely to leave a trail. We have no way of knowing how many instances of de-grammaticalization may have occurred without leaving traces, nor whether a certain fraction of the reconstructed cases could reflect de-grammaticalization rather than grammaticalization.

It must be emphasised here that I am not attempting to argue that de-grammaticalization is as common as, or more common than, grammaticalization. My intention is merely to point out that our understanding of the relative frequencies of the two directions of change is not necessarily as solid as we tend to assume, and that therefore it is important to keep an open mind about the respective roles of grammaticalization and de-grammaticalization. I share the position expressed by Newmeyer (2001:205), who explains, 'My sense is that [cases of de-grammaticalization] are rampant, though I would not hazard a speculation about their statistical import.'

The failure to remain agnostic about the relative frequencies of grammaticalization and de-grammaticalization has created a situation in which rather little attention has been paid to the actual properties of de-grammaticalization; Norde (2009) is a welcome book-length exception. Most of the argumentation surrounding de-grammaticalization concerns nothing more than the status of its existence and the possible implications of this status for grammaticalization. Therefore, although we do have a reasonable understanding of the circumstances under which grammaticalization can occur, our understanding of what is actually happening in de-grammaticalization, when it can occur, &c., is considerably less advanced. This is an unfortunate consequence of regarding the relevant examples as counterexamples rather than interesting objects of study in their own right.

Why is there so much investment in maintaining unidirectionality as a central tenet of the study of morphological change? Janda (2005:55) notes that those who work in this particular sub-field ordinarily have no trouble working with probabilistic predictions, and therefore would be expected to be content if changes were to follow the predicted direction,

²⁵ Some aspects of Latin morphology are also impossible to reconstruct, including the passive voice, future tense, deponent verbs, and some non-finite verbal forms. These constructions were ubiquitous in Latin, but have left no traces in Romance. Cf. Hall (1983:5).

say, two-thirds of the time. It would also be expected that the remaining minority cases should still be of theoretical interest.²⁶ Why should the loss of unidirectionality as a central principle be so threatening?

One factor here is unquestionably the potential veracity of reconstructions. If it were true that grammaticalization-type changes were irreversible and unidirectionality a basic fact, then whenever a linguist perceived a potential relationship between e.g. a modal and a lexical verb, she would likely be correct to postulate that the former developed out of the latter. Once counter-directional change is admitted as a viable possibility, the degree of certainty is diminished, since it would also be possible for the lexical verb to have developed from the modal.²⁷

That this is a real concern can be seen from the following statement of Haspelmath (2004:21-2); emphasis added:

It seems to me that it is undeniable that the unidirectionality in grammaticalization is *by far the most important constraint on morphosyntactic change*, simply because grammaticalization changes are so ubiquitous... [U]nidirectionality in grammaticalization is very important in practical terms for the historical-comparative linguist. Suppose we have two related languages with no historical documentation, and one of them has a future-tense affix that looks similar to a future-tense auxiliary of the other language. If both directions of change were equally likely, we would not know what to reconstruct for the ancestor language. But because grammaticalization is overwhelmingly irreversible, the historical linguist can safely reconstruct the future auxiliary for the protolanguage in this case.

(Haspelmath 2004:21–2)

It is certainly true that admitting a larger number of possibilities does make it more difficult to be certain that one's reconstructions are secure. However, convenience for reconstruction seems an insufficient reason to completely ignore or marginalise counterexamples. As Joseph (2005:5) points out, 'it would be possible to do historical linguistics and study language change without ever doing any reconstruction; that is, reconstruction is a nicety that arises out of the linguist's intellectual curiosity, but it is hardly an essential part of understanding language change per se.'

For some, eliminating unidirectionality as a central premise threatens more than simply reconstructions. As discussed in 2.2, some researchers (e.g. Hopper) have formulated theories of grammar in which unidirectionality plays a key role. For them, grammaticalization reveals something very deep and important about the way grammar is organised; it has even been argued (cf. Heine et al. 1991, Traugott and König 1991) that the very existence of grammaticalization demonstrates the complete invalidity of the generative movement. It has also been suggested (cf. Janda 2005:56) that unidirectionality seemingly provides its adherents with a theory as constrained and precise in its predictions as theories put forth by the generative tradition. Such researchers have a deep ideological commitment to the notion that morphosyntactic change runs in only one direction.

The upshot of this ideology – both the preoccupation with unidirectionality and the “grammaticalization theory” programme in general – is that it perpetrates a serious conceptual error by essentially elevating a perfectly valid descriptive tendency to the status of axiom. Grammaticalization theory assumes that grammaticalization exists as a specific force underlying linguistic changes, and that therefore this force is undermined by every demonstration of change moving in the opposite direction. This position is essentially the reification of an observable phenomenon (or, more accurately, a constellation of observable phenomena) to explanatory status: we see these things happening because this is the way

²⁶ Janda (2005:56): '[W]hat a scientist must seek to predict is the frequencies of both major and minor trends, rather than treating everything as either due to a universal principle or else just an accident.'

²⁷ Such a case is attested in a Canadian dialect of Pennsylvania German; cf. Burridge (1995, 1998).

things happen. A moment's reflection reveals that this line of reasoning is approximately as circular as claiming reconstructions as admissible evidence for validating grammaticalization.

In addition, this way of thinking obscures to an unhelpful extent the more interesting properties of these kinds of phenomena, because it concerns only changes happening on the surface level of the grammar and omits any explanation of what is happening structurally. Furthermore, as alluded to above, any morphosyntactic changes not labelled "grammaticalization" tend to be ignored by theorists, even if they don't constitute "exceptions".

Various other issues and fallacies to do with the grammaticalization theorist position are discussed in eloquent detail by Newmeyer (1998, 2001); see also the scathing critique in Janda (2005). I will not rehearse them further here; rather, this dissertation starts from the assumption that Newmeyer is correct to label grammaticalization an epiphenomenon resulting from deeper structural changes and addresses instead the nature of these deeper structural changes.

Chapter Three

Essential Properties of Affix-Genesis

3.1 Overview

With very few exceptions (cf. Kiparsky 2011), discussions of diachronic affixation in the literature seem at times to take the process almost for granted, as simply the last stage in a deterministic sequence of increasing “grammaticality”. In reading the literature, one sometimes gets the impression that anything can become an affix given enough time. Rather less attention has been given to the finer technical details, particularly those that concern deeper syntactic structures.

The primary aim of this chapter is to look at diachronic affixation from a structural perspective and dissect some of its essential properties, building on the theoretical principles laid out in the previous chapter. No additional mechanisms will be introduced; instead, the focus will be on the syntactic contexts in which innovative affixes appear. The intuition advanced and defended here is that novel affixes derive from the following situation: generation *P* has two independent M-words; generation *P+1* analyses one of these as a Subword within the other M-word. The resulting “change” is discrete and punctual, and occurs at the level of syntax, not the surface – although more superficial considerations may bias learners in that direction. There is no long-term drift and no diachronic operations involved; instead, there is a conservative grammar and an innovative grammar.

This chapter also aims to highlight just how complex and variable developments of this nature can be. Many claims have been made in the literature about the ease and naturalness of some of the phenomena surveyed in this chapter; in particular, when one reads the literature, one gets the sense that the transition from postposition to case suffix is a very simple, almost trivial development. When we examine the actual data in detail, however, the picture becomes far more complicated than the standard literature on grammaticalization would have it. This unexpected complexity underscores the importance of grounding our conclusions about diachronic morphosyntactic phenomena in data whose development is either actually attested in historical documents or has very strong cross-linguistic support. Not all of the changes discussed here as case studies are directly attested in progress, but all of them are supported by comparative evidence and/or textual evidence, and none of them are considerably older than the documents in which they are attested. They were also chosen for their relative simplicity, and yet only one of them is a true example of the straightforward “lexical item > affix” development of the type the grammaticalization tradition treats as ubiquitous. It would be an exaggeration to say that these straightforward developments are myths, but certainly the available data challenge the view that they are the norm.

Case suffixes, tense/aspect affixes, and other affixes may be omnipresent in the world’s languages, and for the most part their behaviour is predictable – but the assumption that they have *always* behaved so predictably, from the beginning of their history as affixes, is unwarranted, as shown by data from affixes of more recent provenance. The most likely explanation for the apparent discrepancy is simply that the standard well-behaved affixes are so old that all traces of their wilder youth have long since been eradicated. Devising a plausible etymology for some affix or other may offer us a (hypothetical) point of departure, and clues into the potential origins of affixes, but such etymologies, even the most brilliant, reveal nothing about *how* a proposed development took place. To truly understand how affixes are generated, knowing more than a potential origin is absolutely crucial.

In fact, the hypothesis that the development from lexical item to affix is simple and straightforward is not merely overly simplistic; it is actually misleading, because it completely ignores important questions about the structures involved. We call this kind of change *morphosyntactic* for a reason; these are not cosmetic changes, but changes that affect

the structure of a sentence, sometimes in very significant ways. When a language develops a new tense suffix from an earlier temporal auxiliary, it has not only gained a new suffix; it has also developed V-to-T movement. The latter is obviously a very important fact about the syntax of this language.

Some concrete examples of reasonably well-documented instances of affix-genesis will establish the relevant patterns clearly, and therefore, before discussing more abstract generalisations, I begin in 3.2 with a series of reasonably well-documented case studies, paying attention to the syntactic and phonological properties of the conservative and innovative grammars. 3.3 deals with issues of learnability and transmission, which leads into a discussion of linguistic preconditions necessary for the development of affixes in 3.4. 3.5 links the discussion of this chapter with that in the previous chapter. In 3.6, I develop the beginnings of a typology of the kinds of innovative analysis that result in affix-genesis. The implications of this chapter's results for SOV languages specifically are briefly discussed in 3.7.

3.2 Case Studies

In this section, I will discuss five case studies involving affixation: the development of a new locative case suffix in a dialect of Oscan (3.2.1); the development of a new dative marker in Persian (3.2.2); the brief appearance of an accusative direct object *prefix* in Classical Armenian (3.2.3); the development of subject agreement prefixes in Piattino, a dialect of Italian (3.2.4); and, finally, the development of a new tense suffix in Amharic (3.2.5).

These cases were chosen because they are all reasonably well-documented, either appearing within the historical record or supported by comparative evidence. As we will see, none of them are precisely the same in their details, and several different kinds of structural development are represented between them.

3.2.1 Oscan²⁸

A very simple illustration of what happens in the process of affixation is found in the emergence of a locative suffix in Oscan, a now-defunct Italic language. The development seems to have been limited to the dialect of Agnone; it did not occur in the dialect of the Tabula Bantina. Owing to the general paucity of the Oscan corpus, there is exactly one relevant example; however, this makes it useful for illustrative purposes before turning to more complex cases.

The development of postpositions into case suffixes is extremely common cross-linguistically. McFadden (2004:78–81) suggests that this is predictable because of the similarity in the function of adpositions and semantic case markers. As he points out, postpositions tend to be destressed and eventually phonologically reduced; as their dependence on the noun they govern increases, the learner begins to analyse them as case markers instead. Following e.g. Marantz (1991), McFadden assumes that case markers are not inserted by the syntax, but rather are dissociated morphemes like agreement markers. In his system, then, semantic case endings are assigned at PF by a null P. The change of postposition to semantic case marker is therefore a change between a syntactic head governing a DP to a dissociated morpheme.

Proto-Indo-European had a number of adverbial particles which gave rise to adpositions in many of its daughter languages.²⁹ The Oscan adposition *en* is descended from

²⁸ I am grateful to Don Ringe (p.c.) for his assistance in determining which examples are secure. The majority of the rest of the discussion comes from Buck (1904:117).

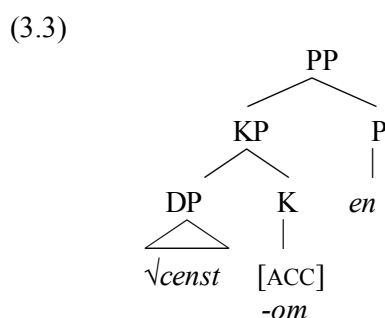
²⁹ For discussion cf. e.g. Sihler (1995:438-9).

one of these particles, **en*, which also gave rise to Latin (and English) *in* and Greek *en*. In Oscan it occurs as both a preposition (3.1a) and an enclitic postposition (3.1b), and it seems to be used more broadly than its Latin counterpart: when used with a locative noun, it means ‘in’, as in Latin; but when used with the accusative it can also mean ‘at’ or ‘to’. The examples in (3.1) come from the Tabula Bantina.

- (3.1a) *en eitu-as*
 in fine-ACC.PL.
 ‘for a fine’
- (3.1b) *censt- om= en*
 census-ACC.SG.=in
 ‘to the census’

In the dialect of the Tabula Bantina, *en* is clearly only an adposition; as seen in (3.2), when the noun it governs has a modifier, the *en* is not repeated.³⁰ The structure of (3.1b) above would be something like that seen in (3.3).

- (3.2) *exais-c= en lig- is*
 this- DAT/ABL.FEM.PL.=in law-DAT/ABL.PL.
 ‘in these laws’



However, in the dialect of Agnone, the clitic appears not only on the noun it governs, but also on an adjectival modifier, which indicates that in this dialect, the locative incarnation of *en* is a case suffix. This example comes from the Dedicatory Tablet of Agnone.³¹

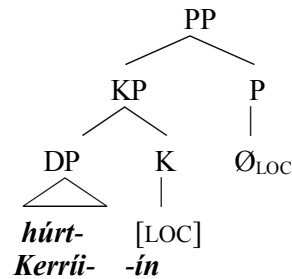
- (3.4) **Stat- ús p- ús set húrtn ín Kerríi- ín:**
 Stat- o:s p- o:s sent hort- e:n Kerre:i- e:n
 erected-NOM.PL. REL-NOM.MASC.PL. be.3rd.pl. grove-LOC Cereal-LOC
 ‘[Those] which have been set up in the grove of Ceres: [list follows]’

The structure of the Agnone locative DP **húrtn Kerríiín** in (3.5) should be contrasted with that of (3.3). In the generations prior to the acquisition of structures like (3.5), speakers of the Agnone dialect would have had the same grammar (3.3) as contemporary speakers from Bantia.

³⁰ Oscan had a separate locative singular, but not a locative plural; there has been syncretism of dative, ablative, and locative in the plural.

³¹ By convention, inscriptions in the native Oscan alphabet are transcribed in boldface. “ú” represents [o] (which did not originally have a character in the native alphabet, as the alphabet was taken from that of the Etruscans, who did not have this phoneme); “i” represents a raised [e:] that had not merged with [i:].

(3.5)



(3.3) and (3.5) differ only in that in the dialect of Bantia, *-en* is a syntactic head governing a DP, while in the dialect of Agnone, the syntactic head is null, and the former preposition has been reanalysed as the exponent of a lower head.

Phonological factors also played a role in the reanalysis, in that the original locative ending and the postpositional clitic had effectively fused by regular phonological processes. The original locative ending in Oscan was *-ei*; when combined with the postposition *-en*, the resulting **-ey-en* became *-ēn* (= “**in**”). Speakers could thus no longer easily segment the locative ending into locative + postposition, making it easier for them to analyse the former postposition as a case suffix.

Despite its simplicity, the Oscan case nevertheless illustrates several key points. First, there is the importance of phonological input in influencing the speaker towards a particular analysis of the data: when phonological change left the locative postposition hard to segment from the locative ending, speakers had additional motivation to analyse the result as a single case suffix. However, the fact that this reanalysis had *not* occurred in the Bantine dialect shows that phonological considerations are influential, but not necessarily deterministic: the data must have been ambiguous between one analysis and the other.

Another important point is the minimal difference between the postposition dialect and the case suffix dialect, as illustrated by (3.3) and (3.5). The only difference between these structures lies in whether *en* has been analysed as postposition or as case suffix – a difference entirely on the structural level. In terms of the linear string, the output of the two dialects is identical, and the reanalysis itself is string-vacuous. The only evidence that it has occurred in Agnone and not in Bantia lies in the innovated adjectival agreement in the former and not in the latter.

3.2.2 Persian

A more complicated and better-documented example of the development of postpositions into case suffixes can be cited from Persian, in which an erstwhile postposition developed into a dative marker *-râ* which, in Modern Persian, is used primarily as a differential object marker. As will become clear, the DP structure of Persian had a direct contribution in motivating the reanalysis.

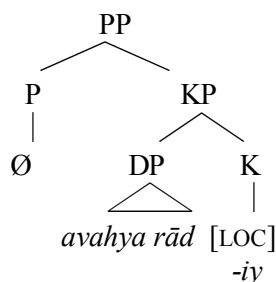
Since Persian has a very long written history, the development of the postposition itself can be traced from an earlier noun, *râdiy*. Since linguistic change is discontinuous (cf. Chapter Two), the development from noun to postposition has no bearing on the subsequent developments affecting the postposition and could therefore be omitted from the discussion; however, for the sake of completion, and because it is rare for a proposed noun > adposition > case marker sequence of reanalyses to be actually attested within recorded history, I have chosen to include the prologue in the discussion anyhow.

Proto-Indo-European is generally believed to have had a system of eight or nine cases: nominative, accusative, vocative, dative, instrumental, ablative, genitive, locative, and possibly allative (attested as a separate case only in Hittite). Most of this system is still preserved in Old Persian, with one clear exception: the dative had become syncretised with

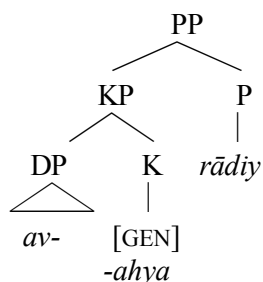
the genitive (Kent 1950:58). The absence of a separate dative and subsequent appearance of a dative postposition in Persian may not be coincidental.

Old Persian was primarily a prepositional language (cf. Kent 1950:87), but it also had two postpositions, one of which was *rādiy*, glossed by Kent as ‘on account of’, which governed an object in the genitive. Formally, *rādiy* still looks like a locative nominal case form³², and in fact it is not completely clear whether it was structurally a noun (3.6a) or a postposition (3.6b) at this stage. It is clear that both (3.6a) and (3.6b) represent grammars of Old Persian at some point or other, however; *rādiy* did begin as a noun and end as a postposition. The ambiguity is only which of these stages is attested in the Old Persian documents.

(3.6a)



(3.6b)



(3.6b), with the reflex of *rādiy*, is the starting point for the later development into a case suffix.

Although *rādiy* most frequently means something like ‘on account of,’ as in (3.7), there are also two tokens (3.8), both occurring in the same context in the same text (the Naqš-i-Rustam B. of Darius), which appear to indicate the agent of a passive verb.³³ The status of the latter two examples in the synchronic grammar of Old Persian is uncertain.

- (3.7) Av- ahya rādiy naiy nipištam, mā- tya hya aparam
 that-GEN.SG. on.account.of NEG inscribed.PTCP lest-COMP REL afterwards
 im- ām dip- im patipars-ā- tiy, av- ahya paruv
 this-ACC writing-ACC read- SUBJ-3rd.sg. that-GEN much
 θaday-ā- taiy, tya ma- nā kartam.
 seem- SUBJ-3rd.sg. REL 1st.sg.-GEN done.PTCP
 ‘For this reason it was not inscribed, lest whoever afterwards read this
 writing, to him it might seem (too) much, what was done by me.’

³² Kent (1950:205) connects *rādiy* to the Sanskrit root *rādh* ‘effect, complete’. He does not give a meaning for the noun in Old Persian.

³³ I am grateful to Don Ringe for his assistance with the Old Persian data.

- (3.8a) Nai- mā kāma tya skauθ- iš tunuvat- ahyā
 NEG-1st.sg.ACC desire.NOM COMP weak.person-NOM strong.person-GEN
 rādiy miθa kar-iy- ai- š.
 by evil.ACC do- PASS-OPT-3rd.sg.
 ‘[It is] not my desire that a weak person should be done evil at the hands of a strong person.’
- (3.8b) Nai- mā ava kāma tya tunuvā
 NEG-1st.sg.ACC that.NOM desire.NOM COMP strong.person.NOM
 skauθ- aiš rādiy miθa kar-iy- ai- š.
 weak.person-GEN by evil.ACC do- PASS-OPT-3rd.sg.
 ‘That is not my desire, that a strong person should be done evil at the hands of a weak person.’

Between the Old Persian and Middle Persian periods, most of the remaining case system was lost. In Middle Persian, *rādiy* had developed into the postposition *rāy*, and was used primarily to mean ‘on account of’. Although there are examples of *rāy* acting as a dative marker (3.9), most dative functions were indicated via prepositions. Even at this early date, however, there are examples, albeit rare, of *-rā* as a differential object marker (3.10).³⁴ This is counter to the claim (cf. Bossong 1985:61) that the original use of *-rā* was entirely dative-benefactive.³⁵

- (3.9a) U ōi Vīrāf rāy haft xvah būð.
 CONJ that DAT seven sisters be
 ‘And to that Viraf there were seven sisters.’
- (3.9b) Āðōn amā haft xvah rāy brāð ēn ēvak hast.
 thus 1st.pl seven sisters DAT brother this one only
 ‘Thus is to us the seven sisters as brother this one, the only.’
- (3.10) U pas Axt ī yāðūk framūð brāð ī
 CONJ? then EZF magician order.PAST.3rd.sg. brother EZF
 xvēš rāy āwur- tan ōza- ðan.
 3rd.sg.MASC.POSS DAT brought.PASS-INF slay.PASS-INF
 ‘Then the magician Axt ordered his brother to be brought and slain.’

In Modern Persian, *-rā* has become a dative suffix, placed further from the root than the indefinite article (3.11a). It is a phrase-level suffix rather than a word-level suffix, as evinced by the fact that it appears on the final word in the phrase to which it applies (3.11b) and nowhere else (3.11c). Nevertheless, it is better analysed as a case marker than an adposition, as will be demonstrated shortly. It is frequently used as a differential marker, occurring with definite direct objects and specific indefinite objects; its presence is not obligatory but seems to be connected to animacy (cf. Lazard 1982).³⁶

³⁴ Examples (3.9)-(3.10) from Salemann and Bogdanov (1930:58), who provide only translations. They also provide several examples of Middle Persian prepositions expressing dative functions, but these examples are neither glossed nor translated. I am extremely grateful to Ron Kim (p.c.) for his help with the gloss of (3.10).

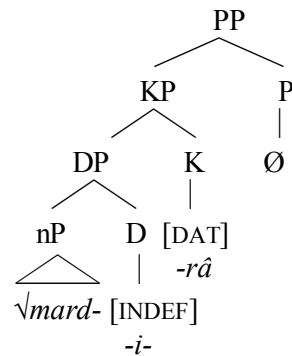
³⁵ EZF in (3.10) and subsequent examples stands for “ezafe”, a clitic indicating that its host is possessed or modified by an adjective; cf. Lazard (2006:53ffn.).

³⁶ The Modern Persian examples come from a variety of sources, no two of which are working with the same system of transliteration. I have made no attempts to standardise. Examples in (3.11) are from Lazard (2006:64). The original source is unglossed; I have therefore glossed it myself based on a searchable Persian-English dictionary (Steingass 1892) and my translation of Lazard’s (French) translation.

- (3.11a) mard-i- rā
 man- INDEF-DAT
 ‘a man’
- (3.11b) Xâne- ye Hasan-râ mišenâs-am.
 house-EZF Hasan-DAT know- 1st.sg.
 ‘I know Hasan’s house.’
- (3.11c) Ân ketâb-e kamyâb-râ xarid.
 that book-EZF rare- DAT bought
 ‘He bought that rare book.’

The structure of (3.11a) is therefore (3.12). Here, *-râ* is the head of KP, in contrast to the earlier (3.6b), where its equivalent was treated as the head of PP.

(3.12)



Unfortunately, the details of the developments that occurred between Middle Persian and Classical Persian are somewhat murky. New or Modern Persian is divided into several stages (Pre-Classical Persian, Classical Persian, and the present-day variety), and there are some clear differences in the syntax of *-râ* between these stages. In his discussion of Pre-Classical Persian, Lazard (1963:356ffn.) demonstrates that differential marking had already developed (3.13a), but *-râ* was also used in a wide variety of dative contexts, including indirect object (3.13b), possessor (3.13c), experiencer (3.13d), and subject of passive (3.13e).^{37, 38} Differential marking was much less common at this stage of Persian; cf. Lazard (1963:357).

- (3.13a) Yeki az moluk- e Xorâsân Mahmud Sobaktegin-râ be-xâb
 one of king.PL-EZF khorasan mahmud sobaktegin- DAT in- sleep
 did.
 see-PAST-3rd.sg.
 ‘One of the kings of Khorasan saw Mahmud Sobaktegin asleep.’
- (3.13b) Hakim-i pesar-ân-râ pand hami- dâd.
 wise- INDEF boy- PL-DAT advice CONTIN-give.PAST
 ‘A wise man gave advice to the boys.’
- (3.13c) Ma- râ dar šahr dust- ân besyâr-and.
 1st.sg-DAT in city friend-PL many- 3rd.pl

³⁷ The examples in (3.13a-d), taken from Bossong (1985:58ffn.), are actually from Classical Persian rather than Pre-Classical Persian, owing to the superiority of Bossong’s glosses over those of any other source I have found. Translations from Bossong’s German are mine. (3.13e) is taken from Lazard (1963:374), which is unglossed; for the source of the glosses cf. the previous footnote.

³⁸ *-râ* appears to have also functioned as an information structure element or focus marker, judging from its use in left dislocation, topic-marking, contrastive environments, &c. For details cf. Lazard (1963:371-3).

- ‘I have a lot of friends in the city.’
 (3.13d) Darviš- i- rā zarurat-i piš-âmad.
 dervish-INDEF-DAT peril- INDEF happen.to.PAST.
 ‘A peril befell a dervish’ = ‘A dervish met with serious difficulties’
 (3.13e) Jam‘ karda šuđ in kitāb-rā.
 gather.VN made be this book-DAT
 ‘This book has been gathered.’

Lazard also gives examples which contain two tokens of *-râ*. In some of these cases *-râ* serves the same function twice; in (3.14a), for instance, both dative objects are marked separately with *-ra*. Examples of this type are no longer found in Modern Persian. Other examples feature tokens of *-ra* with different functions, as in (3.14b). In (3.14b), the first DP is an experiencer subject, while the second is a definite direct object.

- (3.14a) Mā in Muhammad-rā va yār- ān-i ō- rā az
 1st.pl. this M.- DAT CONJ follower-PL- EZF 3rd.sg.POSS-DAT from
 Makka biyāvard-īm.
 Mecca bring- 1st.pl
 ‘We brought this Muhammad and his followers from Mecca.’
 (3.14b) Peyğāmbār-râ ân tadbir-râ xoš âmad.
 Prophet- DAT that plan- DAT good come.3rd.sg.PAST
 ‘The prophet approved that plan.’

(3.14b) is particularly good evidence for the status of *-râ* as a case marker rather than simply a postposition: adpositions are not expected with dative subjects, and *peyğāmbār-râ* is best analysed as a dative experiencer.

Further evidence that *-râ* is no longer a postposition can be seen in its ability to co-occur with prepositions. This construction is frequent in Pre-Classical Persian, though it is obsolete in Modern Persian. Most of the prepositions which co-occur with *-râ* are glossed ‘for’ or ‘by reason of’ (3.15). The preposition which occurs most commonly, however, is *mar* (3.16), which does not seem to have any actual semantic function; Lazard (1963:382) says that there is no apparent difference in function between *mar...râ* and *-râ* alone.³⁹ The provenance of *mar* is unclear to me; there are no traces of it in Middle Persian (Salemman and Bogdanov 1930:58).⁴⁰

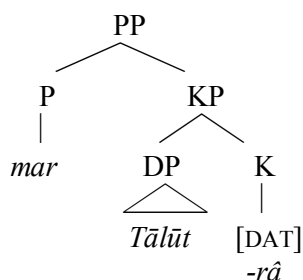
- (3.15) az in jihat- rā
 for this reason-DAT
 ‘for this reason’
 (3.16a) Išmū’īl mar Tālūt-rā biyāvard va Banī Isrā’īl gird
 Samuel PREP Saul- DAT bring.3rd.sg.PAST CONJ son.PL Israel gather
 kard.
 do.PAST.3rd.sg.
 ‘Samuel brought Saul and assembled the Israelites.’
 (3.16b) rašad- hā-i Yūnāni-yān mar kavākib-rā
 observation.VN-PL- EZF Greek- PL PREP star.PL- DAT
 ‘the Greeks’ observations on the stars’

³⁹ *Mar* itself rarely occurs without *-râ*, though Lazard (1982:449) has some examples of it preceding subjects or predicate nominals. However, it was used very frequently in Judeo-Persian texts, especially translations of the Bible, because it was seen as equivalent to Hebrew *’et*. Cf. Bossong (1985:59ffn.) with references.

⁴⁰ Bossong (1985:59) suggests that it originally meant ‘number’.

(3.17) shows that the structure of *mar...râ* phrases is identical to that of *-râ* phrases as given in (3.12), apart from the headedness of the PP; the only difference between them is whether the preposition is overt or null.

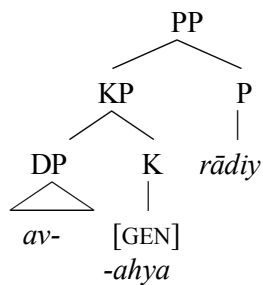
(3.17)



In Modern Persian, *-râ* is used in a narrower set of contexts than in older Persian, though it is still clearly a dative marker; prepositions are used more frequently for e.g. indirect objects (Lazard 2006:163–79). It is mostly used as a differential direct object marker, although in Classical Persian it was equally common as an indirect object marker (cf. Lazard 1970). In addition to the narrowing of permissible contexts for *-râ*, present-day Persian *-râ* differs from Pre-Classical Persian *-râ* in not co-occurring with prepositions and in appearing only on the last conjunct rather than each conjunct individually.⁴¹

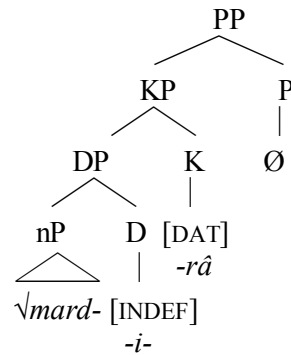
To summarise, then, the development of a dative suffix in Persian occurred in a fashion similar to that of Oscan *-ēn*, but seems to have been connected with the loss of overt dative case-marking, as well as the erosion of phonological material. Another potential factor favouring the reanalysis may have been the fact that Old/Middle Persian was predominantly a prepositional language. The postposition *rādiy* was reanalysed as a phrasal dative marker marking the object of a preposition, which was often null, particularly as time progressed. The trees representing these structural changes are repeated in (3.18). (3.18a) shows the earlier postpositional grammar and (3.18b) the more innovative dative grammar.

(3.18a)



⁴¹ For a very detailed account of the conditions on the use of *-râ* in Modern Persian, cf. Lazard (1982).

(3.18b)



The structure of PPs and DPs in Persian made it easy for language learners to treat *-râ* as a case marker rather than a postposition. First, Persian was essentially a prepositional language, not a postpositional language, so that from the perspective of the grammar, *rây/-râ* was anomalous. If *rây/-râ* were a case marker rather than an adposition, this irregularity would be eliminated. This is not to say that such irregularity would have *forced* learners to this conclusion; modern German, to cite a single example, has both prepositions and postpositions, so clearly uniformity in the structure of adpositions is not essential. However, Persian innovators had an additional motivation, in that their case system was largely broken down, including the dative – which, as noted above, was the first casualty. Children learning German are unlikely to analyse German postpositions as case markers because the German case system remains fully functional; children learning Persian were learning a language with only a residual case system at best, which meant that there was nothing in the way (so to speak) of taking *-râ* to be a case marker, including the phrasal nature of *-râ*.

An interesting detail in the Persian case is that *-râ* never, at any point in its history, became a word-level suffix; its status as a phrase-level suffix remains stable, even though the accustomed view of this type of development is that the transition from postposition to word-level suffix is a fairly trivial one. Once again, however, the structure of the DP in Persian makes the behaviour of *-râ* explicable: the order of elements in Persian DPs is such that that the M-word to which *-râ* attaches is not always the head noun, since genitive and adjectival modifiers both follow the head, and *-râ* attaches to them when they are present. This is not to say that Persian language learners could not possibly have devised a rule moving *-râ* to the head noun, but the fact is, they *didn't*, and moreover, given the structure of the DPs they were acquiring, there is no reason they should have had to.

Therefore, the presence of nominal postmodifiers may prevent learners from acquiring a rule placing a novel case suffix on the head noun (and everything agreeing with it). However, nominal postmodifiers do *not* prevent learners from acquiring a new case marker at all. That is to say, if the syntax of postpositions in a given language is such that nouns are not always linearly adjacent to the postposition, learners may not leap to an analysis whereby the postposition is a case suffix attached to the noun, but they may still conclude that the postposition is a case marker that simply attaches to phrases. This is a surprising circumstance, when we consider that the cross-linguistic asymmetry between case suffixes and case prefixes is frequently ascribed to the fact that premodifiers are likely to interfere with the reanalysis of a preposition as a potential prefix. It would appear that either the syntax of premodifiers is structurally different from postmodifiers in a significant way, *or* the general hypothesis of the paucity of prefixes is wholly or partially incorrect.

Although Persian and Oscan differ somewhat in their details, the basic type of change is the same in both cases. Generation P has an analysis in which the terminal *X* is the head of PP; Generation P+1 has an analysis of *X* as the head of KP. KP is hierarchically lower than PP, but string-vacuously; there has been no change in the surface linear string.

3.2.3 Classical Armenian⁴²

While the development of case suffixes from postpositions is quite common, the development of case *prefixes* from prepositions is considerably less so. One of the few attested examples can be found, briefly, in Classical Armenian, described by Wilhelm (2008). Classical Armenian retained many of the inherited Proto-Indo-European case suffixes, but had lost a formal distinction between nominative and accusative singular, except in some personal pronouns (rather like Modern English), as well as a formal distinction between accusative and locative plural. Armenian also had a small set of prepositions, including both free-standing words and proclitics.

Of chief interest here is the proclitic *z-*, which had no independent form and governed the accusative case. Its original meaning was something like ‘concerning, around’. However, it could also be used with definite direct objects, and in this case it was semantically empty. Wilhelm (2008:292–3) illustrates with the contrast in (3.19):

- (3.19a) *Z*-hreštak-n tesan-ein.
Z-angel- that see- IMPF.3rd.pl.
 ‘They saw the angel.’
- (3.19b) Hreštak tesan-ein.
 angel see- IMPF.3rd.pl.
 ‘They saw an angel.’

z- is also used on those pronouns and determiners otherwise lacking a formal nominative/accusative distinction, including the pronoun *na* (3.20), the demonstrative *ayd* (3.21), and the relative pronoun *or* (3.22).

- (3.20a) Ew **na** sks- aw nzov-el.
 CONJ **3rd.sg.MASC.** begin-AOR.3rd.sg. curse-INF
 ‘And he began to curse.’
- (3.20b) Zi matn- isc’- ē **z-na** noc’a.
 COMP betray-PRES.SUBJ-3rd.sg. *Z-* 3rd.sg.MASC. 3rd.pl.DAT
 ‘That he would betray **him** to them.’
- (3.21a) **Ayd** ē marmin im.
 this COP.3rd.sg. body 1st.sg.POSS.
 ‘**This** is my body.’
- (3.21b) Mart’ ēr **z-ayd** ewł vačar°-el.
 possible IMPF.3rd.sg. *Z-*this oil sell- INF
 ‘It was possible to sell **this** oil.’
- (3.22a) Ekn kin mi **or** un- ēr sis imł-oy
 come.AOR.3rd.sg. woman one **REL** have-IMPF.3rd.sg. bottle oil- GEN.SG.
 nardean axnow-i mecgn- oy.
 of.nard fine- GEN.SG. very.precious-GEN.SG.
 ‘There came one woman who had a very precious fine bottle of nard-oil.’
- (3.22b) Da **z-or** un- ēr arar.
 that *Z-REL* have-IMPF.3rd.sg. carry.out.AOR.3rd.sg.
 ‘That **which** she had, she carried out.’

⁴² I am grateful to Jean-François Mondon for his assistance with the examples in this section, particularly with the glosses.

The evidence indicates that *z-* was a differential accusative marker, not a dative marker like Persian *-râ*. As a preposition, it was limited to governing the accusative case; it was never used with the dative, which did exist in Armenian. Furthermore, there was a class of Armenian verbs that required dative case (cf. Thomason 1975:14), and *z-* was never used in this environment either.

As discussed in 3.2.1, the innovative Oscan locative spread to modifiers; as noted in 3.2.2, Persian *-râ* did not. Armenian *z-* occupies an intermediate position between the two. In (3.23), the adjective *het'anos* is not marked with *z-*, unlike the noun it modifies, but in the examples in (3.24), the modifiers are marked with *z-*.

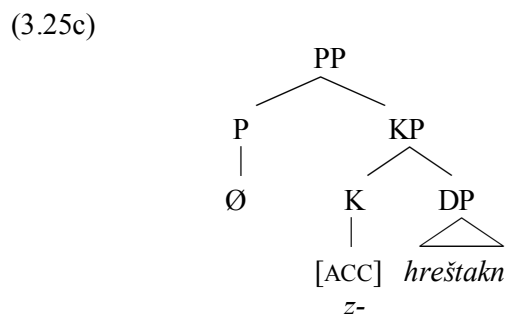
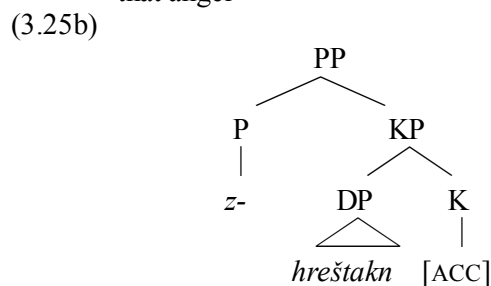
- (3.23) Gnac' -ĕk' aysuhetew ašakertec' -ĕk' z- amenayn
 go- AOR.IMPV.2nd.pl. henceforth teach- AOR.IMPV.2nd.pl Z-all
 het'anos-s.
 gentile- ACC.PL.
 'Go henceforth and teach all the Gentiles (peoples)!'

- (3.24a) Ġit- em zi z-Yisus z-xač'eleal-n xndr-ĕk'
 know-PRES.1st.sg. COMP Z-jesus Z-crucified-that seek- PRES.2nd.pl.
 'I know that you seek the crucified Jesus.'

- (3.24b) Anc'o z-bažak-s z-ays y- inĕn.
 transfer.AOR.IMPV.2nd.sg. Z-cup- this Z- this from-1st.sg.ABL.
 'Transfer this cup from me.'

The diachronic analysis of *z-* is rather more complicated than that of Oscan *-en* or Persian *-râ*, because the synchronic status of *z-* is unclear. If *z-* were a basic preposition governing the accusative, as it is usually analysed, then the only difference between it and the previous two cases is the fact that it is a preposition rather than a postposition. The change in *z-* would therefore be the change between (3.25b) and (3.25c), i.e., preposition to case marker.

- (3.25a) *z-* hreštak-n
 ACC-angel- that
 'that angel'



However, the analysis of *z-* as a preposition is somewhat problematic. Though it is not unheard of for languages to mark direct objects with prepositions, this is rare for Indo-European languages and seems to be only marginally possible in Armenian. Furthermore, prepositional *z-* is traditionally assigned a meaning ‘concerning, around’, and it is not at all clear how this semantics lends itself to reanalysis as a basic object marker.

In light of these problems, a better solution is to analyse *z-* as some sort of information structure marker, probably a topic. A discourse marker analysis makes more sense semantically, and also explains why *z-* was limited to only definite DPs: indefinite topics are not usually allowed. The association of *z-* with the accusative could be due to the fact that objects are more likely to require topic marking than subjects.⁴³ In addition, *z-* is unusual within Classical Armenian, in that it is apparently the only preposition in the language to govern only one case rather than two or more.

The internal structure postulated for a topic marker analysis of *z-* is minimally different from that already given for *z-* as preposition; the only difference lies in the identity of the node labels in the earlier pre-change stage (presumably TopP rather than PP). The motivations for reanalysis thus remains the same, structurally speaking.

Despite its marginal status, *z-* is of interest because there are very few examples of case prefixes in the literature. It is *also* interesting in that it is an example of an innovation which never quite made it to completion and subsequently all but vanished; such ephemeral innovations are probably quite common, but it is very rare to find one with a documented history. In the modern Armenian languages there is only a single trace of *z-*: in Western Armenian, many (though not all) accusative pronouns have an initial *z* which does not occur in the nominative. Wilhelm reports that many of these are recognisably of Classical Armenian provenance.

(3.26)	<i>Nominative</i>	<i>Accusative</i>
	1s	zes
	2s	zpez
	3s	zimpə
	1p	zmez
	2p	zcez
	3p	ziremp
	rel.s	or, zor
	rel.p	oromp, zoromp, zors

The three case studies discussed thus far all demonstrate the ease with which adpositions can be reanalysed as case markers: the innovating generation of speakers is simply identifying the adposition with a lower position on the tree, closer to the DP. In the Armenian and Oscan cases, phonological factors undoubtedly played a role in facilitating this reanalysis: Oscan speakers could no longer easily segment the postposition *en* from the earlier locative ending because of regular sound changes, and Armenian *z-* was a single segment, not even an entire syllable. The Armenian case shows that this reanalysis is possible with prepositions as well as postpositions, although it is rarer.

3.2.4 Piattino

Most modern Indo-European languages mark subject agreement as a verbal suffix at the end of the verbal complex, and not elsewhere; this pattern is extremely archaic and can be traced back to the proto-language (cf. e.g. Sihler 1995). However, in some non-standard

⁴³ My thanks to Aviad Eilam (p.c.) for pointing this out. He notes that Amharic also has formally identical object and topic markers, though as yet it is not clear whether the two are etymologically connected.

modern dialects of Germanic and Romance languages, agreement prefixes have developed. Since the development of prefixes is not always correlated with an accompanying loss of suffixed agreement, these cases represent the development of a form of multiple exponence. Various cases of this type have been closely studied by Fuß (2005), including that of Piattino, a dialect of Lombardian Italian originally studied by Gerlach (2001, 2002). Piattino is particularly interesting because in the process of developing agreement prefixes, it also developed verbal inflection in gender, a distinction that previously did not exist.

Subjects are marked in two ways in Piattino: by verbal suffixes and pre-verbal clitics. The suffixes, consistent with verbal suffixes in other Italian dialects, are obligatory; the clitics are innovatory, and are optional in some circumstances (3.27) but obligatory in others (3.28). *Mi* and *noaltri* are ordinary subject pronouns.

- (3.27a) *Mi* (a) guard-i.
 1st.sg. 1st.sg. watch-1st.sg.
 ‘I am watching.’
- (3.27b) (*Mi*) a guard-i.
 1st.sg. 1st.sg. watch-1st.sg.
- (3.28a) (*Noaltri*) an guard-a.
 1st.pl. 1st.pl. watch-1st.pl.
 ‘We are watching.’
- (3.28b) * *Noaltri* guard-a.
 1st.pl. watch-1st.pl.

The optionality of a particular subject clitic depends on its ϕ -features; this is summarised in Table 3.1, adapted from Fuß (2005:257). Thus, in Piattino, subject clitics are best analysed as agreement prefixes in the third persons and the first person plural, but not elsewhere, even though presumably the clitics originally occupied the same syntactic position in both cases.⁴⁴

	Singular	Plural
1 st .	(a) guard-i	an guard-a-Ø
2 nd .	(te) guard-esc	(ve) guard-é
3 rd . Masc.	al guard-a-Ø	i guard-en
3 rd . Fem.	la guard-a-Ø	li guard-en

Table 3.1: *Subject agreement and clitics in Piattino, present indicative*

Table 3.1 reveals a clear correlation between obligatory subject clitics and formally distinct agreement suffixes: the clitics are mandatory in the homophonous first plural and third singular. However, as Fuß points out, subject clitics are also mandatory in the third plural, which *is* formally distinctive from the perspective of earlier stages of Piattino, when verbs were inflected only for person and number.

Explaining what has happened in Piattino requires an analysis of two stages of the grammar: one before the innovation, one after. In the pre-affixation stage, the first plural is subject to radical impoverishment (perhaps through a *[1 pl] filter; cf. Noyer 1997),⁴⁵ and therefore no subject agreement morpheme is inserted. Thus, the set of Vocabulary Items for subject agreement in Piattino at this stage would be as in (3.29).

⁴⁴ Fuß follows Gerlach (2002) in analysing the *-a* found in the third singular and first plural as a theme vowel rather than a bona fide agreement marker. The theme vowel is deleted when followed by an agreement suffix.

⁴⁵ Historically, the Piattino first plural was actually an impersonal construction involving the third singular and the noun ‘man’, much like French *on* + third singular. This change has happened across Italian as well. Cf. Fuß (2005:293), notes 36-38.

(3.29a) [+2 +PL]	↔	-é
(3.29b) [+2 +SG]	↔	-esc
(3.29c) [+1 +SG]	↔	-i
(3.29d) [+ PL]	↔	-en

The Piattino subject clitics are proclitic to the verb and are therefore already part of the same phonological word as the verb; therefore, they are in a position susceptible for reanalysis as agreement morphemes. However, as we saw at the beginning of this subsection, not all the subject clitics have been reanalysed as agreement prefixes, since only some of them are obligatory. Moreover, all of the optional clitics have in common the fact that they are uniquely identified by a person-number suffix; with the exception of the third plural, which we will return to momentarily, the obligatory clitics are those whose suffixed endings are homophonous. On the basis of this observation, Fuß (2005:258) and Gerlach (2002) argue that the clitic is reanalysed in order to provide the missing distinctions. Fuß suggests,

...[S]ubject clitics became obligatory in contexts where the finite verb is underspecified for agreement features, presumably to recomplete a defective agreement paradigm. In contrast, the clitic is merely optional in 1sg and 2nd person contexts, where the verbal agreement morphology is still fully distinctive, reflecting unambiguously the subject's set of ϕ -features.

Fuß (2005:258)

Although Fuß's phrasing implies a somewhat teleological view of linguistic change, one can envision a scenario compatible with the assumptions about linguistic diachrony laid out in Chapter Two. In the pre-prefixation grammar of Piattino we are currently working with, all clitics are optional, but they may be used more frequently in the third singular and first plural, since these endings are homophonous. Children acquiring the grammar would therefore have more evidence for the use of clitics with the third singular and first plural, and may in turn use these clitics even more frequently than their parents. At some point, the combined evidence of a) particularly frequent use of a specific proper subset of clitics and b) a lack of distinctive agreement suffix in exactly these cases leads the speakers of some generation, call it Generation *P+I*, to analyse those clitics as prefixed agreement markers rather than as pronominal clitics, unlike their parents in Generation *P*.⁴⁶

Children do not, however, reanalyse the other clitics as prefixes. These clitics double the information already provided on the agreement suffixes, and therefore analysing the clitics as prefixes builds redundancy into the verb forms. The argument that children are biased away from duplicating feature information is based on the broader hypothesis that children do not posit redundancy in their grammar unless they are forced to it; rather, they will prefer (at least initially) a one-to-one mapping of function and/or semantics. It has been repeatedly observed, starting with Bréal (1897), that exact synonymy between lexical items is quite rare; more recently, acquisition specialists like Clark (1987) have argued for a Principle of Contrast: 'Every two forms contrast in meaning.' Though intended to explain how children assign word meanings to word forms, it is not unreasonable to extend the hypothesis to the realm of morphology; cf. Carstairs-McCarthy (1994). This intuition has been formalised into the Morphological Anti-Redundancy Condition (MARC) by Diertani and Eilam (2010):

⁴⁶ This is quite likely a situation that would involve competing grammars, though there is no data available on this point.

- (3.30) *Morphological Anti-Redundancy Condition (MARC)*: Learners will disprefer a morphosyntactic word (M-word) in which one morpheme is assumed to be identical in its feature content to another morpheme.

The MARC is applicable in determining which Piattino subject clitics are reanalysed as prefixes: those which contribute unique morphological information are interpreted as affixes, while those which merely duplicate featural information already expressed as suffixes are analysed as clitics.

Another analysis of the data is potentially available to the children: the feature content could be distributed between prefix and suffix – i.e., number could be indicated by the suffix and person by the prefix, or vice versa. This would eliminate redundant information and create a symmetrical system in which all verbs have both prefixes and suffixes. For the linguist, this kind of symmetry is pleasing, but children do not seem to be biased in this direction.⁴⁷ In this case, they have evidence from their parents' speech that these clitics are optional, and therefore can hardly be encoding essential feature information. Therefore, the children have more syntactic evidence that the first singular and second person clitics are indeed clitics than they do for them as prefixes. Note that it is entirely plausible that, in Generation $P+I+n$, the evidence available will have changed (i.e. perhaps the use of the optional clitics will increase), so that the children are now biased in favour of analysing all preverbal terminals with person/number information as prefixes; however, the present-day grammar of Piattino does not seem to have reached this stage.

There is another instantiation of this bias in the Piattino agreement system, namely in the case of the third plural. The third plural prefix is anomalous within the system: it is obligatory, and yet the third plural agreement suffix is unique. This would seem to represent counterevidence to the arguments made here; yet, as Fuß (2005:259) observed, the third plural prefixes are actually conveying unique information: gender. Piattino developed gender marking on the verb with the reanalysis of the third singular clitics *al* and *la*; therefore, Fuß argues, the development of gender marking with third singular verbs must also result in gender marking with third plural verbs, since this creates a new category: 'Due to their specification for gender, the new [third] person agreement formatives count as stronger agreement exponents, even if they do not carry any person specification.'

The argument that children *must* analyse the third plurals in this way is rather too deterministic. I prefer to modify his phrasing slightly, saying instead that the child encounters evidence for gender marking in both the singular and plural forms, and has no reason to differentiate between them syntactically, since they are each contributing the same information. This can be seen clearly in the post-prefixation grammar of Piattino, which contains the inventory of Vocabulary Items in (3.31).

(3.31a) [+1 +PL]	↔	<i>an-</i>	
(3.31b) [+2 +PL]	↔	<i>-é</i>	
(3.31c) [+2]	↔	<i>-esc</i>	
(3.31d) [+1]	↔	<i>-i</i>	
(3.31e) [+ PL]	↔	<i>-en</i>	
(3.31f) [+MASC]	↔	<i>i-</i>	/ [PL]
(3.31g) [+FEM]	↔	<i>li-</i>	/ [PL]
(3.31h) [+MASC]	↔	<i>al-</i>	
(3.31i) [+FEM]	↔	<i>la-</i>	

Although this could be seen as a type of multiple exponence (cf. Chapter Four), (3.31) reveals that the third plural affixes are *not* redundant: plurality is expressed by the

⁴⁷ Speakers of languages seem to be much more tolerant of such inconsistencies in grammar than linguists; cf. for example the "exceptional" third plural in the Irish case study in 5.2.3.3.

suffix, and gender is expressed by the prefix, though conditioned by the presence of the feature [+PL] elsewhere on the verb. Rather than multiple exponence, this is a type of Fission, as introduced by Noyer (1997). There is only a single Agr associated with each verb, but multiple Vocabulary Items are required to discharge all of the features. Each of these is inherently specified as either prefix or suffix, but must be adjacent to the verbal complex.

Piattino provides another example of clitic pronouns adjacent to the verbal complex developing into affixes, with an additional twist: agreement affixes already existed. The innovation here is not agreement affixes per se, but more highly specified agreement affixes. However, a new category does arise with the innovation of gender agreement.

3.2.5 Amharic⁴⁸

Amharic has two types of “compound” verb forms, transparently formed by the fusion of two finite verbs. In this section I will discuss one of these, the compound gerund; in Chapter Four I will discuss the other, the compound imperfect.

The Amharic “gerund” is confusingly named, as it is a finite verb form inflected for person, number, and gender. Its primary function is in subordinate clauses; Leslau (2000:78) describes it as ‘express[ing] an action that precedes that of the verb of the main clause’, as in the following example:

- (3.32) Lemat-u- n käft- o dabbo-w- ən
 basket-DEF-ACC open.GER-3rd.sg.MASC. bread- DEF-ACC
 wässäd- ä.
 take.PERF-3rd.sg.MASC.
 ‘Having opened the basket, he took the bread.’

The “compound gerund” consists of the fully inflected gerund and the auxiliary *allä* ‘to exist’; the form of the latter is largely the uninflected *-all*, except in the first persons and third singular feminine; the loss of subject inflection on *-all* will be discussed in Chapter Four (cf. Diertani and Eilam 2010; in progress). The verb *allä* is used elsewhere in Amharic as an independent main verb with inflection for all person/number/gender combinations. It is unusual in Amharic, however, in that it is formally a perfect, but semantically an imperfect indicating present or future.

Outlines of the paradigms of the simple and compound gerunds follow in Table 3.2.

	Singular		Plural	
	Simple	Compound	Simple	Compound
1 st .	-e ⁴⁹	-e ^y -all-äh ^w 50	-än	-än-all
2 nd . Masc.	-äh	-äh-all	-aččəhu ⁵¹	-aččəh ^w -all
2 nd . Fem.	-äš	-äš-all	-äw	-ä ^w -all
3 rd . Masc.	-o	- ^w -all ⁵²		
3 rd . Fem.	-a	-a-all-äčč		

Table 3.2: Inflection of Amharic Simple and Compound Gerunds

In Amharic, object pronouns are suffixed to the main verb; in the case of the compound gerund, these pronouns are suffixed to the gerund, not to the entire compound, as

⁴⁸ The data and discussion in this section first appear in Diertani and Eilam (2010; in progress).

⁴⁹ The first singular gerund uses a different template from the rest of the paradigm, e.g. 3rd.sg.masc. *säbr-o* but 1st. sg. *säbərr-e*. This is retained in the compound gerund.

⁵⁰ The sequence *-e^y-* may be elided.

⁵¹ Pronounced *-aččuh* (Leslau 2000:77).

⁵² Surface form; the underlying form is *[/ROOT]o-all/*.

illustrated in Table 3.3 with the forms used when the subject is third singular masculine. (The verb used is from the root *sbr* ‘break’.)

	Singular	Plural
1 st .	säbr-o-ññ-all	säbr-o-n-all
2 nd . Masc.	säbr-o-h-all	säbr- ^w -aččəh ^w -all
2 nd . Fem.	säbr-o-š-all	
3 rd . Masc.	säbr-o-t-all	säbr- ^w -aččäw-all
3 rd . Fem.	säbr- ^w -at-all	

Table 3.3: Amharic Compound Gerunds with Suffixed Object Pronouns

Leslau (2000) describes the compound gerund as a sort of “present-perfect”, indicating an action that started in the past but whose outcome continues into the present; it thus contrasts with the Amharic perfect tense, which indicates only that an action occurred in the past. This function can be plausibly derived from the combined semantics of the two verbs, with the imperfect present/future sense of *allä* providing the present component and the subordinate-prior-action sense of the gerund contributing the perfect component.

The following examples illustrate the use of the compound gerund (b examples) as contrasted with that of the perfect (a examples).⁵³

- (3.33a) Əda-ye- n kəffäl- ku.
debt-1st.sg.-ACC pay.PERF-1st.sg.
‘I paid my debt.’ (in the past)
- (3.33b) Əda-ye- n kəfəyy- Ø- all- əh^w.
debt-1st.sg.-ACC pay.GER-1st.sg.-PRES-1st.sg.
‘I paid up my debt (and am at present free of debt)’
- (3.34a) Abbat-e mot- ä.
father-1st.sg. die.PERF-3rd.sg.MASC.
‘My father died.’
- (3.34b) Abbat-e mot- ^w- all.
father-1st.sg. die.GER-3rd.sg.MASC.-PRES
‘My father is dead.’

That the relationship between the auxiliary *allä* and the compound gerund suffix *-all-* is not simply chance resemblance is indicated by the retention of the subject agreement on the former auxiliary in two of the members of the paradigm, as well as the retention of agreement in other forms in Old Amharic texts (3.35) (cf. Goldenberg 1977:495). The construction sans affixation is also found unchanged in the related language Tigrinya (3.36).

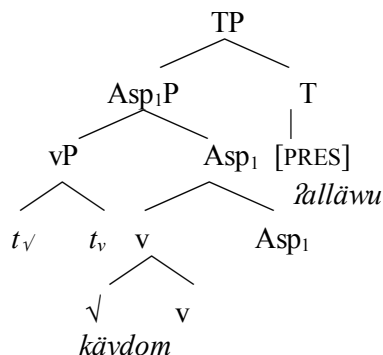
- (3.35) bəl- äw- all- u
say.GER-3pl-PRES-3pl
‘They have said.’
- (3.36) kəyd- om ?all-äwu.
go.GER-3pl COP-3pl
‘They have gone.’

Like many Semitic languages, the basic distinctions made by Amharic verbs relate to aspect rather than tense, the primary distinction being between imperfect and perfect. Since *-all* provides a clear temporal component to the semantics of the sentence, Diertani and Eilam

⁵³ Examples from Leslau (2000:81–2).

(2010, in progress) argue that it is an exponent of T. This suggests that the original clausal structure in Amharic was something like the modern structure of Tigrinya, with *all-* as a temporal auxiliary. This analysis gives the structure in (3.37), illustrated with the Tigrinya sentence from (3.36).

(3.37)



Subject agreement is not indicated in (3.37); as in most Semitic languages, subject agreement in Amharic is contingent in form and location (prefix versus suffix) on aspect and therefore is likely a dissociated morpheme attached post-syntactically to Asp₁. The gerund, as a former nominal form, may be neutral with respect to aspect; it surfaces with the default agreement affixes rather than those associated with the perfect or imperfect.

Further evidence that *-all* originated as an auxiliary comes from relative clauses in present-day Amharic, where affixation of *-all* never occurred. In relative clauses, the auxiliary raises to C and gains a relative prefix, while the gerund remains lower in the clause. Both verb forms bear subject agreement suffixes.

- (3.38) Səra- w- ən č'ərrəs- äw y- all- u- t məsa y- əbl- u.
 work-DEF-ACC finish.GER-3pl REL-COP-3pl-DEF lunch 3pl-eat.JUS-3pl
 'Let those who have finished the work eat lunch.'

The process of affixation of *-all* appears to have been completed only recently, and possibly only in certain dialects, although the sociolinguistic situation has not been investigated. Leslau (1995:388) indicates that two particles (*-mm* 'and' and the contrastive topic marker *-ss*) were optionally allowed to intervene between the gerund and *-all*; however, he also says that, although this intervention is allowed in older texts and in some dialects, it is not allowed in others. None of the speakers surveyed in recent fieldwork conducted by Aviad Eilam allowed the intervention, which suggests that for them, the process is complete.

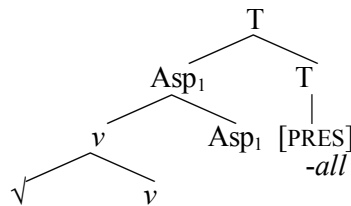
- (3.39a) Nägr- w- all- əmm
 tell.GER-3rd.sg.MASC.-AUX-CONJ
 'And he has told.'
 (3.39b) (*) Nägr- o- mm- all
 tell.GER-3rd.sg.MASC.-CONJ-AUX
 (3.39c) Nägr- w- all- əss
 tell.GER-3rd.sg.MASC.-AUX-FOC
 'He has indeed told.'
 (3.39d) (*) Nägr- o- ss- all
 tell.GER-3rd.sg.MASC.-FOC-AUX

Even in the dialects discussed by Leslau, it is clear that *-all* had already acquired affix-like behaviour. First, the set of elements allowed to intervene between the gerund and -

all is highly restricted even in those dialects that permit intervention at all. This is in fact to be expected in a head-final language, given the way the clausal syntax works. As will be discussed below, linear adjacency is a necessary precondition for the development of a new affixation relationship. Second, *all* is in the process of losing its agreement marking (thus becoming less verb-like) even in those cases when it is not linearly adjacent to the gerund; the loss is complete in all but two of the eight forms of the paradigm. We will see in Chapter Four that the compound imperfect differs from the compound gerund in both respects. Indirect corroboration for an affix analysis comes from native speakers, who, when asked, agree that these forms constitute one “word”; this intuition is reflected in the orthography.

This evidence points to an analysis in which the relationship between the gerund and *-all* is affixation via head movement. If *-all* is, as seems reasonable, an exponent of T, then the internal structure of the modern compound gerund is that in (3.40).

(3.40)



The question, then, is how Amharic developed from a language with a Tigrinya-like construction with a temporal auxiliary (3.37) into a language with affixation via head movement.

Amharic is an Infl-final SOV language, which means that in most clauses the verb and exponents of all higher projections will be clause-final. This means that they are very likely to lean on each other phonologically; it also means that there are relatively few elements that will be allowed to intervene between them. The combination of prosody and very frequent linear adjacency is thus very likely to bias language learners towards analysing the verb and auxiliary together as a single M-word. A further factor here may be that the auxiliary is vowel-initial and all of the subject agreement markers of the gerund are either consonant-final or glide-final. When pronounced in sequence, therefore, these endings would form a single syllable with the vowel of the auxiliary.

In the generation *P+1* in which reanalysis of (3.37) as (3.40) occurred, therefore, language learners heard the very frequent combination of gerund and auxiliary, probably already reduced into a single phonological word, with a syllable boundary that did not correspond to the underlying morphological structure. Moreover, they learned from the speech around them that only a very small set of items were allowed to intervene between the auxiliary and the gerund. This led them to postulate that the morphological structure was as close as the phonological structure. They were working with exactly the same elements that the speakers of generation *P* used; the only difference in the innovative grammar is the nature of the dependency.

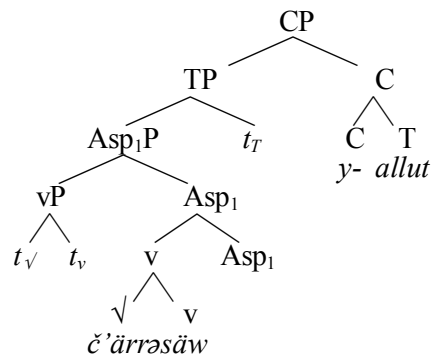
There is very little evidence available as to whether Amharic speakers actually proceeded directly from a Tigrinya-like grammar to the modern grammar; it is entirely possible that they passed through an intermediate stage involving a rule of Local Dislocation, allowing them to extend the right edge of the M-word boundary of the main verb to include the auxiliary when no focus particle intervened. This could have underlain the Amharic grammar until very recently, so that only speakers for whom no interposition was possible had a head movement grammar, while other speakers had an LD grammar. Speakers who allow both possibilities would have access to both grammars. However, it is also possible that learners *did* move from a grammar with simple linear concatenation to a grammar with head movement, and the forms with interposed particles merely reflect competing grammars. In this scenario, speakers who do not allow interposition have finally lost the older grammar.

- (3.41a) *Scenario A:*
 Linear concatenation > Local Dislocation > Head movement
- (3.41b) *Scenario B:*
 Linear concatenation > (Variation) > Head movement

There is one complication which remains to be discussed. Recall from above that the development of affixation discussed here never occurred in relative clauses; the structure of (3.36), repeated here as (3.42a), is still (3.42b).

- (3.42a) Səra- w- ən č'ərrəs- äw y- all- u- t məsa y- əbl- u.
 work-DEF-ACC finish.GER-3pl REL-COP-3pl-DEF lunch 3pl-eat.JUS-3pl
 'Let those who have finished the work eat lunch.'

(3.42b)



The crucial difference between the main and relative clause compound gerunds is the presence, in the latter, of a relative prefix on the auxiliary. The Amharic gerund was originally a nominal form, and as such it would not have been permitted to undergo verbal operations such as movement to C, nor to bear a verbal affix. Even after the gerund became more verbal in nature, learners of Amharic had no evidence that it was allowed to move to C; rather, all the evidence available to them showed the auxiliary moving to C while the main verb remained in situ. Further, in relative clauses, the auxiliary appeared with the relative clause prefix *y-*, which could have been assumed by speakers to indicate the beginning of an M-word, thus leading them to the conclusion that affixation via head movement was restricted to main clauses.

Although it may seem undesirable to have the same set of elements forming one M-word in main clauses, but two elements in relative clauses, such grammars apparently pose no difficulties for native speakers. The language most notorious for permitting this kind of inconsistency is present-day English, where *do*-support in negative past tense clauses is not only an integral part of the grammar, but a relatively innovative one that developed from what could be considered a more “consistent” grammar (for discussion of *do*-support, cf. 6.3.1). Amharic and English are mirror images in this respect: English abandoned a consistent “one-M-word” grammar, while Amharic abandoned a consistent “two-M-word” grammar.

The diverging developments of compound gerunds in main and relative clauses suggests that language learners use information such as the presence or absence of prefixes to assist them when deciding which Sub-words make up an M-word. In this particular instance, they used the presence of a relative prefix in relative clauses to decide that the auxiliary constituted a separate M-word from the gerund, but they did not generalise from this case to the case of main clauses, which lack the tell-tale prefix.

Thus far I have not addressed the most difficult problem presented by the Amharic data, which concerns the subject agreement suffixes. Even a cursory glance at Table 3.2 is enough to show that some of the affixes on the auxiliaries have been lost, while others

remain, apparently giving rise to multiple exponence. I will return to these affixes in Chapter Four.

3.2.6 Summary

In each of the case studies discussed in this section, the grammar learned by the innovators is not radically different from that of their elders. In fact, the crucial grammatical changes occur at a fine level of detail: adding a specific rule to the derivation, or a specific step to an existing rule, or simply placing a morphological exponent in a slightly different structural position. All of these changes are discrete and precise. Turning now from specific case studies to a broader, more general perspective, we will consider the factors that enable these kinds of changes to occur.

In the interest of convenience and readability, I will henceforth be using the terms “P-speakers” and “innovators”, defined as follows:

(3.43) *P-speakers*: speakers in Generation *P* and the generations prior to Generation *P*, who use the older, more conservative grammar.

(3.44) *Innovators*: language learners in Generation *P+1* who acquire an innovative grammar that differs from that of Generation *P*.

3.3 Learnability

The first point that must be addressed is a simple one: how is it that innovators acquire a grammar different from that of their predecessors? The operating assumption in the study of child language acquisition is that children eventually converge on the adult grammar so as to replicate it perfectly. When children do *not* replicate their parents’ grammar exactly, linguistic changes result. What is different about those cases?

One notion that has frequently been invoked in grammaticalization-type phenomena in general is that of structural simplification (cf. e.g. Roberts and Roussou 2003), the idea being that innovators tend to initiate changes that simplify the grammar in some way: Merge rather than Move; one word rather than two (in lexicalist theories); one clause rather than two; and so on. Translated into the framework in use here (in which a child language learner could not knowingly act to simplify a grammar he has not yet acquired), this amounts to saying that children learning a language opt for the simplest hypothesis consistent with the data. This sounds very well, but is rather difficult to quantify, absent a rigorous definition of “simplicity”.

Part of the problem is that adult linguists and children learning their native language have different intuitions about the constitution of “simplicity”. As linguists, we like our analyses to be descriptively consistent, with as few exceptions as possible. Children do not seem to regard the problem in quite the same way, and we have already seen this fact illustrated in just the few case studies discussed above. In Piattino, for instance, some preverbal person/number morphemes are prefixes, but others are clitics. This is quite unsatisfying for an adult linguist (particularly those for whom “paradigm” is a valid theoretical concept); we would prefer an analysis in which all of these morphemes were of one kind or the other. Similarly, in Amharic we find that the compound gerund is a synthetic construction in main clauses, but an analytic in relative clauses; one might also mention English *do*-support and Irish subject agreement (cf. 5.2.3) in this context. Yet children seem to acquire these grammars all the same – sometimes as innovations.

The question we must ask, therefore, is something like this: what, for a child, constitutes the simplest analysis? To address this, we must first consider the nature of the data available to them, and immediately we have to confront the sparse data problem.

Children learning their native language have access to notoriously poor data. They do not have access to their parents' internal grammars; even information as simple as the boundaries between words is not directly provided to them. The only evidence available to them is a linear string of continuous sounds (or signs). Onto this linear string of sounds, they must somehow identify units of sound-meaning and impose a hierarchical structure on the result. Given that children are remarkably good at doing this, any errors they make must be either equally compatible with the data in their input or so very similar that the deviations which may appear in their own speech must be slight. This is consistent with the case studies we reviewed in the previous section: in each case, the difference between the grammars of Generations P and $P+I$ is quite small. We need to identify the clues that language learners are using to acquire their grammar in order to understand what allows innovators to innovate.

Since the linguistic data available to them is basically a string of sounds, it is reasonable to assume that children depend primarily on phonological data, especially when they are first identifying which chunks of sounds comprise a semantic/functional unit. In particular, prosodic information is almost certainly very valuable to them, as it allows a continuous string of sound to be broken into smaller chunks. In languages where unstressed vowels are reduced, unreduced vowels are another source of information about how strings of sound should be divided up. In naturally-occurring speech, functional elements and old information are less likely to receive word-level stress and more likely to be subject to allegro speech phenomena. These unstressed entities are also more likely to lean on adjacent lexical items.

If learners are highly sensitive to phonological information because it is their primary source of information, they may be biased towards analysing phonological words as morphosyntactic words, whenever the data allows it. The data may not always allow it: a clitic with a relatively free distribution, able to lean on virtually anything, is not likely to be analysed as part of a morphosyntactic word because there is too much information available to the contrary. But a clitic with a highly restricted position is another matter. The smaller the set of items a clitic is allowed to attach to, the greater the likelihood that it will be analysed as part of a morphosyntactic word as well as a phonological word.

Avoid Accidental Homophony, a cognitive principle introduced by Embick (2003:156), is potentially relevant here.

(3.45) *Avoid Accidental Homophony (AAH)*: Learners seek to avoid accidental homophony; absent evidence to the contrary, identities in form are treated as systematic.

The AAH biases learners against treating two identical forms as different unless they must. Suppose a learner encounters a particular form X_1 in a context Y , and an identical form X_2 in a context Z . Since X_1 and X_2 are identical, the learner would prefer that they be instantiations of the same entity. Any evidence that X_1 and X_2 are similar in meaning and function will confirm the learner in this hypothesis. Suppose that, in this hypothetical universe, there *is* a semantic or functional relationship between X_1 and X_2 : then the learner will assume they are the same beast. Now suppose that X_1 is clearly an independent M-word – it always has its own stress – whereas the analysis of X_2 is slightly more ambiguous, because it tends to lean on adjacent lexical items, and sometimes is pronounced with reduced vowels (though it is clear to learners that the reduced and unreduced versions are the same). If learners have already identified X_2 with X_1 , they will not analyse X_2 as some sort of affix, because they already know that X_1 is not an affix. However, if this situation were to change, speakers might come to a very different analysis. Suppose now that X_2 is *always* given a reduced pronunciation, and X_1 never is. The AAH is inapplicable here, since the forms are no

longer precisely phonologically identical; knowing that X_1 is an independent M-word tells the learner nothing about X_2 . In this situation, he could conclude that X_2 is an affix. Thus, the AAH has a conservative influence on the kinds of conclusions a learner might make about his grammar: he is less likely to analyse a particular form as an affix as long as he has some evidence identifying that form with an independent M-word.

Of course, the AAH is only a cognitive bias; it is not an inviolable rule. What kind of evidence might cause a learner to go against this bias? Consider, in this context, the case of Oscan *-en*: it was formally identical to a postposition which performed a very similar function, and yet it became a case suffix. Recall, however, that there had been some fusion between *en* and some of the forms to which it had been cliticised, thus making it less easily segmentable from its host. This was enough for the innovators of the Agnone dialect to analyse *-en* as a case suffix, even though in some contexts (the plural, for instance), the phonological conditions were different, and *-en* could still have been identified with the postposition – as was still true in the conservative Bantine dialects.

The nature of the data available to language learners gives primacy to phonology as a guide to the acquisition of grammar. But although phonological data may be primary, it is not always given the utmost consideration, since the mapping between phonology and morphosyntax is not perfectly correlated. The highly controversial Romance adverbial suffix *-mente* illustrates this point particularly well: in both Spanish and Portuguese (cf. Torner 2005, Vigário 2003), *-mente* bears tonic stress, and it does not seem to be subject to close phonological processes; nevertheless, its syntactic behaviour is more like an affix than a clitic.⁵⁴

Nor are cues from phonological data the only tools children have available to them. Semantics is also important. In 3.2.4, I introduced the Principle of Contrast (Clark 1987), which biases children towards finding a unique meaning for every linguistic terminal they are able to identify: ideally, there should be a one-to-one relationship between meanings and morphemes. Clark was working primarily with the acquisition of lexemes, but the MARC, proposed by Diertani and Eilam (2010), extends her insights to functional morphemes as well. The MARC appears to be active in determining e.g. whether or not a clitic prefix in Piattino is considered part of the same M-word as its host. Learners are less likely to conclude that a terminal belongs in the same M-word if it is duplicating information from another morpheme they have already identified as part of that M-word.

Piattino also demonstrated that where marking of one morphosyntactic feature on a category is present, learners are likely to mark the same feature on other items of that category where they have the means to do so. The third plural suffix in Piattino is unambiguous, unlike the third singular, but third plural prefixes are mandatory. They appear to have been affixed because when innovators concluded that the singular morphemes were prefixes, they also concluded that verbs in their grammar were marked for gender.

Returning to the questions addressed at the beginning of this section, we can now identify some of the factors that make an analysis “simpler” in the sense of the child learner. The child learner would prefer M-words to correspond as closely as possible to phonological words. She will try to minimise accidental homophonies, and therefore will identify identical forms with each other to the extent that semantics and function allow it. She will try not to duplicate information within an M-word, even if this leads to some irregularity in the system; at the same time, he may introduce redundancy if it allows for a particular feature to be marked on a category across the board.

Clearly none of these tendencies have the status of laws or absolutes; we know, for instance, that inflectional distinctions may be lost, and they may be lost piecemeal, as in the case of SAg morphemes in English. Contextual factors can and do override them, sometimes

⁵⁴ This is not uncontroversial, of course; Spanish *-mente* has been variably analysed as a derivational suffix, a phrasal suffix (Torner 2005), or a compounded element, and Vigário (2003:361n65) discusses similar controversy for the Portuguese equivalent.

in ways still inexplicable to us.⁵⁵ Nevertheless, innovations do occur in these directions, which suggest that these biases do exist.

3.4 Preconditions for Affixation

The case studies discussed at the beginning of this chapter share a number of features, in terms of the nature of the linguistic changes that occurred and the context in which they occurred. In the previous section, we discussed various factors that might influence language learners in their acquisition of a native grammar. In this section, we will examine the preconditions necessary for affixation to occur given what we have said about the nature of learner bias.

First, assuming it was originally lexical, the proto-affix must have begun to develop some sort of abstract functional or grammatical sense, regardless of its semantics.⁵⁶ These semantic changes may be indirectly responsible for the eventual formal changes, in that as a lexeme begins to acquire grammatical functions, it is likely to become de-stressed, thus introducing a contrast between it and its still-stressed lexical cousin.⁵⁷

This can be illustrated by the Amharic temporal suffix *-all*, which developed from a copula used as an auxiliary, as in Tigrinya. This is unlikely to be the original construction, however, since the ordinary gerund is a nominal form used in subordinate clauses; it does not even have aspect of its own, but is parasitic on that of the main verb with which it is used. It is easy to see how the temporal construction could have arisen, however: it is a relatively short step from ‘I am in a state of having X-ed’ to ‘I have X-ed’, with a concomitant syntactic change whereby the erstwhile subordinate verb is analysed as the main verb, and the former main verb copula reduced to a temporal auxiliary. The important point here is that the preliminary semantic and functional changes must have preceded the later reduction of *-all* from an independent auxiliary to a suffix.

Linear adjacency between a proto-affix and the incorporating M-word is the second precondition. This was an omnipresent theme in 3.2: all of the syntactic terminals later reanalysed as affixes had been adjacent to their eventual hosts, on a purely surface level, prior to their eventual affixation. If at least one element always intervened between a potential affix and its potential stem, a speaker would have no motivation for analysing two units as a single word even if such a reanalysis would be semantically appropriate. Such adjacency need not be *mandated* by the syntax, but it must be allowed at least some of the time.

The Amharic case study is particularly illustrative of this point, thanks to its contrast between relative and main clauses. Even though the relevant M-words were next to each other in relative clauses, the presence of the relative clause prefix meant that *-all-* itself is *not* linearly adjacent to the gerund. In principle, speakers could have analysed the entire sequence as a series of suffixes, but they did not; therefore, no affixation occurred here. In main clauses, by contrast, *-all-* was almost always linearly adjacent to the gerund.

⁵⁵ It is not impossible that different people have different degrees of bias, or that cognitive differences between individuals may result in identical inputs receiving different grammatical analyses by different children. In a sense, it would be surprising if this *weren't* the case, inasmuch as different people frequently diverge in their analyses of many other varieties of data.

⁵⁶ Most of the cases discussed in this dissertation involve what is traditionally considered “inflectional” morphology, but the same processes are expected to hold true in with morphology of the more “derivational” type.

⁵⁷ Lexemes do not cease to exist when they develop functional senses, but rather the lexical and functional variants quite commonly co-exist as two homophonous Vocabulary Items. The copular forebear of Amharic *-all*, for example, is still very much in usage. Often the two items continue to co-exist even after they have ceased to be formally identical.

An unanswered question is just how much linear adjacency constitutes “enough” in the relevant sense. Suppose, for instance, that a language allows the sequence Determiner-Noun, but adjectives are prenominal, so that the sequence Det-Adj-N is frequent. At what point is the frequency of Adj such that any potential reanalysis of Det as a nominal prefix is precluded? In Persian, as we saw, *-râ* was reanalysed as a dative suffix, but on the phrasal level rather than the word-level, and this is likely connected to the fact that modifiers follow the head noun in a DP. Certainly there were instances of *-râ* attached directly to a noun, but apparently these were not sufficient evidence for innovators to reanalyse this as its default positioning. Nevertheless, the fact that *-râ*, being an erstwhile postposition, was always on the right edge of the DP *was* sufficient evidence for them to reanalyse it as a phrasal dative suffix. However, there are plenty of other cases where the two items are not always linearly adjacent and yet innovators *do* apparently have enough evidence to generate a new affix. Thus, the exact level of adjacency necessary to be “enough” for innovators to conclude they have an affix is unclear.

The Scandinavian languages provide an interesting illustration of this problem.⁵⁸ Both Danish and Swedish mark definiteness via a nominal suffix, but the details are slightly different in each language. In Swedish, the suffix is always present, whether the noun is the only item in the DP (3.46a) or not. If the DP also contains an adjective, an overt determiner is also required, but the definiteness suffix remains (3.46b). If there are postmodifiers rather than premodifiers, the suffix still appears on the head noun, thus proving that the suffix is not simply associated with the rightmost element in the DP in the manner of Persian *-râ*.

- (3.46a) mus- en
mouse-DEF
‘the mouse’
- (3.46b) den gamla mus- en
DEF old mouse-DEF
‘the old mouse’
- (3.46c) gris-en med lång svans
pig- DEF with long tail
‘the pig with a long tail’

A slightly different pattern is found in Danish. Here again, solitary nouns take the definiteness suffix (3.47a), and nouns with premodifiers require an overt determiner; but when the overt determiner is present, the noun is unsuffixed (3.47b). This complementary distribution of definiteness markers is obligatory (3.47c)

- (3.47a) mand-en
man- DEF
‘the man’
- (3.47b) den unge mand
DEF young man
‘the young man’
- (3.47c) *den unge mand-en
DEF young man- DEF

Embick and Noyer (2001) suggest that the underlying syntactic structure of the Scandinavian DP is much the same: N moves to D if it is not dominated by a modifier, thus gaining a suffix; and a definite D must have a host, so that if N cannot move to D, an overt determiner is used instead. The difference between Swedish and Danish is that Swedish has

⁵⁸ The Scandinavian problem has been the subject of frequent discussion in the literature. The analysis followed here is that of Embick and Noyer (2001:580ffn.), with examples from Börjars (1998).

an additional requirement that the head N be marked definite when D is definite, and therefore gains a dissociated definiteness agreement marker post-syntactically if it fails to be thus marked in the course of the syntactic derivation. Danish requires only that definite D have a host, and therefore lacks definiteness agreement.

The Scandinavian data is useful here because it illustrates two things. First, when an adjacency relationship between proto-affix and host is frequently disrupted by other elements, analysis of the proto-affix as an affix may not be prevented outright, but the immediate outcome is not necessarily a garden-variety affix. Second, there are different strategies available; the outcome is hardly deterministic. In Danish, the determiner is affixed to the noun when the syntax permits it, and not otherwise. Swedish – which may well have gone through a Danish-like stage – has developed a type of doubling.

What is particularly interesting about the Swedish case is that – thus far, anyway – Swedish speakers did not eliminate the overt determiner upon concluding that nouns always take a definite suffix, presumably because there was so much evidence for its existence. This raises interesting questions for transmission: what led Swedish speakers to conclude that all definite nouns required overt definiteness marking, and what would it take for Swedish speakers to eliminate the overt determiner?

Similar questions are raised by the definiteness clitic in Bulgarian, also discussed by Embick and Noyer (2001:568ffn.). This clitic shows second-position effects, in that it lowers to suffix itself to the first head within the DP. If there are no premodifiers, it surfaces on the noun; when there are premodifiers, it surfaces on the first adjective (3.48b), even when the adjective is itself modified by an adverb (3.48c).

- (3.48a) kniga-ta
 book- DEF
 ‘the book’
- (3.48b) xubava-ta kniga
 nice- DEF book
 ‘the nice book’
- (3.48c) dosta glupava-ta zabeležka
 quite stupid- DEF remark
 ‘the quite stupid remark’

Bulgarian differs from Scandinavian in that the determiner is always a clitic, and never surfaces in its presumed original position as the head of D, and that, therefore, the clitic appears on adjectives as well as on nouns. Since *-ta* does sometimes appear on the noun, it is not inconceivable that future generations of Bulgarian children might decide that definite nouns should always have *-ta*, as in (3.49), but again this raises the question of what evidence they might need to do make this decision; and in fact, given the syntax of the Bulgarian DP, the most likely potential innovation is probably (3.50), where both the noun and its modifiers are marked.⁵⁹

- (3.49a) ◇ xubava kniga-ta
 (3.49b) ◇ dosta glupava kniga-ta
- (3.50a) ◇ xubava-ta kniga-ta
 (3.50b) ◇ dosta glupava-ta kniga-ta

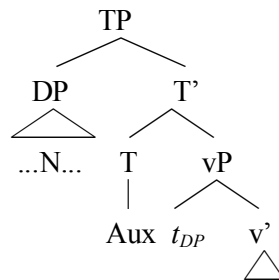
A proto-affix and its potential host need not always be linearly adjacent for affix-
 genesis to occur; this much is clear. Most of the other questions surrounding potentially

⁵⁹ Out of a desire to avoid adding to the polysemy of the diacritics already in use, particularly “*”, I’m using “◇” to mark “preconstructed” hypothetical forms.

intervening material remain unresolved. It is not clear how frequently material can intervene to preclude affix-genesis entirely, or to generate a phrasal affix rather than a head affix; nor is it clear what evidence allows innovators to restrict the set of potential interveners more stringently than their forebears. Something seems to allow the proto-affix and host to come together regardless, but it is not at all clear what this something is. Therefore, although linear adjacency is clearly essential for affix-genesis, exactly how this is arrived at – or circumvented – is still uncertain.

While linear adjacency is necessary for reanalysis, it is not itself *sufficient*, and here we need to make a distinction between the strict use of “affixation” as a structural term and the use of affixation as a descriptive term, the latter including phenomena like LD. Nouns, for instance, are relatively unlikely to acquire temporal affixes even if tense markers must always be immediately preceded by them. The sufficient conditions for affixation in the stricter sense are structural. Consider the relative position of the noun and auxiliary in (3.51).

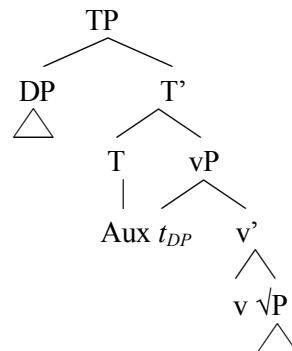
(3.51)



The noun is embedded in a specifier; the auxiliary is a head. No conceivable syntactic analyses of (3.51) would allow the auxiliary to be affixed to the noun by head movement because there are no legitimate syntactic movement operations combining elements in these two positions. Therefore, no reanalysis of the auxiliary as a suffix can occur, for structural reasons. However, it *would*, of course, be possible for T to be analysed as a post-syntactic affix via LD.⁶⁰

By contrast, auxiliaries are likely to be reanalysed as tense on the verb, as shown by (3.52).

(3.52)



In (3.52), tense and verb are both heads (and potentially linearly adjacent), and therefore they could be brought together by head movement. Two additional analyses available for the relation of the two nodes in (3.52) are linear concatenation and affixation under adjacency.

⁶⁰ For discussion of nominal TA affixes, cf. Nordlinger and Sadler (2004).

The contrast between (3.51) and (3.52) is crucial, because it explains the restrictions on the typology of affixes. As discussed above, in the view of morphosyntax adopted here, the term *affixation* is used in a structural sense to refer to the syntactic process of head-adjunction. Thus, for a structural reanalysis of an item as an affix, it must be the case that the candidate for reanalysis could be put together with its host by deeper structural principles. Linear adjacency alone is insufficient.

3.5 Retrospective

At this point, it will be useful to combine the insights provided by our case studies and the rest of the discussion from this chapter with the theoretical assumptions from Chapter Two, and thus outline a broader perspective on how languages develop new affixes. In this way, we will also address the apparent “gradualness” often attributed to grammaticalization-type changes, and reconcile the time-depth required for “gradualness” with the more punctual nature of change as described thus far.

In Chapter Two, I argued that linguistic change is non-deterministic, and may be halted or even reversed at any time. Clitics need not necessarily turn into affixes; whether they do depends on other features of the grammar, other changes in the language (phonological or syntactic), and factors external to the language. There is also an element of chance involved. The case studies discussed in this chapter all involve complete affixation, but along the way, we observed contrasting cases where no affixation occurred: the development of the Oscan locative occurred in the dialect of Agnone, but not that of Bantia; not all Piattino subject clitics became affixes; the affixation of Amharic *-all* did not occur in relative clauses, and never occurred at all in closely related Tigrinya, even though the latter has the same construction.⁶¹ Each of these examples points to the complexity of the factors involved in affixation: it is *not* a deterministic process, and it does not proceed as smoothly or as evenly as it is often described in the literature.

It *is* true that M-words that become affixes tend to have passed through various intermediate stages first. These intermediate stages are what grammaticalization theorists are speaking of when they write about “clines” of grammaticality or the “pathway” from independent lexical item to bound affix. Their error lies in the teleological assumption that each stage necessarily leads to another.

In 3.3, phonological primacy was cited as a very important factor in determining whether learners analyse a particular morpheme as a Sub-word or an M-word. Speakers tend to de-stress non-lexical forms (except in cases of emphasis), so when a former lexical item acquires grammatical functions, it is likely to develop both reduced and unreduced forms, which may co-exist for centuries, or longer. As long as speakers have the intuition that the reduced and unreduced forms are the same lexeme, they are unlikely to interpret the former as a bound affix. After several generations, however, only the reduced form may be left in a particular structural position or functional capacity, thus giving rise to a clitic.⁶² If the

⁶¹ Tigrinya also shows that linguistic forms which have acquired grammatical functions may remain phonologically unchanged independent M-words.

⁶² The finer details of the mechanism underlying this stage are complicated and somewhat problematic, in that the line between contraction and allomorphy is not well delimited. Similarly, it is not clear whether the variation between reduced and unreduced forms is on the grammatical level (along the lines of the competing grammars discussed in the work of Kroch 1994) or the phonological level (e.g. an optional reduction rule, like the rule reducing *I would* to *I'd*). Also unclear is the role of phonological processes in obscuring the link between the reduced and unreduced forms, or whether these processes are phonological at all; extreme allomorphy is something speakers are generally accustomed to (e.g. both the Old Irish verb *téit* ‘goes’ and its compound *do-tét* ‘comes’ are seven ways suppletive – and not entirely in the same ways). MacKenzie (in prep) demonstrates that auxiliary contraction in English can be sensitive to properties of the host (e.g. auxiliaries are far more likely to

reduced form of the clitic has become the underlying phonological form for speakers, it becomes a special clitic (in the terminology of Zwicky 1985).⁶³ At this point, the relationship between special clitic and the original lexeme will be etymological only; for speakers, they are now two items. This is particularly probable if the clitic has undergone further phonological changes. Then, if a new clitic meets the structural requirements for potential affixation, the set of its potential hosts may become progressively more coherent⁶⁴, so that its distribution becomes much narrower than it was previously, and the number of elements which may intervene between the clitic and its future stem is reduced. Now the probability that the proto-affix is linearly adjacent to its stem increases, and with it the potential for speakers to analyse them as a unit.⁶⁵

It is reasonable to say that each development described in the previous paragraph builds on the previous one. If the lexeme had never developed a functional usage, it is rather unlikely that it would have acquired a reduced variant. If, for whatever reason, the connection between the reduced and unreduced forms remains transparent to speakers, the unreduced form is unlikely to gain special status in the grammar. But it is *not* reasonable to say that each development ensures that the “next” development must take place. Again: changes can halt, or reserve themselves, at any stage. For instance, while the set of elements to which a special clitic attaches may narrow, it is equally plausible that syntactic changes in the grammar will cause the set of elements to *broaden*, thus reducing the likelihood of potential affixation rather sharply. The existence of preconditions for particular diachronic developments does not render a change teleological.

Recall, as well, that the structural changes involved in this type of morphosyntactic change are actually quite small: an additional movement operation, or a slightly different structural position. If the data can accommodate both analyses, there is no reason *a priori* why the children of a generation of innovators would necessarily acquire the innovative variant, thus “undoing” the change.

This brings us to the issue of “gradualness”. The changes illustrated here are both punctual and discrete: a child acquires a grammar slightly different from that of his elders. There is no room for glacial-type gradual changes under this account, yet it is true that it can take centuries for commonplace garden-variety affixes to develop. As discussed in the previous chapter, this apparent paradox is largely a consequence of identifying the change as “independent lexical item to bound functional affix” rather than recognising that what is actually happening is a series of micro-changes punctuating long stretches of stasis.

Innovations can take a long time to spread, and an innovative grammar can take generations to completely replace an older grammar. The synchronic situation is complicated by the inevitable contact between conservative speakers and innovators; it is likely that innovators will acquire the conservative dialect and become bi-dialectal. The astute reader will have noticed that, with the exception of the “toy” example of Oscan, none of the case studies discussed in this chapter are as straightforward as the usual stereotype. In fact, the only case studies cited in the literature that do not have any complicating factors are those that are completely reconstructed and, consequently, have existed as such for a very long time,

contract with pronouns than with nouns), thus showing that contraction is not a purely phonological process.

⁶³ Zwicky (1985) identifies two kinds of clitics: “simple” clitics, which result from allegro speech, and “special” clitics, which are underlyingly clitics.

⁶⁴ Cf. the selectivity criterion of Zwicky and Pullum (1983): affixes are more selective about their hosts than clitics.

⁶⁵ This could be accompanied by further phonological changes, particularly the longer the clitic remains in the language; in particular, it may cease to be a full syllable due to e.g. syncope or apocope. The affix-to-be may also become subject to word-level phonological processes like vowel harmony (and may start to affect the base as well). Phonological changes may also affect the stem-to-be. The closer the phonological relationship between the stem and affix, the tighter the impression of unity between them becomes.

possibly millennia in some instances. The examples here are of more recent origin, and therefore demonstrate that there are many more complications in affix-genesis than the standard picture allows for.

The variation resulting from the co-existence of two discrete grammars could, in principle, result in a synchronic situation which resembled some intermediate stage on a cline, but only on a superficial level. Ideally, closer examination of empirical data ought to allow for the detection of principled variation emerging in a few crucial contexts.⁶⁶ For example, it could be the case that some speakers had a grammar *P* featuring an affix-like entity *Q* which is optional in a context *C*, while other speakers have an innovative grammar *P'* in which *Q* is obligatory in *C*.⁶⁷

Hypothetically, conjunction is a good potential candidate for a diagnostic *C*, since affixes and clitics often pattern differently under conjunction. Suppose, for instance, that the item of interest is an innovative tense suffix *-T*. A speaker using grammar *P* might allow *-T* to appear on only one verb in a sequence, with the others understood to be under its scope, while a speaker using grammar *P'* might require *-T* to appear on *each* verb in a sequence to which it is relevant. Innovators may also change the contexts in which the affix is found. If the new affix is a case marker, for instance, they may, for the first time, begin to produce it on adjectives agreeing with the case-marked noun, as the speakers of the Agnone dialect of Oscan did with their new locative suffix *-en* (in contrast to the more conservative speakers of the Bantina dialect). Further innovations are possible; speakers might e.g. restrict the use of an affix to particular conjugation classes, or extend it to new environments where previously it could not occur. One of the two competing hypotheses about the origin of the Aeolic Greek consonant-stem dative plural ending *-essi* is an example of the latter type. The older ending was *-si*, and therefore the dative plural of an *s*-stem noun would have ended in *-es-si*; the hypothesis is that this was reanalysed as a single morpheme *-essi* and then extended to stems ending in other consonants.⁶⁸

Even after an innovative analysis has spread throughout a speech community, all speakers are using it, and variation has stabilised, the affix in question still may not necessarily behave like a stereotypical affix, as the examples presented in the previous section illustrate. If there is enough evidence for independence of a particular terminal somewhere in the grammar, speakers will acquire it. Affixes which give no direct syntactic evidence for ever being other than affixes (e.g. the Germanic dental preterite, the “classic” Indo-European inflectional affixes, or Semitic verbal agreement affixes, &c.) may well require centuries if not millennia to evolve as such.

The social aspects of this situation are beyond the scope of this dissertation.

⁶⁶ This variation, both within the idiolect of the individual and between speakers in the larger community, raises some interesting theoretical questions. In theories which explain diachronic affixation through inviolable universals, one might predict that speakers would be forced to choose one analysis over another, since UG ought to support only one analysis if universals are implicated. The existence of variation at the level at which it occurs seems to run counter to that prediction. On the other hand, variation is not a problem for theories in which UG is a set of parameters with toggle-settings.

⁶⁷ Assuming, of course, that textual and/or comparative evidence makes clear that the clitic grammar is the elder; cf. Chapters Two and Five for the problem of determining the direction of change in the absence of solid comparative or historical evidence.

⁶⁸ This particular innovation occurred in the Aeolic dialects (Boeotian, Lesbian, Thessalian) as well as the Delian, Locrian, and Pamphylian dialects. The alternative hypothesis is that *-essi* was formed on the nominative plural *-es*, by analogy with the vocalic stems, which had nominative plurals in *-ai/-oi* and dative plurals in *-aisi/-oisi*. Regardless of its veracity, however, the *s*-stem hypothesis is useful for illustrative purposes.

3.6 Typology of Affix-Genesis

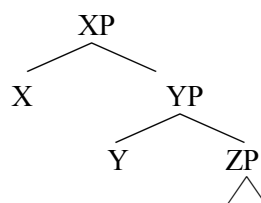
The case studies discussed in this chapter are not a homogeneous sample: although they have a number of features in common with each other, the structural details are not precisely the same in each case. However, as the definition of affixation is restricted to specific structural configurations, there are only a few logical possibilities.⁶⁹ In this section, therefore, I will construct a typology of the different structural possibilities involved in affix- genesis.

The case studies discussed in this chapter fall into three basic types⁷⁰:

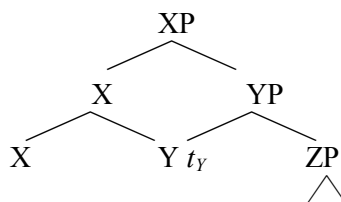
- (3.53a) *Type I*: Acquisition of an additional movement operation;
- (3.53b) *Type II*: Reanalysis of a terminal as an exponent of a different structural position;
- (3.53c) *Type III*: Extension of an M-word boundary to include a former clitic.

Type I results when innovators add an additional operation of head movement in constructing an M-word, where P-speakers had been content to stop the derivation lower in the tree. Amharic *-all* is an example of this type; it is merged in T, as it was before, but now the rest of the verb undergoes movement to adjoin to it. (3.54) schematises this situation.

(3.54a) *P-speakers' Grammar*



(3.54b) *Innovators' Grammar*



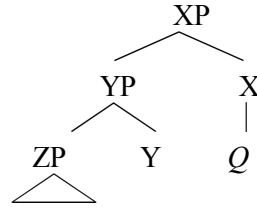
Type I is likely characteristic of verbal affixes in general, as it has been observed repeatedly (cf. e.g. Baker 1985) that verbal morphology in particular tends to reflect the order of projections customarily assumed.

The crucial difference between Type I and Type II is that Type I does not involve any changes in the structural position of the neo-affix, whereas Type II does: innovators place the neo-affix in a structural position that is taken to be within the relevant M-word. This is illustrated below, with *Q* representing the exponent affected by the innovation.

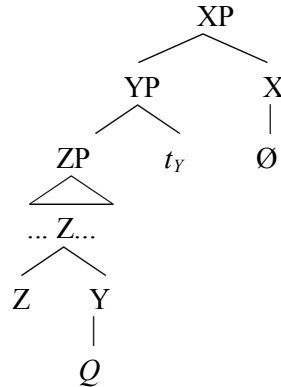
⁶⁹ Note that, owing to the strictly structural definition of *affixation* employed here, some morphemes traditionally analysed as “affixes” are not included in this typology, including those that are the products of Local Dislocation.

⁷⁰ Absent from this typology is reduplication.

(3.55a) *P-speakers' Grammar*



(3.55b) *Innovators' Grammar*



Each of the examples involving case affixes discussed in this chapter (Oscan, Persian, and Armenian) were examples of this type. It seems reasonable to suppose that case morphology is typically the result of Type II innovations, although further research is necessary to support this supposition.

The third type of innovation involves the extension of an M-word boundary to include a former clitic; this was the case in the Piattino example in this chapter, where the neo-affix is a dissociated morpheme. I have included a distinct Type III here because of the notorious complexity and variability in the syntax of clitics cross-linguistically, but ultimately this may prove unnecessary, as it may well be the case that all apparent examples of Type III can be subsumed under one of the others.

In addition to the three types of structural innovations discussed in this chapter, there is a fourth, which results from compounding and incorporation.

(3.56) *Type IV: Reanalysis of a compounded element as an affix.*

Owing to restrictions on time, I will not be discussing any cases of affixation resulting from compounding; cf., however, the discussion of the Swedish berry suffix in 7.3.2.3. Type IV innovations are nevertheless important, as they are a likely source for the varieties of morphology traditionally dubbed “derivational”, such as e.g. diminutives.⁷¹

All four types of affixation are similar in an important way: each of them involves the extension of an M-word boundary to incorporate a former M-word as a new Sub-word. Sometimes this involves a novel movement operation, other times a change in location for a terminal, and sometimes merely a change in status. Nevertheless, the end result is the same: what had been two structurally adjacent M-words in the P-speakers’ grammar are now a single M-word in the innovators’ grammar.

⁷¹ Some such reanalyses are more plausible than others; it seems unlikely, for instance, that “shark” in “bear-shark” might become a derivational suffix. But language-specific conditions, often completely unrecoverable, sometimes lead to results that are not what one might predict.

3.7 Implications for SOV Languages

If the preceding discussion is correct, there is a significant association of structural affixation with several conditions: a syntax in which the neo-affix was frequently adjacent to the M-word which engulfed it, and in which the two terminals could be legitimately combined via head movement; and a possible bias for speakers to equate morphosyntactic words with phonological words when the syntactic evidence warrants such a conclusion. Phonological cues are arguably primary in this case; even if two M-words are both linearly adjacent and plausibly combinable via head movement, if both of them are polysyllabic and clearly constitute independent phonological words, language learners have no motivation to put them together.

These observations about the ideal conditions for the innovation of affixation coincide nicely with basic intuitions regarding verbal morphology in SOV languages. It has been frequently noted (cf. e.g. Bybee et al 1990, Julien 2002) that verbal suffixes are particularly ubiquitous in head-final SOV languages. In such languages, all the inflectional projections are, naturally, piled up at the end, for structural reasons, thereby creating linear adjacency. Also for structural reasons, each of the relevant morphemes is the head of its projection, and therefore potential landing sites for verb movement. Finally, given the nature of their content, these morphemes are highly likely to be de-stressed, and thus to lean on each other phonologically.

The potential exception to this fortunate coincidence are dissociated morphemes like subject-agreement. If linear adjacency is a prerequisite for diachronic affixation, and if SOV languages tend to have subject-agreement *suffixes*, then it must be the case that, under some conditions and at some previous stage in the language's history, clause-final subjects were possible in this language. Unfortunately, documented instances of such cases are not as plentiful as one might wish.

The language most frequently cited in the literature as an example of innovative subject-agreement suffixes in an SOV language is Buryat Mongolian, which was discussed at length by Comrie (1980). At first glance, Buryat seems an ideal test case: it is an SOV language with suffixed subject agreement markers indicating non-third person. These suffixes are obligatory, as demonstrated in the paradigm in (3.58).

- (3.57a) Exe -ń xübü:- ge: daxin xile:men-de el'ge:-be.
mother-3sg son.REFL.-ACC again bread -DAT send -PAST
'The mother sent her son again for bread.'
- (3.57b) Bi damdiny-iiyi xaraa- b.
1st.sg. -ACC. see.PAST.-1st.sg.
'I saw Damdin.'
- (3.58a) Bi jaba-na- b.
1st.sg. go -PRES-1st.sg.
'I am going.'
- (3.58b) Jaba-na-b.
- (3.58c) * Bi jaba-na.

Even better, these suffixes have an obvious formal relationship with the personal pronouns, as shown in Table 3.4.

	Pronoun (Nominative)	Verbal Ending
1 st .sg.	bi	-b
2 nd .sg.	ši	-š
1 st .pl.	bide	-bdi
2 nd .pl.	ta	-t

Table 3.4: *Pronouns and Verbal Endings in Buryat*

Most fortunately of all, the Mongolian languages have an extensive written history, and the data would appear to fall out the way we would like it. As shown in (3.59),⁷² Classical Mongolian also has a basic word order of SOV, but lacks subject-agreement suffixes. Subject-agreement suffixes have not developed in all of the Mongolian languages, either (they are absent, for instance, from Khalka). Therefore, it would seem that we have in Mongolian an ideal test-case.

- (3.59) Bi tegün-i ese mede-müi.
 1st.sg. it -ACC NEG know-NARR.PRES.
 ‘I don’t know it.’

Comrie argued that the Buryat AgrS suffixes developed from sentences with a marked VS word order. VS sentences *were* allowed in Classical Mongolian, as shown in (3.60).⁷³

- (3.60a) Tere metü jalbarin ügülemüi bi.
 This like prayer say 1st.sg.
 ‘I am praying in that manner.’
 (3.60b) Inegeldüküi-yi yekin tayalamu či?
 laughter -ACC why like 2nd.sg.
 ‘Why do you like laughter?’

These postverbal subjects could co-occur with preverbal subjects, although Poppe (1954:125) gives only a single, unglossed example, reproduced here with the relevant pronouns in bold and question marks representing morphemes I am unable to gloss.

- (3.61) Čimayi **bi** ene edür minu dergede sayu geǰü ese kele-be- üü
 2s.ACC 1s.NOM ? ? 1s.GEN by sit.? ? NEG say- PAST-Q
bi.
 1s.NOM
 ‘Did I not tell you to sit by me today?’

The problems with Mongolian begin here. First, this is the only example I have found of a doubled subject in Classical Mongolian – though all the secondary sources agree that such examples exist – and it is an interrogative. Working out the syntax of these double-subject sentences from a single non-declarative example cannot be done with reliable precision. Second, there is also some question as to whether Classical Mongolian is really the most reliable source of data on this point. The Classical Mongolian period covers approximately the seventeenth to nineteenth centuries (Janhunen 2003:32), and is considered “the prototypical form of Written Mongol”. However, the history of Written Mongol dates back some eight hundred years, with the Preclassical period marked by “a greater degree of regional and individual variation” than the later texts. Moreover, Written Mongol itself is

⁷²(3.59) comes from Poppe (1954:124). Poppe, like Skribnik, translates his examples but does not gloss them; again, any errors in the gloss are mine.

⁷³ Examples from Poppe (1954); glosses are from Fuß (2005:211).

essentially a written lingua franca, much in the way of Chinese; it is used by speakers of many Mongolian languages but is the native language of none.

This passage from Janhunen (2003:30) gives some indication of the problem of using Classical Mongolian as the model for the earlier stage of Buryat.

The basic property of Written Mongol is its conservatism. During the entire duration of its use, Written Mongol has undergone only slight changes... At the same time, the spoken language has undergone intensive evolution and diversification, leading from the Middle Mongol stage to the various Modern Mongolic languages and dialects. Written Mongol has always kept a distance from the spoken vernaculars, though, at the same time, it has been influenced by them... In reality, the use of Written Mongol involves a special type of diglossia, in which the speaker of an oral form of Mongolic employs a related, but clearly distinct, idiom... It is particularly important to note that, although its recorded history dates back to the Middle Mongol period, Written Mongol was never identical with Middle Mongol. ... [T]herefore, some peculiarities of Written Mongol may well reflect the specific features of the Naiman dialect, later extinguished by the unification of the Mongols under Chinggis Khan.

Janhunen (2003:30)

From this description, Written Mongol sounds suspiciously artificial. The period of what is called Classical Mongolian falls fairly late in the history of Written Mongol; Written Mongol appears to have been based on a now-defunct dialect; and it has been affected by various vernaculars since its inception. When Janhunen later writes (p. 52) ‘syntax, and especially morphosyntax, has always been the area of Written Mongol grammatical structure that most easily has absorbed influences from the spoken language,’ the case against using Classical Mongolian as an earlier stage of Buryat is sealed. Although Janhunen (2003:31) does say that Buryat speakers are among those who employ Written Mongol, we cannot consider Classical Mongolian a reliable, plausible direct ancestor to modern Buryat.

Instead, we could turn to Middle Mongol, the language of the Mongol empire, in the thirteenth through early fifteenth centuries (Rybatzki 2003:57). Although it is known from historical documents, Middle Mongol was never used as a literary standard, and therefore is more likely to accurately reflect a historical vernacular than Written/Classical Mongol.

The problem with Middle Mongol, however, is that there has been rather little work published on its grammatical structure, and the available descriptions are not very comprehensive. The only full-length treatment of Middle Mongolian grammar, Street’s (1957) dissertation, has *no* (!) discussion of sentences with verbal predicates. Rybatzki (2003:78) discusses syntax only briefly; he says that Middle Mongol allowed for freer word order than either Written Mongol or the modern languages, but gives no examples. Nor does he give examples of sentences with enclitic first- or second-person subjects, though he says they existed and are written with the verb in late Arabic sources.

Therefore, although it may well be the case that Middle Mongol *does* contain important clues to the development of AgrS suffixes in Buryat, it has not yet been made available by current scholarship⁷⁴, and a thorough study of the depth required lies far beyond the scope of this dissertation.

Before abandoning the subject of Buryat, one misunderstanding of the crucial Buryat data should first be cleared up. Although it is true that modern Buryat (and several other Mongolic languages) has developed AgrS suffixes absent from either the written classical language or Middle Mongol, these AgrS suffixes, contrary to the impression given by the linguistic literature, are in fact *not exclusively verbal suffixes*. Rather, they are used on all predicates, be they verbal, nominal, or pronominal. The examples in (3.62) are taken from Skribnik (2003:120), who says that adverbs, numerals, and inflected nouns can also take these

⁷⁴ With the possible exception of some work done in Russian.

suffixes (though her examples do not show this directly because they are mostly third singular).

(3.62a) Bi bagsha-b.
1S teacher-1S
'I am a teacher.'

(3.62b) Bagsha bi-b.
teacher 1S-1S
'I am the teacher.'

In short, despite its many intriguing properties, Mongolian cannot be used as evidence for the development of AgrS suffixes in SOV languages.⁷⁵ It does, however, raise some interesting questions about the development of these suffixes: do they often originate as suffixes on all predicates, verbal or otherwise? If so, what evidence motivates language learners to later restrict them to verbal predicates only? If not, what properties of the specific language influences the analysis of the language learners?

It is the last question that is of particular interest to us here, and returns us to the primary topic of this chapter. Since diachronic affixation is not deterministic, language-specific (and possibly speaker-specific) properties and biases play a crucial role in the changes that can occur when a language is transmitted to a new generation of speakers. In this chapter, I have identified a number of linguistic properties which correlate with novel affixes, but there is a crucial ingredient missing: why, when all of the necessary ingredients for innovation may have been present in a language for centuries, does innovation happen in a particular time and with a particular speaker or speakers? This is, of course, the actuation problem, the most recalcitrant issue in historical linguistics. The discussion in this chapter has not solved the actuation problem, but I hope that it has identified more clearly the context in which the problem is set.

3.8 Chapter Summary

I have argued in this chapter that novel affixes result when an innovative language learner makes an analytic decision that results in a single M-word in an area of his grammar where previous generations of speakers had two M-words. These analytic decisions fall into several basic types:

- (3.63a) *Type I*: Acquisition of an additional movement operation;
- (3.63b) *Type II*: Reanalysis of a terminal as an exponent of a different structural position;
- (3.63c) *Type III*: Extension of an M-word boundary to include a former clitic;
- (3.63d) *Type IV*: Reanalysis of a compounded element as an affix.

All of these types are similar in one respect: each new analytic decision involves a morpheme boundary. For Types I–III, the relevant boundary is an M-word boundary, and each of these types ultimately results in the extension of an M-word boundary to include Sub-words that previously constituted a different M-word – in other words, by introducing a new Sub-word into a complex head. Type I achieves this by adding a new movement rule; Type II by replacing a null exponent within the M-word with what was previously an independent M-word, but without adding any new movement operations. It may be possible to subsume Type III into the other two categories.

⁷⁵ The same conclusion was reached by Muriel Norde; cf. Norde (2009).

Type IV has not been dealt with in this chapter, because it involves morpheme boundaries in a different way: here, the nature of the boundary itself is analysed differently. This type of affix-genesis arguably has more in common with the phenomena dealt with in Chapter Seven than with Types I–III.

One of the important points to take away from this chapter is that although surface phenomena do play an important role in affix-genesis, the actual changes themselves are structural. Non-structural linguistic factors which contribute to the learner's analysis include linear adjacency and phonological factors such as stress, vowel reduction, and segmental changes due to contact at morpheme boundaries. These are extremely important, since the only direct evidence available to a language learner is linear strings of sounds. But the analysis itself is an analysis of a structural nature: the learner must decide where M-word boundaries lie in his language. If his ultimate conclusion about M-word boundaries includes one more Sub-word than the grammar of earlier speakers, a novel affix has been created.

This view of affix-genesis is consistent with the idea that language change is not continuous but proceeds in punctuated equilibria. The illusion of continuous change results from the fact that each new development *does* depend on input from an immediately previous grammar (which in turn develops from the context of the one before it); nevertheless, language learners only have direct evidence from the speech of those around them and are blind to historical developments that may have occurred centuries before. The result is a succession of (synchronic) grammars; there are no underlying diachronic "principles".

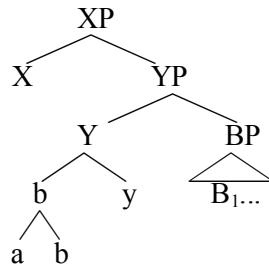
One of the concepts frequently invoked in motivating linguistic change is a sort of desire – on the part of the language, or the speakers – for the language to become as "simple" as possible. "Simple" is not a particularly well-defined metric, but various definitions have been put forth, such as the desire to eliminate layers of structure or reduce the number of words. The contention in this dissertation is that "simplicity" is not really a relevant notion in the domain of linguistic change. Rather, language change and transmission operate in the way they do because phonological reduction exists and plays a role in determining a speaker's analysis, and because communication never proceeds quite as perfectly as human agents assume it does.

The discussion thus far has virtually ignored post-syntactic affixation, but this is an omission that should be remedied. Since linear adjacency is an essential prerequisite for affix-genesis, and since the crucial difference between affixation by head movement and affixation under adjacency is that the former is subject to structural conditions and the latter to strictly local, linear conditions, the idea that a chronologically intermediate period of LD in the etymological history of an eventual affix makes a certain intuitive sense. Furthermore, this intuition is consistent with the Uniformitarian Principle: since we observe instances of LD in modern languages under study, we must assume that it was present in earlier languages as well.

The plausibility of such a development increases when we examine the mechanics of LD operations. Embick (2007) schematises LD as follows, starting from the structure (3.64).⁷⁶ After syntax, linearization mechanisms apply to (3.64), arranging the M-words in the derivation in a linear order with respect to each other (3.65a), as well as the various Sub-words within the M-word Y (3.65b), again with respect to each other. These linearization mechanisms first convert hierarchical representations ((i) examples) to statements of headedness and left adjacency ((ii) examples, with the *-notation), and finally to concatenation statements ((iii) examples; \wedge for M-word concatenation and \oplus for Sub-word concatenation).

⁷⁶ This presentation is simplified; for the full derivation cf. Embick (2007).

(3.64)



- (3.65a) i. [X [Y BP...
 ii. (X * (Y * BP, ...
 iii. (X ^ Y), (Y ^ B₁)

- (3.65b) i. [[a b] y]
 ii. ((a * b) * y)
 iii. (a⊕b), (b⊕y)

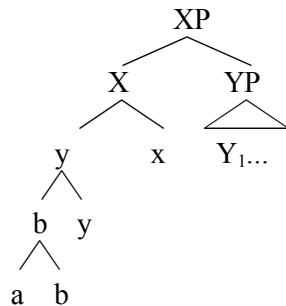
LD can apply where two M-words have been concatenated, replacing the concatenation with new hierarchical information adjoining X to Y (3.66). As a result, X becomes a Sub-word within the M-word Y, and therefore the Sub-words of Y are re-concatenated (3.67).

$$(3.66) \quad X \wedge Y \rightarrow \quad [[Y]X]$$

- (3.67) ii. (((a * b) * y) * X)
 iii. (a⊕b), (b⊕y), (y⊕X)

Compare this derivation with (3.68), in which head-movement adjoins Y to X. The underlying structure is very different, but the surface linear order is the same despite the fact that x is now a Sub-word on the structural level and X, rather than Y, is the projection to which the complex head has moved.

(3.68)



The principal difference between syntactic and post-syntactic affixation is the stage at which the operation takes place. It would be fairly trivial for an innovator to interpret a conservative speaker's post-syntactic adjunction as syntactic, particularly if the M-words involved were almost always linearly adjacent to begin with; the primary empirical difference would be that, for the innovator, the operation would be mandatory throughout the grammar, while the P-speakers' grammar might contain tokens in which a disruption of adjacency results in a clear non-adjunction.

Both intuitively and mechanically, then, there is a plausible historical connection between LD and affix-genesis. The problem lies in finding clear empirical evidence in which an LD grammar must have chronologically preceded a grammar with syntactic affixation in

the history of the same language. None of the case studies discussed in this chapter qualifies: each has its own peculiarities, but in every instance a sequence of two M-words becomes a single M-word with a demoted former M-word now included as a Sub-word on the periphery of the surviving M-word. Given the theoretical assumptions of DM, this is precisely what we would expect. But not all affixes are found on the periphery, and not all affixes are quite so straightforward.

In 3.2.5, we saw that the Amharic compound gerund suffix *-all*, despite its clear behaviour as a temporal suffix, is still very much an independent M-word in relative clauses, complete with relative prefix and the AgrS suffix (mostly) absent from its main-clause incarnation. How should *-all* be classified: is it an affix, or isn't it? This case is fairly simple: the status of *-all* as Sub-word or M-word is strictly determined by properties of the grammar. Therefore, *-all* is not an affix in relative clauses, but it is in main clauses.

By contrast, recall that many of Leslau's speakers allowed the optional intervention of two particles between the gerund and *-all* (Leslau 1995:388), as was shown in (3.39), reproduced below for convenience:

- (3.69a) Nägr- ^{w-} all- əmm
 tell.GER-3rd.sg.MASC.-AUX-CONJ
 'And he has told.'
- (3.69b) (*) Nägr- o- mm- all
 tell.GER-3rd.sg.MASC.-CONJ-AUX
- (3.69c) Nägr- ^{w-} all- əss
 tell.GER-3rd.sg.MASC.-AUX-FOC
 'He has indeed told.'
- (3.69d) (*) Nägr- o- ss- all
 tell.GER-3rd.sg.MASC.-FOC-AUX

Here, there is no clear structural criterion; this looks very much like an instance of genuine variation. Do we say that dialects permitting intervention, *-all* is not "really" an affix? The clearest analysis of this situation takes advantage of the competing grammars model, so that the particle intervention examples represent an older grammar with clitic *-all* while the others are generated in a newer grammar with affixal *-all*.

The older grammar raises some important questions, because it is ambiguous in an interesting way: what is the underlying structure of sentences without particles? There are multiple possibilities; the list in (3.70) is not intended to be exhaustive. Note that e.g. (3.70c-d) invoke multiple grammars with no consequences for surface order.

- (3.70a) *-all* is always a clitic, regardless of the presence of particles;
- (3.70b) *-all* is always a clitic in the presence of particles, but when there is no particle, it is *always* subject to Local Dislocation;
- (3.70c) *-all* is always a clitic in the presence of particles, but when there is no particle, it is *sometimes* subject to Local Dislocation (and other times remains a clitic);
- (3.70d) is always a clitic in the presence of particles, but when there is no particle, it is *sometimes* subject to Local Dislocation (and other times there is head movement).

Similar questions can be raised in the context of a more familiar example that does not exhibit synchronic variation. The future and conditional tenses in Romance languages are of more recent origin than the rest of the synthetic verbal forms; they developed from analytic constructions consisting of the infinitive with the auxiliary *habeō* 'have'. The original analytic construction is preserved in a few languages, including Sardinian and Sicilian, but most of the modern Romance languages (including French, Spanish, Italian, and Brazilian

Portuguese) have lost syntactic evidence for the original construction. European Portuguese stands somewhere in between: it allows mesoclis, in which clitic clusters composed of pronominal verbal objects are allowed to intervene between the future or conditional tense marker and the subject agreement, as shown in (3.71).⁷⁷

- (3.71a) mostra-r- emos
 show- FUT-1st.pl.
 ‘We will show’
- (3.71b) mostra-r- lho- emos
 show- FUT-3rd.sg.MASC.ACC./3rd.sg.MASC.DAT-1st.pl.
 ‘We will show it to him’
- (3.71c) queixa- r- nos- emos
 complain-FUT-1st.pl.REFL-1st.pl.
 ‘We will complain’

Mesoclis occurs *only* with the future and conditional tenses. The corresponding present tense forms show enclisis in this environment.

- (3.72a) mostra-mos
 show- 1st.pl.
 ‘We show’
- (3.72b) mostra-mos= lho
 show- 1st.pl.- 3rd.sg.ACC./3rd.sg.MASC.DAT
 ‘We show it to him’
- (3.72c) queix- amo= nos
 complain-1st.pl.-1st.pl.REFL
 ‘We complain’

What is the status of the European Portuguese future and conditional, and is there covert structural variation imperceptible to us because of its string-vacuous nature? I will use the term *quasi-affix*, defined in (3.73), as a general descriptive term for these erratic or irregular affixes; this is not intended as a technical term, as the phenomena to which it may be applied may be attributed to very different structural properties. From the perspective of a linguist, quasi-affixes can be difficult to analyse, as they appear to constitute systematic irregularities; but for native speakers, they appear not to pose a problem, although they may not show long-term stability.

- (3.73) *Quasi-affix*: a linguistic terminal variably exhibiting both M-word or Sub-word behaviour

The contention here, and elsewhere in the literature, is that affix-genesis requires linear adjacency in at least some contexts. Quasi-affixes such as those discussed here demonstrate that adjacency is not required in *all* contexts; by doing so, they also raise interesting questions about when, why, and how innovators decide to “ignore” apparent evidence for intervening elements – as eventually happened in both Amharic and the majority of Romance. They may also shed some light on the diachronic status of LD. These more difficult examples, and others, will be discussed in the following chapter.

⁷⁷ Data taken from Luís and Spencer (2004).

Chapter Four

Further Points of Interest in Affix-Genesis

4.1 Overview

Chapter Three dealt with the underlying principles and overarching generalisations about affix-genesis, illustrated by five relatively straightforward examples. Not all examples are quite so simple, and these more complicated cases raise interesting questions and challenges which any theory of affix-genesis must be able to address. The purpose of this chapter is not to provide answers to all of these questions; in many instances we simply do not have the necessary information. My goal here is rather to address the issues insofar as the data allow, and show how these issues intersect with the larger themes of this dissertation.

This chapter is divided into two broad sections, each with several sub-topics. Section 4.2 is concerned with the locus of affixation. The theory presented in the previous chapter predicts that a new affix should occur in the same linear position in which it occurred prior to achieving affixhood, which implies that new affixes should occupy a peripheral position within the M-word. This is not always the case, however, and the deviations from the expected pattern are both interesting and revealing.

Section 4.3 centres upon the problem of redundancy within an M-word. It is generally held that new affixes are created in part to encode some new information inside an M-word; however, various types of multiple exponence do exist, which raises some interesting questions about the motivation for creating redundancy in the face of conventional wisdom about the desire for economy in language.

4.2 Interpolation

One of the key themes of previous chapters has been that innovators do not set out to make radical changes to the language they are acquiring; they come up with a hypothesis that suits the data as they perceive it, and it happens to be the case that their hypothesis is novel. This means that, although the structural nature of a new affix has changed, its linear position relative to the M-word that has incorporated it has *not* changed. As a result, a non-lexicalist piece-based theory predicts that new affixes will appear on the periphery of their new M-word home.

In the case of garden-variety affixation, this prediction is largely borne out; in all of the case studies from the previous chapter, for instance, affixation occurred on the periphery of the relevant M-word. Setting aside the actual details of their structure, compound verbs in Classical Greek – that is, verbs with an adverbial prefix – are another example. In Homeric Greek, the entities which became prefixes were still independent adverbial particles which could occur at some distance from the verb, but at some point after Homer, the particles lost their independence and became part of the verb. As one would expect, these prefixes are positioned further from the root than tense/aspect prefixes, which include the reduplicating perfect prefix and the so-called “augment”, a prefix occurring with indicative past tenses. This is illustrated in (4.1) and (4.2). (4.1) shows the present, aorist, and perfect tense forms of the simplex verb *bállō* ‘throw’, while (4.2) shows the corresponding forms of the compound verb *eisbállō* ‘throw into’.⁷⁸

⁷⁸ The adverbial prefix *eis* appears elsewhere in Greek as the preposition ‘into’.

- (4.1a) *Present:* bál- l- ō
 throw-PRES-1st.sg
- (4.1b) *Aorist:* é- bal- on
 AUG-throw-1st.sg.
- (4.1c) *Perfect:* bé- blē- k- a
 REDUP-throw-PERF-1st.sg.
- (4.2a) *Present:* eis- bál- l- ō
 into-throw-PRES-1st.sg.
- (4.2b) *Aorist:* eis- é- bal- on
 into-AUG-throw-1st.sg.
- (4.2c) *Perfect:* eis- bé- blē- k- a
 into-REDUP-throw-PERF-1st.sg.

Although some of these prefixes later migrated closer to the root (cf. Chapter Seven), the crucial point is that their initial location in the verbal complex was peripheral. This point may seem trivial at first glance; it would be extremely odd for a language to spontaneously develop infixes. Nevertheless, there are two important considerations to take into account here. First, the (initial) peripherality of affixes is crucial to the theoretical assumptions made here. The present conception of morphosyntax requires that each morpheme be positioned syntactically, while the implications of the previous chapter require morphosyntactic changes to occur on a structural level rather than in a linear string. The same is not true on a lexicalist theory of morphology, where words are assembled in a separate lexicon prior to their insertion in syntax. Under a lexicalist theory, once a word has acquired a new affix, the affix could be positioned anywhere within the word; it could, for instance, be placed in such a way as to allow for optimal phonotactics. It is harder to make precise predictions about the locus of affixation in a lexicalist theory without stipulation, whereas in DM the predictions fall out automatically.

That said, there *are* well-known examples of infixes or what appear to be interpolated clitics within M-words; these must arise from somewhere, and we must be able to not only accommodate them within the present framework, but be able to account for how they came to be permitted in the first place.

In this section, I will show that infixes and interpolated clitics are in no way incompatible with the framework of language change advocated here. I will begin with the clearest cases, which involve clitic interpolation within an M-word, sometimes called *mesoclysis* in the context of European Portuguese. Section 4.2.1 will look at examples from Lithuanian, European Portuguese, and Old Irish, all of which involve clitic interpolation, though the outcome of each case is rather different. I will argue that clitics can come to be trapped when the relationship of the elements around them changes.

The phenomena discussed in 4.2.1 can all be described as infixation, but of a morphological type: Sub-word boundaries are respected. The classic case of infixation is phonological, rather than morphological; this results when a prefix or suffix interpolates within another Sub-word for phonological reasons. All of the attested examples I know of are ancient, dating back to the earliest reconstructed stages of their language family, which means that unravelling the circumstances under which phonological infixation becomes possible is extremely difficult. In 4.2.2 I will discuss some of the issues involved, as well as one of the possible fates of an infix: reanalysis as the output of a readjustment rule.

Finally, 4.2.3 deals with the possibly-unique case of endoclysis in Udi, a Nakh-Dagestanian language which allows subject clitics to occur in multiple places within a clause and within the verbal complex, up to and including inside the verbal root itself. In other words, Udi subject clitics behave sometimes like morphological infixes, sometimes like phonological infixes, and sometimes like mundane clitics.

4.2.1 Morphological Infixation

Most familiar clitics cliticise to the left or right of an M-word, but some clitics can also occur *inside* an M-word. This is a type of morphological infixation, sometimes referred to as *mesoclisys* in the context of Romance. For a theory of morphology which assembles words in a separate lexicon, morphological infixation is a serious problem, because it challenges notions of lexical integrity: if a word is a syntactic atom, then nothing should be allowed to permeate it. Theories like Distributed Morphology are less troubled by morphological infixation, since words are built up in the syntax; there is nothing to stop a clitic from ending up inside an M-word if the structure of the clause places it there.

The analysis of Lithuanian reflexives in Embick and Noyer (2001) and Embick (2007) shows how morphological infixation might be accounted for within DM. Lithuanian has a reflexive affix *-si*. When *-si* is added to a simple verb, it appears as a suffix (4.3a), but with compound verbs (similar in nature to the Greek verbs briefly introduced in 4.1), *-si* appears between the prefix and the verb (4.3b). If there are two prefixes, *-si* is placed between them (4.3c), and if the verb is negated by the prefix *ne-*, *ne-* precedes all of the preverbs with *-si* to its immediate right (4.3d).

(4.3a)	laikaũ 'I consider, maintain'	laikaũ- si 'I get along'
(4.3b)	iš-laikaũ 'I preserve, withstand'	iš- si -laikaũ 'I hold my stand'
(4.3c)	su-pa-žinti 'to know [someone], recognise'	su- si -pa-žinti 'to become acquainted with'
(4.3d)	ąš ne-lenkiù 'I do not bend'	ąš ne- si -lenkiù 'I do not bow'

Essentially, *-si* is a second-position suffix. It is always suffixed to the first verbal prefix; if there is no such prefix, it is suffixed instead to the verb, after the inflection. Although it is not allowed to intervene between the verb and the inflection, it *is* allowed to penetrate the M-word.

Embick and Noyer (2001:579) assume that preverbs are adjoined to V, with the resulting complex moving first to Neg, when it is present, and then further to T (4.4a). They analyse *-si* as a dissociated morpheme which left-adjoins to the highest segment of the M-word containing V and then undergoes LD so as to be *right*-adjoined to the leftmost Subword in the verbal complex (4.4b).

$$\begin{aligned}
 (4.4a) \quad & [_{\text{TP}}[\text{Neg}_1 + [\text{Pr} + \text{Pr} + \text{V}]_2] + \text{T} [_{\text{NegP}} t_1 [\text{VP} \dots t_2]] \\
 (4.4b) \quad & [-si * [\text{Pr} \dots \text{V} * \text{T}]] \quad \rightarrow \quad [[\text{Pr} \oplus si \dots \text{V} * \text{T}]]
 \end{aligned}$$

One further step in the derivation is necessary to eliminate the incorrect prediction that *-si* would be between the verb and inflection when there are no prefixes. Embick and Noyer suggest that T undergoes string-vacuous LD, forming a complex Sub-word which henceforth behaves like a single Subword for purposes of positioning. Since LD is predicted to behave cyclically, building from lower to higher, the LD involving V and T precedes that involving *-si* according to that principle, so that *-si* occurs in the expected position suffixed to the entire [V*T] unit.

An interesting diachronic question arises in this context, as alluded to at the end of the previous chapter. Linear adjacency may be required for speakers to create a new affix, but clitics sometimes seem not to “count” in this context; that is, clitics are sometimes allowed to regularly intervene between proto-affix and proto-stem. Why is this? Moreover, if this gives rise to a situation in which a particular terminal behaves sometimes as an M-word and other times as a Sub-word, why are speakers happy to tolerate this inconsistency?

In this section, two case studies involving trapped clitics are discussed: European Portuguese (4.2.1.1) and Old Irish (4.2.1.2). In both cases, the clitics in question are object pronouns; however, as will become clear, the eventual fates of these clitics are quite different. In the Romance languages, mesoclitics were generally eliminated in favour of the clitic patterns prevalent with other verb forms; they were a systemic anomaly, in a sense, and their presence was a throw-back to an archaic grammar before affixation occurred. The Old Irish case is somewhat different. Here, the clitics seem to have been incorporated into the verbal complex, with very complicated results.

4.2.1.1 *European Portuguese*

As discussed at the end of Chapter Three, the synthetic future and conditional forms found in the various Romance languages developed from an earlier analytic construction involving an infinitive and the inflected auxiliary *habēre* ‘to have’. In modern European Portuguese, the future and conditional verb forms still allow pronominal object clitics to interpolate between the two pieces, as shown in (4.5). In fact, not only are the clitics *allowed* to appear in this position, but they *must* appear there. This is in contrast with the rest of the verb forms in this language, which take enclitics under these conditions, as in (4.6). This description of the data leaves out a number of important details, but will be elaborated further shortly.

- (4.5a) mostra-r- emos
show- FUT-1st.pl.
‘We will show’
- (4.5b) mostra-r- lho- emos
show- FUT-3rd.sg.MASC.ACC./3rd.sg.MASC.DAT-1st.pl.
‘We will show it to him’
- (4.5c) queixa- r- nos- emos
complain-FUT-1st.pl.REFL-1st.pl.
‘We will complain’
- (4.6a) mostra-mos
show- 1st.pl.
‘We show’
- (4.6b) mostra-mos= lho
show- 1st.pl.-3rd.sg.ACC./3rd.sg.MASC.DAT
‘We show it to him’
- (4.6c) queix- amo= nos
complain-1st.pl.-1st.pl.REFL
‘We complain’

The problem of Portuguese is not that of peripherality; the affixed auxiliaries do appear as the outermost suffix in the future and conditional, exactly as predicted by this account. But these forms present three other problems: first, the more general problem of quasi-affixation; second, that the differing position of the clitics suggests that the future/conditional forms in (4.5) either have a different internal structure than those in (4.6), or do not have internal structure, counter to the claims of the theoretical framework assumed here; and third, issues concerning linear adjacency more broadly. These problems are discussed in the following sub-sections, which deal briefly with what is known about the history of these forms (4.2.1.1.1), the problem of their synchronic analysis (4.2.1.1.2), and their broader implications (4.2.1.1.3).

- (4.9b) There is only one grammar, which generates sentences with the two pieces linearly adjacent some of the time and not others; whether they are written as one word or two reflects orthography rather than grammar.

Spitzer (1918) pointed out that only in Northern Gallo-Romance – that is, the ancestor of modern French – are analytic-type forms never attested amongst the western Romance languages: all dialects of Italy, Iberia, and Occitania show analytic forms at some point in their history. Moreover, all of these languages show a similar construction in which the infinitive is construed with a preposition at some point in their history, as does French, which otherwise does not allow analytic futures. The following examples are taken from Fleischman (1982:73).

(4.10a) <i>Spanish</i>	hè de cantar	‘I am to sing’
	AUX.1 st .sg. sing.INF	
(4.10b) <i>Italian</i>	ho da cantare	‘I am to sing’
	AUX.1 st .sg. sing.INF	
(4.10c) <i>French</i>	j’ ai à travailler	‘I have work to do’
	1 st .sg. AUX.1 st .sg. work.INF	

Mesoclitic forms, meanwhile appear in Old Italian, Old Occitan (traces persist in Middle Occitan), and pre-seventeenth century Spanish; there are also a few examples in Old Catalan. Fleischman cites the following forms; the modern equivalents are in brackets.

(4.11a) <i>Old Spanish</i>	dar le has	[le darás]
	give MASC.SG.DAT. AUX.2 nd .sg.	
	‘You will give to him.’	
(4.11b) <i>Old Catalan</i>	trobar-s’ich-a	[s’hi trobarà]
	find- REFL-AUX	
	‘It will be found there.’	
(4.11c) <i>Old Occitan</i>	donar lo t’ ai	[te lo donarai]
	give.INF MASC.SG.ACC. 2 nd .sg. AUX.1 st .sg.	
	‘I will give it to you.’	

Clitics could co-occur with prepositions in Spanish, Italian, and Portuguese, with two different potential word orders: the clitic could precede the auxiliary or be suffixed to the infinitive, as in (4.12). This construction appears to have been semantically distinct. Spanish also allowed the clitic to follow the auxiliary (4.13); this was also possible in Italian.

(4.12a) <i>Italian</i>	ho ancora da veder- lo	
	AUX.1 st .sg. still PREP see.INF-3 rd .sg.MASC.	
	‘I still have to see him.’	
(4.12b)	l’ho ancora da veder	
(4.12c) <i>Portuguese</i>	ha de fazê- lo	
	AUX.3 rd .sg. PREP do.INF-3 rd .SG.MASC.	
	‘He is to do it.’	
(4.12d)	o ha de fazer	
(4.12e) <i>Spanish</i>	ha de mandar- lo	
	AUX.3 rd .sg. PREP send.INF-3 rd .sg.MASC.	
	‘He is to send it.’	
(4.12f)	lo ha de mandar	

- (4.13) a lo a decir
 AUX.3rd.sg. 3rd.sg.MASC. PREP say.INF
 ‘He is to say it.’

The point is that even a brief sketch of the history of these forms, such as the present one, reveals a great deal of complexity and analytic uncertainty. In fact, the status of mesoclis in modern European Portuguese is itself more than slightly uncertain. They appear primarily in educated, literary registers and thus are likely to be artificial. Duarte and Matos (2000:117) give examples of enclisis replacing mesoclis in the language of speakers who have acquired the pattern incompletely (or, perhaps, not at all). (4.14a) was produced by a twelve-year-old sixth-grader, and (4.14b) was submitted in a written exam for admission to university. The relevant clitics are italicised; the grammaticality indications are those of Duarte and Matos.

- (4.14a) ? Telefonar-ei- *te* mais vezes.
 phone- FUT.3rd.sg.-2nd.sg.DAT. more often
 ‘I shall call you more often.’
- (4.14b) ? Na conjuntura sócio-económica, pode-rá- *se* verificar um
 in.the situation socio-economic may- FUT.3rd.sg.-REFL. obtain.INF a
 saldo bastante positivo.
 very balance positive
 ‘Given the socioeconomic situation, a very positive balance may well obtain.’

The precise life status of modern European Portuguese mesoclitics may be uncertain; their synchronic analysis is even more so, as we will see in the next sub-section.

4.2.1.1.2 The Synchronic Status of Mesoclis in Modern European Portuguese

In this section, I provide a general introduction to the problematic status of mesoclis in modern European Portuguese, which requires a discussion of the rules of clitic placement in this language. Though descriptively the facts are clear, there is nothing resembling consensus among experts in Portuguese syntax as to the proper syntactic analysis of this data. In fact, as we will see, there is even contention over whether Portuguese clitics are proclitics or enclitics by default. However, even scholars with dramatically different analyses do agree on one point: Portuguese forms with mesoclis have a different status from those without it. This supports the approach to mesoclis argued for in this dissertation.

The description of European Portuguese outlined below is based on that of Duarte and Matos (2000). The default position of a clitic pronoun in this language is enclisis; as discussed above, mesoclis appears as an obligatory variant of enclisis when the verb is in the future or conditional tense. Unlike most other Romance languages, this is true in both finite and non-finite clauses; contrast the Italian forms in (4.15) with the corresponding Portuguese in (4.16).⁷⁹

- (4.15a) *Lo* conosco.
 (4.15b) Vorrei conoscere-*lo*.
- (4.16a) Ele viu- *a*.
 3rd.sg.MASC. see.PAST-3rd.sg.FEM.ACC
 ‘He saw her.’

⁷⁹ All examples are from Duarte and Matos (2000), unless otherwise noted; the glosses are also theirs, with minor cosmetic changes to conform to the practice of this dissertation. Clitic pronouns continue to be italicised.

- (4.16b) O João pens- a vê- la mais tarde.
 DEF intend.PRES-3rd.sg. see.INF-3rd.sg.FEM.ACC later
 ‘João intends to see her later.’

However, Portuguese also requires proclisis in certain contexts, namely when an operator precedes and c-commands the verb. Such triggers include sentential negation (4.17), negative phrases (4.18), overt complementizers (4.19), *wh*-operators (4.20), quantified subject NPs (4.21), focused constituents (4.22), and certain adverbs (4.23). In the following set of examples, the (a) sentences show that proclisis is grammatical, and the (b) sentences show that enclisis is not. (4.17c–d) show that the future and conditional verbs show the same pattern. The underlined element is the trigger for proclisis.

- (4.17a) O João não o comprou.
 DEF NEG 3rd.sg.MASC.ACC buy.PAST.3rd.sg.
 ‘João didn’t buy it.’
- (4.17b) * O João não comprou-o.
- (4.17c) O João não o comprar-á.
 DEF NEG 3rd.sg.MASC.ACC buy.FUT-3rd.sg.
 ‘João will not buy it.’
- (4.17d) * O João não comprá-lo-á.
- (4.18a) Ninguém se lava sem sabonete.
 nobody REFL wash.PRES.3rd.sg. without soap
 ‘Nobody washes himself without soap.’
- (4.18b) * Ninguém lava-se sem sabonete.
- (4.19a) Eles dissera- m que os amigo-s *lhes* dera- m
 3rd.pl. say.PAST-3rd.pl. COMP DEF friend- PL 3rd.pl.DAT. give.PAST-3rd.pl.
 livro-s.
 book-PL
 ‘They said their friends gave them books.’
- (4.19b) * Eles dissera-m que os amigo-s dera-m-*lhes* livro-s.
- (4.20a) Que mentira *lhe* contaste?
 which lie 3rd.sg.DAT tell-2nd.sg.
 ‘Which lie did you tell him/her?’
- (4.20b) * Que mentira contaste-*lhe*?
- (4.21a) Todos os aluno- s se rira- m.
 all DEF student-PL REFL laugh.PAST-3rd.pl.
 ‘All the students laughed.’
- (4.21b) * Todos os alunos riram-se.
- (4.22a) Até a ele *lhe* contara- m (elas) mentira-s.
 even to 3rd.sg.MASC. 3rd.sg.DAT tell.PAST-3rd.pl. (3rd.pl.FEM.NOM.) lie- PL
 ‘They told lies even to him.’
- (4.22b) * Até a ele contara-m-*lhe* (elas) mentira-s.
- (4.23a) Ele também o leu.
 3rd.sg.MASC. also 3rd.sg.MASC.ACC. read.PAST-3rd.sg.
 ‘He read it also.’
- (4.23b) * Ele também leu-o.

The class of triggers does not include all logical possibilities; that is, some lexical items that might be predicted to be triggers for proclisis, based on their semantic properties, are not triggers. Contrast the behaviour of *muitas vezes* ‘often’ in (4.24a–b) with *raras vezes* ‘seldom’ (4.24c–d), both apparently quantified phrases. Proclisis is mandatory for the former, but enclisis is mandatory for the latter.

- (4.24a) O João muita-s veze-s dá- *me* razão.
 DEF many-PL time-PL give.3rd.sg.-1st.sg.DAT reason
 ‘João often agrees with me.’
- (4.24b) * O João muitas vezes *me* dá razão.
- (4.24c) * O João rara-s veze-s dá- *me* razão.
 DEF rare-PL time-PL give.3rd.sg.-1st.sg.DAT reason
 ‘João seldom agrees with me.’
- (4.24d) O João raras vezes *me* dá razão.

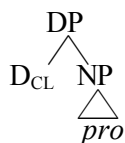
The preverbal position of the trigger is also crucial; the examples in (4.25) show that when a potential trigger is postverbal rather than preverbal, the usual enclisis appears.

- (4.25a) Eles lera- m- *no* a todos.
 3rd.pl. read.PAST-3rd.pl.-3rd.sg.MASC.ACC. to all
 ‘They read it to everyone.’
- (4.25b) Ele leu- *o* também.
 3rd.sg.MASC. read.PAST.3rd.sg.-3rd.sg.MASC.ACC. also
 ‘He read it also.’

The first analytical obstacle to an account of mesoclisism is to determine whether enclisis or proclisis is neutral in this language, and here expert opinions differ. In order to illustrate this, I will briefly summarise two accounts from the same volume of papers on Portuguese syntax which come to radically different conclusions about the status of clitics: Raposo (2000), who argues for proclisis as default, and Duarte and Matos (2000), who argue in favour of enclisis. I will not adopt either account here, nor will I attempt to solve the problem of Portuguese clitics once and for all. My goal in summarising these accounts is twofold: first, to provide a glimpse of the difficulty of the problem and the sorts of solutions proposed for it (as we will see, both accounts discussed here must resort to somewhat unusual measures in order to account for the empirical data); and second, to demonstrate that even authors with diametrically opposed analyses for this data agree that the Portuguese mesoclitic forms are best analysed as containing two separate pieces.

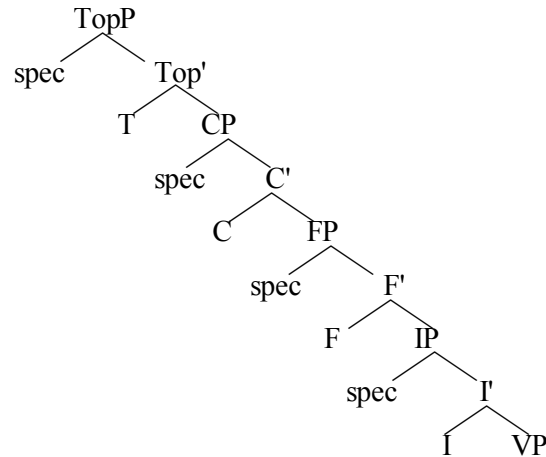
Both accounts adopt the DP Hypothesis of Corver and Delfitto (1993), depicted in (4.26), according to which clitics are transitive Ds which take *pro* NP complements. In most other particulars, their analyses are quite different.

(4.26)



I will first discuss Raposo (2000)’s argument favouring proclisis as the default behaviour of clitics in European Portuguese. His motivations are partially architectural, as he assumes that clitics move as heads and always adjoin to the left. He also assumes the clausal structure in (4.27), following Benincà (1995).

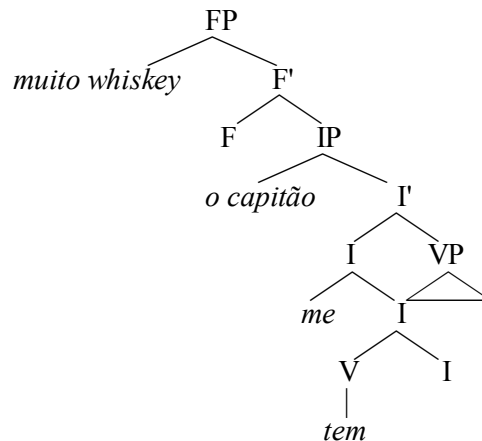
(4.27)



Raposo's argument centres around the projection of F. He argues that European Portuguese requires that the spec of FP must be filled by an overt element. Triggers of proclisis are attracted to this position, leaving the verb and its proclitic *in situ*. Under his account, the structure of a proclitic sentence such as (4.28a) is (4.28b) (his (22a–23)).⁸⁰

(4.28a) Muito whisky o capitão *me* tem servido!
too.much whiskey DEF captain 1st.sg.DAT AUX.3rd.sg. serve.PTCP
'The captain has served me too much whiskey!'

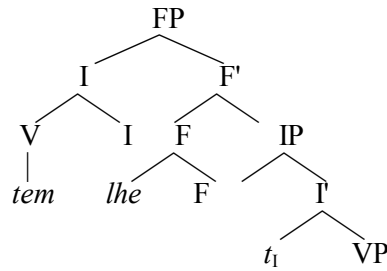
(4.28b)



Enclisis, on Raposo's account, occurs when Spec-F is not filled by some sort of operator. Spec-F must be filled in this language because the structural position F is an enclitic and must have something to its left; therefore, the verb raises to fill the position. This is rather difficult to implement without violating theory-prohibited operations such as excorporation or the verb skipping IP. Raposo's solution is to have I, containing the verb, move directly to Spec-F, skipping over the clitic, which is positioned in F, yielding (4.29), Raposo's (56). (4.29) is nevertheless somewhat unusual, in that it involves a head raising to a specifier position; Raposo is aware of this.

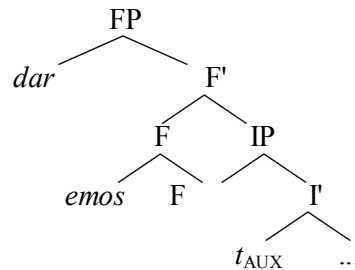
⁸⁰ I am giving only the barest essentials of Raposo's account; for the motivations of some of the finer details, cf. the original paper.

(4.29)

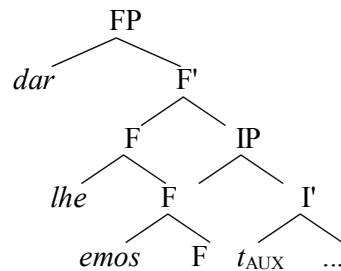


As for mesoclitisis, Raposo, as mentioned above, takes the position that the future and conditional forms are still two pieces; under his account, the infinitive must incorporate with the auxiliary to get Case, as shown in (4.30a), his (73), using the verb *daremos* ‘we will give’. If there is a clitic, as in (4.30b) (his (71)), it remains in F with the auxiliary while the infinitive moves alone to Spec-F.

(4.30a)



(4.30b)



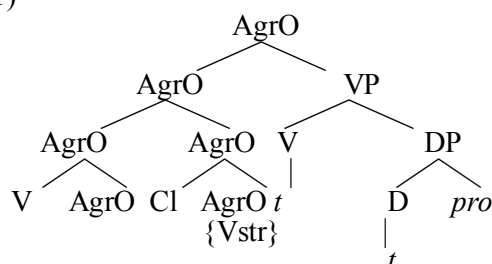
Raposo is working within a framework in which clitics *must* originate as proclitics; his paper, therefore, is about how to account for Portuguese clitics given this initial assumption. Duarte and Matos start with the assumption that Portuguese clitics are underlyingly enclitics, even though they, like Raposo, are working with Kayne’s assumption that clitics cannot be right-adjoined. Their contention is that Portuguese is able to allow enclitics because of their position on the clitic > affix grammaticalization cline – i.e., pronominal clitics in Portuguese are more affix-like than their counterparts in other Romance languages.⁸¹

Duarte and Matos (2000:129–30) suggest that clitics are generated under VP, which should predict that the clitic D adjoins to V, except that if the resulting complex moves to AgrOP, the clitic cannot check Case. Therefore, rather than adjoining to V, their account moves clitics directly to AgrO first (leaving its *pro* complement *in situ*), and then moves the verb. The verb could not move first, because the clitic could not then get its Case.

⁸¹ Duarte and Matos (2000:127) provide potential evidence for this position by noting that enclisis is used categorically by children under forty-two months of age. Though interesting, this data is not probative, since it could equally reflect a situation in which acquiring the adult proclitic pattern is simply hard enough and subtle enough to require a relatively long period of time to learn.

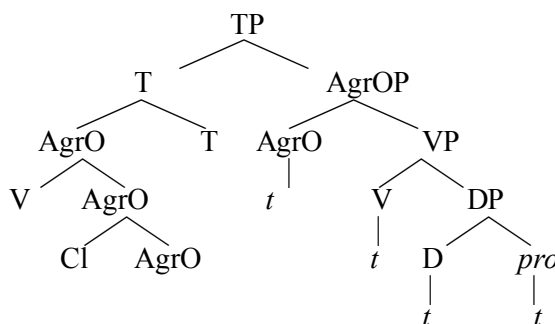
The resulting configuration is shown in (4.31), their (42). This structure should not allow the verb to check Case, but Duarte and Matos argue that this is allowed because the clitic is actually a “quasi-verb inflectional affix”.⁸²

(4.31)



Unlike Raposo, Duarte and Matos analyse future/conditional forms differently with and without mesoclis. Their suggestion, which is compatible with the competing-grammar hypothesis, is that forms without mesoclis are ordinary synthetic forms, akin to verb forms elsewhere in the language, while forms with mesoclis are two pieces. In the latter instance, the auxiliary is a “lexicalised T-affix” treated syntactically much like English *do*. The derivation is shown in (4.32).

(4.32)



Proclisis, rather than enclisis, is the odd case out under Duarte and Matos’s account. They follow Frota and Vigário (1996) in proposing a phonological explanation fed by hierarchical data: all clitics start off as enclitics, and proclisis occurs whenever a “heavy function word”, defined as an item with either focus or structural branching, both precedes and c-commands the verb, with the switch from enclisis to proclisis occurring between Spell-Out and LF. However, they *also* have to allow for syntactic movement in order to account for cases where e.g. the clitic must c-command an empty category, as Last Resort, which rather weakens their overall account.

The relative strengths and weaknesses of these two accounts are not, however, at issue here; as I said at the outset, my aim is to provide a glimpse of the diversity of accounts proposed for these data. However, in order to provide more than an illustration of the Portuguese situation, it is necessary to decide whether to treat Portuguese pronominals as proclitics by default or enclitics by default. I will assume the latter, although I make no serious theoretical commitment to this position; my reasons for it are as follows. First, there is a fairly short list of items that trigger proclitics, and enclitics appear elsewhere; generally treating the “elsewhere” condition as the default is preferable in the absence of clear evidence to the contrary. Second, the phonological evidence favouring the enclitic analysis is

⁸² It is not clear to me why the quasi-affixal status of the clitic is not a problem for Case-checking in this configuration if it has to bypass the verb in order to get Case.

compelling, as demonstrated by Vigário (1999, 2003). Proclitic future/conditional forms and forms without clitics form a single prosodic word, with stress on the “auxiliary” and vowel reduction in the root, and the presence of the clitic does not change the location of the primary stress. Mesoclitic forms, however, have *two* primary stresses, one on each element, and no radical vowel reduction. Furthermore, the mesoclitic behaves phonologically as though it were a suffix.

The phonological evidence is not necessarily probative; Klavans (1985) has pointed out that the syntactic and phonological attachment of clitics can differ (although the Portuguese data is not analogous to the cases Klavans discussed). But Raposo’s motivation for proclitics as the default is largely theory-internal, and his theoretical framework is not that used here. The available empirical evidence favours the case for enclitics, albeit subtly, and therefore I adopt that assumption here.

My second goal in the discussion above was to note that both papers agree that the Portuguese future/conditional mesoclitic forms are bipartite – even though the authors disagree on virtually every other point, including whether the future/conditionals are ever synthetic. An advantage of using DM in this context is that we are not, as Duarte and Matos are, forced to say that future/conditional forms are sometimes assembled in the lexicon and sometimes in the syntax. On a DM account, the future/conditional forms are always assembled in the syntax, and the non-mesoclitic forms are the result of garden-variety head movement. A further advantage of DM here is that the syntax need not be responsible for the placement of the clitic; that can be accomplished via post-syntactic operations. This is the same intuition put forward by Duarte and Matos, but the post-syntactic options allowed them by their framework are less sophisticated.

This is not to say that we can quickly solve the problem of the correct derivation of the Portuguese mesoclitic forms. Several alternatives are logically possible, and the empirical motivation for choosing one over another is subtle. The first thing to determine is whether the clitic should be initially associated with V or with T. Vigário (2003:148–9) argues in favour of the former, on morphological grounds; her conclusion is based on some issues of allomorphy.

The future/conditional forms are generally described as being built from the ordinary infinitive, which is largely true; however, there are three irregular forms where the first portion does *not* correspond to the usual infinitive. This is shown in (4.33a), using third singular forms. (4.33b) shows that the mesoclitic forms allow both possibilities, although the forms with the regular infinitive are not standard.

(4.33a) <i>Infinitive</i>	<i>Future</i>	<i>Conditional</i>	
dizer	dir-á *dizer-á	dir-ia *dizer-ia	‘to tell’
fazer	far-á *fazer-á	far-ia *fazer-ia	‘to do’
trazer	trar-á *trazer-á	trar-ia *trazer-ia	‘to bring’
(4.33b) <i>Infinitive</i>	<i>Regular w/Mesoclis</i>	<i>Irregular w/Mesoclis</i>	
dizer	dir-lhe-ia	(?) dizer-lhe-ia	
fazer	far-lhe-ia	(?) fazer-lhe-ia	
trazer	trar-lhe-ia	(?) trazer-lhe-ia	

Derived verbs built from this root behave somewhat differently. The non-mesoclitic forms are built after the simplex forms, using the irregular base. The mesoclitic forms again allow both possibilities, but this time speakers have a clear preference for the *regular* infinitive⁸³:

⁸³ The forms displayed are the third singular conditionals of *desdiria* ‘contradict’, *refaria* ‘do again’, and *satisfaria* ‘satisfy’.

(4.34a) <i>Regular</i>	* desdizeria * refazeria * satisfazeria	<i>Regular w/Mesoclis</i>	desdizer-te-ia refazer-lhe-ia satisfazer-se-ia
(4.34b) <i>Irregular</i>	desdiria refaria satisfaria	<i>Irregular w/Mesoclis</i>	*? desdir-te-ia *? refar-lhe-ia *? satisfar-se-ia

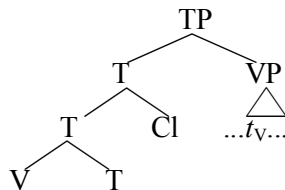
Vigário (2003:149) argues on the basis of these data that the clitic must attach directly to the infinitive, rather than to the fully inflected form or to T. However, there is also reason to think that the clitic might instead be associated with T. With other auxiliaries, the clitic cliticises onto the auxiliary rather than the non-finite verb, regardless of the locus of the clitic.⁸⁴

- (4.35a) Tivesse- *me* o Pedro ajudado, isto não teria
had.SUBJ.3rd.sg.-1st.sg.ACC. DEF help.PTCP this NEG have.would
acontecido.
happen.PTCP
‘Had Pedro helped me, this wouldn’t have happened.’
- (4.35b) Se o Pedro *me* tivesse ajudado, isto não teria
if DEF 1st.sg.ACC. had.SUBJ.3rd.sg. help.PTCP this NEG have.would
acontecido.
happen.PTCP
‘If Pedro had helped me, this wouldn’t have happened.’

It is difficult to see why clitics should be attracted to T some times and not others. Arguably, the future/conditional T could have different properties from other auxiliaries; perhaps the latter raise to T from *v* and the former start out in T. But if clitics are attracted to T, they should be attracted to T regardless of what fills it. Reducing the allomorphic problem to locality effects on Vocabulary Insertion rather than the association of the clitic with V seems a better solution.⁸⁵

Even upon deciding that the clitic is associated with T, there are still several derivational possibilities. One option would be to have the clitic adjoined to T prior to verb-movement; the verb then moves to adjoin to T, creating the linear string V-T-Cl. The clitic would then undergo two separate operations of post-syntactic Local Dislocation, causing it to invert with T and thus giving rise to the surface order. The tree in (4.36) is deliberately only schematic.

(4.36)



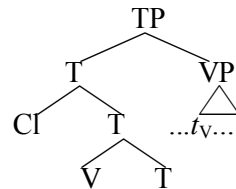
⁸⁴ Examples from Madeira (1992:103). For another example, cf. (4.28) above.

⁸⁵ However, this may still be problematic, because the clitic occupies the same position in non-finite clauses as in finite clauses.

- (4.37a) i. [[V T] Cl]
 ii. ((V * T) * Cl)
 iii. (V⊕T), (T⊕Cl)
- (4.37b) i. [[Cl]T]
 ii. (V * (Cl * T))
 iii. (V⊕Cl), (Cl⊕T)

It is not necessary, for our purposes, to determine the exact synchronic structure of Portuguese future/conditional verbs. The point of this discussion is to show that so long as the syntax positions the clitic adjacent to the verbal complex, we can derive the surface order via post-syntactic operations. This is true regardless of whether we choose either or neither of the derivations above, or even whether we believe that Portuguese clitics are underlyingly enclitics or proclitics. (4.36) could be minimally altered to be consistent with Raposo's position that Portuguese clitics are inherently proclitic.⁸⁶ The derivation shown in (4.38)–(4.39) is the counterpart to that in (4.36)–(4.37), where the clitic adjoins to T prior to verb movement; the last lines of both derivations are identical.

(4.38)



- (4.39a) i. [Cl [V T]]
 ii. (Cl * (V * T))
 iii. (Cl⊕V), (V⊕T)
- (4.39b) i. [[Cl]V]
 ii. ((Cl * V) * T)
 iii. (V⊕Cl), (Cl⊕T)

Duarte and Matos (2000) and Raposo (2000) disagree on many fronts, but agree that mesoclitic structures should be analysed as two words; Vigário (1999,2003) has shown that the phonological evidence supports this. Since the current framework is not lexicalist, we already have two separate pieces here, and the structural difference between a future or conditional and e.g. a synthetic past is not that there are more pieces involved, but that post-syntactic operations on the former, but not the latter, result in clitics seemingly interpolating themselves into a complex head. As we discussed in the previous chapter, phonology and morphosyntax are not always perfectly in tune with each other, so the fact that there are two phonological words in a mesoclitic form, when our analysis suggests one morphosyntactic word, is part of this larger issue.

The point here is that the position of the clitic is not a problem for us: not only can we get the desired surface word order via post-syntactic operations, but there are multiple potential derivations available, each with advantages and disadvantages. Because of considerations of time and space, the decision of which of these is the best for the data will have to be left for another project. The more pressing problem, from the present perspective, is the position of the verb, not the position of the clitic, and the difficulty is more a diachronic issue than a synchronic issue. This will be the starting-point of the following sub-section.

⁸⁶ All of the problems with an underlying-proclitic analysis remain, of course.

4.2.1.1.3 Implications

In the last chapter, the importance of linear adjacency in subsequent affix-genesis was repeatedly stressed: for two M-words to be analysed as a single M-word by a new language learners, the syntax of P-speakers must allow (but not necessarily require) the two M-words to be linearly adjacent. Linearity is an issue in the Portuguese case on two fronts, and in fact the clitic is the smaller of the two.

It is not difficult to devise a coherent story for the “entrapment” of the clitic. Suppose, for the sake of argument, that clitics were always associated with T⁸⁷; then the clitic would intervene between T and V at least some of the time. It would not *always* intervene, for a number of reasons: not all direct objects are pronominal, and clitics could adjoin to either side of T. Thus, there was plenty of evidence for the linear adjacency of V and T, enabling innovators to conclude that V and T formed a complex head, just as in e.g. the past tense. Nevertheless, they were also aware of the evidence that clitics could intervene. The earliest innovating generations were almost certainly bi-dialectal, with one grammar in which V and T formed a complex head, and another grammar where they did not. “Proto-mesocclisis” would indicate the use of the second grammar, but other sentence tokens would potentially have been ambiguous. At some point, a new innovative grammar, where mesoclitic forms resulted from post-syntactic operations⁸⁸, would also be introduced into the speech community.

But this quick narrative omits an important piece of the puzzle, which is that the linearity of V and T is questionable even when the clitic is not considered. In modern European Portuguese, verbs simply do not appear immediately to the left of auxiliaries⁸⁹:

(4.40) Ten- ho guardado as tu- as carta-s.
 have-1st.sg keep.PTCP DEF.PL. 2nd.sg-FEM.PL. letter-PL
 ‘I have kept your letters.’

One could argue that this is a trick question, since it is well-known that, despite the prevalence of SVO word order in the modern Romance languages, Latin was both SOV and head-final. We saw this in the Augustinian example (4.7), repeated here for convenience:

(4.41) Aliquando Christian-i non er- unt et
 sometime Christian-MASC.NOM.PL. NEG be-FUT.IND.3rd.pl. CONJ
 idol-a rursus col- i habe-nt.
 idol-NEUT.PL. again cultivate-PASS.INF. have-PRES.IND.3rd.sg.
 ‘Some day there will be no more Christians, and idols will be cultivated again.’

Since Latin had fairly free word order, the auxiliary could either precede or follow the verb; nevertheless, Latin had the requisite structure to yield surface linear adjacency between verb and auxiliary. Estimates for the shift from SOV to SVO range between Pre-Plautine

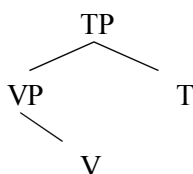
⁸⁷ The argument remains the same if clitics were instead associated with V, or if they had more mobility in earlier stages of the language. In fact, the latter possibility actually makes the argument all the easier.

⁸⁸ Post-syntactic operations are open to potential criticism as “ad hoc” from those who prefer all movement to occur in the syntax. From the perspective of a language learner confronted with baffling or contradictory data, however, this could be an advantage, because the possibility allows for an analytic scenario in which a learner fits all the data to a single underlying syntactic structure and then uses post-syntactic operations as rescue strategies for the empirical data that do not quite conform to his analysis.

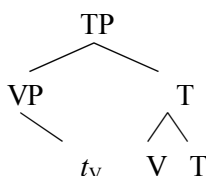
⁸⁹ Example from Ambar (2000:22).

(Addams 1976:99) and the ninth century CE (Muller 1929:7), with Harris (1978:34) taking the moderate position that unambiguous SVO word order did not appear until the fifth century CE. Even if Addams' very early figures are correct, there was clearly still enough evidence for SOV for the future/conditional innovation to occur. The grammatical shift, therefore, was approximately a change from (4.42a) to (4.42b); for the sake of clarity, the trees are overly simplistic. In terms of the typology laid out in the previous chapter, this is an example of Type I; note that it is also an example of the frequency with which new verbal suffixes are accrued in a head-final language.

(4.42a)



(4.42b)



The key point in this scenario is that it requires the complex future/conditional head to have already entered the grammar *before* language learners concluded decisively that their language was SVO. Otherwise, they would have had no motivation to create these forms, and there is a non-negligible possibility that the synthetic future/conditional forms of the western Romance languages would have never have developed. But this is something of a paradox. The continued existence of mesoclisism in Portuguese could be taken to indicate that the final stages of the change were recent and/or ongoing in the Romance languages with this construction; worse yet, some Romance languages never developed a synthetic future or conditional. We know that changes in word order can affect morphology and potentially lead to the break-up of complex heads (cf. Chapters Five and Six); if the future/conditional forms were not yet considered complex heads when the word order shifted, why would they behave as one?

The paradox is particularly problematic from a lexicalist perspective, since a lexicalist must postulate a stage in which two pieces become one. In this respect, the non-lexicalist piece-based nature of DM gives us some advantage; from a DM perspective, there are always two pieces. Nevertheless, we still have to explain why those two pieces behave as though they were a complex head if mesoclisism is to be taken as an indication that they were not.

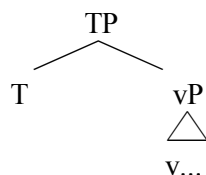
Fortunately, there is phonological evidence to assist in the resolution of this paradox. Valesio (1968) suggested that stress may have played a role in marking verb + auxiliary collocations as units, since the primary stress would have fallen on the auxiliary. Vigário (1999:230, 2003:345) corroborates that this is still the case in modern Portuguese, where in non-mesoclitic forms, the “auxiliary” receives primary stress and the radical vowel is reduced.

Perhaps even more important than stress, however, is the fact that the initial consonant of the auxiliary *habeō* is somewhat illusory: even in classical Latin, [h] was well on its way to being lost. Hall (1976) does not reconstruct [h] for Proto-Romance, while Sturtevant (1920[1940]:§130) notes that ‘From the time of our earliest documents... Lat. *H*

was an unstable sound.⁹⁰ As the changes that concern us were happening many centuries after the classical period, it is highly probable that the “h” of *habeō* was a purely orthographical fiction. This hypothesis is supported by the spelling of the earliest textual examples of synthetic futures (cf. (4.8) and surrounding text): *daras* and *prindrai/salvarai*.

This means that during the relevant period, the auxiliary was vowel-initial. It combined with an infinitive form ending in [r], which meant that to separate the two forms was to divide a syllable – and not just any syllable, but the syllable bearing nuclear stress. In other words, despite the possibility of pronominal interpolation, there was considerable motivation for language learners to conclude that the infinitive and auxiliary were to be kept together – and in that order. Conceivably, the change in word order *could* have occurred prior to the innovation of head movement, with the surface order V+T representing an operation of LD. This is illustrated in (4.43). (4.43a) shows a partial structure of the T-medial clause, (4.43b) the LD operation. The mesoclitic forms could be derived in several ways; for instance, it could have undergone string-vacuous LD to become a Sub-word within v, so that T, being an M-word, would end up on its right.

(4.43a)



(4.43b) T*v → v⊕T

Because of the phonological facts outlined above, the resulting forms would be indistinguishable on the surface from the V-to-T head movement of the other verbs in the language. This would leave innovators with very strong motivation for the conclusion that the future and conditional were also the result of V-to-T movement.

The key points of this section can be summarised as follows.

- The linear adjacency that led speakers to innovate synthetic future/conditional forms was a feature of Latin syntax and does not reflect the modern syntax;
- The infinitive and auxiliary had a strong phonological relationship, in that the morphosyntactic boundary between them fell in the middle of a syllable bearing nuclear stress;
- The phonological coalescence between the infinitive and auxiliary must have been solidified prior to the change in word order that occurred in later Latin, because otherwise the relationship between them would have been obliterated once the language was no longer verb-final;
- Surface linear adjacency between the verb and T could easily have been achieved via LD, following the change in word order;
- The resulting forms would be superficially identical to verb forms formed by head movement, so that language learners should be forgiven for failing to notice the underlying syntactic distinction.

The synchronic peculiarities of Portuguese are the result of two changes: a morphosyntactic creation, namely new synthetic future/conditional forms, and a syntactic shift from SOV to SVO. Although the synchronic situation has not been resolved here, it is clear that there are multiple possible solutions available. Meanwhile, despite its peculiarities, Portuguese is not a serious problem for the framework developed in this dissertation. The

⁹⁰ For full discussion, cf. Allen (1965:43–45).

new temporal affix appears on the periphery, although clitics are allowed to intervene. In the modern language, the intervention is post-syntactic; it reflects an earlier stage when the infinitive and auxiliary were still separate M-words. Finally, the respective linear order of T and V reflects in part the grammar of Latin, when T first became an affix, and does not imply a failure of the linear adjacency condition.

4.2.1.2 *Old Irish*

Old Irish pronominal objects, traditionally called “infixed pronouns”, have a requirement that they appear to the right of the first particle in the sentence, by brute force if necessary. The particle in question is sometimes a plausible C element (e.g. negation), but sometimes a verbal particle that otherwise appears as an adverbial preverb. This means that the pronominal infix sometimes intervenes between the verb and its prefix, and sometimes doesn’t, depending on the structure of the clause. Complex patterns of allomorphy and a phenomenon similar to *do*-support make it difficult to determine exactly what is going on underlyingly, and calls into question the status of the preverb. Although it is entirely reasonable to say that the position of the pronoun is due to its having been trapped at some point in its history, the actual details of how this may have happened, not to mention their present synchronic status, are not entirely straightforward. In this section, I review the Old Irish data and examine the roles of Local Dislocation and learnability in the scenario that may have given rise to it. The relevant data are described in 4.2.1.2.1 and analysed in 4.2.1.2.2; the ramifications of the conclusions drawn in 4.2.1.2.2 and the evidence we have for possible earlier grammars of Old Irish are discussed in 4.2.1.2.3. In 4.2.1.2.4, I demonstrate that the account described here has non-trivial advantages over previous accounts in the literature.

4.2.1.2.1 The Data

The syntax of infixed pronouns⁹¹ in Old Irish is so closely connected to the syntax of verbs in general that any discussion of the latter depends entirely on an understanding of the former; consequently, both this sub-section and the following sub-section start with an overview of the clausal structure (4.2.1.2.1.1) and verbal system (4.2.1.2.1.2) of this language. How the two sub-systems fit together will be discussed along with the theoretical considerations in 4.2.1.2.2.

4.2.1.2.1.1 Surface Sentence Structure

Although Old Irish was significantly different from Modern Irish in many respects – including most of the morphosyntactic issues discussed in this chapter – the basic surface clausal structure has not changed. The language is VSO in both main (4.44) and subordinate clauses (4.45).⁹²

- (4.44) Beoig-idir in spirut in corp in fecht so.
 vivify-3rd.sg. DEF.NOM spirit.NOM DEF.ACC body.ACC now
 ‘The spirit now vivifies the body.’

⁹¹ Eska (to appear) has argued that what appear to be pronominal objects in Old Irish were actually non-referential agreement markers; this will be discussed in 4.2.1.2.3 below. For the moment, I will use the traditional term, “infixed pronouns”.

⁹² (4.44) from the Wurzburg Glosses (Wb.) 13^d7; (4.45) from Milan Glosses (Ml.) 127^a7.

- (4.45) ...arna- ro chret-ea mbiás ícc do hua
 COMP.NEG-PERF. believe-3rd.sg. be.3sg.REL salvation to-3rd.sg.MASC. from
 dia.
 God
 ‘...that he did not believe that his salvation would be from God.’

The underlying structure is more difficult to determine; either SVO or SOV could underlie the surface order. McCloskey (1983) demonstrated that Modern Irish, despite its surface order, is underlyingly SVO, and as the same arguments can also be applied to the older language (cf. Adger 2006:607ffn.), it is generally assumed that Old Irish was SVO as well. The first of McCloskey’s arguments for Modern Irish is that non-finite verbs are clearly SV (4.46)⁹³; the second is that a verbal noun and its object behave as a constituent in cleft constructions.⁹⁴

- (4.46) Is bés leo- som in daim do thúarcuin.
 COP custom with.3rd.PL-EMPH. DEF.NOM ox.PL.NOM. PREP thresh.VN
 ‘It is a custom with them that the oxen thresh.’

- (4.47) Is [oc precept soscéli] attó.
 COP ASP preach.VN gosepl.GEN REL.BE.1st.sg.
 ‘I am preaching the gospel.’

Though the verb always precedes the subject and object in finite clauses, it is not always the first element in a sentence, and this is of vital importance for the problem at hand. There is a class of particles, including negation (4.48a) and interrogation (4.48b), which always precede the verb when they appear.⁹⁵ The presence of one of these particles interacts with the verb both phonologically and morphologically, as we shall see.

- (4.48a) Ní déna- t firt- u úil-i.
 NEG work.PRES-3rd.sg. miracle-NOM.PL. all- NOM.PL.
 ‘Not all work miracles.’
- (4.48b) In coscr- am- ni.
 Q destroy.PRES-1st.pl-EMPH.1st.pl
 ‘Do we destroy?’

In order to understand the interactions between these particles and the verb, as well as the positioning of the pronominal infixes, it is helpful to see them schematically as a sort of verbal cluster, here following the conventions of Adger (2006:610). [Y + Z + W] represents the verb stem. X is a slot occupied by complementizers and other particles and the first preverb of compound verbs. The nuclear stress always falls on Y, while X is always unstressed.

- (4.49) [X] . [Y + Z + W]

X is optional. This apparently simple fact is actually quite important, because the presence or absence of an overt X has morphosyntactic consequences. A simplex verb, not a compound, has two available sets of inflection, and the choice between them is determined entirely by whether or not the sentence has an X. In (4.50a), the verb *berid* ‘he/she/it carries’ is in absolute sentence-initial position, and consequently, it takes what is traditionally called

⁹³ Wb. 10^d6.

⁹⁴ Wb. 21^c19.

⁹⁵ (4.48a) from Wb. 12^b20, (4.48b) from Wb. 2^b20.

“absolute” inflection: the ending *-id*. The same verb, in the same tense and mood, is also used in (4.50b), but (4.50b) is a negative sentence, and therefore the verb appears in second position after the negative particle *ní*. As a result, the verb takes “conjunct” inflection and appears as *beir*.⁹⁶

- (4.50a) Ber -id in claideb sin.
 carry.PRES-3rd.sg.A DEF sword.ACC.SG that
 ‘He carries that sword.’
- (4.50b) Ní ·beir in claideb sin.
 NEG carry.PRES.3rd.sg.C DEF sword.ACC.SG. that
 ‘He doesn’t carry that sword.’

Table 4.1 compares the absolute and conjunct paradigms for the present indicative active of *berid*, omitting the impersonal/passive and relative forms. The absolute forms are, generally speaking, one syllable longer than the conjunct forms.⁹⁷

	Absolute	Conjunct
1 st .sg	biru	·biur
2 nd .sg	biri	·bir
3 rd .sg	berid	·beir
1 st .pl	bermai	·beram
2 nd .pl	beirthe	·berid
3 rd .pl	berait	·berat

Table 4.1: *Present Indicative (Active, Matrix) Paradigm of berid ‘carry’*

Old Irish has a large number of compound verbs, formed from the verb stem and prefixed preposition-like elements called *preverbs*. The semantics of the combination of preverb and stem are quite often idiomatic: *berid*, for example, can combine with the preverb *as* to mean ‘says’, or with *do* to mean either ‘brings’ or ‘gives’. Many compound verbs have only one preverb, but some can have several; McCone (1997) gives the examples *for-cum-ga(i)r-* ‘command’ and *to-ar(e)-in-ga(i)r-* ‘promise’.

Compound verbs, like simplex verbs, show allomorphy conditioned by X, but since preverbs can fill the X slot, compound verbs take conjunct inflection by default. The important factor, then, is the identity of X rather than its presence or absence. If there is no complementizer present, the first preverb of the compound becomes X. In (4.51a), the preverb, *do*, is set apart from the rest of the verbal cluster (indicated in traditional orthography by a raised dot). This runs counter to the usual stress rules in this language. Old Irish, as a general rule, places primary stress on the initial syllable, but since X is always unstressed, stress falls on the next preverb or on the root, here *beir*; hence, verbs in this construction are called *deuterotonic* verbs. These contrast with *prototonic* verbs, which are stressed on the first preverb as expected.⁹⁸ Prototonic forms appear when the verb is not in absolute initial position. (4.51b) is the negative equivalent of (4.51a).⁹⁹

⁹⁶ From this point forward in the discussion, I will be indicating this distinction in the glosses of simplex verbs: A for absolute, C for conjunct.

⁹⁷ The raised dot in the conjunct forms is an orthographic convention that will be explained momentarily.

⁹⁸ Under some circumstances, the phonological interaction between adjacent preverbs produces slight deviations from this pattern. McCone (1997) contrasts the deuterotonic *do-AIR-n-gar* ‘he promises’ with its prototonic equivalent, *(ní/in)-T-AIR-n-gar*. Here, the vowel of the first preverb, *do*, has been elided in the prototonic form and reduced to *t*, moving the stress to the vowel of the second preverb.

⁹⁹ This distinction will henceforth be expressed in the glosses of compound verbs: CD for deuterotonic forms and CP for prototonic forms. Following Adger (2006), I have glossed preverbs as simply PRV,

- (4.54b) It- ius.
eat.PRES.3rd.sg.A-3rd.sg.FEM.
'He eats it.'
- (4.54c) Táth- ut
be.PRES.3rd.sg.A-2nd.sg.
'You have.'

Verbs with conjunct inflection, including all compound verbs, require infixed pronouns rather than suffixed pronouns. Infixes occur in the slot between X and [Y + Z + W]. In simplex conjunct or prototonic forms, like (4.55a), the verb cluster itself is not disturbed, which essentially makes the “infixes” a product of phonology. Complementizers and other particles may be phonologically dependent on the verb, but they are located in a different syntactic head; therefore, while the pronominals are “internal” to the verbal cluster phonologically, in morphosyntactic terms they attach to the outside of a syntactic position. Our problem is deuterotonic forms like (4.55b), the positive counterpart to the verb from (4.55a). Here, the element to the left of the pronominal is a preverb, and therefore the pronoun appears to be allowed to interpolate itself into the verb itself. When there are multiple preverbs, the pronoun appears after the first in the string, rather than at the end of the string, which indicates that the pronoun is not simply targeting a position closer to the verb than the preverbs.¹⁰¹

- (4.55a) Ní- s· nim-dich.
NEG-3rd.pl.-PRV-protect.PRES.3rd.sg.CP
'He does not protect them.'
- (4.55b) Imm-us· ndich.
PRV- 3rd.pl-protect.PRES.3rd.sg.CD
'He protects them.'

Example (4.56) shows that infixed pronouns can occur with verbs and pronominals in the first or second person, rather than exclusively with third persons.

- (4.56) Ar- ut· neith- ius.
PRV-2nd.sg.-await.PAST-1st.sg.CD
'I waited for you.'

The careful reader will have noticed an apparent gap in this presentation: infixed pronouns can appear with all conjunct forms, but suffixed pronouns must be third-person and can only attach to third person verbs. What happens if the form is a first- or second-person absolute? The answer, essentially, is that the verb is made conjunct by brute force: the semantically vacuous particle *no* is inserted as X, triggering conjunct inflection, and infixed pronouns are used.¹⁰²

- (4.57a) No- m· íslig- ur.
PRV-1st.sg.-abase.PRES-1st.sg.C
'I abase myself.'
- (4.57b) No- s· comalna- mar.
PRV-3rd.pl-fulfil.PRES-1st.pl.C
'We fulfil them.'

¹⁰¹ Examples (4.55) and (4.56) from McCone (1997:11).

¹⁰² (4.57a) from Wb. 17^d22, (4.57b) from McCone (1997:12).

It is tempting to view this system as one of complementary distribution: suffixed pronouns are used when they can be used, and infixes are used elsewhere. Unfortunately, the pattern is not this simple. Suffixed pronouns are vanishingly rare in Old Irish, particularly in later texts. Ordinarily, infixes are used even when the pronoun in question is a third person form used with a third person absolute verb; this construction can be found even in the earlier texts. An account designed to produce complementary distribution would predict that examples like (4.58) do not exist, when in fact they are the most common construction.¹⁰³

(4.58) no- s· ber- ed
 PRV-3rd.sg.FEM-carry.IMPF-3rd.sg.C
 ‘he was carrying it (fem.)’

Here we come at last to the problem of Old Irish infixes: they sometimes appear to the left of the entire verbal complex, but other times intervene between elements *within* the verbal complex. Moreover, the language does have an alternative strategy for positioning these pronouns, but it is of restricted distribution and is usually not used even when the conditions for it are met. Worst of all, by inserting semantically vacuous dummy particles with absolute, simplex verbs, the grammar seems to go out of its way to create contexts of infixation.

4.2.1.2.1.2. The Verbal System

Four different stems make up the Old Irish verbal system: imperfective (traditionally “present”), perfective (traditionally “preterite”¹⁰⁴), subjunctive, and modal (traditionally “future”).¹⁰⁵ Setting aside for the moment the imperative, which is formed from the imperfective stem, each of the four stems is used in two different verbal forms, thus:

(4.59)	<i>Imperfective</i>	<i>Perfective</i>	<i>Subjunctive</i>	<i>Modal</i>
	present indicative	preterite	present subjunctive	future
	imperfect indicative	perfect	past subjunctive	conditional

The forms in the third line of (4.59) are the so-called secondary tenses. Except for the perfect, the secondary tenses differ from the primary in two respects: they have different subject agreement markers, and they have only conjunct forms; stated differently, only primary tenses can occur in absolute form. These contrasts are illustrated in Table 4.2¹⁰⁶; the particle used here is the dummy support particle *no*, but any other preverbal particle will suffice.

¹⁰³ (4.58) from Turin Glosses (Tur) 134. I have chosen to illustrate this point with a feminine object because it is easiest to see. The masculine and neuter infixes are phonologically null, but they trigger initial mutations on the first segment of the verbal stem: nasalisation in the case of the masculine, and lenition in the case of the neuter.

¹⁰⁴ Readers familiar with the Old Irish verbal system may be somewhat alarmed by this terminology. I assure these readers that I have not forgotten the perfect forms with prefixed *ro-*, which will be discussed below.

¹⁰⁵ The conditional form is sometimes called the “secondary future”.

¹⁰⁶ I have given third plural forms because they show the relevant distinctions with particular clarity, and have tried to select verbs which give good examples of different stem types; for many verbs, the imperfect and past subjunctive are identical and the present indicative and subjunctive nearly so.

	Imperfective	Perfective	Subjunctive	Modal
<i>Primary</i>	benait, ·benat	marbsait, ·marbsat	*gessait, ·gessat	mairbfit, ·mairbfet
<i>Secondary</i>	no·bentais	ro·marbsat	no·gestais	no·mairbfitis

Table 4.2: *Third Plural Forms of benaid ‘strikes’, guidid ‘prays’, and marbaid ‘kills’.*

This system is interesting, because the primary division in the system – between “primary” and “secondary” forms – is not purely based on temporal distinctions, as is made clear by the fact that the preterite, a primary form, is a past tense. Rather, the secondary forms appear to be contributing an additional layer of features, which indicates that their structure has an additional syntactic projection absent from the structure of primary forms. They share aspectual and modal features with their primary forms, which further indicates that the nature of TP in this language is more heavily aspectual than temporal; the difference between the “present” and “preterite” is more in the way of “imperfective” and “perfective”.

There is another indication that the secondary forms contain an additional layer of structure. The perfective forms stand in a different relationship to each other than do the other primary/secondary pairs: they, and they alone, take the same set of subject agreement markers. The difference between these two forms is simply a matter of whether a perfective prefix is present.¹⁰⁷ This suggests that the difference between the perfect and the other secondary forms is that the former makes use of an auxiliary while the latter require head movement. For this to be the case, there must be another projection above TP. I will refer to this projection as AugP in order to capture the intuition that the secondary forms are “augmented” in comparison with the primary forms; this should be taken as a decision of nomenclature rather than anything more profound.

Before turning to the following section, however, it is worth mentioning that conjugation is not particularly orderly in Old Irish. Each of the four stems can be formed with multiple allomorphs, so that one speaks of e.g. the “f-future”, “reduplicated future”, “ē-future”, and “s-future”, each of which usually can be further divided into at least two subtypes (usually but not always for phonological reasons). Subject agreement morphology is sensitive to this allomorphy as well as to stem type and primary/secondary designation. The fact that a verb forms a particular type of imperfective does not necessarily enable one to predict what its other three stems will look like. In addition, Old Irish shares with the more familiar Indo-European classical languages Latin and Greek a distinction between “active” and “deponent” conjugation, with the complicating factor that in Old Irish only third person mediopassive forms survive, so that much of the “deponent” paradigms are formally unique.

The upshot of this allomorphic complexity is that, while there is some degree of formal correspondence between the secondary subject markers of the various stem classes, and some, albeit less, between the primary subject markers, there are also a number of differences. The subject allomorphs used for the perfective forms are especially distinctive as compared to the rest of the system.

4.2.1.2.2 Structural Properties of Declarative Clauses in Old Irish

This sub-section is concerned exclusively with the synchronic grammar of Old Irish declarative clauses; diachronic speculations are reserved for the next sub-section. For reasons of time and space, only declarative clauses will be considered here, though similar arguments can be made for relative and imperative clauses. The crux of my analysis of is that the various formal differences between the different types of verb forms are conditioned by local contexts, in some cases involving LD. It should be stressed at the outset that this is not the only possible analysis; some minor variations account for the data about equally well. What

¹⁰⁷ This prefix is usually *ro* – as here – but is somewhat idiosyncratic; some specific verbs require a different prefix. All of these prefixes, including *ro*, also function as preverbs.

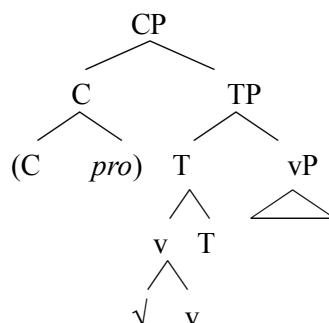
is shared by the analyses that best account for the full range of data is the responsibility of local post-syntactic operations, verb movement, and a phenomenon similar to *do*-support for surface variations.

The basic assumption about the structure of the clause is that primary verbs (cf. 4.2.1.2.1.1) move only as far as T, and secondary verbs only as far as Aug (or T, in the case of the perfect). The preverbs are adjoined to C, rather than to *v*; there are some outstanding problems with how exactly they come to be there, but at any rate they do not originate as elements of *v*.¹⁰⁸ Pronominal objects are also generated in C. The one element that moves to C during the syntactic derivation is the perfect auxiliary (*ro* and its compatriots); this is necessary in order to capture its syntactic behaviour, which is essentially that of a preverb. Absolute morphology and the variability of the surface position of the pronoun are due to post-syntactic operations. These are the essential ingredients; however, because there are so many variables that must be taken into account here, it is necessary to show how this system derives a rather large series of verbs.

The simplest cases are the primary non-compound verbs, where preverbs are not an issue and the clause has one fewer inflectional projection. Three different cases must be derivable on the basic structure (4.60): the conjunct form (*ní beir*), the absolute form (*beirid*), and the declarative pronominal form (*nom·beir*). There are two variables to control for: the form of the AgrS suffixes on the verb, which I assume to be dissociative morphemes post-syntactically adjoined to T, and the presence of a pronominal object.

- (4.60) Conjunct: *ní beir* = C_{neg} STEM.AgrS
 Absolute: *beir-id* = STEM-AgrS
 Pronominal: *nom·beir* = C_{decl}-OBJ-STEM.AgrS

(4.61)



The simplest case is the conjunct forms. If C is negative or interrogative, it spells out in C, and the verb spells out in T. The form of AgrS is conditioned by the features on T.

Absolute forms have the same underlying structure as conjunct forms, but require LD post-syntactically. The absolute forms appear only when the verb is both in surface sentence-initial position and not governing a pronominal object. This happens exactly when C is declarative and phonologically null, and nowhere else, which corresponds to exactly those cases where there is a strict linear adjacency relationship between C_[decl] and the rest of the verb. So we can safely infer that the verb undergoes Local Dislocation with C.

¹⁰⁸ Most accounts of Old Irish (cf. 4.2.1.2.4) adjoin the preverbs to *v*, effectively treating them like Germanic verbal particles. Maintaining such an analysis would be desirable, insofar as it reduces Old Irish to part of a known problem; and it *is* possible to get the analysis to work with preverbs in *v*. However, adjoining the preverb to *v* results in a number of significant problems later in the derivation, all of which vanish by moving them to C. There is some historical support for this decision.

- (4.62a) *Partial Structure*: [C [[[√ v] T] AgrS]]
 (4.62b) *Local Dislocation*: [C^T] → [[T]C]
 (4.62c) *Re-linearisation*: (((√ * v) * T) * C)

AgrS is adjoined to T after this has occurred, which leaves it adjacent to both T and C, conditioning absolute verbal endings. Vocabulary Insertion for the third singular present indicative absolute and conjunct endings of *beirid* is shown in (4.63).¹⁰⁹

- (4.63a) [3rd.sg.] ↔ -id / [PRES. IND.]_ [C]
 (4.63b) [3rd.sg.] ↔ -∅ / [PRES. IND.]_

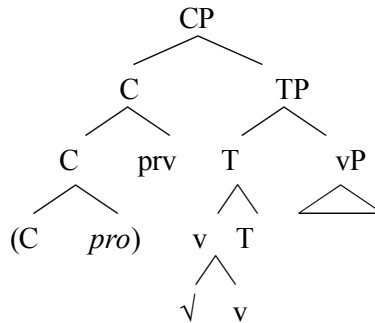
An alternative derivation for the absolute/conjunct distinction would involve positing T-to-C movement, as Carnie et al. do (cf. 4.2.1.2.4.1). This would be simpler, but it then becomes difficult to derive the pronominal declarative forms: head movement is a purely hierarchical operation, and therefore linear adjacency should not interfere with it, and yet the presence of a pronominal object intervening between C and T blocks it. Since the pronominal object is not a head, it must be disruptive because it precludes linear adjacency between C and T, which can only mean that the operation triggering absolute verbal endings is based on linearity and necessarily post-syntactic.

If C undergoes LD, it spells out as zero. When this is prevented, it spells out instead as *no*. This correctly captures the impression that *no* is essentially equivalent to *do*-support (an observation reached independently by Newton; cf. 4.2.1.2.4.3); it is inserted when C-features are not otherwise instantiated by any overt exponent.

Primary compound verbs differ from simplex verbs only in that one or more preverbs are adjoined to C; how they get there is an unsolved problem. There are four cases to consider: the prototonic and deuterotonic forms, with and without pronouns.

- (4.64) Prototonic: ní(-m)·e-pir = C_{neg}(-OBJ)-PRV-STEM.AgrS
 Deuterotonic: do(-m)·beir = PRV(-OBJ)-STEM.AgrS

(4.65)



As before, when C is not declarative, the forms are easy, because everything simply spells out in place. When C is declarative, the situation is a bit more complex. Clearly the preverb and pronoun undergo LD; this can be formulated as a rule whereby pronouns invert with a following preverb just in those cases when C is declarative.

- (4.66a) (C_{decl} * pro * prv) ↔ [C_{decl} [[prv] pro]]
 (4.66b) (C_{decl} ⊕ prv ⊕ pro)

¹⁰⁹ This has been simplified; a full account would require information about declensional class.

By itself, this rule takes care of the *dom-beir* cases, since we could posit that different phonological rules apply to the preverbs before pronouns; however, prototonic and deuterotonic verbs are formally distinctive regardless of the presence of the pronoun. In addition, we might expect *no*-insertion to apply, since there is no overt expression of C-features. We can circumvent this problem by assuming that deuterotonic forms of preverbs are conditioned by a linear-adjacency relationship with declarative C. If no pronoun is present, C and the (first) preverb are already linearly adjacent and the conditioning is straightforward. If a pronoun *is* present, it will have inverted with the first preverb, thus placing the preverb into the conditioning environment. Negative and interrogative C have no effect on the preverb, just as they do not condition agreement on simplex verbs. Meanwhile, the features on C now have overt expression, so *no*-support is not needed.

An alternative would be to say that C always undergoes Local Dislocation with a preverb to its right and conditions it in much the same way it conditions absolute agreement on the verb. This is possible even when a pronoun intervenes, since pronouns must also undergo LD with the adjacent preverb, thereby creating an environment feeding the LD operation between C and the preverb. The order in which the rules must apply for the feeding to occur is consistent with our understanding of cyclicity: the inner rule applies first, then the outer. However, this cyclic application would not allow the derivation of the correct forms.

$$\begin{array}{ll}
 (4.67a) \text{ (C}_{\text{decl}} * \text{pro} * \text{prv}) & \leftrightarrow \quad [\text{C}_{\text{decl}} [[\text{prv}] \text{pro}]] \\
 (4.67b) [\text{C}_{\text{decl}} [[\text{prv}] \text{pro}]] & \leftrightarrow \quad [[[\text{prv}] \text{pro}] \text{C}_{\text{decl}}] \\
 (4.67c) \text{ (prv} * \text{pro)} * \text{C}_{\text{decl}} &
 \end{array}$$

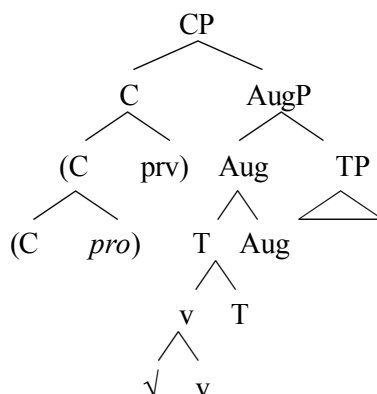
As we see from (4.67b–c), C ought to invert with the entire preverb+pronoun complex, since that is now a unit. But this is a problem, because C ends up suffixed to the pronoun, not to the preverb. Since C is not then adjacent to the preverb, it cannot be conditioning its formal change.

Based on the above data, one might be tempted to simplify matters by specifying that *no*-support is conditioned by the presence of a pronoun to the right of C; when the pronoun undergoes LD, it removes the conditioning environment for *no*. The secondary verbal forms are not consistent with such an analysis, however, because secondary forms require *no*-support whenever the clause is declarative and there is no preverb, regardless of the presence of a pronoun.

Recall from the previous section that secondary forms differ from primary forms in containing an extra projection I am informally calling an AugP. Six cases must be accounted for (i.e. simplex verbs, prototonic verbs, and deuterotonic verbs, with and without pronouns), but as the behaviour of pronouns is precisely the same as with primary verbs, I will concentrate only on C, the preverb, and the verb.

$$\begin{array}{ll}
 (4.68) \text{ Simplex:} & \text{no(-m)·ber-ed} = \text{C(-OBJ)-STEM-AgrS} \\
 \text{Prototonic:} & \text{ní(-m)·tai-br-ed} = \text{NEG(-OBJ)-PRV-STEM-AgrS} \\
 \text{Deuterotonic:} & \text{do(-m)·ber-ed} = \text{PRV(-OBJ)-STEM-AgrS}
 \end{array}$$

(4.69)



The secondary compound forms are derived in exactly the same way as their primary counterparts, modulo the addition of Aug and of T-to-Aug movement. Once again, if C is not declarative, nothing happens and everything spells out in situ; if C *is* declarative, then it conditions the form of the first preverb and causes an object pronoun, if present, to undergo LD. The verbal complex shows phonological effects of presence or absence of non-declarative C, but this is primarily because C and the verb form a single phonological word. All of the action, in this case, lies within C itself.

Simplex forms are only slightly more complex. The rule conditioning absolute agreement requires a linear relationship between C and T, and this is impossible here because an entire inflection projection is in the way. There is no rule for LD between C and Aug, and no preverbs for overt expression of the features on C. Therefore, just as in the case with a pronoun blocking LD, C features surface via *no*-support. *No* is a syntactic device rather than an expression of tense features; the fact that it routinely occurs with past-tense verbs is a coincidence.¹¹⁰

- (4.70a) C_{neg} ↔ *ní*
(4.70b) C_{interrog} ↔ *in*
(4.70c) C_{decl} ↔ ∅ / *_prv*; AgrS_¹¹¹
(4.70d) C ↔ *no* / elsewhere

This leaves the perfect forms, which are unique within Old Irish in that they require an auxiliary preverb, usually *ro*. The presence of a perfect particle, and the fact that AgrS is identical in primary and secondary forms built on the perfective stem, indicates that in the perfect, the verb raises no higher than T. What does seem to be the case, however, is that the perfect particle raises to C: all of the exponents of the perfect are formally identical to some preverb or other (and clearly developed from preverbs), and the perfect prefix behaves syntactically just as the innermost preverb in a sequence would behave.

All of this suggests the structure given in (4.71). The pronouns behave here exactly as they do elsewhere, so I will set those forms aside for reasons of space. This leaves four forms to account for: non-declarative simplex, declarative simplex, non-declarative compound, and declarative compound.¹¹²

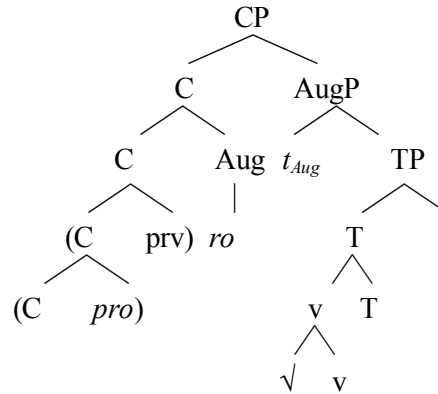
¹¹⁰ (4.70) has been somewhat simplified, since it doesn't take into account imperative or relative clauses.

¹¹¹ I'm not particularly pleased with this disjunction.

¹¹² Note that in demonstrating the derivation of the latter two cases, we can see how non-perfect forms with more than one preverb are derived.

- (4.71) Declarative simplex: ro(-m)·b-i = PERF(-OBJ)-STEM-AgrS
 Non-declarative simplex: ní(-m)·ru-b-ai = C_{neg}(-OBJ)-PERF-STEM-AgrS
 Declarative compound: do(-m)·ri-gn-i = PRV(-OBJ)-PERF-STEM-AgrS
 Non-declarative compound: ní(-m)·é-ru-bart =
 C_{neg}(-OBJ)-PRV-PERF-STEM.AgrS

(4.72)



The derivations of the non-declarative forms are essentially the same: everything spells out in situ, without post-syntactic operations. The declarative simplex form behaves just like any other deuterotonic form, with the form *ro* conditioned by the adjacent C_{decl}. Finally, *ro* in the declarative compound form is simply spelled out in situ; all of the action is between C, the pronoun (when present), and the first preverb. Any formal differences are due to the fact that C and the verb form a single phonological word, so that whatever is happening in the C domain has phonological implications for the rest of the verbal complex (largely a result of differing stress patterns).

(4.73) summarises the crucial points of the preceding discussion.

- (4.73a) Declarative C undergoes LD when it is adjacent to T.
- (4.73b) Pronouns undergo LD with preverbs when C is declarative.
- (4.73c) Declarative C conditions absolute AgrS and deuterotonic preverbs.
- (4.73d) The perfect exponent raises to C and then otherwise behaves as a preverb.
- (4.73e) The additional inflectional projection in augmented clauses blocks LD.
- (4.73f) Pronouns also block LD.
- (4.73g) If it doesn't leave an overt exponent, C spells out as *no*.

Having now more or less established the basic clausal structure of Old Irish, we are finally in a position to address the actual topic of this section: the behaviour of the object pronouns.

4.2.1.2.3 Implications

I introduced the case of infix object pronouns in Old Irish because of their variable position relative to the other elements of the verbal complex, sometimes proclitic to the verb and sometimes apparently inside it. As established in the previous section, this behaviour relative to the verb is rather superficial: in fact, pronouns are never part of the verbal complex, because their position is actually variable to preverbs, which are also never part of the verbal complex. This is an example of a mismatch between the phonological word and the morphosyntactic word. Both the pronoun and the preverbs are positioned in C, with the pronouns displaying a surface second-position effect within the C domain. All the elements

in C lean phonologically on the verb, but except in the case when C is directly adjacent to a (simplex) verb in T, C and the verb are in two separate morphosyntactic words.

What we need to explain, then, is not why the pronoun can interpolate between elements of the verb, but why it can infiltrate into the cluster of preverbal elements, and what the preverbs are doing in clause-initial position at all. Neither of these questions can receive a definite answer, because of the lack of information available on Old Irish's immediate ancestors; however, there *is* enough available information from comparative evidence and archaic (particularly poetic) internal constructions to arrive at an approximation of the history.

There are three relevant archaic constructions. The first of these we have already seen: the suffixed pronouns, which are still grammatical in Old Irish, though probably part of a separate, rather moribund grammar. As discussed above, when speakers need to construe a simplex verb in a declarative clause with a pronominal object, it is possible to use a suffixed pronoun rather than *no*-support.¹¹³

- (4.74) Beir-th-i.
bring-3rd.sg.-3rd.sg.NEUT.
'He applies it.'

Cowgill (1987) demonstrated posthumously that the suffixed and infix pronouns were not in free variation. The suffixed variant appears with third singular/plural and first plural verbs (and with the first singular future) when the pronoun is masculine or neuter throughout the Old Irish period¹¹⁴; infix versions of these forms are not found. In earlier periods, suffixed pronouns were required in a wider range of contexts (e.g. with third singular feminine and third plural objects of third singular verbs), but infix pronouns gradually took over in these contexts. Eska (2003) argues the extensive phonological changes occurring in the language prior to the Old Irish period had left most of the suffixed forms identical to unsuffixed forms, and that the suffixed constructions which remained in use were those which had remained phonologically distinctive.

Suffixed pronouns only ever occurred on simplex verbs with absolute endings – that is, verbs not preceded by either an overt C particle or a preverb. A grammar capable of generating both suffixed and “infix” pronouns is a grammar in which the pronoun can be either proclitic or enclitic depending on the position of the verb. Therefore, in this grammar, finite verbs can move to C unless they are blocked from doing so by an overt element already in C. Pre-Old Irish had T-to-C movement. The implication is that the post-syntactic LD operation uniting C and the verb in T has replaced an earlier grammar in which this movement occurred in the syntax and the difference between absolute and conjunct endings was owing to a difference in the syntactic position of the verb.¹¹⁵

The other two archaic constructions both involve non-VSO constructions in Old Irish and are considerably more archaic than the suffixed pronouns. They are attested entirely in poetry and non-metrical “rhetorical” prose, and it is entirely possible that they were not actually part of the native grammar of the poets who made use of them; by the Old Irish period, these constructions could well be only half-remembered and may not have been construed as speakers of earlier generations, for whom they were native, would have used them.¹¹⁶ It is best to use them cautiously, but what they reveal is quite interesting.

¹¹³ Example from Ml. 42^b7.

¹¹⁴ Cowgill suspects that this variant was replaced directly by the innovative Middle Irish construction with free pronouns.

¹¹⁵ For discussion of the etymological origins of the absolute and conjunct endings, cf. Cowgill (1975a,b), McCone (1982), Schrijver (1994), Russell (1995), Newton (2006).

¹¹⁶ As a point of comparison, I have a strong impression that Modern English speakers attempting to approximate language from earlier centuries tend not to understand the rules well enough to reproduce something authentic.

The first type of construction, the so-called “Bergin’s Law” sentence (cf. Bergin 1938:197,201), features the finite verb in clause-final position; simplex verbs take conjunct form and compound verbs are prototonic.¹¹⁷

- (4.75a) Ceso femmuin m-bolgaig m-bung.
 although seaweed.ACC blistered.ACC reap.PRES-1st.sg.C
 ‘Although I reap seaweed.’
- (4.75b) Fri aingel n-acall- astar.
 to angel.ACC speak.PRET-3rd.sg.P.DEP
 ‘He spoke to an angel.’
- (4.75c) Óencharide fon Eilg n-árag- ar.
 one.treaty throughout Ireland.ACC establish.PRES-3rd.sg.PASS
 ‘One peace-treaty is established throughout Ireland.’

Whether Old Irish was underlyingly verb-final or had become underlyingly SVO is difficult to determine; but comparative evidence, both within Celtic and across older Indo-European languages, suggests that Old Irish was originally verb-final. Bergin’s Law sentences, therefore, are presumably remnants of that earlier grammar. They provide further evidence in favour of the postulated connection between suffixed pronouns, absolute agreement, and movement into C: since there is no evidence that any Indo-European language has ever been C-final, we would not expect to find absolute agreement on the verb when it is not sentence-initial, and indeed, since these verbs are clause-final, we do not.

The second archaic construction is “*tnesis*”. In a *tnesis* construction, the preverb and verb are separated by at least one constituent, an option not possible in typical Old Irish prose. The preverb may be in sentence-initial position, with the verb sentence-final, as in (4.76a), or it can be preceded by another constituent (4.76b). Object pronouns are, as in verb-initial constructions, attracted to the first preverb, as shown in (4.76c)¹¹⁸, which, interestingly, has *no*-support.

- (4.76a) Ath (mór) cath- u fri crícha comnámát
 PRV (great)battalion-ACC.PL. to border.ACC.PL neighbour.GEN.PL
 ·cuire- thar
 send.PRES-3rd.pl.CD
 ‘He dispatches (great) battalions to the borders of hostile neighbours.’
- (4.76b) Ónd ríg do rea rúas- at.
 from.DEF king.DAT.SG PRV space.ACC.PL create.PRES-3rd.sg.CD
 ‘from the king who has created (celestial) spaces’
- (4.76c) No- m choimmdiu ·coím- a.
 PRV 1st.sg. lord.NOM.SG. cherish.PRES-3rd.sg.
 ‘The Lord cherishes me.’

Although *tnesis* sentences cannot tell us *why* the preverbs were attracted to C, or even how they came to be there syntactically, they do indicate a connection between C and preverbs and (in the case of (4.76c)) between C and pronouns independent of any connections between C and the verb. They also imply the *lack* of a fixed syntactic connection between at least some preverbs and the verb, although the preverbs other than the first may have occurred later in the clause.

¹¹⁷ Thurneysen 1949:327 notes a peculiar feature of (4.75a): the subordinating conjunction *ceso* contains a fossilised copula. This is the rule for Bergin’s Law sentences; negative particles in such sentences also include fossilised copulae.

¹¹⁸ From Sg 204.

Taken together, these three constructions indicate that pronouns were enclitic to whatever was in C, be it a C-particle, a preverb, or the verb itself. Though it is not clear how Old Irish developed default VSO syntax (for a review of the literature on this problem cf. Newton 2006), it does appear to be the case that the rather odd behaviour of pronouns in Old Irish is due to their having been trapped when the basic syntax of the language changed. The position of the pronouns relative to other elements present in C has not changed, but the pronouns appear to be trapped inside the verbal complex because the changes in syntax that resulted in V1 structures interacted with the prosodic structure in such a way that the entire C domain became part of the phonological word containing the verb.

It is interesting to note that when the syntax changed, such that V-to-T movement was lost, speakers preserved the superficial appearance of it by introducing post-syntactic operations. The interaction between pronouns, absolute endings, and *no* support indicates that Old Irish no longer has V-to-T movement, but absolute endings were not lost when this happened. This raises interesting questions about the development of LD operations in general. Do language learners postulate these rules in order to produce sentences that are superficially like those of their elders once they have acquired a different analysis of the underlying structure?

There is one additional variable worthy of mention here. I have assumed throughout this discussion that the infix pronouns are, in fact, pronouns; however, Eska (2010) has recently argued that they are actually AgrO markers. While considerations of space prevent me from rehearsing his paper, his arguments deserve to be taken seriously, and thus we should consider the implications for the clausal syntax if the pronouns are agreement morphemes rather than pronouns. If Eska is correct, then the infix pronouns are not present in the syntax, but inserted post-syntactically. This is primarily a problem for the derivation of absolute verb forms, where the pronouns seem to have syntactic repercussions: they must be inserted early enough to block LD of C to the verb. The complication is that C is attracted to T, not to AgrS, which means that AgrS cannot be inserted until after LD has occurred. It is not yet clear to me whether these issues can be reconciled.

4.2.1.2.4 Previous Accounts

In this section I will demonstrate that my analysis of the clausal structure of Old Irish avoids many of the empirical and theoretical problems faced by previous generative accounts. 4.2.1.2.4.1 deals with the suggestion by Carnie, Harley, and Pyatt (2000) that the Old Irish pattern reflects a filled C requirement, such that verbs move sometimes to T and sometimes to C; 4.2.1.2.4.2 concentrates on the argument by Newton (2006) that Agree, syntactic features, and the Stray Affix Filter are responsible for the data; and 4.2.1.2.4.3 focuses on the post-syntactic account in Adger (2006).

4.2.1.2.4.1 Carnie, Harley, and Pyatt

The earliest and simplest generative analysis of Old Irish clausal structure concludes that the grammar is a “weak” variant of the well-known V2 parameter. Presented first in Carnie, Pyatt, and Harley (1994) and later further articulated as Carnie, Harley, and Pyatt (2000) (henceforth CHP), this analysis is intuitively appealing, since it extends a well-known analysis of geographically close languages with only minor modifications. Unfortunately, it encounters significant empirical difficulties on closer examination.

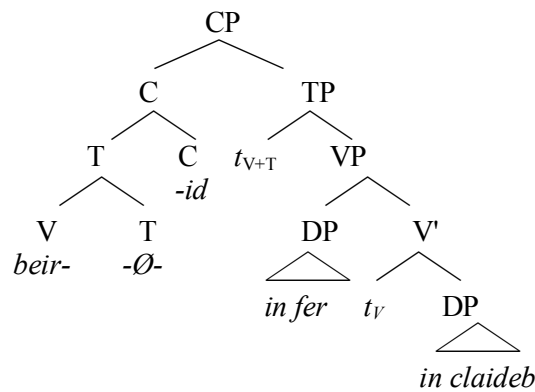
In classic V2 languages like German and Dutch, the verb appears in second position in tensed main clauses following an XP of any category, but appears in clause-final position in subordinate clauses containing complementizers. The standard analysis of V2 (e.g. den Besten 1981) maintains that these languages require C⁰ to be filled in tensed clauses. In subordinate clauses, this position is filled by the complementizer; in matrix clauses the verb

raises to fill the position. An additional requirement mandates that the specifier of a matrix complementizer also be filled by some constituent or other.

- (4.77a) Karl kaufte gestern dieses Buch.
 bought yesterday this book
 (4.77b) Dieses Buch kaufte Karl gestern.
 (4.77c) Gestern kaufte Karl dieses Buch.
 ‘Karl bought this book yesterday.’
 (4.77d) Ich dachte daß Karl gestern das Buch gekauft hat.
 I thought that yesterday the book bought has
 ‘I thought that Karl had bought the book yesterday.’

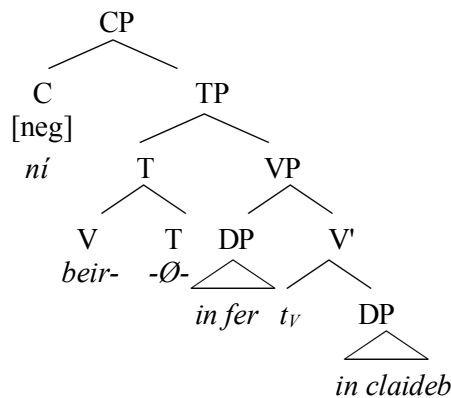
CHP argue that Old Irish clausal syntax differs from that of German only in lacking the requirement that spec-CP be filled. Thus, the structure for the sentence (4.78a) is (4.78b).

- (4.78a) Beir- id in fer in claideb.
 carry.PRES-3rd.sg.A DEF man.NOM DEF sword.ACC
 ‘The man carries the sword.’
 (4.78b)



Following Chung and McCloskey (1987), CHP assume that conjunct particles (such as negation) occupy C⁰. Therefore, when these particles are present, the verb need not raise as far as C, settling instead in T.

- (4.79a) Ní beir in fer in claideb.
 NEG carry.PRES.3rd.sg.C DEF man.NOM DEF sword.ACC
 ‘The man doesn’t carry the sword.’
 (4.79b)



The position of the verb determines its morphology: when it is in C, it takes absolute endings; when it is in T, it takes only conjunct endings. This is because in the former case, the verb is actually incorporating with a null C, and this additional morphological information must be realised overtly. The positioning of pronominal objects correlates well with this view. If pronominal objects adjoin to the right edge of C – in accordance with Wackernagel’s Law – then they ought to appear to the verb’s right when it is in C and to its left when in T. This, of course, is precisely the pattern we find.

CHP’s system accounts quite elegantly for the distribution of simplex verbs (though cf. below), but encounters significant difficulty when applied to compound verbs. The problem is not the prototonic forms, which can be accounted for in exactly the same fashion as the simplex conjunct forms: with the presence of overt material in C, the verb raises only so high as T. Deuterotonic forms, on the other hand, require more explanation. It must be the case that the first preverb of a deuterotonic form is filling C, while the rest of the verb remains in T. CHP suggest that there is good evidence for this in the form of relative clauses, where some preverbs show a special form in exactly those cases where there is no preverbal complementizer.

- (4.80a) imm·rádi
 PRV-speak.3rd.sg.C
 ‘he thinks/meditates’
- (4.80b) imm-a·rádi
 PRV-REL-speak.3rd.sg.C
 ‘who thinks/meditates’

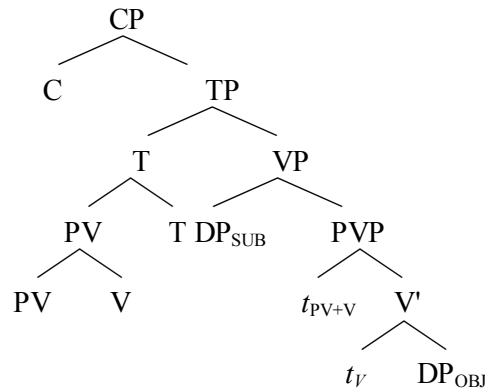
The problem is how to get the preverb into the required position. CHP, following Hale and Keyser (1993), assume that (4.81b), their (31), is the basic pre-movement structure of (4.81a), which contains a deuterotonic verb.

- (4.81a) As· biur in so.
 PRV-say.PRES.1st.sg.CD this
 ‘I say this.’
- (4.81b) [_{CP} [Ø [_{IP} [Infl] [_{VP} *pro* [_V as [_V biur in so]]]]]]]

This makes sense semantically, but presents something of a problem, because it requires the preverb and verb to move independently to C and T respectively, in a violation of the Head Movement Constraint. In addition, Adger (2006:625-6) argues that the CHP account makes the wrong predictions about when movement to C is licensed. V must be allowed to raise to adjoin to preverbs, since when C is filled, this is precisely what happens; in addition, this is what happens to all but the first preverb when C is *not* filled.¹¹⁹

¹¹⁹ (4.82) is taken from Adger’s (55) [p. 625] with only minor changes.

(4.82)



If this structure were simply raised to C, it would satisfy the fill-C requirement, just as absolute simplex forms do. This would wrongly predict a prototonic form with absolute agreement and suffixed pronouns, and therefore must be blocked. Blocking this, as Adger points out, is not easy, because simply outlawing T-to-C movement would not allow the movement in simplex verbs. His contention is that there is no way of allowing both possibilities without stipulation.

CHP offer an alternative solution using Long Head Movement (LHM; cf. Rivero 1991, 1994, 2000; Roberts 1994; Borsley et al. 1996; Holloway-King 1997). LHM is found in a number of Slavic and Romance languages, in addition to the Celtic language Breton. Under LHM, a participle moves across a number of intervening positions into C. In the case at hand, this would mean that C must be an A' position, thus enabling it to bind traces across A positions (including T). But this is not without problems, particularly because the data do not actually behave like LHM data from other languages. LHM generally involves participles, not particles, and usually involves licensing a tense feature in C (Rivero 2000). In addition, LHM does not usually occur in embedded clauses, and it would have to in Old Irish.

In short, there is no easy way to account for Old Irish deuterotonic verbs via LHM, unless the existing theories were extensively revised. The weak-V2 analysis might nevertheless be worth retaining were this the only obstacle, but it also fails to account for the most productive pattern of object pronouns. CHP are correct in saying that only absolute forms can take suffixed pronouns, but they fail to observe that suffixed pronouns are not allowed with all absolute forms: in most cases, simplex verbs in need of object pronouns are made conjunct by brute force. There is no way to explain this in CHP's system, which predicts that suffixed pronouns should be possible with all absolute forms. Instead, as Newton (2006:32) points out, the presence of object pronouns seems to block T-to-C movement in the ordinary case. She argues that this is evidence for verb movement only as far as T in Old Irish.

A similar argument can be made from relative clauses. There are several different ways of marking relative clauses in Old Irish, one of which involves a special form of the verb. This is restricted to simplex absolute forms, which could be taken to support the CHP account; except that, once again, the distribution of these forms is limited to the third person (although, in the very earliest texts, first person plural forms are also found; cf. Thurneysen 1946:313).

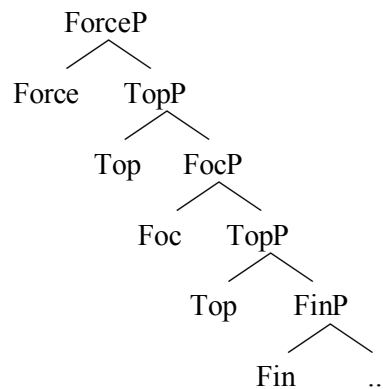
The upshot is that the weak-V2 analysis advocated by CHP cannot be upheld, at least in its present formulation, despite its considerable intuitive appeal. My analysis avoids these problems by treating the interaction between C and the verb as a post-syntactic operation rather than a syntactic operation. However, it is important to note that CHP's analysis is quite plausible for the stage of grammar immediately preceding Old Irish, when suffixed pronominal forms were still productive.

4.2.1.2.4.2. Adger

Like the current account, Adger (2000, 2006) argues that the Old Irish data cannot be accounted for via an exclusively syntactic account and require post-syntactic operations; he also jettisons V-to-C movement and appeals to LD. However, as we will see, his analysis encounters some of the same empirical problems as CHP's weak-V2 analysis, in addition to requiring a somewhat non-standard use of LD.

Adger adopts the articulated CP structure of Rizzi (1997) shown in (4.83), with discrete positions for Force, Topic, and Focus. Topic and Focus are only projected in the syntax when they are required semantically, but Force is always present and is taken to encode whether or not the clause is embedded rather than whether it is imperative, interrogative, or negated. In this respect, it differs from Newton's [force] feature, which seems to allow a greater variety of values.¹²⁰

(4.83)



The crux of Adger's proposal is stated in (4.84), his (73). The remainder of his argument concentrates on how (4.84) interacts with the basic schematisation of the Irish verbal cluster (4.49), repeated here as (4.85) for the sake of convenience.

(4.84) Force is subcategorised to be enclitic to an X^0 .

(4.85) [X] . [Y + Z + W]

(4.84) and (4.85) interact as follows. The Force projection is higher in the structure than anything that will occupy the X slot; therefore, in the immediate linearization of the clause, Force linearly precedes X. However, (4.84) requires that Force be enclitic to something, and therefore it is subject to Local Dislocation, thus:

(4.86) Force X Y Z W → [X + Force] Y Z W

This means that the prosodic difference between prototonic and deuterotonic verb forms does not reflect an underlying difference in the syntactic position of the first preverb under Adger's analysis. Recall that Old Irish words are almost always stressed on the initial syllable; Green (1997) accounts for this via a left-headed foot at the left edge of the prosodic word. The theories previously discussed supposed that deuterotonic forms do not fit this pattern because the first preverb is not actually part of the lexical word. Adger's analysis

¹²⁰ Newton (2006:59) criticises Adger's Force projection as unmotivated, maintaining that her proposal is superior because [force] is a feature rather than a projection in her analysis. It is not clear to me that this is in fact a significant difference, as the use which the two authors make of Force/[force] is very similar.

supposes that preverbs are always part of the same morphosyntactic word. To account for the apparent paradox, he suggests that Force interferes with the ordinary mechanisms of stress assignment. He argues (2006:634–6) that Force typically marks the left edge of large phonological constituents (i.e. phonological phrases or intonation phrases) as stated in (4.87):

(4.87) Align the left edge of a phonological phrase (or perhaps an intonational phrase) with the right edge of Force.

Since a phonological phrase cannot be contained within a prosodic word (cf. Selkirk 1995), the ordinary prosodic structure cannot be maintained, as it would result in the illicit (4.88). Instead, the initial preverb becomes extrametrical, and the verb is footed with the remaining preverbs (if any), as shown in (4.89). Only deuterotonic verbs must be irregularly stressed in this way because in all other cases, Force and ordinary prosody align. The prosodic structures Adger supposes for the remaining types of verbs are shown in (4.90): (4.90a) is the structure for an absolute verb, (4.90b) for a simplex conjunct verb, and (4.90c) for a prototonic verb.

(4.88) (ω PV+Force (φ PV V

(4.89) PV+Force (φ (ω PV V

(4.90a) (ω V ω) Force (φ ...

(4.90b) C+Force (φ (ω V

(4.90c) C+Force (φ (ω PV V

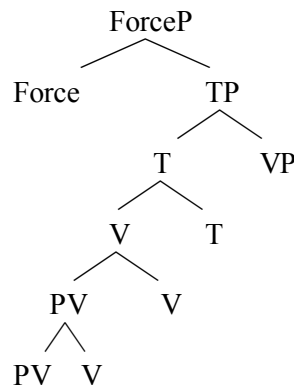
Adger (2006:637ffn.) also argues that it is the Force feature which conditions verbal morphology: when Force immediately follows the verb, as it does when X is a simplex verb, absolute verbal endings are required, as in (4.91a). When Force cliticises to a conjunct particle, as in the case of simplex conjunct forms and compound prototonic forms, conjunct endings are required, as in (4.91b).

(4.91a) [absolute] ↔ T / $_$ Force

(4.91b) [conjunct] ↔ T

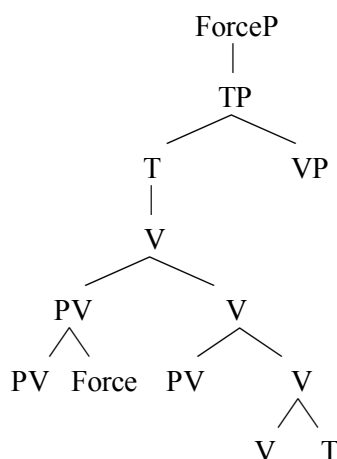
Once again, deuterotonic verbs require a bit more explanation. Adger (2006:638) assumes that the basic syntactic structure of a compound verb is (4.92).

(4.92)



Following the treatment of Lithuanian in Embick and Noyer (2001) (cf. above), he then uses LD to adjoin T and V, and then again to adjoin Force to the first preverb. This configuration does not meet the conditions for absolute endings in (4.91a), and therefore conjunct endings are used. Adger's derivation of this is given in (4.93), his (99).¹²¹ Force cannot condition the preverb and stem allomorphy between prototonic and deuterotonic forms, which therefore must be purely the result of differing stress patterns under Adger's account. It is not actually clear from the available evidence that the two forms could have been synchronically derived from each other in this manner.

(4.93)



Newton (2006:59) criticises this use of Force on grounds of learnability. The Force head Adger postulates has no overt reflection in the syntax and does not seem to have any semantic, pragmatic, or discourse effects. Nor does Old Irish provide the same strong empirical evidence for an articulated CP that Rizzi discusses in Italian. She questions, therefore, whether children learning Old Irish would have been able to acquire a ForceP at all, and further how they would acquire the fact that the null Force head is an enclitic. This is the crucial difference between Adger's analysis of Old Irish and Embick and Noyer's analysis of Lithuanian (as well as the analysis of Old Irish argued for above); the latter dealt with LD involving overt phonological material.

The positioning of pronouns is accounted for in Adger's system by his Topic projection. Following Rizzi (1997) and Roberts (2005), he argues that pronouns, as old information, first move syntactically into TopP, immediately to the right of Force, and then must lower post-syntactically. The post-syntactic movement of the pronouns is motivated in much the same way as that of Force; they are enclitics. Adger (2006:646) identifies two possible derivations here:

(4.94a) Force lowers to the pronoun, and then the newly complex pronoun lowers to the verb.

(4.94b) The pronoun lowers to the verb, and Force lowers to this complex.

Adger takes the first position because of the restriction of suffixed pronouns to verbs with absolute inflection, arguing that (4.95) represents the structure of simple verbs with suffixed pronouns. He argues that suffixed pronouns condition absolute agreement, as in

¹²¹ Adger's presentation here is somewhat idiosyncratic; most DM authors use bracket notation for post-syntactic operations rather than the tree structures Adger employs.

(4.96)¹²², and therefore must be closer to T than Force. This, in turn, explains why relative verbs have absolute rather than conjunct endings.

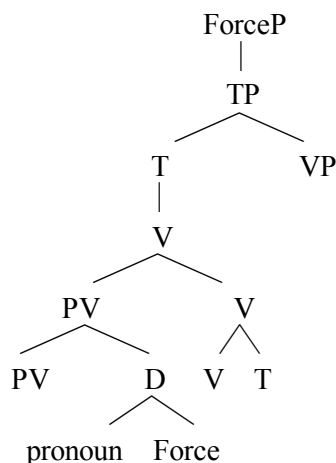
(4.95) [[V+T]+[_D pronoun+Force]]

(4.96) [absolute] ↔ T / pronoun

There is additional, theory-internal motivation for Adger's choice of (4.94a). He needs Force to create a phonological boundary to its left in order to account for deuterotonic forms. If Force were closer to the verb than the pronoun, this would incorrectly predict that the pronoun and verb must be in separate prosodic words; in fact, verbs and suffixed pronouns are clearly part of the same prosodic word.

So far the discussion has concentrated on suffixed pronouns. Adger does not discuss the interaction between infixes and conjunct particles in detail, but says that when a conjunct particle is present, the pronoun-Force complex suffixes to this rather than to the verb. He does discuss infixes in conjunction with deuterotonic verbs, giving (4.97) (his (131)) as the structure after the lowering of the pronoun and Force.

(4.97)



Adger (2006:647-8) argues that this structure makes the correct predictions about the interaction between pronouns and the entities directly to their right. Each of the pronominal infixes triggers initial mutations on the verb; for instance, the first singular lenites the following the segment (4.98a) while the feminine singular nasalises (4.98b).¹²³ In these examples, the mutated segment is in boldface.

(4.98a) Ma immi-m· **thabar-** thar.
 if PRV- 1st.sg.-surround.PRES-3rd.sg.C.DEP
 ‘If I am surrounded.’

(4.98b) Du- s· **ngni.**
 PRV-3rd.sg.FEM.-make.PRES.3rd.sg.CD
 ‘He makes it (fem).’

¹²² Similarly, the presence of a suffixed pronoun triggers active endings on deponent verbs: *bertaigidir* ‘shakes’ with the deponent third singular ending *-idir* becomes *bertaighth* with the active ending in the presence of the suffixed (3rd.pl.) pronoun *-ius*.

¹²³ (4.98a) from Ml. 41^{c2}, (4.98b) from Ml. 29^{a3}

If the pronoun adjoined to the preverb before Force adjoined to it, Adger argues, then Force, rather than the pronoun, would be hierarchically dominant, and therefore it would block the pronoun from triggering initial mutations, and possibly trigger its own.

This account, and particularly the respective ordering of pronoun and Force head, encounters a number of problems, some empirical and some theoretical. These problems begin with the portion of the account just described. The pronoun must condition the initial mutations, but it is linear adjacency, *not* hierarchical dominance, which is likely to be responsible; this is the sort of effect predicted to be conditioned locally. This means that the intervention of Force between pronoun and verb ought to *block* the pronoun from triggering initial mutations on the verb, particularly when the Force head is supposed to cause a prosodic boundary to form to its left.

This is not the only problem, however. Saying that Force attaches to the pronoun first effectively creates an undesirable disjunction: it requires Adger to say that *both* pronouns and Force condition absolute endings (4.99). If the opposite ordering were adopted, leading to the structure in (4.100a), the simpler (4.91), repeated for convenience as (4.100b), could be postulated instead.

- (4.99a) [[V+T]+[_D pronoun+Force]]
 (4.99b) [absolute] ↔ T / pronoun, Force
 (4.99c) [conjunct] ↔ T
- (4.100a) [[V+T]+[_D Force+pronoun]]
 (4.100b) [absolute] ↔ T / Force
 (4.100c) [conjunct] ↔ T

The most serious problem, however, is empirical, and has to do with the distribution of suffixed pronouns. Adger's account essentially predicts complete complimentary distribution between absolute and conjunct forms, triggered in part by pronouns; but while there *is* complimentary distribution, it does not really work in this way. As previously discussed in connection with the CHP analysis, the use of these pronouns is extremely restricted. Simplex verbs without clearly motivated conjunct particles are made conjunct more or less by brute force whenever they need an object, unless they are third singular and the object is third singular masculine or neuter.

Adger's account is simply too deterministic; there is no obvious way to implement these necessary restrictions. Pronouns and their accompanying Force heads lower to a specific position and thereby force certain endings; but where is the specification of their position? In Adger's system, this is implemented mechanically, without reference to the ϕ -features on both pronoun and verb. But the dummy conjunct particle *no* only manifests if the simplex verb has been made conjunct by virtue of its bearing a pronoun which cannot be suffixed. *No*, therefore, must be inserted before the pronoun moves, so that the pronoun knows it must be suffixed to C rather than to the verb; yet the pronominal ϕ -features *as well as* verbal ϕ -features are relevant in determining whether *no* must be inserted. This is clearly rather circular. Thus, although Adger's account has some advantages, it encounters serious difficulties with the object pronouns, beginning with the necessity of moving them out of TopP into the positions in which they surface.

4.2.1.2.4.3. Newton

Newton's analysis of this data, presented in her 2006 dissertation, is based on observations very similar to those on which I have based mine. For instance, she, too, identifies the similarity between Old Irish *no*-support and English *do*-support, and suggests that the answer to the Old Irish problem lies in post-syntactic operations rather than in the syntax proper. However, her theoretical framework, particularly in its definition of "post-

syntactic operation”, is significantly different from that assumed here, with the result that her analysis is ultimately very different in character. She argues that the post-syntactic operations of DM are illicit because they involve movement, relying instead upon a combination of Agree and the Stranded Affix Filter. As a result, her account is very intricate, and the following summary necessarily omits many of the details. Since her theoretical assumptions are very different from those assumed in this dissertation, I will summarise the key points before summarising her treatment of Old Irish.

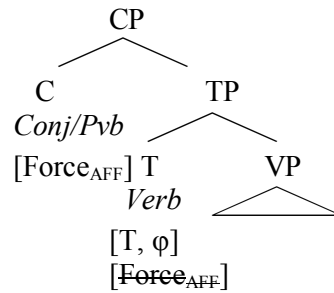
The first thing to note is Newton’s treatment of affixation, which is very different from that assumed elsewhere in this dissertation. Rather than a syntactic relationship, she assumes that ‘[affix] is a morphological property that is associated with individual features, i.e. a morphological subfeature relevant at the PF-interface’ (Newton 2006:64). She then argues that a feature marked with the subfeature [affix] must be able to combine with another morphosyntactic feature, either in the syntax or at PF. This is to satisfy the Stranded Affix Filter (SAF), which requires that features have hosts. The simplest way of satisfying this requirement in the case of [affix] is for the [affix] to appear under the same syntactic node as another morphosyntactic feature. The feature that provides a host for the [affix] diacritic must have a positive value – that is, it cannot have a default setting.

The other theoretical assumptions crucial to Newton’s analysis concern agreement, which works as follows. The two heads participating in the Agree relation – say, X and Y – both have the features *x* and *y*, but at the beginning of the derivation, one of these heads has unvalued features. When Agree has taken place, however, both *x* and *y* will be present on X and Y with identical values. At Spell-Out, one set of these features will receive a phonological realisation. This can happen at either X or Y, and the operation Chain Reduction is responsible for determining which. Under ordinary circumstances, Chain Reduction marks the leftmost copy of a moved entity for phonological realisation and deletes the rest; however, Newton (2006:62) extends this operation to include all features in an Agree relation. Following Chomsky (2001:11), she says that identical features resulting from Agree fill the same requirements as moved elements because their feature content is, by necessity, identical, and because in order for Agreement to occur in the first place, the Probe and Goal must be in a c-command relation. Newton then argues that Chain Reduction need not always delete the leftmost entity in a chain. Since Chain Reduction is a post-syntactic operation, requirements at PF are responsible for selecting which copy is realised, and these requirements may choose either member of the Agreeing pair. This is Newton’s version of Lowering or Affix Hopping; the latter is the term she uses.

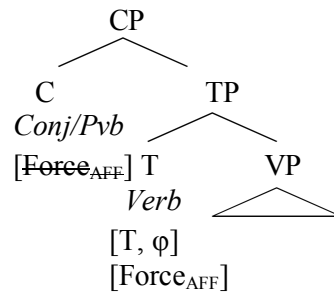
Newton’s analysis of Old Irish builds on these theoretical assumptions, in addition to another specific to Old Irish, namely that Old Irish had V-to-T movement, as in Modern Irish, but not V-to-C movement, *contra* CHP. She suggests that absolute morphology is the result of Affix Hopping. The feature conditioning these endings is shared by both C and T, and, as it has the property [affix], it therefore must be realised. Newton identifies this shared feature as [force]; it cannot be simply \varnothing -features, since conjunct forms show subject agreement. [force] indicates whether a clause is affirmative, negative, interrogative, and so on, and can only be spelled out in one position in a clause at a time. If C has some overt material, then the feature is realised on C (4.101), but when it is not, then the features on T are realised instead (4.102).¹²⁴

¹²⁴ (4.101–102) are taken directly from Newton (2006:68), her (50) and (51).

(4.101) *Derivation of Conjunct Forms*



(4.102) *Derivation of Absolute Forms*



Newton (2006:72–3) also has recourse to the SAF to account for the *no*-insertion phenomenon. Following the intuition that *no* is a Last Resort strategy, the simplest insertion condition for *no* would be for C when it does not have more specific features, as in (4.103). However, Newton rightly points out that this cannot be correct, because it would predict that *no* would always appear to the exclusion of absolute forms, which is clearly false.

(4.103) *Vocabulary Insertion for C*

[C [+ negative]]	↔	ní
[C [+ conjunction]]	↔	co ⁿ , dia ⁿ , ara ⁿ
[C [+ preverb]]	↔	do, fo, as, ro...
[C]	↔	no

Instead, she argues that when C contains only an affixal [force] feature, without a host, this feature cannot be realised, in accordance with the SAF, and the restriction extends to the expression of the entire head C, rather than simply just the feature bearing the [affix] diacritic. If C contains only the affixal feature [force], which cannot receive expression, it is therefore deleted entirely. It can be realised only when it bears additional features. This allows her to use the VI articulated above while still allowing for the null realisation of C in clauses with absolute verbs, since she is assuming that C in these clauses has been deleted prior to VI.

Since preverbs have C features in this account, the conjunct agreement on compound verbs is expected; it thus remains to account for the position of the preverb. Like CHP, Newton is assuming that preverbs begin as the heads of light verb projections. She differs from them in also assuming that the preverb never moves to C. The only movement involving preverbs, she argues (p. 74–5), is that by which the preverb and verb are incorporated into a single head.

Instead of movement, Newton derives the deuterotonic forms via another Agree relation, this time between C and the light verb. C, she argues, has an unvalued verbal feature. Although it is always present, this feature is only phonologically realised when there are no other more specific features in C. The closest entity in the sentence with a feature that can value this feature on C is the highest preverb in the structure. When C receives its value

from the preverb, then this feature is spelled out in C, and the lower copy is deleted by Chain Reduction. Newton argues that the relevant feature is one of verb class, and that in fact preverbs are not the reflexes of *v* per se, but rather specifically the expression of the verb class feature.

A potential problem with this account is that when C does have other features, then its features are not identical to the features on the lower copy, so that the latter should not be deleted. Newton argues (p. 83) that in fact all the features that might be realised on C are also realised on the lower copy: secondary tenses involve aspect, which is associated with *v*; possible object agreement features on C are also associated with *v*; and *wh*-features can be marked on *v*. In all these cases, then, the result is a deuterotonic form. Only other features (like negation) are not marked on *v* and trigger prototonic forms.

What Newton does not seem to address is the fact that if C always has an unvalued *v* feature, then it has an unvalued *v* feature in clauses with simplex verbs as well as compound verbs – including simplex verbs with absolute agreement. Newton’s account maintains that the entire C head is deleted when absolute verbs are inserted because they have only an affixal [force] feature which cannot be valued. She does not explain why the unvalued verb class feature on C does not prevent this deletion.

Newton also does not discuss directly the placement of object pronouns, apart from noting that they move into the C projection (fn. 61).

4.2.2 The Elusive Nature of (Phonological) Infixation

The idea that affixes appear in the same linear position in which they appeared before they were affixes makes intuitive sense when the affixes in question are prefixes or suffixes: obviously, prefixes develop from M-words that linearly preceded the M-word to which they now belong, and suffixes from M-words that linearly followed. If we took phonological infixes at face value, their very existence would challenge the theory that affixes are generated in specific locations under specific principles: wherever their pre-affix ancestors may have appeared in the clause, it surely wasn’t *there*. But since it has been recognised repeatedly that “classic” infixation is really a phonological process operating on underlying prefixes or suffixes, the problem of infixation is not as difficult as it appears at first glance. Infixes must have originated as affixes of a less-exotic character.

Phonological infixes are, unfortunately, difficult for other reasons. The previous subsection dealt with cases of clitics interpolating into M-words and essentially becoming “trapped” there, continuing to respect the morpheme boundaries of the M-word they belong to. Though the precise details of how this came to occur are murky, there are enough clues to allow us to paint a reasonable picture of how such a situation may have come to pass. Phonological infixes are a trickier case. They do respect the morpheme boundaries of the M-word they belong to, and they are very difficult to account for historically because there is little information available as to how they may have come to be there; all of the attested infixes seem to have existed in the earliest reconstructed stages of the languages in which they occur. Moreover, they do not serve the same function as the trapped clitics from the earlier portions of this chapter, so we cannot simply extrapolate from more recent examples that they were once trapped clitics.

A clear example of an infix which is syntactically a prefix comes from Tagalog. As shown in (4.104), the Tagalog affix *um* is a prefix when attached to a vowel-initial form, but emerges as an infix with consonant-initial verbs. McCarthy and Prince (1993b) suggest that this is because the alignment requirement (that the *m* not emerge as a coda) outranks *um*’s specification as a prefix.

(4.104a)	<i>um</i> -aral	(u.ma.ral)		‘teach’
(4.104b)	<i>s-um</i> -ulat	(su.mu.lat)	* <i>um</i> -sulat	‘write’
	<i>gr-um</i> -adwet	(gru.mad.wet)	* <i>um</i> -gradwet	‘graduate’

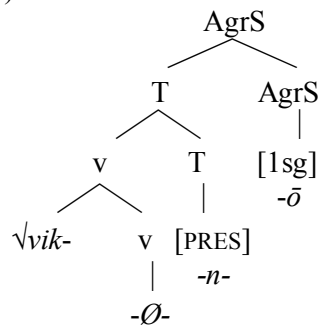
Though it sometimes surfaces as an infix, in structural terms *um-* is clearly a prefix; its infixation occurs under very specific circumstances which can be stated in phonological terms, and therefore must occur at PF. All of this is clear. The origin of the infixation rule, however, is completely obscure.

A further complication is that some “infixes” may not be synchronically affixes at all, but the output of a readjustment rule. The diagnosis is not always straightforward. One example of this type is the Proto-Indo-European (PIE) “nasal infix”, a present-forming affix which occurs before the root-final consonant in some PIE roots; the resulting syllabification is not otherwise permitted in PIE. Relics of the nasal infix can be found in various Indo-European languages; some examples are given in (4.105).

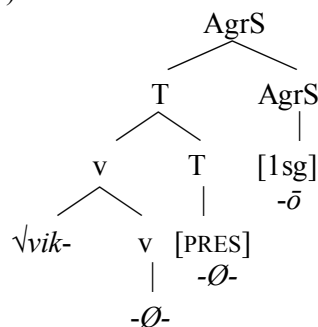
(4.105a) Latin:	present	vi-	n-	c-	ō	‘I conquer’
		conquer ₁ -PRES-conquer ₂ -1 st .sg.				
	perfect	vīc-			ī	‘I conquered’
		conquer.PERF-1 st .sg.				
(4.105b) Sanskrit:	present	yu-	ná-	k-	ti	‘He joins’
		join ₁ -PRES-join ₂ -3 rd .sg.				
	perfect	yu-	yój-		a	‘He joined’
		RED-join.PERF-3 rd .sg.				

Roots which form nasal presents all contain a medial sonorant,¹²⁵ which suggests that the locus of the nasal infix is determined by the phonological shape of the root. If this is the case, then either the infix is syntactically a suffix, appearing in the root through a subsequent phonological operation (4.106a), or it is the product of a readjustment rule rather than a bona fide exponent of the category [pres] (4.106b); the illustration in (4.106) uses Latin.

(4.106a)



(4.106b)



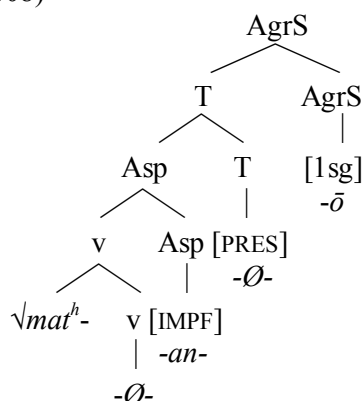
¹²⁵ The surviving roots known to have formed nasal presents have a second apparent restriction, in that they cannot end in a liquid or nasal. Don Ringe (p.c.) has pointed out that this may be partly a statistical accident (since *l and *r cannot occur in the same root). The only *clear* constraint, however, is the restriction to roots with medial sonorants.

It is not difficult to see that the most likely source for a readjustment rule like (4.106b) is a migrant suffix like that in (4.106a), and how such a reanalysis might have happened is quite clear. Infixes violate the Mirror Principle: a child learning this language knows that tense suffixes ought to be outside of the verb and *v*, and the nasal infix, counter to expectations, is in the root instead. A child learning an older Indo-European language would have also learned that verbal (and nominal) morphology frequently involved null suffixes accompanied by readjustment rules adding or changing the quality of vowels in the root. In that context, analysing nasal-infix presents as a null present suffix and a nasal added to the root by readjustment rule might have been very much in keeping with the general system. Thus, although it is impossible to say which analysis was correct for the reconstructed proto-language, we can at least say that it passed through a stage (4.106a) at some point.

The reflexes of the nasal infix in PIE's non-reconstructed daughters tend to be easier to analyse.¹²⁶ Perhaps the most interesting development occurred in Classical Greek, where the original nasal infix rule was replaced by something quite different. Unlike Latin, Greek has no direct reflexes of nasal infix presents; what Greek has, synchronically¹²⁷, is an innovative construction in which a nasal infix co-occurs with the suffix *-an-*. The Greek present of *mant^hánō* 'I learn' is contrasted with the corresponding aorist *émat^hon* ('I learned') in (4.107); the underlying structure is shown in (4.108).

(4.107) Greek:	<i>present</i>	ma- n- t ^h - an- ō
		learn ₁ -PRES-learn ₂ -PRES-1 st .sg.
	<i>aorist</i>	é- mat ^h - on
		AUG-learn.AOR-1 st .sg.

(4.108)



How exactly this particular species of present was derived is unclear, but the fact that the nasal readjustment always co-occurs with a suffix *also containing a nasal* is very interesting, because the pattern looks rather as though language learners, confused about whether the nasal was a suffix or the product of a readjustment rule, decided to use both options just to be on the safe side. The complete absence of data on how this might have happened is decidedly unfortunate.

As suggested at the outset, the most logical hypothesis concerning the origins of Greek-type readjustment infixes is that they were once Tagalog-type surface infixes – that is, they were originally syntactically prefixes or suffixes. If this hypothesis is correct, we are left with unanswered questions regarding the origin of the Tagalog type: under what

¹²⁶ However, Latin, where the examples are marginal, is not one of these clearer cases, although the relic status of the attested Latin forms probably makes (4.106b) a better choice.

¹²⁷ Most of the apparent examples of nasal infix presents in Greek derive historically from other sources (Don Ringe, p.c.).

circumstances do ordinary prefixes and suffixes begin to interact with the phonology of other Sub-words to the extent that they become surface infixes?

Note that this is actually a morphophonological problem rather than a morphosyntactic problem: while it requires an explanation, it does not require a readjustment of the scenario outlined in Chapter Three, both because surface infixes were originally ordinary affixes and because structurally they still are. This does not make the problem any easier to solve, of course; but at least we can identify the level of the grammar at which to look for a solution.

4.2.3 Endoclititics: Udi

Infixes are potentially controversial, but their existence is well-supported. Much more controversial is the subject of endoclititics, which by their very existence challenge not only notions of lexical integrity, but notions of morphemic integrity. Endoclititics are clitics which interpolate themselves not only within an M-word, like mesoclititics in European Portuguese, but within a Sub-word. Most theories of morphosyntax predict that endoclititics should not exist, but the Lezgian language Udi appears to be the exception. As discussed by Harris (2002), Udi has very complex rules determining the placement of subject-agreement enclitics, which occur in different places depending on various structural properties of the sentence. The variable position of these clitics is itself a challenge.

My purpose in presenting the Udi data is not to provide an account of the problem; the data are complex enough that this lies well beyond the scope of this dissertation, and their prehistory lies too far in the realm of speculation. The goals of this section are more modest: to determine whether the Udi data are truly problematic for the theoretical position taken here, and to evaluate Harris's stance on the matter. I will conclude that, in fact, endoclititics in Udi are a problem, but only as a sub-case of the more general problem of infixes.

I begin by quickly laying out the essential properties of the Udi grammar (4.2.3.1) before elucidating the seven rules of clitic placement formulated in Harris (2002) (4.2.3.2). As the written attestation of Udi extends back into the mid-nineteenth century, I discuss some of the properties of earlier texts, as they differ from the modern language, in 4.2.3.3. 4.2.3.4 discusses diachronic issues, using the proposals of Harris (2002) as a point of departure.

4.2.3.1 Basic Grammar

An endangered language belonging to the Lezgian branch of the Nakh-Daghestan family, Udi is currently spoken only in a few villages in Azerbaijan, although it is believed to be the descendant of a language known as "Aluanian" or "Caucasian Albanian", which was spoken over most of modern Azerbaijan. As shown in (4.109), Udi has an ergative case system with differential case marking, so that definite direct objects are marked dative rather than absolutive, and its basic word order is SOV, although, as shown in (4.110), it does allow scrambling.¹²⁸

- (4.109) pačay-un čuby- on eš- ury-o t'ak'a-**ne**- p- e...
king- GEN woman-ERG apple-PL- DAT place- **3SG-SAY-AORII**
'The king's wife placed the apples...' (40)

¹²⁸ All examples are taken from Harris (2002), with her glosses reproduced faithfully. Examples are marked with the page number on which they appear, due to the length of the book. For convenience, all subject clitics are rendered in boldface type.

- (4.110) pačay-un čuby- on sa q'əzəl-on manatluy
king- GEN woman-ERG one gold- GEN coinCOLL.ABSL.
ta- **ne-** d- e q'oja čuby- o
thither-3SG-DO-AOR old.woman-DAT
'The king's wife gave a cold manat [coin] to the old woman.' (40)

Negation usually precedes the verb (4.111). Some forms of the verb also allow post-verbal negation, but never mandate it (4.112). Negation can also appear in other positions for reasons of scope, but in these cases it must also appear in its ordinary location. In (4.113), the subject *un* 'you' is the focus of negation, but negation also appears immediately before the verb.

- (4.111a) un ma č'e- ke
you.ABSL NEG out-GO.IMPV
'Don't go out.' (52)

- (4.111b) un t'ya te- **n** bake
you.SG there NEG-2SG be
'You have not been there.' (51)

- (4.111c) šet'a c'i- ax te- **q'o** aba
3SG.POSS name-DAT NEG-INV3PL know
'They don't know her name.' (51)

- (4.112) bez k'wa taš- al te- **zu** vax
my house.DAT take-FUTII NEG-1SG you.DAT
'I will not take you to my house.' (52)

- (4.113) te un te- **n** aš-b-e, ama zu- z va q'ulluy-b- e.¹²⁹
NEG 2SG NEG-2SG work but 1SG-1SG 2SG.DAT service-DO-AORII
'It is not you who have worked, but I who have served you.' (53)

Wh-words are immediately pre-verbal, except (optionally) 'why', which can be separated from the verb but cannot follow it.

- (4.114a) xinär-mux ma- **q'un** taysa?
girl- PL.ABSL where-3PL go
'Where are these girls going?' (48)

- (4.114b) * ma xinär-mux-q'un taysa?

- (4.114c) * ma-q'un xinär-mux taysa?

- (4.114d) * xinär-mux taysa ma-q'un?

- (4.114e) * xinär-mux ma taysa-q'un?

The focus position in Udi is also pre-verbal.

- (4.115a) äyel-en p'a eš- **ne** aq'- e
child-ERG two apple-3SG take-AORII
'The child took *two apples*.' (55)

- (4.115b) äyel-en- **ne** aq'- e p'a eš- n- ux
child-ERG-3SG take-AORII two apple-OBL-DAT
'The child took the two apples.' (56)

¹²⁹ For the unusual syntax of the second clause, cf. (4.146c) below.

- (4.115c) *naŋne bez-laxo te k'aba-ne bake, zu-_____z e-be*
 yesterday my- on that dress- 3SG be 1ST.SG.ERG-1SG sew
 'The dress that I had on yesterday, I sewed.' (56)
- (4.115d) *sa- al t'ya- ne beysa je adamar-ux te- ne bu, je cac.*
 once-CONJ there-3SG see or person-PL.ABSL. NEG-3SG. be or thorn.ABSL
 'And at once *there* he sees that there is neither a person nor thorns.' (57)

The Udi verbal complex consists minimally of a root and TAM marker (the latter sometimes complex), but most verbs are complex. Complex verbs consist of some root (or infinitive), a light-verb, and the TAM marker.¹³⁰

- (4.116a) *lašk'o- b- esa*
 wedding-DO-PRES 'marry'; *lašk'o* = noun
- (4.116b) *kala-bak- e*
 big- BECOME-AORII 'grew up'; *kala* = adjective
- (4.116c) *boš-t' - al*
 in- LV-FUT 'will put in'; *boš* = adverb
- (4.116d) *oc' - k' - e*
 wash-LV-AORII 'washed'; *oc'* = verbstem
- (4.116e) *fikir- b- sa*
 think-DO-PRES 'think'; *fikir* = borrowed verb
- (4.116f) *k'al-p- e*
 call-SAY-AORII 'called'; *k'al* = bound root(?) (65)

The AgrS clitics in Udi are variable in both form and position. Table 4.3, taken from Harris (2002:28), shows the most common allomorphs of these clitics. All sentences, except (singular) imperatives, must have an AgrS clitic.

	General	Inversion	Possession	Question
<i>1st.sg.</i>	-zu, -z	-za	-bez, -bes	
<i>2nd.sg.</i>	-nu, -n, -ru, -lu	-va	-vi	
<i>3rd.sg.</i>	-ne, -le, -re	-t'u	-t'a	-a
<i>1st.pl.</i>	-yan	-ya	-beš	
<i>2nd.pl.</i>	-nan, -ran, -lan	-va, -vaŋ	-eř	
<i>3rd.pl.</i>	-q'un	-q'o	-q'o	

Table 4.3: *Subject Clitics in Udi*

The "Inversion" series of AgrS clitics are required by a small set of verbs which take experiencer subjects, including *buq'* 'love, want', *ak'* 'see', and *ababak* 'know'. This is illustrated in (4.117); (4.117c) shows that the clitic cannot be omitted. In modern Udi, the experiencer is in the ergative case and the stimulus in the dative, but examples from nineteenth century texts have experiencers in the dative and stimuli in the absolutive, and the form of the clitics descends etymologically from the dative (Harris 2002:28).

- (4.117a) *q'i- za- b- sa*
 fear-INV.3rd.sg.-DO-PRES
 'I am afraid.' (28)
- (4.117b) *äyl- en a- t'u- k' - e k'učan-ax*
 child-ERG see₁-INV.3rd.sg.-see₂-AORII puppy- DAT
 'The child saw the puppy.' (28)

¹³⁰ From page 65, with AgrS clitics removed for simplicity.

- (4.117c) * äyl- en ak'-e k'učan-ax
 child-ERG see-AORII puppy- DAT

The “Possession” series of clitics is used in possessive constructions (which may or may not contain an overt verb) and with a handful of other verbs, including *köfil-* ‘like’. Possessors are expressed in the genitive, and they show a clear formal relationship to the Agr clitics, as seen in (4.118b). Harris notes that sometimes the General clitics are substituted for this series.

- (4.118a) p'a xunči bez- bu
 two sister.ABSL POSS.1st.sg.-BE
 ‘I have two sisters.’ (29)
- (4.118b) vi günäh gölö- vi
 2nd.sg.GEN. sin.ABSL. many-POSS.2nd.sg.
 ‘You have many sins.’ (29)
- (4.118c) udin maḡ- bes köfil-esa
 Udi song-POSS.1st.sg. like- PRES
 ‘I like Udi songs.’ (29)
- (4.118d) ši ba- t'a- k- e šum...
 who.GEN. BE₁-POSS.3rd.sg.-BE₂-AORII bread.ABSL
 ‘Whoever had bread...’ (29)
- (4.118e) * ši bak-e šum...
 who.GEN. BE- AORII bread.ABSL

The “Question” clitic is used only in content questions and ‘either/or’ questions, and only in the third singular. It only substitutes for the General clitic, not the Inversion type.

- (4.119a) mähl-in- a xod nut' boš- t'- al- le
 yard- OBL-DAT tree.ABSL NEG bury-LV-FUT-3rd.sg.
 ‘She will not plant a tree in the yard.’ (30)
- (4.119b) ma boš- t'- al- a?
 where bury-LV-FUT-Q
 ‘Where will she plant it?’ (30)
- (4.119c) * ma boš- t'- al- le?
 where bury-LV-FUT-3rd.sg.

Most sentences contain one of the “General” clitics. These have a pronounced formal relationship with independent ergative pronouns.

- (4.120a) eḡel nut' šam- k'- al- zu
 sheep.ABSL NEG slaughter-LV-FUT-1st.sg
 ‘I will not slaughter a sheep.’ (29)
- (4.120b) * zu eḡel nut' šam- k'- al
 1st.sg.ERG sheep.ABSL. NEG slaughter-LV-FUT
- (4.120c) un vi- laxo abuz günäh-nu q'azamiš-b- esa
 2nd.sg. 2nd.sg.GEN-on more sin- 2nd.sg. commit- LV-PRES
 ‘You are committing [one] more sin.’ (29)

The astute reader will have already observed that if Udi subject clitics have a fixed position in the clause, the location of this position is not straightforward. Even just comparing (4.120a) to (4.120c), we see that in (4.120a) the subject clitic is clause-final and attached to the verb, while in (4.120c) it comes immediately before the verb and is

phonologically attached to the previous word. Meanwhile, in (4.117b) and (4.118d), the clitic occurs in the middle of the verbal root. The next section explores the rules for clitic placement in more depth.

4.2.3.2. Clitic Placement

Prior to Harris (2002), no comprehensive attempt at describing the rules for clitic placement in Udi had ever been made, and in fact Schulze-Fürhoff (1994) claimed that the position simply could not be specified. Undaunted, Harris successfully formulated a series of seven descriptive rules (some with codicils) that do capture the data. In this section, I first present Harris's rules and then turn to the question of how to reformulate her rules to reflect a deeper level of structure.¹³¹

Rule I. If the form of the verb is future II, subjunctive I/II, or imperative, the clitic is final in the verbal complex.

(4.121a) q'ačay-γ- on bez tänginax bašq'-al- **q'un**
 thief- PL-ERG my money.DAT steal- FUTII-3rd.pl
 'Thieves will steal my money.'

(117)

(4.121b) eγ- a- **q'un?**
 come-SUBJI-3rd.pl
 'Will they come?'

(117)

(4.121c) besp'-a- **nan**
 kill- IMPV-2nd.pl
 'You (pl.) kill [her].'

(117)

(4.121d) tak-e- **nan**
 go- IMPV-2nd.pl
 'You go.'

(117)

Rule II. If there is a focused constituent, the clitic is enclitic to it. If, however, the sentence contains a verb that fits the requirements of Rule I, Rule I takes precedence: this is seen in (4.122b–c), (4.123c), and (4.124d). Of the various entities which can be in focus, there is a hierarchy: the clitic attaches first to negation (4.122), then to *wh*-elements (4.123), and then to other items in focus (4.124). Also note the irregularities with *why*, which doesn't have to immediately precede the verb when it has a subject clitic (4.123d).

(4.122a) nana- n te- **ne** bɣa-b- e p'a ačik'alšey
 mother-ERG NEG-3rd.sg find- DO-AORII two toy.ABSL
 'Mother did not find two toys.' (117)

(4.122b) nana- n bɣa-b- e te- **ne** p'a ačik'alšey
 mother-ERG find- DO-AORII NEG-3rd.sg two toy.ABSL (117)

(4.122c) *p'ap'a-n eš- ax te- **ne** eč- al k'oya¹³²
 father- ERG apple-DAT NEG-3rd.sg bring-FUTII house.DAT
 'Father will not bring apples to the house.' (117fn.)

¹³¹ All examples are taken from Harris; the parenthetical numbers refer to her page numbers.

¹³² This sentence is grammatical in the Niĵ dialect, but was rejected by speakers of the Okt'omber dialect, where the majority of Harris's research was done.

- (4.123a) šu- a a- r- e mya?
 who-Q hither-R-AORII here
 ‘Who came here?’ (118)
- (4.123b) okt’omber- a evaxt’-t’u täy- sa?
 Okt’omber-DAT when- 2nd.sg thither-PRES
 ‘When are you going to Okt’omber?’ (118)
- (4.123c) šu- x k’al- k’- al- a
 who-DAT call- SAY-FUTII-Q
 ‘Whom will she invite?’ (119)
- (4.123d) ek’aluy-nu mya are?
 why- 2nd.sg here come
 ‘Why have you come here?’ (49)
- (4.124a) äyel- en p’ä eš- ne aq’- e
 child-ERG two apple-3rd.sg take-AORII
 ‘The child took two apples.’ (119)
- (4.124b) täzä k’oĵ- q’un biq’- e išq’ar-muy-on
 new house-3rd.pl build-AORII man- PL- ERG
 ‘The men build a new house.’ (119)
- (4.124c) äyel- en k’uč’an beγ- al- le
 child-ERG puppy.ABSL watch-FUTII-3rd.sg
 ‘The child will watch a puppy.’ (120)
- (4.124d) * äyel- en k’uč’an- ne beγ- al
 child-ERG puppy.ABSL-3rd.sg watch-FUTII (120)

The interactions of the three types of focus are shown in (4.125). Negation has first dibs on the clitic, both when it co-occurs with a *wh*-word (4.125a) and when another item is in focus in the same sentence (4.125b). (4.125c) shows that when a sentence has both a *wh*-word and another kind of focus, it is the *wh*-word that attracts the clitic.

- (4.125a) ek’alu te- n maγ- exa?
 why NEG-2nd.sg song-SAY.PRES
 ‘Why aren’t you singing?’ (119)
- (4.125b) * äyel-en k’uč’an- ne te beγ- sa
 child- ERG puppy.ABSL-3rd.sg NEG watch-PRES
 ‘The child is not watching a puppy.’ (120)
- (4.125c) * evaxt’ okt’omber- a- nu täy- sa
 when Okt’omber-DAT-2nd.sg thither-PRES
 ‘When are you going to Oktomber?’ (121)

Rule III. If there is a null copula, the clitic is enclitic to the predicate nominal (unless the subject is in focus, in which case Rule II will have already applied). This seems to be true when the copula is *not* null, as in (4.126b), although Harris does not discuss the relevant examples.¹³³ The interaction of this rule with earlier rules is demonstrated in (4.127): if a *wh*-word or negation is present, it takes the clitic.

- (4.126a) nana k’wa- ne
 mother house.DAT-3rd.sg
 ‘Mother is at the house.’ (121)

¹³³ Arguably, this is a sub-case of Rule II and the predicates here are in focus; cf. below.

- (4.126b) pə- q'at- t'e bak-i
two-piece-3rd.sg BE- AORI
'It was in two pieces.' (121)
- (4.126c) me gamxo k'onjux zu- zu
this place.PL.GEN proprietor.ABSL 1st.sg.-1st.sg.
'The proprietor of this estate is me' = 'I am the proprietor of this estate' = 'It is I who am the proprietor of this estate.' (122)
- (4.127a) nana ma- a?
mother.ABSL where-Q
'Where is mother?' (121)
- (4.127b) kano vi borj te- ne
that.ABSL 2nd.sg.POSS. matter NEG-3rd.sg
'That is not your concern.' (121)

Rule IV. If the verb is composed of a Root and light verb, the clitic is placed between them. There are two subtypes to this rule, depending on whether the verb is a causative or not.

Rule IVa. Udi forms productive causatives by adding a causative morpheme to an infinitive; subject clitics are placed between the infinitive affix and the causative morpheme, as shown in (4.128a). There is also an older type of causative which uses the suffix *-ev* rather than an infinitive, and in this case the clitic goes between *-ev* and another light verb, as shown in (4.128b).

- (4.128a) me pasčay-en eč- es- ne- st'a... kul
this king- ERG bring-INF-3rd.sg-CAUS.PRES earth.ABSL
cip- es- ne- st'a pak- i
spread-INF-3rd.sg-CAUS.PRES garden-DAT
'This king has brought earth....; he has it spread in the garden.' (122)
- (4.128b) zer- ev- ne- k'- sa
decorate-CAUS-3rd.sg-LV-PRES
'She arranges [the house].' (122)

Rule IVb. When the verb is not a causative, but has incorporated a noun, adjective, adverb, or whatever else, the clitic goes between that element and *v*.

- (4.129a) zavod- a aš- ne- b- sa
factory-DAT work-3rd.sg-DO-PRES
'She works in a factory.' (122)
- (4.129b) bez vič- en aš- b- al- le zavod- a
1st.sg.POSS brother-ERG work-DO-FUTII-3rd.sg factory-DAT
'My brother will work in a factory.' (123)
- (4.129c) ma- n aš- b- esa
where-2nd.sg work-DO-PRES
'Where do you work?' (123)
- (4.129d) zavod- a- z aš- b- esa
factory-DAT-1st.sg work-DO-PRES
'I work *in a factory*.' (123)
- (4.129e) nana- n buya-ne- b- e p'a ačik'alšey
mother-ERG find- 3rd.sg-DO-AORII two toy
'Mother found two toys.' (122)

(4.129f) nana- n te- **ne** b̥ɣa-b- e p'ə ačk'alšey
 mother-ERG NEG-3rd.sg find- DO-AORII two toy
 'Mother did not find two toys.' (123)

Causatives can, of course, be formed to complex verbs of this type. (4.130a) shows the non-causative form, with the clitic placed according to Rule IVb. In the causative variant (4.130b), two other affixes have been added between the light verb and tense: the infinitive suffix and the causative suffix, with the subject clitic placed between the two. That is, Rule IVa applies before IVb. However, Harris reports that sentences like (4.130c), where the clitic is placed between the Root and the first light verb, are also marginally acceptable.

(4.130a) mzia-n arux-**ne**- b- e
 Mzia-ERG fire- 3rd.sg-DO-AORII
 'Mzia built a fire.' (123)

(4.130b) baba- n mzia-x arux-b- es- **ne**- d- e
 father-ERG Mzia-DAT fire- DO-INF-3rd.sg-CAUS-AOR
 'Father had Mzia build a fire.' (124)

(4.130c) baba- n mzia-x arux-**ne**- b- es- t'- e
 father-ERG mzia-DAT fire- 3rd.sg-DO-INF-CAUS-AOR
 'Father had Mzia build a fire.' (124)¹³⁴

Rule V. Harris identifies a category of verbs "M", which form intransitives, and which require the clitic to be placed between Root and the present tense marker, as shown in (4.131). For comparison with the corresponding transitives, cf. (4.138).

(4.131) bix-**ne**-sa 'is born'
 box-**ne**-sa 'boils (intr.)'
 lax-**ne** 'lies, is'
 uk-**ne**-sa 'is edible' (127)

Harris (2002:130) argues convincingly that these verbs are actually only a sub-type of the previous rule, in that they have a null light verb specifying their intransitivity. This morpheme has overt allomorphs in other tenses; in the future I it appears as *-eɣ*, and in the aorist II as *-c*:

(4.132a) box-**ne**- (Ø)- sa
 boil-3rd.sg.-INTR.-PRES

(4.132b) box-eɣ- al- **le**
 boil-INTR.-FUTI-3rd.sg.

(4.132c) box-**ne**- c- e
 boil-3rd.sg.-INTR.-AORII

Rule VI. There are two situations which require the clitic to be enclitic to the entire verb complex.

Rule VIa. If the verb does not contain a light verb, and the shape of the Root (or allomorph of the root) is of the shape C or CV, the clitic is enclitic to the entire verb complex.

(4.133) b-esa-**ne** 'she makes'
 bu-**ne** 'she is'
 bi-esa-**zu** 'I am dying'
 p-e-**ne** 'she said' (128)

¹³⁴ Harris reports that this example is only barely acceptable.

Rule VIb. There is also a set of irregular verbs (Harris dubs them “category B”) which require that the clitic be enclitic to the entire verb complex.

(4.134) aba- za	‘I went out’	
ex- ne	‘she says’	
p’ur-e- ne	‘he died’	
č’e-re- ne	‘she went out’	
a-re- ne	‘she came’	
ci-re- ne	‘she went down’	(128)

These verbs are an eclectic bunch. The first was originally simplex, but now typically requires a light verb; however, in the present tense it often lacks both the light verb and a tense marker. The second, *ex-ne*, is defective, appearing only in the present and imperfect and lacking a present tense affix. The last three verbs consist of a locative/directional preverb, a light verb, and no overt Root. All of these verbs place their clitic according to regular rules when in other TAM categories.

Rule VII. This is the class of verbs that allow endoclitics. If the verb root is monomorphemic, and its phonological form is not simply an open syllable, the clitic appears immediately before the final consonant.

(4.135a) pasčay-un γar- en gölö bę- ne- γ- sa met’a- laxo king- GEN boy-ERG much look ₁ - 3rd.sg -look ₂ -PRES this.GEN-on ‘The prince looks at this for a long time.’	(125)
(4.135b) kayuz-ax a- z- q’- e letter- DAT receive ₁ - 1st.sg -receive ₂ -AORII ‘I received the letter.’	(125)
(4.135c) q’ačay-γ- on bez tänginax baš- q’un- q’- e thief- PL-ERG 1 st .sg.POSS money.DAT steal ₁ - 3rd.pl - steal ₂ -AORII ‘Thieves stole my money.’	(125)
(4.135d) ba- ne- k- sa sa pašč’ay-k’ena adamar BE ₁ - 3rd.sg -BE ₂ -PRES one king- like person.ABSL ‘[Once upon a time, there] is a person like a king.’	(125)

This rule is only operative if previous rules have not applied. In (4.136a), the verb is in the futureII, so the clitic is on the right edge of the verb and the root is not broken up.¹³⁵ The clitic in (4.136b) has been drawn to negation, and in (4.136c) to a *wh*-word.

(4.136a) sa xinär-en... a=q’- al- le k’alpesun-un p’iz- ax one girl- ERG receive-FUTII- 3rd.sg read.MAS- GEN prize-DAT ‘A girl... will receive the prize for studying.’	(126)
(4.136b) q’ačay-γ- on ek’k’al te- q’un baš=q’-e thief- PL-ERG nothing NEG- 3rd.pl steal- AORII ‘Thieves stole nothing.’	(126)
(4.136c) šin- a a=q’- sa šel nišan- ux? who-Q receive-PRES good grade-DAT ‘Who gets good grades?’	(126)

When a causative is formed to one of these Roots, the clitic appears in its expected position between the infinitive and causative suffixes, as in (4.137b). (4.137a) shows the non-causative version with endoclitisis.

¹³⁵ “=” indicates the place at which a potentially divided root would divide, when it is not divided.

- (4.137a) ek'at'e man- **ne-** d- o oš:a, zu u- **z-** k- o
 whatever remain-**3rd.sg-LV-FUT**I after 1st.sg eat₁-**1st.sg-eat₂-FUT**I
 'I will eat whatever is left over.' (126)
- (4.137b) sa šel u=k-es- **ne-** st'a
 one good eat- **INF-3rd.sg-CAUS.PRES**
 'She feeds [him] one good [one].' (126)

All of the verbs that obey Rule VII are transitive; their intransitive counterparts obey Rule V instead. The contrast between some transitive/intransitive pairs is illustrated here:

(4.138) <i>Transitive</i> (Rule 7)		<i>Intransitive</i> (Rule 5)	(127)
a- t'u -k'-sa	'sees'	ak'- ne -sa	'shows, is visible'
a- ne -q'-sa	'receives'	aq'- ne -sa	'is surprised'
bi- ne -t'-sa	'sows'	bit'- t'e -sa	'is sown'
bi- ne -x-sa	'gives birth'	bix- ne -sa	'is born'
bo- ne -t'-esa	'cuts'	bot'- t'e -sa	'is cut'
bo- ne -x-sa	'boils, cooks'	box- ne -sa	'boils'
bq- ne -q'-sa	'gathers'	bq'- ne -sa	'gathers, is gathered'
ču- ne -k-sa	'breaks'	čuk- ne -sa	'breaks off, is ripped out'
la- ne -x-sa	'lays, puts'	{lax- ne	'lies, is'}
u- ne -k-sa	'eats'	uk- ne -sa	'is edible'
u- ne -γ-sa	'drinks'	uγ- ne -sa	'is drinkable'

Optional “Permissive Placement” Rule. Instead of using Rules IV or VII, the clitic can be placed enclitic to the verbal complex as though the verb were one of the irregular verbs that follow Rule VIb. Speakers who do not produce these forms do accept them; the difference appears to be one of register (and possibly dialect?), and “permissive” forms are more frequent in fast speech. (4.139) contrasts verbs obeying Rule VII with their “permissive” counterparts; (4.140) shows exceptions to Rule IV.

- (4.139a) bę- **ne-** γ- sa ~ bęγ- sa- **ne** (137)
 watch₁-**3rd.sg**-watch₂-PRES
 'he watches' watch-PRES-**3rd.sg**
- (4.139b) ta- **ne-** d- i ~ ta- d- i- **z**
 give-**3rd.sg-LV-AORI**
 'he gave' give-LV-AORI-**1st.sg**
 'I gave'
- (4.140a) k'oc'-bak-e- **ne**
 bent- BE- AORII-**3rd.sg**
 'he stands bent' (137)
- (4.140b) tay- sa- **ne**
 thither-PRES-**3rd.sg**
 'he goes'
- (4.140c) γač'- p- e- **nan**
 yoke-SAY-AORII-**2nd.pl**
 'you have yoked'

The past-tense clitic can optionally go after the AgrS clitic.

- (4.141a) bex- ec- e- **ne-** y
 swell-LV-AORII-**3rd.sg-PAST**
 'It had swollen.' (137)

(4.141b) bit- i- ne- y
 fall-AORI-3rd.sg-PAST
 ‘It had fallen.’

Compound Verbs. Udi allows a type of verbal compound in which the TAM affixes of each verb are preserved. As shown in (4.142), when the verb is compound, the clitic is enclitic to the entire compound.

(4.142a) tay- sa- bɛɣ- sa- ne
 thither-PRES-watch-PRES-3rd.sg
 ‘he goes and watches’ (138)

(4.142b) čap- a- e- č- a- nan
 drag-IMPV-hither-carry-IMPV-2nd.pl
 ‘drag and bring [it]’

(4.142c) bay-ɣ- a- bɛɣ- a- z
 in- go-SUBJ-watch-SUBJ-1st.sg
 ‘I will go in and watch’

Harris’s rules are summarised in (4.143). The rules are to be read the same way that rules for VI should be read, with each rule taking precedence over any rules that follow it.

(4.143) *Harris’s Descriptive Rules for Clitic Placement in Udi*

1. When the verb is future II, subjunctive I/II, or imperative, the clitic is verb-final.
2. Clitics are enclitic to focused elements, with priority to negation, then *wh*-items, then other constituents in focus.
3. Clitics are enclitic to predicate nominals (with or without an overt copula?).
4. Clitics are placed between a Root and light verb.
 - a. In productive causative verbs, the clitic is placed between the infinitive and causative suffixes; in older causatives, between the causative suffix *-ev* and another light verb.
 - b. Elsewhere, the clitic goes directly between the Root and light verb.
5. Verbs which form intransitives via a null suffix in the present tense place the clitic between this null suffix and present tense (which looks superficially as though the clitic were placed between the Root and tense).
6. Some exceptional cases require the verb to be enclitic:
 - a. Verbs with no light verb and Roots of the shape C or CV; and
 - b. A set of seven irregular verbs.
7. The clitic appears before the Root-final consonant in monomorphemic verbs ending in closed syllables.

(4.144) *Optional Rule of Permissive Placement:* the clitic can be enclitic to the verbal complex in lieu of following rules IV or VII, and in the case of compound verbs.

(4.145) *The -y Exception Rule:* the past tense clitic can optionally follow the otherwise-verb-final subject clitic.

These rules can be amended or condensed in several places. Some of the likely alterations are mentioned by Harris herself: Rule VII should be ordered after Rule V, making the position enclitic to the verb the default, which is already implied by Harris’s presentation

of the data (although she doesn't renumber the rules); Rule V is already a subcase of Rule IV. In addition, Rules II and III can probably be collapsed into a single rule; they appear to target the same syntactic position, and predicate nominals are likely to be in focus. The "Permissive Placement" rule is a good candidate for a novel grammar competing with the earlier grammars to establish a default position for clitics.

Rule VII is the primary source of interest here. There is an intuitive connection between Rule VII and Rule IV; in both the clitic is associated with the Root, with the primary difference that the element to the right of the clitic is *v* in Rule IV and a Root-final consonant in Rule VII. It is already clear that phonology is involved in the surface output, since whether Rule VII or Rule VIa applies depends on the phonological shape of the Root; given that infixes are the output of phonological rules applying to prefixes or suffixes, it requires no particular stretch to imagine that phonological rules might also be responsible for producing Udi endoclitics.

I said at the outset that my aim was not to provide an account of the Udi data, but rather to determine how much of a problem it poses for the theory. The affix/clitic distinction prominent in other theories of morphosyntax is not important in DM, so the fact that the intrusive elements here are classified as clitics does not make them any more difficult than the more customary infixes. M-words are not islands or atoms; they can be invaded. If it were the case that a phonological explanation for the positioning of endoclitics seemed to be precluded by the data, then we would have a serious problem; but it would be a problem no more serious than if the matter at stake involved infixes rather than endoclitics. As the matter stands, even this potential problem is not at issue, because it *does* seem more than likely that phonology is involved in the relevant rules of clitic placement in this language. More work will be required in order to arrive at a proper account of the data, but a solution ought to be possible.

4.2.3.3 Attested Earlier Grammars

As mentioned at the outset, Udi has been attested since the mid-nineteenth century. The earlier texts reveal a different syntax for focus constituents and a wider variety of mobile clitics which figures prominently in Harris's account of the historical developments that produced endoclitics in modern Udi. Nineteenth-century Udi, recorded in Schiefner (1863), allowed negation, *wh*-words, and other focused constituents in sentence-initial position, as well as in the modern pre-verbal position. The clitics were attracted to focus just as in modern Udi.

This is illustrated in (4.146). (4.146a–b) show clitics attached to *wh*-words which are not immediately pre-verbal, though note that in (4.146a) *or-q'un* 'how' is preceded by the subject and therefore not truly clause-initial; Harris (2002:235) does not discuss this case. (4.146c–f) show the clitics attaching to other items in focus, usually clause-initial, though in (4.146f) the subject is yet higher. (4.146c) shows both the modern pattern and the archaic pattern, in that in the first clause, the clitic attaches to negation in the immediate pre-verbal position, while in the second clause, the clitic is attached to a clause-initial focused constituent.

- (4.146a) a- **t'u-** k- i Ili- in **ai-** ux or- **q'un** čal- la
 see₁-INV3rd.sg-see₂-AORI Ili-a-GEN child-PL.ABSL how-3rd.pl fence-on
 laxo lay-c- i
 on up-LV-AORI
 'He saw how Ili's children climbed up on the fence.' (235)
- (4.146b) ek'a- **n** gena b- o?
 what-2nd.sg contrast DO-FUT
 'What will you do?' (235)

- (4.146c) te un te- **n** aš- b- e, ama zu- **z** va
 NEG 2nd.sg NEG-2nd.sg work-DO-AORII but 1st.sg-1st.sg 2nd.sg.DAT
 q'ulluy-b- e.
 service-DO-AORII
 'It is not you who have worked, but I who have served you.' (53)
- (4.146d) šel- **lu** un ĥazir- b- esa
 well-2nd.sg 2nd.sg prepare-DO-PRES
 'You prepare food well.' (235)
- (4.146e) un- **nu** lek'er- ax ba-sak- e
 2nd.sg-2nd.sg container-DAT in- push-AORII
 'You pushed the container [a bucket] in...' (235)
- (4.146f) γε baba či- č- čun- **t'u** čeč buq'-sa
 today father.DAT out-carry-MAS-INV3rd.sg honeycomb.ABSL want-PRES
 'Today father wants to take out the honeycomb.' (236)

Early twentieth-century Udi, recorded in Dirr (1904), also has a few examples of initial focus, although these are rarer in Dirr's texts. Again, the clitic continues to obey the modern rules of placement even though the focus position is different; it attaches to negation rather than *et'e* 'why'.

- (4.147) et'e te- **n** vi vič- e baxt'in čubux e- č- sa?
 why NEG-2SG 2SG.POSS brother-GEN for woman.ABSL hither-LV-PRES
 'Why don't you bring a wife for your brother?' (236)

This is important because it shows that Udi subject clitics are attracted to focused constituents regardless of the syntax of focus; when the syntax changed, so did the position of the clitics.

Earlier texts also reveal additional clitics which interact with the subject clitics. In the following examples, these clitics have been italicised. The modal clitics *gi-* (forming the now-moribund "particle conditional" in conjunction with aorist I or II) and *-q'a-* (forming the so-called "particle subjunctive" in conjunction with the aorist I) are shown in (4.148) and (4.149) respectively. Both clitics always appear immediately before subject clitics, regardless of where the latter appear.

- (4.148) jahil- *gi-z* bak-e-y
 young-COND-1SG be-AORII-PAST
 'If I were young.' (132)
- (4.149a) gölö ma- *q'a-* **n** box-ec- i
 much NEG-SUBJ-3rd.sg boil- LV-AORI
 'It should not boil much.' (132)
- (4.149b) te- **t'u** aba ek'a-*q'a-* **n** b- i
 NEG-INV3rd.sg know what-SUBJ-3rd.sg do-AORI
 'She doesn't know what she should do.' (132)
- (4.149c) gam-*q'a-* n- ec-i
 hot- SUBJ-3rd.sg-LV-AORI
 'it should become hot' (133)
- (4.149d) e- *q'a-* **n-** č- er-i
 hither-SUBJ-3rd.sg-carry-R- AORI
 'may he bring' (133)
- (4.149e) ba- *q'a-* **n-** k- i
 be₁-SUBJ-3rd.sg-be₂-AORI
 'may it be' (133)

The “past” clitic *-y/i* combines with other TAM affixes to form secondary tenses (e.g. imperfect past, past perfect). It is the only TAM marker which can occur with null copulas. The past clitic appears final in the verbal complex (4.150), except when it appears with the subjunctive I, in which case the subject clitic can follow it (4.151).

(4.150a) t'e pa xinär-ax ač- es- b- a- **ne-** y
 these two girl- DAT lose-INF-DO-SUBJI-**3rd.sg-PAST**
 ‘...he would lose these two girls.’ (133)

(4.150b) pačšay-en yar- i- baxt'in... kur- **re**
 king- ERG boy-GEN-for hole.ABSL-**3rd.sg**
 kač-p- es- t'- e- y
 dig- LV-INF-CAUS-AORII-PAST
 ‘The king had a hole dug... for the boy.’ (133)

(4.150c) narzux xa- in bap- **ne-** xa- y
 yesterday.evening dog-ERG bark-**3rd.sg-SAY.PRES-PAST**
 ‘Yesterday evening a dog was barking.’ (133)

(4.151a) šin šel nišan aq'- a- y- **n**
 who.ERG good mark.ABSL receive-SUBJI-PAST-**3rd.sg**
 ‘Whoever gets good grades...’ (134)

(4.151b) ek'a-te yač'- k'- a- y- z, zap'-**nu-** k'- o
 what.ABSL-REL bundle-LV-SUBJI-PAST-**1st.sg** pull- **2nd.sg-LV-FUTI**
 ‘Whatever I tie on, you will pull up.’

The conjunctive clitic *-al* is enclitic to the first constituent of the last conjunct in modern Udi, much like the Latin enclitic conjunction *-que*.

(4.152a) ...paščay-un yar- en- *al* zap-i lay-**ne-** šča
 king- GEN boy-ERG-*CONJ* pull-PTCP up- **3rd.sg-CARRY.PRES**
 ‘...and the king’s son, pulling, brings them up.’ (102)

(4.152b) k'ic'i-*al* brilliant
 small-*CONJ* diamond.ABSL
 ‘and a small diamond’ (103)

(4.152c) pak- n- a boš- *al*
 garden-OBL-DAT inside-*CONJ*
 ‘and inside the garden’ (103)

In the earlier texts published by Schiefner (1863), *-al* sometimes behaves as it does in the modern language, as shown in (4.153). However, it can also precede the subject clitic (4.154), even if this places it much lower down in the clause.

(4.153) vaxt'-*al* te- **ne** bu-i
 time- *CONJ* NEG-**3SG** be-PAST
 ‘And she did not have time.’ (135)

(4.154) ägär aš seri-n- *al-* **le** ta- c- i
 if work.ABSL fair-OBL-*CONJ*-**3SG** thither-LV-AORI
 ‘And if the matter had turned out *fairly*...’ (134)

Some of Schiefner’s examples are ambiguous between “*-al* is placed after the first constituent” and “*-al* precedes the subject clitic.”

(4.155a) ta- al- le- c- i kalabal-t'- uč'
 thither-CONJ-3rd.sg-LV-AORI servant-OBL-ALL
 'And she went to the servant.' (135)

(4.155b) evaxt' kinbalo k'ua- ne a- r- i,
 when industrious.ABSL home.DAT-3rd.sg hither-R-AORI
 p- i- al- le
 say-AORI-CONJ-3rd.sg
 'When the industrious one came home and said...?' (135)

Modern Udi also has examples that resemble (4.155), but synchronically these may not be ambiguous. Harris does not commit herself either way.

(4.156a) pirog-ax či- ne- č- er-i la- al- le- x- i
 pie- DAT out-3rd.sg-CARRY-R- AORI put₁-CONJ-3rd.sg-put₂-AORI
 'She took the pie out and put [it] under her arm.' (102)

(4.156b) abaz- in- al- le aš- b- esa
 [coin]-INST-CONJ-3rd.sg business-DO-PRES
 'And he works for an abaza [twenty kopeks].?' (103)

Finally, some of Schiefner's other examples place *-al* somewhere else entirely. In (4.157), *-al* appears inside the verbal complex after the verb root, while the subject clitic appears at the end of the verbal complex, since the verb is subjunctive I. Harris (2002:135) implies (though she does not state directly) that in modern Udi, *-al* must be cliticised to an entire verbal complex rather than to its first morpheme. This suggests that there may have been changes within the verbal M-word that made continued interpolation of *-al* impossible.

(4.157) kinbal- t'- in a- ne- q'- o... gurat bot-a- ne,
 industrious-one-ERG take₁-3SG-take₂-FUTI shirt.ABSL cut-SUBJ-3SG
 e- al- b- a- ne
 sew-CONJ-DO-SUBJ-3SG
 'The industrious one would begin... that she might cut out a shirt, and that she might sew it.' (135)

Interestingly, when all three clitics – subject marker, *-q'a-*, and *-al* – occur in the same sentence in Schiefner's text, the order is *-q'a-AgrS-al*, even though under other circumstances, *-al* precedes AgrS.

(4.158) te- t'u aba ek'a- q'a- n aq'- i ek'a- q'a- n- al- b- i
 NEG-INV3SG know.PRES what-SUBJ-3SG take-AORI what-SUBJ-3SG-CONJ-DO-AORI
 'She did not know what she should take on and what she should do.' (136)

The key points from this section can be summarised as follows:

- (4.159a) The syntax of focus has become more rigid in the last century or so;
- (4.159b) The position of clitics has changed in tandem with the syntax of focus;
- (4.159c) Older Udi had a clitic cluster;
- (4.159d) The position of clitic conjunctions has become more restricted (and Latin-like);
- (4.159e) Some clitics were previously permitted to interpolate into the verbal complex, but not any longer.

We now turn to Harris's conclusions about the origins of Udi endoclitics, which rely on the data from the earlier texts.

4.2.3.4. Harris's Diachronic Scenario

Harris (2002) sketches out a complex derivation of Udi endoclitics and clitic placement. She acknowledges that her analysis is necessarily speculative, since there is no direct evidence for the previous stages; in particular, she sets out a sequence of developments but says that the order may well be wrong. However, she is certain of each of the individual steps. In this section, I will review Harris's sequence and examine each of its stages for inherent plausibility. Not all of her stages are, in my view, well-motivated, but most of them are not inconsistent with the framework developed in this dissertation, and ultimately allow for the conclusion that endoclitics in Udi do not require additional theoretical apparatus. As I argued at the end of 4.2.3.2, they are a sub-case of the infixing problem.

The focus construction discussed in the previous section is the starting point for Harris's scenario. She argues that an earlier stage of Udi (hence "Proto-Udi") possessed a focus cleft construction in which a subject pronoun introduced a dependent clause with which it shared features. Many North Caucasian languages, including relatives of Udi, have constructions of this type, schematised in (4.160), her (5) (Harris 2002:229). The focused element is in the absolutive case.

$$(4.160) \left[\begin{array}{c} \text{s FocC}_i \\ \text{SUBJECT} \end{array} \right] \text{ Copula-Agmt}_i \left[\begin{array}{c} \text{s.... Verb } \\ \text{PARTICIPLE} \end{array} \right]$$

Examples of this construction in a modern North-East Caucasian language are shown in (4.161), from the language Dargi (Kazenin 1994, 1995). The cleft in (4.161b) contrasts with the unclefted (4.161a). A second focus construction, shown in (4.161c–d), is also available in Dargi. Here, the focused element *x'oni* is ergative rather than absolutive, and the copula agrees with the absolutive argument. (4.161c) and (4.161d), despite differences in word order, both have *x'oni* in focus.

- (4.161a) *x'o- ni uzbi arkul- ri.*
 2nd.sg-ERG brothers.ABSL bring.PAST-2nd.sg.
 'You brought the brothers.'
- (4.161b) *x'o saj- ri uzbi arku- si.*
 2nd.sg.ABSL FM[COP-2nd.sg] brothers.ABSL bring-PTCP.SG.
 'You brought the brothers.'
- (4.161c) *x'o- ni sabi uzbi arku- si.*
 2nd.sg.-ERG FM[COP] brothers.ABSL bring-PTCP.SG.
- (4.161d) *uzbi x'o-ni sabi arku-si.*

Harris argues that the (4.161c–d) focus construction developed out of the type in (4.161b), following a reanalysis of the copula as a focus marker and the clefted element as sitting in a FocP, with the structure in (4.162), her (8).

$$(4.162) \left[\text{s } \left[\text{FocP FocC-FM} \right] \dots \text{ Verb } \right]_{\text{PARTICIPLE}}$$

A similar development, she continues, occurred in Udi, reflected in the sentence-initial focus construction found in older Udi texts. Subject markers appear after the focused element because they were encliticised to the copula, which could be null. This use of the pronoun, it should be noted, is unique to Udi (cf. Harris 2002:243). She suggests the following sequence of developments; *that* in (4.163a) represents a pronoun coreferential with the focused constituent; it becomes the AgrS in (4.163b), the construction in nineteenth-century Udi. (4.163c) is the modern pre-verbal construction.

(4.163a) [FocC_i Copula [_s that_i ... Verb]]
PARTICIPLE

(4.163b) [FocC-AgrS ... Verb ...]
FINITE

(4.163c) [... FocC-AgrS Verb ...]
FINITE

This sequence is problematic because it lacks some rather important details: the change of the verb from participle to finite; the change in the case of the focused constituent; the motivation for the subject marker; the loss of non-null copulas. Harris simply says that the first three changes are happened *as a result of the reanalysis* (p. 241) and gives no explanation for the fourth. Furthermore, she offers no explanation for how speakers moved from an initial focus position to a pre-verbal focus position; she simply notes that preverbal focus is common in SOV languages and that “this fact alone may be enough to explain the relocating of the focused constituent in Udi’ (p. 262). Although the overall scenario may well be plausible, too many steps remain unexplained for it to be considered explanatory or even descriptive.

It is somewhat unfortunate that Harris has no explanation for the relocation of focus to before the verb, because it is important to the rest of her argument that “Focus-AgrS-verb” be a linear string in this language.

Harris observes that preverbal focus constructions in Udi have the same surface structure as complex verb forms with incorporated elements. She argues (p. 211) that verbs with focused direct objects were reanalysed as lexicalised phrases, and thence to incorporated elements. Since AgrS was enclitic to the focus position, it could then be trapped when nouns (or other items) were incorporated into the verbs, as schematised in (4.164). (“ComE” stands for a lexicalised complement; “IncE” for an incorporated element):

(4.164a) FocC-AgrS V-TAM

(4.164b) ComE-AgrS V-TAM

(4.164c) IncE-AgrS-v-TAM

Both of the latter two patterns are currently attested in Udi: (4.165a) corresponds to (4.164b), and (4.165b) to (4.164c).

(4.165a) aš- **ne** b- esa
work-3rd.sg. DO-PRES
‘She does work.’

(4.165b) aš- **ne-** b- sa
work-3rd.sg. -LV-PRES
‘She works.’

Though its details need to be made more precise, this stage of Harris’s proposal strikes me as quite plausible, and is very much consistent with the framework presented here. As Harris points out, in constructions with light verbs, most of the semantic information of the predicate is contributed by the direct object (or a predicate adjective, &c.), which means that the light verb is likely to become destressed. In an SOV language, the semantically contentful element will usually be linearly adjacent to the light verb, so that the two end up in the same phonological word; learners are then free to conclude that the light verb is only a *v* affix and that the erstwhile object (or copular predicate) is the Root and has moved into *vP*. I will not be discussing cases of this type in depth here, but the general outline of a solution is intuitively clear, and Harris’s proposal for the birth of compound verbs in Udi falls in neatly with such a proposal. Furthermore, the trapping of a pronominal in this way is something we have seen several times already in this section. One possible analysis would be that the pronoun is actually a proclitic and that it arrives in its surface position via LD.

This leaves one outstanding issue: the endoclitics. Harris (2002:211–215) proposes three logical possibilities.

- (4.166a) *In situ hypothesis*: AgrS clitics developed in situ, by magick (as it were).
- (4.166b) *Univerbation/Simple Movement Hypothesis*: AgrS, having been already trapped as per above, moved into intramorphemic position.
- (4.166c) *Slot Hypothesis*: AgrS clitics have inherited the slot of Proto-Lezgian class marker agreement morphemes.

Harris rejects (4.166a) immediately and concentrates on evaluating (4.166b) and (4.166c) against each other, ultimately concluding that both are necessary. (4.166b) is straightforwardly consistent with the current framework; it amounts to saying that some phonological process occurred that caused the clitic to relocate. (4.166c) requires a bit more unpacking. Modern Udi does not mark class agreement on verbs, but other languages related to it do (cf. e.g. Batsbi in 4.3.2.1); those Northeast Caucasian languages more distantly related to Udi have prefixed class markers, but in the languages of the Lezgian branch, class markers appear before the Root-final consonant in at least some cases. Harris's claim is that, based on the etymologies of a few monomorphemic verbs and their cognates in other Lezgian languages, the position of these class markers was inside the verbal root in the same position as the AgrS clitics in modern Udi. She argues that if some Proto-Lezgian verbs had class markers in the same position that modern Udi uses for AgrS, this cannot be an accident.

If there is a strong indication that a modern structure is a direct reflex of an ancestral one, we must favour this hypothesis over others. If we failed to recognise the correctness of the Slot Hypothesis, we would, in effect, be claiming that it is an accident that contemporary PMs occur in the same position as CMs – in complex verbstems, in verbstems that include a fossilised CM, and in historically simplex verbstems.

Harris (2002:221)

So she concludes that the intramorphemic AgrS clitics essentially inherited a position that had earlier been used for class markers, even though there is no evidence that the two types of marking ever co-existed. She acknowledges some difficulties: AgrS clitics do not appear in *all* of the same places that class markers appear, especially with light verbs; earlier Udi permitted interpolation of an entire clitic cluster; none of the complex verbs in Udi can be traced back to Proto-Lezgian; and allowing one set of morphemes to inherit the position of a vanished set of morphemes is more than a little unorthodox. Nevertheless, in her view these problems are minor when the alternative is to resort to accidental coincidence. She argues that (4.166b) is the correct analysis for complex verbs, (4.166c) for simplex verbs.

I am unable to be quite so sanguine, because this scenario faces some serious learnability issues. As I argued in Chapter Two, language learners have no evidence for ancestral states of their language. Proto-Lezgian may well have had infixed class markers, but unless there was some reflex of them in Udi at the time when intramorphemic AgrS clitics first made an appearance, children learning Udi could not possibly have known of them, let alone considered them sufficient motivation to place AgrS clitics within the Root counter to what they observed in the speech of their elders. The possible existence of a similar phenomenon in Proto-Lezgian is entirely irrelevant: without direct evidence, (4.166c) is no different from the rightly dismissed (4.166a).

Harris does admit of the possibility that there could have been co-existence of class markers and AgrS clitics, and that the latter replaced the former. We have no evidence that this was ever the case, and are not likely to encounter any, so this is pure speculation; nevertheless, such a scenario would not be entirely impossible.

Coincidence cannot be ruled out either, unlikely though it may seem. It *is* probably entirely possible to explain the position of the endoclitics via some morphophonological rule, without reference to anything inherited; this may not be entirely satisfying, but that hardly makes it impossible. Harris (2002:221) objects to a phonological approach because the Udi morphemes are endoclitics (unlike the class markers, which were originally prefixes), but, as discussed earlier (cf. 4.5.3.2), the affix/clitic distinction is not meaningful in the framework discussed here, so this issue goes away.

In short, while I agree that it is striking, even tantalising, to find class marker infixes in cognate languages, I don't believe that this justifies requiring language learners to somehow have an awareness of an inherited "slot" for morphemes when no evidence of this could have been available to them. If there *were*, of course, evidence of some sort, that would be another matter entirely; but this information is not available to us. For this reason, this is not a problem that seems likely to be solved.

This leaves us with the issue of verb-final AgrS placement. Here, Harris's analysis is, once again, very plausible. Her analysis is simple: some of the modern verbal constructions – the subjunctives, future II, and some stative participles – required postposed clitics, some for phonological reasons and some because they had occurred before the copula. From this, language learners began to generalise a rule whereby verb-final placement was the default.

The sequence proposed by Harris can be summarised as follows:

(4.167) *The Origin of Udi Subject Clitic Placement Rules*

1. Pre-Udi has a focus cleft construction whose dependent clause is introduced by a subject pronoun co-referent with the focused element.
2. This was reanalysed (somehow?) into a monoclausal construction with a finite verb and the pronoun cliticised to the possibly-null copula.
3. The overt copulas were lost; the pronouns are now always attached to the focused element.
4. Some sequences of Foc-AgrS-V became lexicalised phrases.
5. Some of the phrases were reanalysed into complex verbs of the shape X-AgrS-v; AgrS clitics are trapped.
6. Once native speakers perceive this as a single complex item, they introduce AgrS clitics to simplex verbs as well.
 - 6a. By extension.
 - 6b. Into the slot where an older class marker would have been when Udi had class marker agreement on verbs.
7. AgrS clitics become obligatory and are placed finally in C or CV simplex verbs by default.
8. Subjunctive clitic *q'a*, from older auxiliary, postposes the clitic because of its shape.
9. Future II develops from former predicate nominals and thus requires a postposed clitic.
10. Out of these, and stative verbs from participles, Udi develops default final position for AgrS clitics ("permissive placement").

Some of these stages strike me as eminently plausible, particularly 5. Others – 1–3, 6–7 – are more problematic. In all cases, the lack of available knowledge about earlier stages of the language makes each stage speculative, but in some places, there are simply too many gaps to form a coherent picture. This is the case with steps 1–3, and even more so with 6–7.

Two goals were laid out at the beginning of this section: to determine whether the synchronic grammar of Udi poses severe difficulties for the theoretical assumptions made here, and to determine whether there were specific historical problems. The first problem is easier: Udi is unusual, certainly, but not so as to call the entire DM research programme into question. Harris's analysis of the history of Udi *is* incompatible with the current framework

at some stages; however, the data are insufficient to allow us a perspective on what might have happened, and an alternative explanation to the one she proposes is available. I conclude that the Udi data are difficult, but that the information available as to what happened is not known in nearly enough detail to suggest that only racial memories of ancestral stages of language can account for them.

4.2.4 General Discussion

The theory of affix-genesis presented here predicts that new affixes tend to appear on the periphery of an M-word, because they were adjacent to the M-word in the same position prior to their affixation. In this section, we have seen a number of cases involving clearly non-peripheral material, from infixes to endoclititics. These phenomena fall into two broad categories: those which appear in their surface position by virtue of morphophonological processes, and those which respect morpheme boundaries but not word boundaries. The latter are typically “trapped” in their surface position by syntactic or post-syntactic operations.

None of this is critically problematic for the theory developed in the previous chapter. We are left with a great deal to explain, of course: infixes (and endoclititics) tend to be extremely old, so the origins of the phonological processes giving rise to them are obscure. But these problems become relevant only after the affix has already begun to exist as such, not in the creation of the affix. This is true even in the case of Udi, where Harris has argued for a partial explanation involving the arcane preservation of ancestral morpheme slots. Her analysis has problems, and the data are too sparse to allow for a positive conclusion.

The existence of data like that presented here is easier to handle in a framework like DM, where each morpheme is active in the syntax. Lexicalist theories have more difficulty, because they have to confront the problem of apparent violations of lexical integrity. If a word is placed fully formed into the syntax, then it is a problem if it proves permeable. A piece-based theory avoids this issue.

Finally, it is worth mentioning here that many of the cases discussed in this section involve Local Dislocation, which raises questions about the acquisition of novel LD rules and why post-syntactic operations exist at all when they create morphosyntactic mismatches. There are some interesting implications for learnability issues here which deserve further investigation. Some researchers (e.g. Newton) object to post-syntactic rules as a sort of cheating; they believe that all placement must be syntactic, and that post-syntactic rules essentially function as a sort of rescue strategy.

Arguably, however, this is exactly what post-syntactic operations like LD and Lowering are for the learners themselves. If a child learning a language has come up with an analysis of his language that predicts a surface structure X , but discovers later that there are tokens of X' , he has three options: he can revise his initial hypothesis; he can maintain his initial hypothesis and produce structures that are obviously different from the speech of his parents; or he can maintain his hypothesis and add a post-syntactic rule to make adjustments and “fix” the output. Generally we think of language learners as always choosing the first and correcting themselves, but the fact that language *does* change and that intraspeaker variation *does* exist is enough to prove that this is not universally true.

In Chapter Six, I will discuss the loss of V-to-T movement in English, and how it led both to *do*-support and to a new rule of post-syntactic Lowering. The Lowering rule is interesting: it appears that speakers knew that their language had past tense forms and wanted to keep these forms even after they could no longer form them in the syntax, and so they adopted a strategy for restoring the past tense forms whenever possible. In this chapter, we saw something similar happening in Old Irish. The empirical data show that Old Irish has lost T-to-C movement, but it continues to generate verb forms that look as though they were generated via T-to-C movement, thanks to a post-syntactic LD rule. Once again, the rule is not always operative, and a strategy strikingly similar to *do*-support is used in those cases.

Both English *do*-support and Old Irish *no*-support involve the development of a novel post-syntactic rule in the context of syntactic change. This is not enough to say that new post-syntactic operations *always* arise in like fashion; more research is needed. Nevertheless, the hypothesis of a connection between syntactic change and new post-syntactic operations seems promising for future study.

4.3 Multiple Exponence

Multiple exponence is a departure from the customary one-to-one relationship between form and function: the same morphosyntactic features appear in multiple places within the same complex head. This is problematic for morphosyntax more generally, not specifically for affix-genesis. In some cases, like the Piattino example from the previous chapter, it can be shown that the feature information contributed by each morpheme is actually different; this is a case of fission rather than true multiple exponence.¹³⁶ Nevertheless, bona fide multiple exponence certainly exists, and where it exists it must have an origin. The goal of this section will be to examine the genesis of multiple exponence and thereby gain a better perspective on other aspects of the grammar.

A naïve view of multiple exponence might consider it problematic for the framework of linguistic change adopted here. One might assume that, if the locus of an affix is taken to reflect a permitted position relative to the form to which it is affixed, for there to be multiple simultaneous positions for it implies that it occupied multiple positions in the same clause prior to its affixation, which is not always the case. If this were true, it would be support for a lexicalist position. As this section will illustrate, however, multiple exponence can arise in a number of ways, but random proliferation concurrent with affix-genesis is unattested. All examples of the origin of multiple exponence known to me are completely consistent with the theoretical approach taken here.

Morphosyntactic changes resulting in multiple exponence appear to be of at least three types. First, it may be the case that an M-word previously marked for a particular category with one affix may later receive another affix marking the same (or a similar) category, a change which we might call “redetermination”. The Piattino example from the previous chapter is an example of this type of change, except that there is no redundancy in the Piattino system. Sometimes, however, redetermination does produce a degree of redundancy, as will be demonstrated in 4.3.1.

The second type of change arises when two independent M-words, each marked individually for the same category, become fused into a single M-word. This phenomenon is not dissimilar from the “trapped” clitics discussed earlier in this chapter, with the addition of redundancy within the M-word. Here it is clear that the redundant affix really did originally occur in multiple places, and it has only been trapped by the vagaries of fate (so to speak). Multiple exponence arising through the fusion of two inflected M-words will be discussed in 4.3.2, which includes a sub-section devoted to a somewhat more detailed case study of the rather extreme case of “exuberant exponence” in Batsbi, a language that allows the same subject agreement marker to occur up to five times within a single verbal complex.

There are also cases in which doubling appears to arise *sui generis*; these are the hardest to account for. The appearance of definiteness marking on nouns in Swedish in the presence of an overt determiner is one example of this type, which I will call *sporadic pleonasm*. This will be discussed in 4.3.3. General issues that come up along the way will be recapitulated in 4.3.4.

¹³⁶ For discussion of fission in synchronic morphology, cf. Noyer (1997).

4.3.1 Redetermination

Some cases of multiple exponence reflect two different historical layers: a language first gains an affix marking a particular category at one point in its history, and then, generations later, acquires another. This process is *redetermination*, defined in (4.168). This section will concentrate on two instances of redetermination: agreement markers in the Italian dialect Vicentino, and plural markers in Breton.

- (4.168) *Redetermination*: state of multiple exponence obtained in multiple stages: generation *P* acquires an affix marking category *C*, and generation *P+n* acquires another affix also marking *C*

Redetermination is often found with specific lexemes; the explanation offered by traditional historical linguists is that the older exponent was no longer felt to be marking the category by language learners. A good example of this is the English plural *children*, whose plural marker *-ren*, unique in the language, can be traced back to a succession of two different markers. The Old English plural of *cild* was *cildru*, where the plural suffix *-ru* is cognate with the *-er* plurals of German. Later, probably because this particular plural formant had become all but obsolete, the more productive plural suffix *-en* was added to *cildru* to produce *children*. The interesting point here is that speakers did not simply swap one plural suffix for another – to give †*childs* or †*children*¹³⁷ – but added a second plural suffix on top of the original – or, rather, to the right of the original, so that the newer suffix *-en* is further from the root from the older one.

This is exactly the prediction the theory advocated here makes for cases such as these: in cases of redetermination, the newer affix may not be closer to the root than the older one. It could, of course, be equidistant from the root, in the cases where one affix is a prefix and the other a suffix; but we should never find cases where the newer affix is closer to the root than the older one.

Redetermination does not always result in multiple exponence; in many cases, the two affixes are not marking precisely the same set of features, giving rise to Fission rather than bona fide multiple exponence. As noted in Chapter Three, some modern Romance and Germanic dialects have developed (or are developing) subject agreement prefixes in addition to the inherited subject agreement suffixes, often in connection with syncretism of the older suffixes. In Piattino, as we saw, redetermination has given rise to Fission, since the affixes are spelling out different features. But in languages with less syncretism than Piattino, redetermination could result in true multiple exponence. Fuß (2005:259ffn.) reports a potential example of this phenomenon based on his fieldwork on Vicentino, a variant of Veneto spoken in Northern Italy. The superficial pattern of subject pronominals in Vicentino is shown in Table 4.4; this will be modified below. Note that, interestingly, the form and position of the pronominals are dependent on the syntax of the sentence: in the second and third persons, a different clitic is used in inversion contexts, as given on second line of the relevant cells of Table 4.4.¹³⁸

¹³⁷ † is used here to mark hypothetically possible but unattested forms.

¹³⁸ The paradigm shown is that of *mangiare* [sic] ‘to eat’. Parenthetical forms are optional.

	Singular	Plural
<i>1st</i>	(a) magn-o	(a) magn-emo
<i>2nd</i>	te magn-i magn-i=to	(a) magn-è magn-è=o
<i>3rd. Masc.</i>	el magn-a-Ø magn-e=lo	i magn-a-Ø magn-e=li
<i>3rd. Fem.</i>	ła magn-a-Ø magn-e=ła	łe magn-a-Ø magn-e=łe

Table 4.4: *Subject agreement and clitics in Vicentino, present indicative*

Vicentino looks to have the following set of Vocabulary Items for the present indicative:

(4.169a) [+2]	↔	-è	/ [PL]
(4.169b) [+1]	↔	-emo	/ [PL]
(4.169c) [+2]	↔	-i	
(4.169d) [+1]	↔	-o	

Like their equivalents in Piattino, the third persons require a pronominal subject. Vicentino agreement suffixes do not distinguish number in the third persons, so each of the third person pronominals marks all of the subject features and there is neither Fission nor multiple exponence. Meanwhile, the first-person pronominals are always optional, and the first-person endings are always distinctive. Again, this looks much like Piattino.

The second-person forms, however, are more challenging. Here, the existing subject agreement suffixes are distinctive, so that the clitics are redundant. The second singular requires clitics across the board, both in inverted and non-inverted sentences (4.170), while the second plural does not require a clitic in non-inverted sentences but does require a clitic in inverted sentences (4.171). For clarity, the clitics are in boldface. As the examples show, the clitic subjects can double non-clitic subject pronouns.

- (4.170a) Ti **te** vien da Montecio.
2nd.sg 2nd.sg. come.2nd.sg from
'You come from Montecio.'
- (4.170b) * Ti vien da Montecio.
- (4.170c) Da 'ndó vien **to** ti?
where-from come 2nd.sg. 2nd.sg.
'Where do you come from?'
- (4.170d) * Da 'ndó vien ti?
- (4.171a) (A) vegn-i da Durlo.
CLIT come-2nd.pl. from
'You come from Durlo.'
- (4.171b) Da 'ndó vegn-i o (voaltri)?
where-from come-2nd.pl 2nd.pl 2nd.pl
'Where do you come from?'
- (4.171c) * Da 'ndó vegni (voaltri)?

There is reason to believe that the optional and obligatory clitics are completely different objects in Vicentino, beyond the matter of their optionality. First, the correlation between homophony and optionality in this system is perfect. All of the formally distinctive clitics - the third persons, the second singular, and the second plural inverted - are obligatory. All of the optional clitics - the first persons and second plural uninverted - are *a*. This could be a coincidence; however, the fact that the two groups of clitics show different syntactic

behaviour suggests otherwise. Fuß (2005:262) reveals that clitic *a* precedes negation, while distinctive clitics follow negation.

- (4.172a) **A** no vegn-o da Vicenza.
CLIT NEG come-1st.sg. from
'I do not come from Vicenza.'
- (4.172b) **A** no vegn-emo da Vicenza.
CLIT NEG come-1st.pl. from
'We do not come from Vicenza.'
- (4.173a) No **te** vien da Vicenza.
NEG 2nd.sg. come.2nd.sg. from
'You do not come from Vicenza.'
- (4.173b) No **la** vien da Vicenza.
NEG 3rd.sg.FEM. come from
'She does not come from Vicenza.'

In fact, *a* can be used for the second singular also. When it is, however, it must co-occur with the obligatory *te*, so that a sentence with a second singular verb can contain up to three subject pronominals.

- (4.174) Ti **a** no **te** vien da Vicenza.
2nd.sg. CLIT NEG 2nd.sg. come.2nd.sg. from
'You do not come from Vicenza.'

This indicates that the clitic *a* is only tangentially relevant to the discussion of the other Vicentino subject markers; whatever it is, it is part of a different system, and it is relevant to the discussion only insofar as it provides clues to the analysis of the second plural.

The alert reader will have noticed that the second singular forms used in the examples above are different from the forms cited in Table 4.4 and identical to the third person forms. This is not an error, but a feature of a few irregular verbs; Fuß (2005:265) cites also *tegnere* 'to hold'. His hypothesis is that second singular clitics became obligatory in precisely this context, to avoid homophony, and subsequently spread to the other second singular forms in the language. This was abetted, in his view, by the fact that another class of irregular verbs indicates changes in person by alternations in the stem vowel rather than by the addition of a discrete subject suffix. There are four such verbs, all very frequent: *dare* 'to give' (2nd.sg. *de*, 3rd. *da*), *fare* 'to do' (*fe*, *fa*), *nare* 'to go' (*ve*, *va*), and *savere* 'to know' (*se*, *sa*). He suggests that children would, when confronted with a choice between stem quality and clitic, choose the clitic as the exponent of subject agreement on the grounds that children prefer affixes over readjustment rules (cf. Clark 1998:384).

Fuß's account of the obligatory second plural enclitic is similar in nature if different in detail. He observes that the second plural indicative and subjunctive forms of four irregular verbs – *dare*, *fare*, *poder* 'to be able to', and *vegnere* 'to come' – are identical, and suggests that speakers first redetermined the second plurals of these verbs and then spread the new obligatory marking to the other second plurals. I find this account less than satisfying, for several reasons. First, all of these irregular verbs save *fare* have first plural forms in the indicative and subjunctive, yet the first plural does not behave like the second plural. Second, one might predict that there would be a distinctive obligatory proclitic in non-inversion contexts as well as inversion contexts, which there isn't. This analysis therefore seems a bit ad hoc.

Both of these accounts run afoul of the MARC from the previous chapter, which theorises that speakers conclude that their language has multiple exponence only if they have no other choice in the matter, if the data leaves them no other conclusion. The second-person

markers are unambiguous; why augment them? As to Fuß's conjectures about pressure from irregular forms, speakers are notoriously comfortable with irregularity even when it involves homophony. From the perspective of the general bias against multiple exponence, the preference of Vicentino speakers for systematic multiple exponence over four homophonous irregular forms seems irregular, if not outright perverse. In addition, MARC aside, Fuß's speculations are rooted in a rather deterministic view of diachronic change. Language change does not happen because speakers decide that it would be nice if all the subjunctive and indicative forms of their language were phonologically distinct.

A different tactic one could pursue in the Vicentino problem is to examine the clitic system from the perspective of someone attempting to acquire it, rather than the verbal system as a whole. Fuß's initial description of the distribution of the non-distinctive *a* is misleading, though he later clarifies the point. *A* is simply not part of the same system as the other clitics: it doesn't have the same syntax, it can co-occur with the other clitics, and so on. But this means that there is a curious gap in the clitic system: there are no first person clitics equivalent to the third person clitics – or, at least, there are none in Fuß's discussion. As I said earlier, the complementarity in this system is perfect. Why is this so? In Piattino, the optional clitics were clearly of the same species as the obligatory clitics; this doesn't seem to be true in Vicentino. Why are there no optional first person clitics filling the same role as *el* or *la*? Something is missing here.

Assuming these data are accurate, then the most likely scenario would be that the language learners acquired the following rule: distinctive clitics are obligatory, regardless of whether or not the verb has another salient marker. Language learners observe, first, that the third-person suffixes are all homophonous, and conclude that the actual exponent of the category of number and gender is the clitic in its various guises. Having made that step, they extend the same analysis to the other clitics that pattern with the third-person forms: the second singular and the second plural in inverted contexts. The fact that all the clitics are phonologically reduced gives them further motivation for interpreting them as affixes. This forces them to make a choice between a grammar with multiple exponence and a grammar where objects of the same syntactic distribution are of different types; in this case, they opted in favour of the former, probably because learners disprefer partial affixation patterns. Had there been distinctive first person clitics as well as second singular, speakers may well have opted for a more Piattinesque grammar. This analysis of the Vicentino data is simpler and more consistent with the understanding of language change set out in Chapter Two than Fuß's.

Further investigation is needed into the syntax of subject markers in Vicentino; a definitive analysis is beyond the scope of this chapter. It is not completely clear whether to analyse the distinctive markers as clitics or affixes, due to the variability in their syntactic position, but the fact that they are always adjacent to the verb regardless of the rest suggests that they are reasonably likely to develop into affixes if they haven't already. Despite this uncertainty, the Vicentino data do illustrate the two basic properties of multiple exponence via redetermination: one set of affixes is demonstrably older than the other, and the newer set is not nearer the root than the older, whether it is prefixed (and therefore equidistant from the root) or suffixed (and thus further). In both cases the newer marker is also peripheral, as predicted.

Different issues arise in the case of plural markers in Breton. Nominal number marking is complex in Breton, which has, in addition to singular and plural, a limited dual and a singulative, used on mass nouns and collectives. In addition, many nouns have, in an addition to a simple plural, a "double plural", with a second plural suffix. Stump (1989:262) notes that nouns which take double plurals often have an irregular simple plural, as in (4.175a–d), though (4.175e–h) show this is not always the case.

(4.175) <i>Singular</i>	<i>Simple Plural</i>	<i>Double Plural</i>
a. louarn ‘fox’	lern	lern-ed
b. gavr ‘goat’	gevr	gevr-ed
c. houarn ‘iron’	hern	hern-ioù
d. troad ‘foot’	treid	treid-où
e. merc’h ‘girl’	merc’h-ed	merch’-ed-où
f. loen ‘beast’	loen-ed	loen-ed-où
g. preñv ‘worm’	preñv-ed	preñv-ed-où
h. roñse ‘horse’	roñse-ed	roñs-ed-où

Both the simple plural and the double plural trigger plural agreement on the verb and receive a plural interpretation.

(4.176a) N’ eo ket mat ar merc’h-ed= se; re vihan int.
 PTCL BE NEG good DEF girl- PL₁=DEM too little BE.3rd.pl.
 ‘These girls aren’t good, they are too small.’

(4.176b) N’ eo ket mat ar merc’h-ed-où= se; re vihan int.
 PTCL BE NEG good DEF girl- PL₁-PL₂=DEM too little BE.3rd.pl.
 ‘These girls aren’t good, they are too small.’

Simple and double plurals also behave alike within the DP. In the presence of a cardinal number, neither plural suffix can appear (De Belder 2010:4).

(4.177a) ugent merc’h
 twenty girl
 ‘twenty girls’

(4.177b) * ugent merc’h-ed
 twenty girl- PL₁

(4.177c) * ugent merc’h-ed- où
 twenty girl- PL₁-PL₂

The data shown so far could be taken to imply that Breton double plurals are much like English *children*, only more widespread; however, other facts indicate that this is not quite true. First, the simple plural can serve as the stem in denominative verbs.¹³⁹

(4.178) merc’h-et- a
 girl- PL₁-VERBAL.SUFFIX
 ‘to womanize’

Second, when a diminutive of a plural is made, the diminutive suffix comes between the two plural suffixes (data from Stump (1989:266).

(4.179a) labous-ed- ig- où
 bird- PL₁-DIM-PL₂ ‘birdies’

(4.179b) merc’h-ed- ig- où
 girl- PL₁-DIM-PL₂ ‘little girls’

(4.179c) paotr-ed- ig- où
 boy- PL₁-DIM-PL₂ ‘little boys’

(4.179d) gwin-où- ig- où
 wine-PL₁-DIM-PL₂ ‘light wines’

¹³⁹ Gloss from De Belder (2010:2).

(4.179e) tie- z- ig- où 'cottages'
house- PL₁-DIM-PL₂

Evaluative suffixes in other languages show similar facts, though in more restricted fashion; Stump (1989:271) notes Yiddish *kinderlex* 'little children', where the diminutive *-l-* comes between two plurals *-er* and *-ex*; and De Belder cites similar facts from Dutch. Elsewhere in Breton, the superlative suffix can also come between the two plurals (Stump 1989:272).

Breton speakers apparently feel that there is a semantic difference between the simple plural and the double plural (Trépos 1957), although the difference does not seem to be predictable (4.180). Mewanwhile, forms which allow duals, as in (4.181), can sometimes form two different plurals, the first ("plural I") by adding a plural to the dual-marked noun, and the second ("plural II") by pluralising the singular; again, the semantic differences are unpredictable.¹⁴⁰ Plurals made to collective nouns in Breton are similarly semantically unpredictable (4.182).

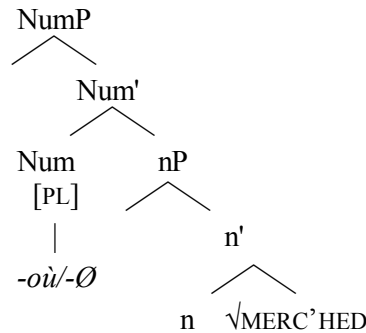
(4.180a) singular:	bugel	'child'
simple plural:	bugal-e	'children'
double plural:	bugal-e-où	'several groups of children'
(4.180b) singular:	botez	'shoe'
simple plural:	bot-où	'pair of shoes'
double plural:	bot-eier	'indeterminate number of shoes'
(4.181a) singular:	lagad	'eye'
dual:	daou-lagad	'eyes'
plural I:	daou-lagad-où	'pairs of eyes'
plural II:	lagad-où	'flecks of fat'
(4.181b) singular:	skouarn	'ear'
dual:	daou-kouarn	'ears'
plural I:	daou-kouarn-où	'pairs of ears'
plural II:	skouarn-où	'handles'
(4.182a) collective:	buzhug	'earthworms (undifferentiated mass)'
plural:	buzhug-ed	'earthworms (set of discrete individuals)'
(4.182b) collective:	dilhac	'ensemble of clothes; outfit'
plural:	dilhac-où	'clothing'

Press (1986), unlike Stump (1989), makes a distinction between semantically alternative plurals and double plurals; for him (4.179a) and (4.179b) are examples of different phenomena. (4.179b) involves the substitution of one plural suffix for another, with a resulting change of semantics, while (4.179a) adds a second suffix when the first is already present.

There has been a great deal of contention over the correct analysis of Breton double plurals; for the sake of brevity I will discuss only the recent analysis of De Belder (2010). She proposes that the simple plural is actually only a stem allomorph, and that the real marker of plurality is either the double plural suffix or, in the case of an apparent simple plural, a zero allomorph. Thus, the apparent simple plural *merc'hed* is underlyingly *merc'hed-Ø*. Her analysis of the structure of plural NumPs in Breton is shown here:

¹⁴⁰ (4.180) from Stump (1989); (4.181) from Press (1986).

(4.183)



One of De Belder's arguments in favour of her proposal is the fact that the grammar must have similar Readjustment Rules elsewhere unrelated to the plural, such as denominative verbs (cf. (4.178)) and singulatives like (4.184):

(4.184) *ster-ed-enn*
star-PL₁-SGLT
'individual star'

Singulatives generally attach to mass or collective nouns to pick out a portion or an individual member of the set, and can be themselves pluralised; one might expect, therefore, that the potential plurality of the simple plural suffix might be relevant in this context. However, the singulative has other uses which do not require its host to be semantically plural. Press (1986:70) says that the suffix can be placed on singular nouns, "say in a figurative sense", giving the example of *kalonnenn* 'heart-shaped object' to *kalon* 'heart'. It can also be used with singulars to stress individuality; Press cites the contrast between *botez* 'shoe' and *botezenn* 'single shoe'. The denominative verb suffix *-a* is slightly more problematic, in that it does appear to require its host be semantically plural in some sense. Press (1986:221) says that *-a* can be added to "collectives, non-counts, some plurals, and to nouns in *-ad* denoting quantity of "blows". Both of these non-plural uses of the simple plural have some complications which would need to be worked out.

Diminutives raise some locality concerns for this analysis, since the diminutive suffix occurs between the two plural suffixes, and one might then predict that the disruption of the linear relationship between the root and the plural suffix might preclude the activation of the Readjustment Rule. The diminutive suffix itself cannot obviously be considered the trigger for the rule because singular diminutives appear with the shorter nominal stem.¹⁴¹

These concerns aside, De Belder's analysis fits nicely with the concept of redetermination. If she is correct that the older plural morpheme has become the output of a Readjustment Rule, then, clearly, they are no longer genuine exponents of plurality. The fact that many of the nouns which form double plurals have irregular simple plurals is probably significant in this context, although some historical study of the double plural will be necessary in order to determine exactly what happened in Breton.

The deeper problem, too, requires further investigation: why, if the older marker was phonologically distinctive, perfectly functional to all outward appearances, do speakers learning the language find it insufficient on its own? Redetermination leading to fission is understandable; the data available to speakers allows them to come up with overt and possibly unique exponents for all the features they feel their language marks. What is the motivation for an analysis that gives rise to ambiguity?

¹⁴¹ Interestingly, Press (1986:72) notes that when diminutives are no longer perceived to be diminutive, the inner plural suffix vanishes: *rannig* 'grammatical particle' forms both *rannoùigoù*, with double plural, and *rannigoù*, with only the outer plural.

4.3.2 “Stepsiblings”

Most of the examples discussed in this dissertation so far have concerned affixes which were, at the time of their incorporation within a larger M-word, essentially monomorphemic; but this need not be the case. Two M-words, both of which contain Sub-words, can also become a single M-word, as in the case of the Romance future and conditional. In the Romance example, none of the Sub-words comprising the new M-words were marking the same features. However, if the infinitive had been marked for subject ϕ -features, the result could have been a new M-word with two AgrS morphemes: a case of multiple exponence. In this case, the exponents are not necessarily the result of different diachronic layers, as in the previous chapter; they are more like stepsiblings in a newly blended family.

We had a preview of this kind of development with the Amharic example from Chapter Three. Although the Amharic compound gerunds mostly involve the combination of an inflected gerund with an uninflected auxiliary, both forms are inflected in the first singular and third singular feminine. There is another compound tense form in Amharic, the compound imperfect, with a higher incidence of doubled inflection. The compound imperfect is probably still structurally two M-words, put together by LD or simple contraction¹⁴²; nevertheless, it is useful for illustrative purposes because the source of each of the pieces is clear, even if the nature of the syntactic dependency between the two M-words is less so.

The compound imperfect combines the inflected imperfect form with the same auxiliary *allä* that underlies the nascent suffix *-all*, but with a crucial difference: in the compound imperfect, only the third singular masculine form uses the uninflected form *-all*. All of the other forms use a fully inflected form of *allä*. This is illustrated in Table 4.5; the triple dashes in some cells indicate placement of the stem in forms with both prefixes and suffixes.

	Singular		Plural	
	<i>Simple Imperfect</i>	<i>Compound Imperfect</i>	<i>Simple Imperfect</i>	<i>Compound Imperfect</i>
1 st .	ə-	ə---all-ähu (-allä ^w h)	ənnə- or ən-	ənnə---all-än
2 nd . Masc.	tə-	tə---all-äh	tə---u	tə---all-aččəhu (-allačəuh)
2 nd . Fem.	tə---i	tə---iy-all-äš / tə---əy-all-äš		
3 rd . Masc.	yə-	yə---all	yə---u	yə---all-u
3 rd . Fem.	tə-	tə---all-äčč		

Table 4.5: Amharic Compound Imperfect

With the exception of the third singular masculine, all of the forms of the compound imperfect are inflected for subject agreement twice: first by the imperfect prefix and then by the perfect suffix on the forms of *allä*; moreover, the imperfect suffix is retained in the second singular feminine. What happens in the second and third plural is rather more difficult. The forms cited in Table 4.5 lack the imperfect suffix *-u*, but these are forms without AgrO suffixes, and when an AgrO suffix is present, the pattern is different.

AgrO suffixes occur between the imperfect and the auxiliary, as shown in Table 4.6.¹⁴³ Since most imperfect forms have prefixes rather than suffixes, this usually positions AgrO to the immediate right of the verb stem.

¹⁴² The data are very difficult, and the actual analysis is beyond the scope of this chapter. For details cf. Diertani and Eilam (2010, in progress).

¹⁴³ The verbal forms are those of the third singular masculine; the root is *ngr* ‘tell’.

	Singular	Plural
1 st .	yə-nāgr-āññ-all	yə-nāgr-ān-all
2 nd . Masc.	yə-nāgrə-h-all	yə-nāgr-aččəh ^w -all
2 nd . Fem.	yə-nāgrə-š-all	
3 rd . Masc.	yə-nāgr-āw-all	yə-nāgr-aččāw-all
3 rd . Fem.	yə-nāgr-at-all	

Table 4.6: *Amharic Compound Imperfect with Suffixed Object Markers*

When the imperfect does have an AgrS suffix, the AgrO suffixes follow it. This is expected in the case of the second singular feminine (4.185a); except for the presence of the AgrO suffix, which is exactly where we would expect it to be, the form is the same as it would have been without the AgrO. But when the verb is second or third plural, the absent imperfect AgrS resurfaces. In the second plural, the suffix on the auxiliary is retained (4.185b), but in the third plural, the outer suffix disappears (4.185c). The same pattern is found when the enclitic *-mm* (optionally) intervenes between imperfect and auxiliary,¹⁴⁴ so this is not simply an attraction between Agr nodes.

- (4.185a) tə-STEM-**i**-STUFF-**all-āš**
(4.185b) tə-STEM-**u**-STUFF-**all-aččəhu**
(4.185c) yə-STEM-**u**-STUFF-**all**
- (4.186a) Yə-nāgr-all- u- mm.
3rd-tell- AUX-3rd.pl.-CONJ
‘And they will tell.’
- (4.186b) Yə-nāgr-u- mm- all.
3rd- tell- 3rd.pl.-CONJ-AUX

Exactly what speakers are doing with AgrS here is unclear; the data are messy, and Aviad Eilam’s fieldwork has produced some evidence that different speakers have slightly different patterns, which indicates that the compound imperfect may be the subject of ongoing change in progress. Further investigation is needed. But the point should be clear: should the compound imperfect develop into a single M-word, and should it retain multiple exponence, the result will not run counter to the scenario developed in the previous chapter. Any multiple exponence here will arise because both of the M-words had an AgrS of their own.

Finding a secure example of this type of multiple exponence is not easy, because most of the available examples are either potential, as in the case of Amharic, or reconstructed. Such a reconstruction serves as the basis for Dixon’s (2002) analysis of the Australian language Yanyuwa on the basis of work by Kirton (1970, 1971). Though they cannot be used as confirmation, the data are sufficiently interesting to be mentioned here.

Nouns in Yanyuwa are marked with the case/classifier prefixes shown in Table 4.7.

	Class	Nominal Case/Classifier Portmanteau Prefixes		
		Absolutive	Dative	Ergative/Locative
<i>Human masculine</i>	1a	nja-		nju-
<i>Non-human masculine</i>	1b	Ø-		dji-
<i>Feminine</i>	2	rra-		rru-
<i>Vegetables, &c.</i>	3	ma-	mu-	mu-ŋgu-
<i>Trees, &c.</i>	4	na-	nu-	nu-ŋgu-

Table 4.7: *Yanyuwa Case/Classifier Prefixes*

¹⁴⁴ The judgements reported in (4.186) are those of Aviad Eilam’s informant GA, which he has kindly made available to me.

In addition to the prefixes, nouns are also marked with case suffixes: the suffixes for disyllabic nouns are dative *-wu*, ergative *-ŋgu*, and locative *-ŋga*.¹⁴⁵ Since the dative, ergative, and locative cases all take the same prefix in classes 1 and 2, the suffixes in these classes are contributing new feature information; similarly, the ergative and locative suffixes are contributing new information in classes 3 and 4, because these prefixes are identical. These are presumably best analysed as Fission. But the class 3 and 4 dative is different: here, both the prefix and suffix are conveying the same information, so that the presence of both affixes is basically redundant. This is a form of multiple exponence.

Dixon (2002:500) suggests that the prefixes in Yanyuwa are of relatively recent origin, though they are not all from the same source. Of interest here is his discussion of the Class 3 prefixes *ma-*, *mu-*, and *muŋgu-*. Class 3 is used for, among other things, vegetable foods; many of the languages related to Yanyuwa also have such a category, with a similar exponent. Other languages without nominal class affixes have a classifier for ‘vegetable food’ whose phonological form is *mayi* (or something similar to it), and Dixon’s discussion (p. 495) implies that languages with class affixes also have either such a classifier or a noun for ‘vegetable food’, again similar in form to *mayi*.¹⁴⁶ This *mayi* is a plausible candidate for the ancestor of the class marker.

Before classifiers became class prefixes, he argues, they were marked for case just as their head nouns were. This meant that the phonological reduction of each case of the classifier was different, and yielded different forms, and speakers continued to learn different forms even after the classifiers had become full-fledged prefixes. This led to the preservation of case marking and, consequently, to multiple exponence in the dative. He postulates the developments from the original two-word stage to the modern Class 3 forms as in (4.187); cf. Dixon (2002:501).

(4.187)	<i>Classifier plus case</i>	>	<i>Intermediate Form</i>	>	<i>Fused Class/case</i>
<i>Abs.</i>	<i>mayi</i>	>	<i>mayi</i>	>	<i>ma-</i>
<i>Dat.</i>	<i>mayi-wu</i>	>	<i>ma-wu</i>	>	<i>mu-</i>
<i>Erg.</i>	<i>mayi-ŋgu</i>	>	<i>ma-ŋgu</i>	>	<i>muŋgu-</i>

The general schema would then be something like (4.188):

(4.188) class-CASE noun-CASE	>	CLASS.CASE-noun-CASE
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Again, this case is too hypothetical to be used as firm evidence of diachronic principles; however, the data are certainly interesting, and Dixon’s analysis is inherently plausible, if somewhat vague on structural details.

The take-home message for this section is that one potential source for multiple exponence is the “collision” of two M-words independently marked for the same category. Actual examples of the phenomenon, however, are not easy to come by. This could be due to a cognitive bias against multiple exponence, but the matter requires further investigation.

4.3.2.1 “Exuberant Exponence”: *Batsbi*

One of the most unusual and challenging examples of stable multiple exponence comes from the Nakh-Daghestanian language *Batsbi*, also known as *Tsova-Tush*, which has been the subject of extensive research by Harris (2008, 2009), who analyses *Batsbi* multiple exponence as an example of the product of colliding M-words. *Batsbi* has multiple realisation of the same features via different morphemes within a single M-word; it also has multiple

¹⁴⁵ Dixon does not cite suffixes for nouns with more or less than two syllables.

¹⁴⁶ The caveat is that Dixon’s discussion here is somewhat difficult to follow.

realisation of the same *morphemes* within a single M-word, sometimes as many as four. The latter is what Harris (2008) has dubbed *exuberant exponence*.

The Batsbi case is interesting from our perspective for two reasons. First, there is enough comparative evidence available from other Nakh-Daghestanian languages to piece together a picture of how the unusual situation of agreement in Batsbi came to be; this work has been done by Harris (2008), and the story she tells is quite plausible. Second, since exuberant exponence not only exists in Batsbi but appears to be stable, this grammar is clearly learnable, and it is therefore worth considering why this should be so if it is indeed the case that children acquire multiple exponence as something of a last resort.

Verbs in Batsbi can take two different kinds of agreement. One of these will not concern us much here; this is subject-agreement, which is marked only once, at the end of the verbal complex, as shown in (4.189).^{147,148}

- (4.189) Duq kaniz y- ayq-n- atx.
 many grape[s](y/y) CM-eat- AOR-1st.EX.ERG
 ‘We (exclusive) ate a lot of grapes.’

What will concern us here is class marker agreement. Batsbi nouns belong to eight noun classes, though three of them are unproductive (Harris 2009:273), the forms of which are given below in Table 4.8.¹⁴⁹ The forms of the class markers show a great deal of syncretism: for the sixteen combinations of features (eight classes, two numbers), there are exactly four distinct markers, each of which consists of a single consonant: *b- d- v- y-*. It is the choice of the two markers that delineates the classes, which are not usually marked on the nouns themselves. Harris’s convention, which I have followed here, is to gloss each noun with an indication of its class membership ‘(singular/plural)’ (“y/y” in 4.189 above) and to gloss the class marker agreement simply as “CM”. The default marker *d* is used in citation forms of verbs with CMs.

Class	Singular	Plural	Example
1	v-	b-	<i>voħ</i> ‘son’
2	y-	d-	<i>ag</i> ‘grandmother’
3	y-	y-	<i>q’ar</i> ‘rain’
4 (unproductive)	b-	b-	<i>kakam</i> ‘wool sheared in the fall’
5	d-	d-	<i>bader</i> ‘child’
6	b-	d-	<i>matx</i> ‘sun’
7 (unproductive)	b-	y-	<i>bšark</i> ‘eye’
8 (unproductive)	d-	y-	<i>lark</i> ‘ear’

Table 4.8: *Batsbi Class Markers*

¹⁴⁷ Though not relevant for the current discussion, subject agreement in Batsbi is of interest in its own right, as Batsbi is an ergative language, and the form of AgrS is dependent on whether the subject is ergative or absolutive, as well as on the subject’s ϕ -features. The other point of interest is that it is not entirely clear whether AgrS is a suffix or not. The morphemes are transparently related to the independent subject pronouns, which do not always appear in the sentence; the first-person inclusive AgrS is an independent clitic.

¹⁴⁸ Unless otherwise noted, all of the examples in this section were taken from Harris (2009), who in turn took them from Kadagiže and Kadagiže (1984). I have followed Harris’s conventions for glosses. The ‘(y/y)’ convention will be explained shortly. Verbs are cited in the masdar or verbal noun form, with the suffix *-ar*.

¹⁴⁹ Table from Harris (2009:274).

Class markers (CM) in Batsbi are conditioned by the absolutive argument: the subject of intransitives and the object of transitives, as shown in (4.190), with the CM in bold.¹⁵⁰ This means that in intransitive sentences, AgrS and CM will denote the same entity, but in transitive sentences AgrS will agree with the (ergative) subject and CM with the (absolutive) direct object.

- (4.190a) Xen-go- ħ potl- i **d-** ek'-iⁿ.
 tree- ALL-LOC leaf(d/d)-PL.ABSL CM-fall-AOR
 'The leaves of the tree were falling.'
- (4.190b) Pst'uyn- čo- v bader **d-** iy- eⁿ.
 married.woman(y/y)-OBL-ERG child(d/d).ABSL CM-do-AOR
 'The (married) woman bore a child.'

Not all verbs take CMs, however. Rather, the ability to take a CM appears to be a property of some roots, but not of others. To some extent, which roots will take CMs and which will not is predictable, because there is a phonological component to the distribution: only roots beginning with a vowel or /r/ can take a CM. But this rule is insufficient, since not all roots that satisfy the phonological requirement actually take CMs, as illustrated by (4.191).

- (4.191) Qaⁿ simind lapsdaⁿ matx ot'- ō.
 tomorrow corn(d/d).ABSL to.dry sun(b/d) spread-FUT
 'Tomorrow (they) will spread the corn in the sun to dry.'

Harris (2009:278) notes that there are even a number of minimal pairs in Batsbi which are distinguished only by whether or not they take CMs. Some of her examples are listed in (4.192). She also notes that some verbs take a CM when imperfective and not when perfective, or vice versa.

- (4.192) *Some minimal pairs in Batsbi:*
- | | | | |
|--------|---------------------|------------------|--------------------|
| ak'-ar | 'burn (intr.stat.)' | d- ak'-ar | 'burn (intr.act.)' |
| ał-ar | 'say' | d- ał-ar | 'give' |
| ot'-ar | 'spread' | d- ot'-ar | 'go, go over' |
| ot:-ar | 'stand, stay' | d- ot:-ar | 'pour into' |

So far, each verb has had a single CM, but derivative verbs can have more. Batsbi has a set of what Harris (2009:276) terms "first order extensions": essentially *v* formants used to derive verbs from other verbs. These are *i*, which derives transitives¹⁵¹, and *al* and *is*, which derive intransitives; the latter is infrequent and probably unproductive. Each of these three morphemes requires a CM, whether or not it is added to a root that requires a CM. This is shown in (4.193)–(4.194) for *i* and in (4.195) for *al*; Harris (2009) has additional examples. (4.193) is to be compared with (4.190a), which features the same root.

- (4.193) Don- e- v taylz- i **d-** ek'-**d-** iy- eⁿ.
 horse(b/d)-OBL-ERG saddlebags(/d)-PL.ABSL¹⁵² CM-fall- CM-TR-AOR
 'The horse threw off the saddlebags.'

¹⁵⁰ In addition to verbs, CMs also appear on adjectives and some other lexical items (Harris 2009:273).

¹⁵¹ Regular phonological processes delete the transitive morpheme *-i* before vowels other than [e]; cf. Harris (2009:277n10). For clarity, I will indicate it by (TR) in the glosses where this is necessary.

¹⁵² The noun for 'saddlebags' is *pluralia tantum* and therefore can never be used with a singular CM.

- (4.194a) K'ab xarc- y- i- n- as.
 dress(y/y).ABSL change-CM-TR-AOR-1st.sg.ERG
 'I change my dress.'
- (4.194b) K'alam xerc- n- as.¹⁵³
 pen(d/d).ABSL change-AOR-1st.sg.ERG
 'I change my pen.'
- (4.195a) P'erang-mak-aňǒ xalat y- opx- ǒ.
 shirt- on- LOC house.coat(y/y).ABSL CM-put.on-PRES
 '[She] puts a house coat on over her shirt.'
- (4.195b) Seⁿ yoň taguš y- opx- y- al- in ǔ.
 1st.sg.GEN. girl(y/d).ABSL beautifully CM-put.on-CM-INTR-AOR CONJ
 'My daughter dressed beautifully and...'

So far, the number of CMs in a single verb can be zero (neither root nor *v* takes a CM), one (either the root or *v* takes a CM), or two (both root and *v* take a CM). Verbal compounding can produce additional CMs, because each of the elements involved in the compound brings along its own CM. In (4.196)¹⁵⁴, the first element of the compound is a *vP*, the derivative verb stem *teg-d-ar* 'do; put in order' (from root *teg-ar* 'be put in order'), and the second is another *vP*, the transitive *d-ol-d-ar* 'begin'.¹⁵⁵

- (4.196) teg- b- a- b- ol- b- ie
 order-CM-EV-CM-begin-CM-(TR)-PAST
 'S/he just began to work.'

Batsbi also has a set of evidential suffixes, some of which come with their own CMs. The verb in (4.197a) is *tet'-ar* 'cut', which does not ordinarily occur with a CM, but does appear with a CM in the present evidential in (4.197b).¹⁵⁶

- (4.197a) Mayqĩ tet'-o- s.
 bread(y/y).ABSL cut-PRES-1st.sg.ERG
 'I am cutting bread.'
- (4.197b) tet'-d- anǒ
 cut-CM-EVIDI
 'Evidently s/he was cutting it.'

The following examples show how the addition of the evidential suffix can increase the number of CMs in the verbal M-word.

¹⁵³ Harris (2009:275n.6) notes that the vocalic difference between the two roots is phonologically regular.

¹⁵⁴ (4.196) is from Harris (2008), taken from Dešeriev (1967:241).

¹⁵⁵ (4.200) below raises the question of whether verbal compounding always involves *vPs*, as it has, on the surface, only CMs and roots. Harris analyses this example as having two null instances of *-i-* with the CMs deleted when two of them are adjacent in the surface phonetic string; that is, (4.200) is underlyingly *d-ic'-d-Ø-d-aq-o-d-Ø-ǔ*, with the second CM deleted so as not to yield **dic'ddaqodǔ*. This raises questions of ordering: if, as seems plausible, the CMs are inserted before VI, let alone the application of regular phonological processes, then the phonetic conditions which trigger the deletion of the transitive suffix *-i* are not met, and the surface form should then be **dic'didaqodǔ*. This requires further investigation.

¹⁵⁶ Harris's (4.197b) was taken from Holisky and Gagua (1994:181) and Č'relašvili (1984, 1990).

(4.198a) K'ab y- ox-y- iy- eⁿ.
 dress(y/y).ABSL CM-rip-CM-TR-AOR
 '[She] ripped the dress.'

(4.198b) y- ox-y- o- y- anö
 CM-rip-CM-PRES-CM-(TR)-EVIDI
 'Evidently she ripped it.'

Harris (2008) analyses Batsbi CMs as the result of various collisions between M-words, much like the type of phenomena mentioned in 4.5.1.2. Although the attestation of Batsbi is late enough that it cannot be certain exactly how this happened, some traces remain, and Harris's argument is quite plausible. First, she derives the derivational suffixes *-i* and *-al* from earlier constructions involving light verbs, evidently a common, if independent, development in Nakh-Daghestanian languages. The frequently deleted *-i* can still be used as a main verb, as shown in (4.199a),¹⁵⁷ which is to be contrasted with the suffixal use in (4.199b).

(4.199a) Vux- k' d- in- o- lo- s!
 what.ABSL-ever CM-(do)-AOR-RPT-SUBJ-1st.sg.ERG
 'Whatever have I done?'

(4.199b) Darĵaⁿ it':- lye- čö k' lasi y- ot'- y- iyeⁿ.
 (y/b).ABSL ten-ORD-in grade CM-go.over-CM-(TR)-AOR
 'They took Darejan over into the tenth grade.'

Synchronically, there is only one verb in (4.199); this shown not only by the fact that the M-word constitutes a phonological word, but by the absence of more than one set of tense/aspect/mood morphology. But at some earlier stage of Batsbi, this construction must have involved two verbs, each bearing its own TAM. This is suggested by the presence of constructions like (4.200), which point to the existence of an older grammar in which both the main verb and the auxiliary had their own tense marker.¹⁵⁸

(4.200) Ma d- ic'- d- aq- o- d- ö is bader...
 NEG CM-forget-CM-raise-PRES-CM-(TR)-PRES this child(d/d).ABSL
 'Don't make this child supercilious!'

Harris (p.c.) says that examples like (4.200) come from the speech of speakers who were elderly when the fieldwork that led to the Batsbi dictionary was undertaken in the 1920's–1960's. Younger speakers (now in their fifties) do not have the internal tense morpheme in examples of this type, but apparently do retain it in others.

Other evidence suggests that whatever happened was not straightforward. Not all of the auxiliaries believed to have been suffixed retained their CM afterwards. For example, Batsbi has a causative morpheme which produces sentences like (4.201b) from non-causative (4.201a). This suffix does not have a CM, and yet the verb from which it is presumably derived, the still-attested *d-it-ar* 'cause', does have a CM.

(4.201a) Nik'ö šayrī naq'bist'-v- a? v- a?- eⁿ.
 (v/b).ABS 3rd.sg.GEN friend- INST-CASE CM-come-AOR
 'Niko came, together with his friends.'

¹⁵⁷ From Harris (2008), taken from Holisky and Gagua (1994:182).

¹⁵⁸ The absence of a second AgrS morpheme is not important, since the development of AgrS is of more recent date than the auxiliary > v development.

(4.201b) Ag- a- s kotam c'- i- n ču
 grandmother(y/d)-OBL-ERG chicken(b/b).ABSL house-OBL-DAT in
 d- a?- it- iyeⁿ.
 CM-come-CAUS-AOR
 ‘Grandmother let a chicken come in the house.’

The exact details of this process are unrecoverable, but the available evidence, particularly the presence of examples like (4.200), does corroborate Harris’s hypothesis for the source of the internal CMs. Once, then, Batsbi had constructions involving an auxiliary and a main verb, each of which was associated with its own CM; and a subset of these CMs were not lost when the construction was reanalysed as a main verb with a suffix.

Harris has similar arguments about the CMs associated with evidentials, which are believed to be of considerably more recent origin. Some of Harris’s sources (Holisky and Gagua 1994; Č’relašvili 1984) describe them as periphrastic but write them as suffixes; another (Holisky 1994) describes them as auxiliaries becoming suffixes and writes them as separate words. Harris (2009) conducted a series of tests for wordhood on the evidentials and discovered that they behave like suffixes insofar as it is possible to tell; under conjunction, for instance, they must be repeated on each conjunct. Historically, the evidentials are said to come from an aorist reported copula (the independent use of the copula is shown in (4.202)).

(4.202) St’ak’ v- a.
 man(v/b) CM-BE
 ‘He is a man.’

Again, though the details are obscure and likely irrecoverable, Harris’s analysis of the origin of exuberant exponence in Batsbi is probably correct. New pieces were added to the verbal M-word via suffixation or incorporation (in the case of the compounds), bringing the CMs associated with them at least some of the time. Whether some of these CMs were lost at some point along the way is unclear; synchronically they appear to be stable.

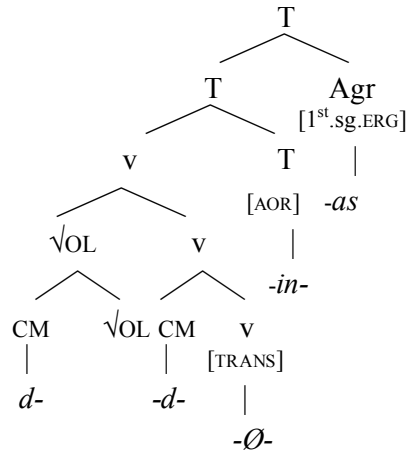
Harris’s (2009) contention is that the existence of a language like Batsbi is enough to call into question the DM enterprise, but this need not be the case. Once we adopt the view of agreement markers as dissociated morphemes attached after the syntax, then there is nothing preventing us from allowing a language to do this more than once. Clearly this is required in the Batsbi case, as there is no question that this is not an example of fission; the CMs in a Batsbi verb are all instances of the same VI inserted repeatedly.

One of the noteworthy properties of the Batsbi CM system is that the CMs are associated with particular Sub-words, even at the Root level. The minimal pairs discussed above (cf. (4.192)) suggest that some Batsbi Roots have a diacritic marking them as requiring a CM, much as Roots in Latin are marked with a diacritic for conjugation class, another non-syntactic feature. This should be contrasted with the AgrS morphemes, which attach to the M-word as a whole. AgrS in Batsbi is completely unremarkable from a cross-linguistic perspective: it appears at the end of the verbal complex and presumably is associated with T, which gives it scope over the entire M-word. The CMs are associated with lower heads, so that the lowest, the CM associated with the Root, has only the Root in its scope.

The structural differences between AgrS and the CM are represented in (4.203). (4.203b) shows the attachment of AgrS to the upper segment of T, which places it on the outside with the entire M-word in its scope. The CMs attach to the Root and to the *lower* segment of v.

(4.203a) d- ol- d- in- as
 CM-start-CM-(TR)-AOR-1st.sg.ERG.
 ‘I started (something).’

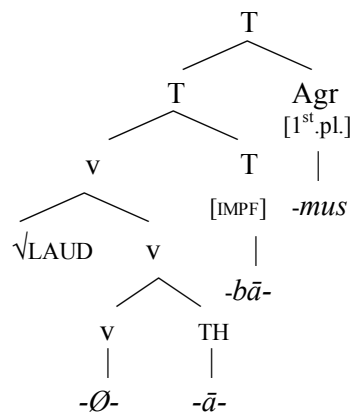
(4.203b)



Compare (4.203) to the structure of the Latin verb depicted in (4.204), taken from Embick and Noyer (2006:305–6). The AgrS morphemes in Latin and in Batsbi are structurally identical; the CM in Batsbi is structurally more similar to the Latin theme vowel.

(4.204a) laud- ā- bā- mus
 praise-TH-IMPV-1st.pl.
 ‘We were praising.’

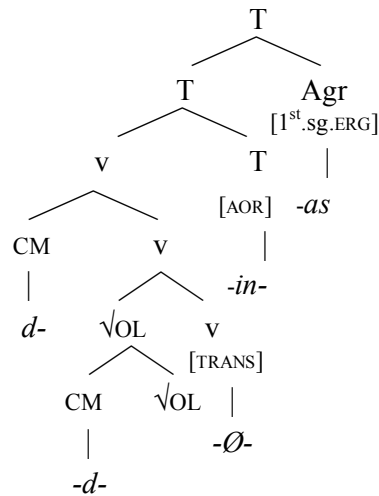
(4.204b)



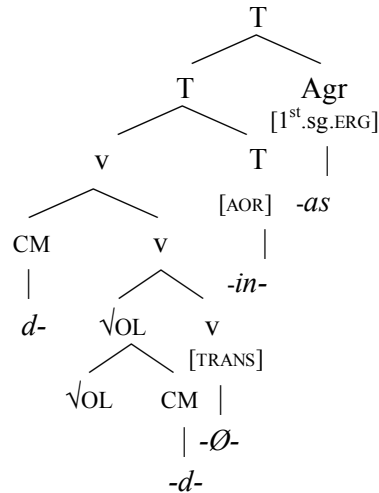
(4.205) shows that the CMs *must* be analysed in this way. If they attached to the upper segment like AgrS, we would predict incorrect surface forms like (4.205a). The incorrect form is phonologically impossible for Batsbi, which does not allow word-initial consonant clusters, but in a sense this is orthogonal, because the likely history of the Batsbi forms would never have given speakers reason to entertain (4.205b) as a possible analysis. Moreover, since verbs can lack a CM in either or both position, speakers have plenty of evidence for recognising which CM is associated with which terminal, ruling out a structure like (4.205c).

(4.205a) *d-d-ol-in-as

(4.205b)



(4.205c)



Although it might be possible to derive the Batsbi surface forms from (4.205b) in some way, the derivation probably would not be straightforward, and there are other reasons to think (4.203b) an appropriate structure for Batsbi. CMs in Batsbi do, in fact, share a number of properties with theme vowels in other languages, and taking them on that level makes some of their more unusual characteristics less exotic. Like theme vowels, CMs surface word-internally; like theme vowels, they are associated with particular Sub-words rather than M-words as a whole. There is a diacritical aspect to their occurrence that is not typically associated with agreement morphology.¹⁵⁹

An interesting consequence of the structural properties of CMs is that *at the level at which they are relevant*, CMs in Batsbi are not redundant at all. That is, a VI associated with a CM is associated with exactly one CM and only one CM. Batsbi has exuberant exponence from the perspective of the M-word, since there are multiple manifestations of the same morpheme expressing the same features with the same phonological content; but there is no multiple exponence in Batsbi at the *Sub-word* level, and CMs are a property of Sub-words. This could be a contributing factor to the relative stability of exuberant exponence in Batsbi.

¹⁵⁹ Note also that the single-segment form of CMs is not dissimilar to a single theme vowel. This may or may not be coincidental.

In a sense, the stability of exuberant exponence in Batsbi is as interesting a problem as its existence at all; one might have predicted that it would be eliminated or that it would be reanalysed. There are likely other contributing factors, one of which is potentially phonological. Each of the CMs consists of a single consonant. Deleting one of these consonants would alter the syllabic structure of the word, either by moving coda consonants into the onset of the next syllable or by eliminating the onset entirely.

Harris (2008) discusses the relatively low probability of a given language developing exuberant exponence in Batsbi fashion, owing to the complex nature of the historical changes giving rise to it. Although her theoretical orientation is very different from that taken in this dissertation, the historical scenario she envisions for Batsbi is not incompatible with the approach to affix-genesis taken here.

4.3.3 Sporadic Pleonasm

The term “sporadic pleonasm” is being used as a catch-all convenience rather than a technical term, hence the scare quotes. An informal definition is something like the following:

(4.206) *Sporadic pleonasm*: the appearance of seemingly redundant exponence with obscure motivation

Most examples of what I would consider sporadic pleonasm are not germane to the current discussion because they happen above the level of the M-word.¹⁶⁰ The development of concord in nominal phrases is an example. Assuming that most case markers were once adpositions, they originally were phrasal in their scope; one of the indications that they are no longer such is that they begin to appear on adjectival modifiers, where they are contributing no new information beyond their affiliation with a particular noun. Why speakers do this is obscure, though one plausible explanation is that they generalise from DPs like *the poor* where an adjective is functioning substantively. Another example is the development of definiteness concord in Swedish when a definite determiner is present. In Danish the noun is not marked in this context; one assumes that children learning Swedish at some stage acquired the rule that definite nouns must be marked as such, but it is not clear how this happened. Finally, something similar might be going on in those varieties of English that regularly produce *more better* – that is, the comparative suffix has been analysed as a type of concord rather than the exponent itself.

The development of various types of concord is interesting, but beyond the scope of this dissertation, since it relies on dependencies between M-words. The relevant question here is whether similar phenomena are possible within the M-word. Clear examples are hard to find, however. One possibility is the Greek incarnation of the PIE nasal-infix presents, discussed in 4.2.2. The nasal infix only appears with a suffix also containing a nasal, as though speakers knew that there was a nasal in the word somewhere, were not entirely certain where this was, and decided to hedge their bets. The lack of evidence for the history of this construction, however, limits its usefulness.

Harris and Halle (2005) have recently discussed an interesting development of sporadic pleonasm in Spanish pronominal clitics, which they treat as a kind of reduplication. The normative pattern is shown in (4.207).¹⁶¹

(4.207a) Vénda= lo.
sell.IMPV=3rd.sg.ACC.
'Sell (sg.) it.'

¹⁶⁰ Fortunately, bear-sharks are not subject to sporadic pleonasm.

¹⁶¹ All examples from Harris and Halle (2005).

- (4.212a) *Sírva-* n= les /los.
 serve.IMPV-PL=3rd.pl.DAT./3rd.pl.ACC.MASC.
 ‘Serve them.’
- (4.212b) * *Sírva(n)=len/lon.*
- (4.212c) * *Sírva=les/los.*

Which other clitics allow VIM depends on the speaker.¹⁶² The reflexive *se* is allowed to participate in VIM by all speakers who have the construction; the remaining clitics obey the hierarchy given in (4.213), where the grammaticality of VIM with one clitic implies the grammaticality of VIM with the clitics to the left of that clitic on this scale, but not the clitics to the right.

- (4.213) *se* > *me* > *le* > *lo, la*

Kopy and VIM can also occur with sequences of two clitics, as shown in (4.214). All of the variants listed are available to some speakers, but not all combinations are allowed by all speakers; the different dialects, attested and unattested, are discussed by Harris and Halle (2005) at some length.

- (4.214a) *Dé-* (n)=**me**n= lo.
 give.IMPV.-PL.=1st.sg.=3rd.sg.ACC.MASC.
 ‘Give it to me.’
- (4.214b) *Dé-(n)=me=lon.*
- (4.214c) *Dé-(n)=**me**n=lon.*

Harris and Halle (2005) propose a formal analysis of reduplication and metathesis by which readjustment rules call for the insertion of square brackets in an underlying phonological string, with the contents of the square brackets then reduplicated in the surface forms (4.215). Partial reduplication, in which some segments are omitted from the surface forms, is dealt with by the insertion of unpaired angle brackets within the square brackets; the segments between the square bracket and angle bracket will not be present in one of the two duplicates. This is depicted in (4.216); the first column shows a derivation in which the first copy is partial, and the second column a derivation where the second copy is partial. A careful combination of left and right angle brackets can derive surface metathesis as well as reduplication within the same formal notation (4.217).¹⁶³

- | | | |
|---|------------------------|-------------------------------------|
| (4.215a) <i>Underlying phonological sequence:</i> | | ABCDE |
| (4.215b) <i>Readjustment rule adds square brackets:</i> | | A[BCD]E |
| (4.215c) <i>Reduplication:</i> | | A-BCD-BCD-E |
| (4.216a) <i>Readjustment rule adds brackets:</i> | A[B>C]D | A[B<C]D |
| (4.216b) <i>Reduplication:</i> | A- BC -BC-D | A-BC- BC -D |
| (4.216c) <i>Surface forms:</i> | A-C-BC-D | A-BC-B-D |
| (4.217a) <i>Readjustment rule adds brackets:</i> | | A[B><C]D |
| (4.217b) <i>Metathesis:</i> | | A- BC - BC -D |
| (4.217c) <i>Surface forms:</i> | | A-C-B-D |

¹⁶² Harris and Halle (2005) suspect that the same holds true for Kopy dialects but cannot assert it with full confidence.

¹⁶³ This description has been truncated for the sake of brevity; for full derivational details, cf. the original paper.

(4.218) is the readjustment rule Harris and Halle (2005:203) propose for Kopy and VIM in Spanish. /*Cl/D* is their variable for the set of clitics allowed to participate in a given dialect. The angle-bracket formulae necessary to produce the surface forms are shown in (4.219), where *X* = *vénda* and *Y* is unspecified.

(4.218) In a string of the form *X/n/Ag_r=/Cl/DY*:
 insert: [to the immediate left of /*n/Ag_r*
] to the immediate right of /*Cl/D*

(4.219a) Kopy: *vénda[n<=lo]*
 vénda-n=lo-n=∅
 véndan=lon

(4.219b) VIM: *vénda[n><lo]*
 vénda-∅=lo-n=∅
 vénda-lon

Some details of the Harris and Halle model have not yet been worked out; in particular, it is not clear how their system can derive fixed segment reduplication of the type found in the perfect forms in various older Indo-European languages. Nevertheless, this analysis does give a decent working model for problems of this type, and works quite well for the Spanish data. It can also account for a superficially similar case involving Georgian indefinite pronouns, discussed by Haspelmath (1993:279–81). The Georgian indefinite pronoun, *rame* ‘anything’, is derived from the interrogative *ra* and an indefiniteness marker *me* – not an uncommon pedigree for this type of pronoun. The earliest Georgian forms show declension on the pronominal element, to the left of the particle; the modern forms all have the declension on the right edge, with a new invariant stem *rame*. In between these stages was an intermediate phase with the declension in *both* positions.

(4.220)	<i>Old Forms</i>	<i>Hybrid Forms</i>	<i>New Forms</i>
<i>Nom.</i>	ra-me	(“ra-me”)	ra-me
<i>Dat.</i>	ra-s-me	ra-s-me-s	ra-me-s
<i>Gen.</i>	r-is-me		ra-me-s
<i>Inst.</i>	r-iti-me		ra-me-ti
<i>Adv.</i>	ra-d-me	ra-d-me-d	ra-me-d

Despite the obvious structural differences between verbs and pronominal clitics on the one hand and indefinite pronouns on the other, the Georgian data is not dissimilar from the Spanish data Harris and Halle discuss. In both cases, we find both pleonastic forms, where morpheme boundaries are obeyed, and non-pleonastic forms with the inflection at the right edge – metathesis, in Harris and Halle’s view. At first approximation, the readjustment rule for the Georgian forms is:

(4.221) In a string of the form *ra-Case=meY*:
 insert: [to the immediate left of *Case*
] to the immediate right of *me*

Derivations of the dative and adverbial forms, as shown in (4.222)–(4.223), are precisely parallel to the Spanish forms in (4.208). The hybrid forms are derived just as the Spanish Kopy forms (4.222), and the new forms are equivalent to the VIM forms (4.223).

(4.222a) Dative: *ra[s<me]*
 ra-s=me-s=me
 ra-s=me-s

(4.222b) Adverbial: *ra[d<me]*
~~*ra-d=me-d=me*~~
ra-d=me-d

(4.223a) Dative: *ra[s><me]*
~~*ra-s=me-s=me*~~
ra-me-s

(4.223b) Adverbial: *ra[d><me]*
~~*ra-d=me-d=me*~~
ra-me-d

The “new” genitive and instrumental forms can be similarly derived, with the addition of a phonological rule deleting vowels after other vowels. An approximation of how such a derivation might proceed is shown in (4.224).

(4.224a) Genitive: *ra[is><me]*
~~*ra-is=me-is=me*~~
ra-me-is
~~*ra-me-s*~~

(4.224b) Instrumental: *ra[iti><me]*
~~*ra-iti=me-iti=me*~~
ra-me-iti
~~*ra-me-ti*~~

We have, therefore, the formal capacity to derive all of the Georgian forms, and all of the Spanish forms, from the same synchronic grammar; and the question is whether or not this is desirable, or whether it would be preferable to postulate multiple grammars. Is it preferable to build optionality into the grammar, or to allow competition between grammars? Since the chronology of the Georgian forms is clear, we know that the “hybrid” forms predate the “new” forms, and it would be easy to analyse the new forms as synchronically an atomic pronominal stem *rame-* with ordinary case endings attached. In Chapter Seven, we will see evidence suggesting that the “atomic *rame-*” analysis may be preferable, even though Harris and Halle’s model is capable of deriving the “new” forms; that said, the situation is highly complex, and variation between individuals cannot be excluded.

Exactly the same reasoning, in principle, could apply to the Spanish case. Suppose Harris and Halle are correct about the derivation of Kopy, but that VIM is the product of a separate grammar. This would imply that in some dialects of Spanish, there is an innovative grammar in which pronominal clitics are part of the same M-word as the verb (as Agr morphemes, presumably), but it is not clear whether this step is desirable.

What Harris and Halle do *not* discuss, and what is of primary concern here, is how innovators acquired the rules necessary to generate the Spanish and Georgian data, given that there was no evidence for the rules in the data available to them. Spanish speakers variably use Kopy, VIM, and standard forms in their speech, and all three series of pronouns are available in modern Georgian: why, if they were able to acquire the conservative forms, did the innovators fail to correct themselves? Where did they acquire the rule in the first place? If the MARC is correct, and speakers only decide on multiple exponence if they have to, why was it allowed here? How do we tell the difference between Harris and Halle-style metathesis and the output of a new grammar? Further investigation is needed into these outstanding issues.

To summarise, there appears to be some sort of correlation between apparently unmotivated pleonasm and the loss of the historically older exponent, where forms of all three types can co-exist synchronically. Apparent migration of affixes will be discussed in more detail in Chapter Seven; for the nonce it will suffice to say that the cause of this species of

doubling is not at all well understood, but it appears to be a real phenomenon both above and below the level of the M-word.

4.3.4 General Discussion

This survey of the genesis of multiple exponence reveals that multiple exponence does not arise in a vacuum; all of the observable phenomena we can see involve either different diachronic tiers of development *or* the collision of independently marked M-words into a single M-word. Even sporadic pleonasm involves the addition of a new marker to an extant marker. All of this can be accommodated naturally within the current framework; there is no particular support for a lexicalist approach. As this was expected, it is not in itself a very interesting result. The larger question remains: what can the appearance of multiple exponence in a language tell us about grammar?

The roots of the answer lie in another, simpler question: why, given the desire to avoid redundancy, do speakers decide that their language has multiple exponence in the first place?

Redetermination phenomena, like the Vicentino case, are the most illuminating in this regard. Multiple exponence can arise out of syntagmatic pressures. If an element or certain class of elements has become phonologically reduced and occurs in (what could be interpreted as) the right syntactic contexts, learners may be highly motivated to analyse that element as an affix. Learners may also decide that a certain member of a given class instantiates a general rule, and then apply that rule to the other members of the class by association. We saw this in Vicentino: the third persons had no overt person marking, and the pronominal clitics were in the right environment and of the right phonological shape to be appropriate person markers. The second singular was then swept along in this process because it was clearly a member of the same syntactic category, even though it could contribute only redundant features. From this, we can conclude that learners are biased against partial affixation patterns and tend to prefer a slightly redundant system generated by a completely regular morphosyntactic rule to a system of partial affixation.

The Vicentino case illustrates another point as well. Phonological analysis and morphosyntactic requirements are independent of each other, and can come into conflict. If the phonology is guiding learners towards an affixal analysis, and the morphosyntax suggests a clitic analysis, learners must choose which to follow. Vicentino learners chose to follow the phonology; this produced only a slight complication of the morphosyntax. Had the morphosyntactic consequences been more dire – outright ungrammaticality, violations of UG – the learners might have chosen otherwise.

Multiple exponence also indicates that phonological reduction is *not*, as is sometimes supposed, driven by a requirement to mark a certain category. It is hard to see how marking-driven phonological reduction would be motivated when the M-word to which the new affix belongs is already marked for that category that affix will instantiate. If it were the case that unmarked M-words regularly and simultaneously gained multiple affixes, this would not *prove* that morphological marking were driving phonological reduction, but it would be compatible with a universe in which that were the case. But this is not what we see: instead we find only examples where the multiple exponents arise via collision, or when there is a significant age difference between the multiple exponents – neither of which is compatible with a morphologically-driven scenario. This lends credence to the idea that phonological reduction is the key to affix-genesis and not the by-product of a need to instantiate categories. If there is a motivation for phonological reduction, it must be rooted in semantics. The effects on the syntax (and the morphology) are almost accidental.

There is another lesson to be learned from multiple exponence as well, but from its maintenance rather than its origins. Amharic has eliminated most of the traces of multiple exponence from its compound gerund, and may (though the evidence is unclear) be in the process of doing the same for the compound imperfect. Multiple exponence, then, is

potentially vulnerable over the course of time, because of the anti-redundancy bias. Cases of exuberant agreement like Batsbi indicate that this need not be the case – and this, again, is almost certainly due to the same tension between phonological requirements and morphosyntactic requirements. All the Batsbi class markers consist of individual consonantal segments. Thus, eliminating exuberant agreement in Batsbi would wreak havoc on the syllable structure of the verbal complex: an undesirable consequence, particularly when there is no apparent precedent for it. The same is not true in Amharic, where the potentially-offending affixes are both syllabic and (mostly) on the periphery.

4.4 Chapter Summary

One of the recurring themes in this dissertation is the problem of learnability. The most difficult problems in historical linguistics reduce to issues in learnability: why does linguistic change occur when and where it does, instead of at other times; how are innovations transmitted; why, when children are so good at acquiring their native language, do they still fail to correct errors. Both of the topics dealt with in this chapter have interesting implications for some of these problems.

Children may use post-syntactic rules to allow themselves to adjust the output of a novel syntactic analysis so as to conform, superficially, with the grammars produced around them. The cases of mesoclitisis discussed in 4.2 involve both syntactic change and post-syntactic rules, where the latter produce results mimicking the surface patterns produced by the earlier syntactic derivation (or, potentially, vice versa). This suggests an interesting hypothesis: that children develop post-syntactic rules when they have already committed themselves to a syntactic analysis that proves unable to capture all the surface patterns they later encounter. Under what circumstances children make this decision is unclear, but the matter is worthy of investigation. These issues will arise again in Chapter Six.

Clitics, particularly pronominal clitics, are not infrequently “trapped” between two M-words as the relationship between the M-words changes and one of the two becomes subordinate to the other. Children are able to acquire innovative affixes even when we might predict that these trapped clitics would preclude such an analysis. They are also able to generate surface infixes; this remains an unsolved problem because we have no evidence of this happening in non-reconstructed languages.

Some types of multiple exponence, like the redetermination in Vicentino, also involve clitics. These cases suggest that speakers are guided by the phonology in taking clitics to be affixes, rather than a need to mark a word with a particular category. In fact, the very existence of multiple exponence, particularly the fact that it doesn’t usually develop via sporadic doubling, is enough to challenge the view that phonological reduction is driven by the desire for speakers to mark a certain word class with particular features.

In the previous chapter, affix-genesis was analysed at its most straightforward and idealised: new affixes are placed on the periphery, and in such a way that a one-to-one relationship between form and meaning is maintained. But both non-peripheral affixes and multiple exponence exist. The goal of this chapter was to demonstrate that these phenomena are not as mysterious as they might appear, and are not ruled out by the framework developed in the previous chapter.

Multiple exponence does exist. There are at least three different types of multiple exponence, each arising differently, but the case that would be problematic for us – a single M-word developing directly into multiple redundant affixes – does not appear to exist.

Affixes do not always appear on the periphery of the M-words that subsume them, and clitics can and do interpolate into an M-word. The latter is not such a problem because of our view of the piece-based nature of syntax. As to the former, affixes typically start off peripheral and then move; there is almost no information available as to how this happens, but it appears to be the result of phonological processes.

Infixation is not the only potential fate of a mobile affix. Sometimes affixes can move to different positions within an M-word; this will be discussed in more detail in Chapter Seven. The most striking case of affix movement, however, occurs when affixes exit an M-word entirely. This phenomenon, which we might flippantly call *affix-exodus*, is the topic of the following chapter.

Chapter Five

De-affixation (or Affix-exodus)

5.1 Overview

As discussed in Chapter Two, many grammaticalization specialists insist that grammaticalization is unidirectional, in spite of numerous counterexamples; many of them even structure their theories so that de-affixation is completely impossible. This is somewhat understandable, given the theoretical assumptions these specialists tend to hold: if affix-genesis is considered a “natural process”, de-affixation, or (as we might also call it) affix-exodus,¹⁶⁴ is somehow exceptional, outside the bounds of nature. One way to account for its exceptionality is to label it a by-product of other types of linguistic change: if the grammar has undergone radical changes, perhaps the usual rules may be temporarily suspended.

The synchronic theory of morphosyntax assumed here, however, has no such restrictions and does not automatically rule out affix-exodus. Morphological words are themselves complex syntactic structures, and they are built up via a series of syntactic derivations. Therefore, in principle, the theory could allow for the derivation to halt at any stage of the process. In diachronic terms, rather than one generation adding a new step to the derivation, as in affix-genesis, in the case of affix-exodus speakers are reducing the number of steps in a syntactic derivation to exclude nodes that were previously included. However, this is not likely to happen sporadically, and the result might even be unpronounceable. Halting a derivation prematurely requires a great deal of motivation. Thus, the theory can account for affix-exodus as readily as affix-genesis; nevertheless, it neither predicts nor implies that affix-exodus should happen as frequently as affix-genesis.

Other consequences follow if the notion that affix-genesis is somehow a privileged, speaker-independent process is set aside. I argued in Chapter Two that grammaticalization was essentially a by-product of various semantic, phonological, and ultimately syntactic changes, rather than a type of change of its own. This means that affix-genesis is, in fact, itself a by-product of changes elsewhere in the grammar, and, therefore, that there would be nothing remarkable about affix-exodus resulting from other linguistic changes either. In fact, we will see that while affix-exodus *sometimes* seems to have arisen in the context of other changes, sometimes it seems to happen more or less spontaneously, with no obvious motivations. While the existence of affix-exodus generally supports the view that linguistic change is in no way deterministic, this particular property of it – that it sometimes appears in contexts where no obvious grammatical changes have occurred – makes the case for the non-deterministic nature of linguistic change particularly compelling.

What *is* a necessary precondition for both affix-genesis and affix-exodus is structural ambiguity from the perspective of the learner (the ambiguity may not be obvious to the adult speaker). The data must support both an analysis in which Morpheme X is an affix and an analysis in which it is not. Learners seem to be biased in favour of the former, but this apparent bias must be overridden in cases of affix-exodus, and why this should be the case will be the focus of this section of my dissertation.¹⁶⁵

¹⁶⁴ I introduced the term *affix-genesis* because the more usual *affixation* and *grammaticalization* are both too polysemous for clarity. Since *de-affixation* is unambiguous, no other terminology is necessary; but this results in an unpleasant lack of parallelism when one speaks of *affix-genesis* and *de-affixation*, which to my ear is utterly abhorrent, as it makes the two sound completely unrelated. Because I care far too much about these things, when I am contrasting the two phenomena, I reserve the right to use *affix-exodus* in lieu of *de-affixation*, as it is nicely parallel to *affix-genesis*.

¹⁶⁵ Kiparsky (2005b) has also attempted to account for affix-genesis and affix-exodus within the same morphological system, but with a very different theoretical framework; cf. Chapter Eight.

Most previous discussions of instances of affix-exodus have neglected to consider the grammatical context in which these changes were situated, or to examine fully what feature or features in the grammar allowed an unusual kind of change to occur. In 5.2, I discuss six case studies with as much attention to structural details as allowed by the available data; in some instances, unfortunately, the structural details are obscure.

In Chapter Three, I developed a structural typology of types of affix-genesis, repeated here as (5.1). One of the obvious questions that arises with regard to affix-exodus is whether a similar typology can be formulated on the basis of known examples.

- (5.1a) *Type I*: Acquisition of an additional movement operation;
- (5.1b) *Type II*: Reanalysis of a terminal as an exponent of a different structural position;
- (5.1c) *Type III*: Extension of an M-word boundary to include a former clitic;
- (5.1d) *Type IV*: Reanalysis of a compounded element as an affix.

Since affix-genesis and affix-exodus are mirror images of each other, the logical place to start is simply by reversing the components of (5.1). This produces something like the typology in (5.2); note that (5.1b) does not need to be revised, which will become important in the course of the chapter.

- (5.2a) *Type A*: Loss of a movement operation;
- (5.2b) *Type B*: Reanalysis of a terminal as an exponent of a different structural position;
- (5.2c) *Type C*: Retraction of an M-word boundary to exclude a peripheral affix;
- (5.2d) *Type D*: Reanalysis of an affix or compound element as an adverbial.

It is intuitively clear what examples of Types A–D would look like. Suppose that P-speakers in Language Q have verb-raising to T, so that T is part of the verbal M-word, but that this rule is lost by innovators, so that T in the innovative grammar is a freestanding exponent of T: this would be an example of Type A. Furthermore, Type B, which is minimally different from Type II affix-genesis, would be exemplified by a case affix in Language R developing into an independent adposition. But while Types A and B are both conceptually and structurally clear (regardless of whether they are attested), Types C and D are markedly less so, in part because it is difficult to think of an illustration of one of them that could not be better described as an example of either Type A or Type B.

Moreover, the actual attested examples do not fit neatly into categories like those in (5.2). As we will see, the case studies discussed in this chapter have a miscellaneous feel to them. Type B is clearly attested, but some examples that might be examples of the other logical categories could also be taken as examples of Type B. Type A, despite being intuitively clear, is not obviously attested. Although it is clear that all of the relevant action occurs on the periphery of an M-word, this does not take us very far in delineating the logical possibilities. 5.3 discusses this problem in the context of the typology from Chapter Three and demonstrates that it fails to provide precise structural categories.

Previous discussions have also failed to take advantage of a valuable opportunity to consider deeper questions about the learnability of grammatical structures and the factors which drive morphosyntactic change; this is the subject of 5.4.

5.2 Case Studies

In this section, I briefly discuss several examples of affix-exodus, and demonstrate that changes in other areas of the language are implicated in only some of the examples.

Since affix-exodus is not necessarily a by-product of other linguistic changes, it must be acknowledged that there is nothing “unnatural” about this type of change. The order in which the case studies are presented has been carefully chosen to highlight this.

The first three case studies *did* all arise in the context of other linguistic changes; these are Estonian C-particles (5.2.1), which developed as a result of phonological changes; the abessive postposition in Northern Saami (5.2.2), originally a case suffix, which was also affected by phonological changes; and the independent Irish first plural pronoun *muid* (5.2.3), originally a verbal ending, which first appeared during a time of massive changes to the verbal and pronominal systems in Middle Irish. Traditionally, the fourth case study – the group genitive in Germanic languages (5.2.4) – is believed to have arisen as the nominal case system was breaking down; however, as we will see, the matter is less certain than often believed. This prepares the ground for the fifth case study, concerning the new English adverb *ish*, which is still transparently related to a fully-productive adjectival and nominal suffix (5.2.5) and which cannot be connected to any other linguistic changes whatsoever. Last comes the mysterious recent development of a quantifier from a former suffix in Frisian, Dutch, and German (5.2.6), the sketchiest of the case studies from this chapter. This last case is discussed in less detail than the others, because few details are known; however, it is worth including because, like English *ish*, there is no obvious connection between it and other linguistic changes in these languages.

5.2.1 Estonian

The Estonian particles *es* and *ep* have long been cited as one of the core cases of counter-directional grammaticalization. According to the standard analysis, *es*, an archaic question particle, and *ep*, an emphatic/affirmative particle, were originally clitics, but have since become independent words located in C. However, of the case studies discussed in this chapter, this is easily the most speculative. Most previous work on these particles, e.g. Campbell (1991) and Nevis (1986a), were rooted in traditional etymologies (Ariste 1973, Alvre 1976). This analysis is based on comparative evidence, but it is only as strong as its etymology, and the etymology is not without contention: recently, Metslang, Pajusalu, and Habicht (2008) have put forth an alternative proposal for the origin of these clitics; unfortunately, their work appears to be available only in Estonian, and I am unable to evaluate it.¹⁶⁶

Despite this controversy, I am choosing to retain this example, with the appropriate caveats. Even were the etymology entirely secure, the Estonian case is less than ideal because it involves clitics rather than bona fide affixes, and because it is not clear what happened structurally, since the changes occurred in the remote past. My reason for including it is simply to demonstrate how language-learners might be influenced by phonology when making their decisions about the location of M-word boundaries, and as an illustration of this kind, the Estonian particles still suffice. Since this is essentially a glorified “toy” example, however, I will not speculate as to the nature of the structural changes involved. As the most basic references on both particles are in Finnish,¹⁶⁷ the description of the data relies on the summaries of Campbell (1991) and Norde (2009).

Traditional philological work traces the Estonian affirmative particle *ep* back to the Balto-Finnic emphatic clitic **-pa*. Although Estonian underwent a process of final vowel apocope, the presence of this clitic was sufficient to bleed the process:

¹⁶⁶ The nature of etymologies of this sort makes it unlikely that there will ever be a truly definitive answer in favour of Metslang et al.’s analysis over the traditional, or vice versa. Metslang et al.’s motives are also unclear to me; if they are of the Haspelmath school of thought, they may well have chosen to formulate an alternative analysis in order to remove a counterexample from the list of “counterdirectional” changes.

¹⁶⁷ There appears to be something of a theme with this particular case study.

- (5.3a) *päällä > *pääll
 ‘on top of’
- (5.3b) *päällä-pä > *päällä-p

Once the process of apocope had ceased to be a surface phonetic rule, and the final vowels were absent from the underlying form, the vowel that had been “protected” by a following clitic was reinterpreted as belonging to the clitic. Various other sound changes occurred, including the loss of vowel harmony, ultimately yielding the form *peall-ep*. Since vowel harmony had been lost, there was no further evidence for *ep* as dependent on the host form, and it was cut loose. Ariste (1973:37) notes:

After this suffix was lexicalised, the word *ep* could change its syntactic position and precede the affirmed words: *see ep* → *ep see*.¹⁶⁸

By the standard account, the history of the interrogative particle *es* is similar but more convoluted. It was not originally an interrogative clitic; it can be traced back to the pan-Balto-Finnic clitic *-s*, said to be a marker of “informal speech”, added to the original Balto-Finnic interrogative clitic **-ko*. The vowel of this suffix was variably lost in some of the Balto-Finnic languages (cf. colloquial Finnish *-ks*, Campbell 1991:291-2), and the suffix itself had become optional in Estonian, so that **-s* had become one of the possible interrogatives. **-s* became *es* in the same way that **-pä* became *ep*, and was then relocated to the C-domain, as in the following examples from Older Estonian.¹⁶⁹

- (5.4a) Nüüd es tee uSSute.
 now Q 2nd.pl. believe.2nd.pl.
 ‘Now do you believe?’
- (5.4b) MiSt es minna Seddä peä tundma.
 it.from Q 1st.sg.NOM. it should know.INF
 ‘How should I know that?’

Even setting aside structural considerations, there are several important pieces to this picture. The first is to note that, in this case, one of the essential ingredients was a change in the location of a Sub-word boundary. P-speakers segmented the sound sequence *peallep* as *pealle-p*, innovators as *peall-ep*, and this decision figured crucially in their subsequent analyses. Knowing that *p* was not a legitimate phonological word in their language, P-speakers could not treat *peallep* as two separate phonological words once they had arrived at the segmentation *pealle-p*. But innovators could: for them, the emphatic Vocabulary Item was an entire syllable, *ep*, which met the qualifications for distinct phonological word.

Note, in addition, that previous phonological changes were essential in allowing this segmentation as a possibility. Estonian had lost final vowels. Prior to apocope, a child learning Estonian knew to segment **päälläpa* as **päälla-pa* because s/he would have learned that **päällä* was a sequence occurring elsewhere with the same semantics as the form **päälläpa*, minus the emphatic component. This would guide him to make the same choice his parents had made. Moreover, even when the surface form had become **pääll*, there was likely to be a period in which the final vowel was still present at some stage of the phonological derivation, and this, too, would enable a child to match the Sub-word segmentation of the earlier grammar, **päälla-p(a)*.¹⁷⁰ However, when apocope was well and

¹⁶⁸ Translation of Campbell (1991:291).

¹⁶⁹ Examples from Alvre (1976:345). Glosses and translations are those of Campbell (1991:290).

¹⁷⁰ Don Ringe (p.c.) has pointed out that the final vowel of the clitic may have been lost in the underlying forms while the final vowel of **päälla* was still underlyingly present, since the former would never have been protected by a following clitic.

truly final, and final vowels had vanished from even the underlying forms of the grammar, this was no longer possible, because the non-emphatic form had now become **pääll*.

It is possible that the final stage of loss of underlying final vowels and the resegmentation occurred in a single generation. This is only one possibility, however; another would be that speakers took the *e* in *peallep* as a connecting vowel between a consonant-final host and consonantal clitic. Another phonological change becomes relevant at this juncture: the loss of vowel harmony. As long as Estonian retained vowel harmony, speakers would be more likely to take the vowel as part of the host, since there was indication for such a segmentation. Loss of vowel harmony meant a loss of another analytical cue, and a more uncertain status for the ambiguous vowel.

And so we return to the innovators, whose analytic decisions did not end with their choice of segmentation. The segmentation itself could easily have had no syntactic consequences whatsoever; in Chapter Six we will see a number of such cases. One could envision a scenario in which both segmentations of *peallep* are current in the same community, even by members of the same generation and family, and quite possibly this variation would escape notice. Ultimately, however, there *were* syntactic consequences, because the particles gained independence and relocated. Even though we are not concerned with structure in this sub-section, there is still much to be said about this change.

A speaker who segments *peallep* as *pealle-p* has little choice about where the clitic will appear, because the clitic is not capable of phonological independence. But for a speaker who segments *peall-ep*, the distribution of the M-word *ep* is no longer restricted for phonological reasons. Here another analytical choice must be made. If the speaker's input data is primarily given him by P-speakers, never will *-p/ep* occur other than adjacent to a host. The learner could very well mark this required adjacency as crucial, and no syntactic innovation would occur. Yet another conclusion is certainly possible. The difference between emphatic and affirmative is rather slight; the English *I **did** return the DVD last night* has both emphatic and affirmative connotations. Therefore, rather than taking *ep* to be a word-level emphatic suffix, a learner might conclude that *ep* has a more general usage and can apply to entire sentences, and fix it in the C-domain accordingly. After one such innovation, any children learning the language who encounter this speaker are likely to be biased in favour of acquiring the innovation themselves, because the status of *ep* is already ambiguous in the speech of more conservative speakers.

One interesting question to ask at this junction is how many stages are necessary in changes of this kind. A scenario in which speakers with the innovative segmentation nevertheless persist in analysing *ep* as having obligatory narrow scope for several generations before someone acquires still-more-innovative wide-scope *ep* is plausible, but there is nothing in principle keeping innovators from leaping to the second innovation once they have acquired the first. Put another way, the three stages listed in (5.5) are obligatory, as is their ordering, but there is nothing at all preventing (5.5b) and (5.5c) from occurring in the same generation.

- (5.5a) *pealle-p*
- (5.5b) *peall-ep*, narrow scope only
- (5.5c) *peall ep*, wide scope possible

Here we again encounter the black box of actuation. When multiple analyses are equally plausible, on some level, why do speakers choose the analysis they choose? We can identify the factors which must have been in place before a given change could occur, but even once these factors are present, there is often nothing telling us why one particular innovation occurred rather than another, or – as in this case – why the conservative analysis was not simply retained if it remained a viable possibility. Even worse, we cannot tell why an innovation occurred when it did. If all of the factors which led generation *Q* to analysis *X*, why did generation *H*, who had the same data available to them, not arrive at *X* themselves?

We confront these questions whenever we tackle historical problems, but they are particularly cogent in the case studies discussed in this chapter, because by their very nature they call these questions into stark relief. We can see the factors that led language learners to make the decisions they made, but we can point to many other cases, apparently equivalent in all ways we can see, where nothing of the sort happened.

One recurring theme, however, will be phonological cues, of the type seen here. When the phonology changes, data that biased learners in favour of one analysis suddenly becomes ambiguous, or even points to another analysis. As we will see later in this section, syntactic changes can have a similar effect.

5.2.2 Northern Saami

The abessive case is typically marked by a case suffix in Finno-Ugric languages, but not universally. The most striking exception is Northern Saami, which has a full-fledged postposition *haga*. Given the common bias towards unidirectionality, one might expect that the Northern Saami situation would be taken to reflect the original, but in fact even hard-core unidirectionality theorists accept that it is Northern Saami which has innovated. As discussed in Nevis (1986a), the comparative evidence indicates that the suffix must have been the original. The example is not without problems, but is nonetheless worthy of discussion. Once again, phonology emerges as an important factor in innovation, this time augmented by something akin to the Principle of Contrast (Clark 1987).

The Finno-Permic branch of Finno-Ugric appears to have built an abessive based on a caritive suffix **-pta* plus a lative **-k*; the cognates, as listed by Nevis, are shown in (5.6).

(5.6)	Southern Saami	-pta, -t'ta
	Northern Saami	-t'ta, -taga [CEAD: -httá, haga ¹⁷¹]
	Finnish	-tta', -ttä'
	Mordva	Ø
	Mari	-tè, -ǎè
	Udmurt	-tek
	Komi	-teg

The caritive element **-pta* also turns up in an adjectival suffix: in Finno-Permic languages it combines with a suffix *-ma* (e.g. Northern Saami *-tabme*, Finnish *-ton*, *-toma*) and in Ugric languages with a suffix *-l* (e.g. Hungarian *-talan*). In addition, most Finno-Permic languages have relic verbal abessive forms consisting of the nominal abessive suffix attached to the verbal base, counter to the usual rules of these languages and therefore likely to be very old.

There is also internal evidence for a suffix *-httá* in Northern Saami itself, also shown by (5.6). Jussi Ylikoski (p.c.) informs me that this suffix is obsolete in all major present-day varieties of Northern Saami with which he is familiar.

For example, you can find some wordlists of a Saami language technology project at <http://giellatekno.uit.no/lex.en.html>, "based upon a corpus of 4116196 wordforms" of the contemporary literary language. In the a tergo list of <http://giellatekno.uit.no/words/lists/sme/wf-atergo.html>, there is not a single abessive-like wordform in *-httá*, even though there are some non-standard "compounded" *haga-PP:s* [sic].

Jussi Ylikoski, p.c.

¹⁷¹ Nevis (1986a) and the works that follow him spell the postposition *taga*. According to Norde (2009), this is an archaic spelling used in the eastern dialects; the western dialects, on which the literary language was based, underwent a sound change of *ht* > *h*. Elsewhere in this section, I am following Norde.

One form bearing the abessive suffix, *gahperahtá* ‘without a hat/cap’, remains current in modern Northern Saami, but has the status of a lexicalised adverb; it is listed in this form in Saami dictionaries, and same meaning can be formed productively with *haga*: *gahpira haga*. This, too, suggests that it is correct to treat Northern Saami as innovative rather than conservative. I will return to *-htá* below.

Norde (2009:208) notes that in modern Northern Saami, *haga* is almost always written orthographically as a separate word, apart from some frequent collocations like *lobihaga* ‘without permission’; the latter is considered non-standard, as Ylikoski mentions. She also says that this convention for writing *haga* independently has been employed since the nineteenth century, which, as Northern Saami has been written for several centuries, implies that it was not written as an independent word prior to the nineteenth century.

Syntactically, *haga* patterns with postpositions rather than with case suffixes. It can be stressed, and it governs the genitive case. The abessive also allows conjunction reduction (5.7a), which other case suffixes, like the comitative in (5.7b) do not.¹⁷²

- (5.7a) áhči ja Issáh-a haga
 father.GEN.SG. CONJ GEN.SG. without
 ‘without father and (without) Issat’
- (5.7b) áhči- in ja Issáhi-in
 father-COMIT CONJ COMIT
 ‘with father and Issat’
- (5.7c) *áhči ja Issáhi-in
 father.GEN.SG. CONJ COMIT

Case suffixes precede possessive enclitics, whereas *haga* follows them.

- (5.8a) bárdná-n haga
 son- 1st.sg.POSS. without
 ‘without my son’
- (5.8b) áhku- i- dasa-n
 grandmother-PL-ILL- 1st.sg.POSS
 ‘to my grandmothers’

Haga can also occur independently as an adverbial.

- (5.9) mun báhcen haga.
 1st.sg. remain.PRET.1st.sg. without
 ‘I was left without.’

Haga can even be used as a preposition in some dialects, probably because of influence from prepositional Norwegian. This use is condemned by contemporary grammarians, but still possible.¹⁷³

- (5.10) haga skuova-id
 without shoe- PL.GEN/ACC.
 ‘without shoes’

¹⁷² The examples in this sub-section are taken from Norde (2009) rather than from Nevis (1986a), as evidently there are some problems with Nevis’s examples. Except where otherwise noted, Norde’s examples were provided by Ante Aikio (p.c. to Norde), a native speaker of Northern Saami.

¹⁷³ Example from Jussi Ylikoski (p.c. to Norde).

All the available syntactic evidence suggests that in modern Northern Saami, *haga* is indeed a postposition, rather than a case suffix. The discussions in Nevis (1986a), Kiparsky (2011), and (to a lesser extent) Norde (2009) do not provide a clear picture for how this situation came about, in large part because of the confusion surrounding the suffixal variant *-httá* and its role in this drama. Kiparsky, for instance, sets up the problem in straightforward prosodic terms: *-haga* is a stress foot of its own, unlike most other case suffixes, which disrupts the typical Saami stress pattern. Therefore, it was detached (in his terms, first as a clitic) by analogy with the new plural comitative plural clitic *-guin*, which was also disyllabic.¹⁷⁴

Yet even a casual glance at this data reveals that something much more complex is afoot. What of the suffixal variant *-httá*, which is monosyllabic? The reader will likely have observed that there are rather significant differences in phonological shape between *haga* and *-httá*. If *-httá* is the only attested abessive suffix in Northern Saami, what is its relationship to *haga*? Kiparsky does not mention *-httá* at all, and Norde glosses over it; yet its existence is crucial to a complete analysis of the scenario.

Nevis (1986a) does discuss *-httá*, but (cf. footnote 172) he is not a very reliable source, and his discussion is confusing, as he apparently believes that the suffixal variant is still productive in the modern language. His contention is that *-httá* was in complementary distribution with *haga*: *-httá* was used for trisyllabic nouns while *haga* appeared on even-syllabled nouns. If true, this would be potentially problematic, as phonological properties of Roots should not affect the syntax; but Nevis's conclusion appears to be based on the discussion in Collinder (1957), and his interpretation of Collinder is not the only possible one.

Collinder (1957:190) does discuss a relationship between phonology and choice of abessive, but crucially he does *not* suggest a complementary distribution. He gives *haga* as the default abessive, with one phonological consequence for trisyllabic nouns. When the noun has an even number of syllables, *haga* is used with the genitive, without alteration. But when the noun is trisyllabic, the stem-final vowel is deleted, as in (5.11b). The alternative to (5.11b) is to use *-httá* with the ordinary genitive/bare stem form: *gabmasa-httá*.

- (5.11a) *Disyllabic*: dola haga
 (5.11b) *Trisyllabic*: gabmas haga (cf. genitive *gabmasa*)

This pattern suggests that Kiparsky's connection with prosody is correct, if incomplete; and indeed, Northern Saami stress, as described in Collinder (1957:187), strongly implies that stress patterns are responsible for both the phonological divergence of *haga* and *-httá* and the exodus of the latter. In Northern Saami, primary stress in non-compounds falls on the first syllable, with secondary stress on the third syllable in four-syllable words.¹⁷⁵ As a result, the first syllable of the suffix *haga* would be stressed when added to a disyllabic or tetrasyllabic nominal stem, but unstressed when added to a trisyllabic stem:

- (5.12) 'do.la.ᵛha.ga vs. 'gab.ma.ᵛsa.ha.ga

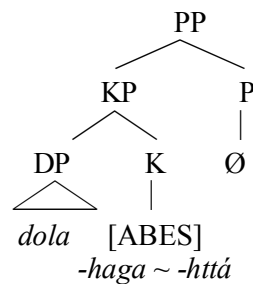
It is entirely reasonable to suppose that the stressed and unstressed variants would come to have different surface phonological shapes – and this gives us a plausible source for the innovation of postpositional *haga*, and the remnant pattern discussed by Collinder and Nevis. At first, the phonological differences were superficial, but at some point innovators must have come to the conclusion that *haga* and *-httá* were two different Vocabulary Items

¹⁷⁴ Kiparsky's analysis will be discussed below in greater detail.

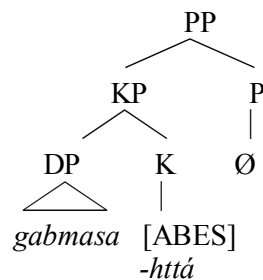
¹⁷⁵ Collinder (1957:187) says, '[W]ords of more than four syllables may have more than one secondary stress,' but does not specify where such stresses would fall. One assumes that the pattern of stressing odd-numbered syllables continues.

with different syntactic properties. Thus, the P-speaker analysis in (5.13) came to contrast with the innovators' analysis in (5.14).

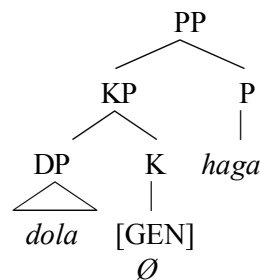
(5.13)



(5.14a)



(5.14b)



This is almost exactly the reverse of the adpositional case studies discussed in Chapter Three; in fact, as we shall see, this case is more parallel to affix-genesis phenomena than is typical for affix-exodus. Instead of analysing *haga* in the lower structural position, the innovators analysed it as the head of a higher projection. In principle, nothing ought to prevent them from doing this. In the case of affix-genesis, language-learners were confronted with a choice of possible positions for a Vocabulary Item *-X*, and could have selected either a higher projection or a lower one. The same choice confronts learners in this case – arguably, the same choice confronts learners *whenever* they acquire a preposition or case marker – but the other alternative was chosen. A child learning its native language has no preconceptions as to whether one analysis is conservative or not, which means that acquiring a case suffix rather than a postposition, or vice versa, is a decision about the data, not about etymology. The question that must be addressed is not ‘why can the child choose either option’ but ‘what factors govern the child’s choice, and why?’ In this instance, as we have already seen, there were phonological motivations for the choice. Due to the effects of stress patterns, the surface forms of *haga* and *-httá* became quite different, different enough that speakers concluded they were two separate items.

Moreover, the divergence in the surface shape of *haga* is not the only relevant phonological fact here. Another important factor is that the genitive/accusative form in

Northern Saami has no overt representation, as indicated by the trees: it is identical to the bare stem, modulo adjustments for the number of syllables.¹⁷⁶ For this reason, speakers would not realise they were producing an innovation by placing an M-word boundary before *haga*, because they would recognise the genitive/accusative form as a legitimate M-word in its own right.

As mentioned above, Kiparsky (2011) has argued that prosodically-motivated analogy was primarily responsible for the exodus of *haga*. Northern Saami has recently developed a new comitative plural case marker, *-guin*, from *guoibmi* ‘fellows’.¹⁷⁷ This form has become increasingly suffix-like in its syntactic behaviour (e.g. by disallowing conjunction reduction), but until recently was essentially a cliticised postposition. According to Kiparsky, since the comitative case is the antonym of the abessive, and since both *-guin* and *haga* are disyllabic (unlike most other Saami case markers), *haga* too became a clitic.

Norde (2009:209) has raised some objections to this argument, on the grounds that, first, *-guin* is strictly plural (the comitative singular is *-in(a)*), while the abessive is not; and second, that *-guin* was unlikely to inspire a new postposition when it was itself becoming a case suffix. Her first objection may or may not be relevant – *haga* can be used with singulars and plurals – but her second objection deserves some attention even though it is spurious given the assumptions about diachrony made in this dissertation. Children have no access to etymological information about their language; confronted with *haga* and *guin*, they have no way of knowing whether one is becoming a suffix. They have the same analytical choice to make about both forms. In this sense, Norde’s objection is trivial; the chronology could well have been such that *-guin* was clearly a clitic postposition when innovators made their innovative decision about the status of *haga*. Nevertheless, it *is* worth considering whether Kiparsky’s account relies on the same assumptions; that is, can Kiparsky’s analogy hold if language learners did not know that *-guin* was destined for affixhood?

As far as I can tell, Kiparsky’s proposed analogy *is* possible, but can be at most a corroborating factor. His account is seriously weakened by his failure to observe the important role of *-httá*. If *-guin* was a clitic postposition, and language-learners were leaning towards analysing *-httá* and *-haga* as separate VIs, then the existence of an antonym prosodically similar to one of the two variants may very well have assisted them in making the decision in favour of a split. Once *haga* was seen as a separate postposition, it would have been treated thus syntactically, biasing the data of the following generation in favour of maintaining it. The continued vagaries of *-guin* need not have necessarily affected it. But Norde *does* have a valid point: if the connection between *haga* and *-guin* was perceived as being so strong, they might have been expected to continue influencing each other rather than appear to salute each other while passing in the dark (so to speak). Moreover, it seems implausible that the proposed analogy was the most important factor in the children’s decision, since they already had considerable motivation to take *haga* as an independent M-word, for the reasons outlined above. *Guin* may have had a minor role in biasing the children’s decision, but is unlikely to have been a significant factor.

What *is* likely to have been a significant factor is something akin to Clark’s Principle of Contrast: ‘Every two forms contrast in meaning’. As discussed in Chapter Three, Clark (1987) has argued that children favour a one-to-one mapping between meaning and semantics, and refrain from positing redundancy unless they are forced to. Thus, when they encounter two different phonological shapes, they assume that the two are semantically different in some way. Here the issue is not semantic, but formal, but it is similar to the Principle of Contrast even though its effects are different. Children must have criteria for deciding whether two objects are the same or not, and phonological shape is one of the best indications available to them. It is clear that children are not slaves to phonology, since they

¹⁷⁶ Northern Saami has a complex system of consonantal ablaut.

¹⁷⁷ The spellings Kiparsky employs here are *-guim* and *guoibme*; the spellings used in the text are the standard Northern Saami spellings, according to Jussi Ylikoski (p.c. to Norde).

are perfectly capable of acquiring often very indirect connections, but there must be some threshold after which the link between two etymologically connected allomorphs is broken and speakers of a language conclude that they are different. Unfortunately, this threshold is a black box: it may be subject to individual variation; it is not clear how to measure it empirically; and we can only really notice that it has been reached after the fact.

In this particular case, *-httá* and *haga* differ in the number of syllables and the identity of the medial consonant, and apparently this was sufficient. Was this necessarily the case? It is impossible to say for certain. This is hardly a satisfying answer, but it is the only answer available at the moment.

One final remark is apropos here. Given a choice between the suffixed and postpositional abessive cases, why would language learners opt so strongly for the latter? As I will discuss in greater length in Chapter Eight, this is a serious problem for Kiparsky (2011), who argues that a single-word construction is always chosen over a two-word construction. Without a searchable corpus, the exact nature of the competition between *haga* and *-httá* cannot be known, but the numbers may well have been against *-httá* from the start. Often when two items are in competition, the “loser” becomes restricted to certain contexts; in this case, *-httá* was *already* restricted to certain contexts, while *haga*, as an adposition, was more versatile.

In the Northern Saami case study, we once again see phonology playing an important role, but not simply in the form of regular sound change, unlike the Estonian example. Here, the crucial factor was the divergent surface forms of a single underlying Vocabulary Item as a result of stress patterns. When language learners were no longer able to recognise *haga* and *-httá* as the same VI, they concluded that the former was an adposition; this analytical decision was corroborated by the fact that the genitive/accusative in Northern Saami is identical to the bare stem, and possibly also by the then-existence of the comitative plural clitic adposition *-guin*.

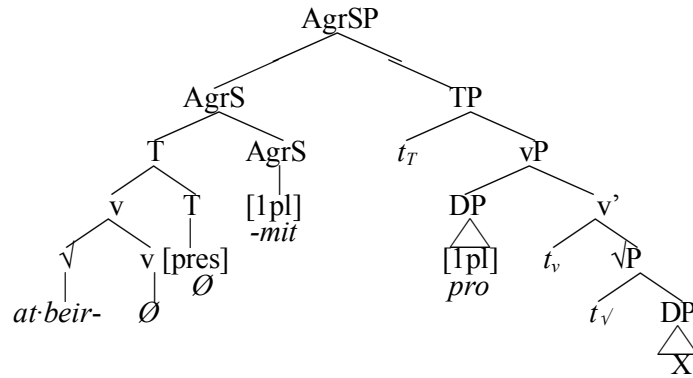
5.2.3 Irish

The emergence of a first plural pronoun *muid* in Modern Irish is both one of the best-known and most widely admitted examples of affix-exodus, because there is no way of explaining it as anything besides affix-exodus. The only possible source for *muid* is the first plural verbal ending, itself relatively new in the history of Irish. While the development of a pronoun from a verbal ending sounds quite exotic at first hearing, in terms of the actual grammar of Irish, the change is well-motivated. First of all, Irish has been VSO for its entire recorded history, so that the reanalysis did not involve a change in linear word order the way it would have had the language been SVO or SOV. Second, the verbal and pronominal systems of Irish have both changed considerably within the historical period, and these changes provided a natural context for the emergence of *muid*. Specifically, the grammar of Irish changed from something like (5.16) to (5.17).¹⁷⁸

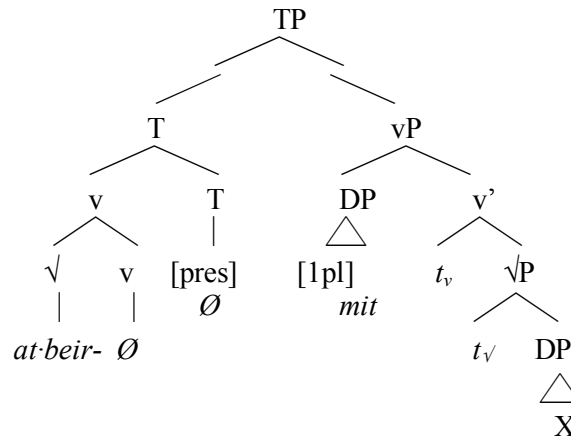
- (5.15) At·beir -mit.
 say.PRES-1st.pl
 ‘We say.’

¹⁷⁸ To simplify the discussion, I have used Middle Irish forms in (5.15)–(5.17).

(5.16)



(5.17)



Before examining these changes in detail, however, it makes sense to look first at the end points; this will make the trajectory maximally clear. The grammars of Old Irish and Modern Irish will be outlined in 5.2.3.1 and 5.2.3.2, respectively; and the Middle Irish developments will be discussed in 5.2.3.3.

5.2.3.1 Old Irish

Like most older Indo-European languages, Old Irish had rich subject agreement marking and *pro*-drop; thus the structure of a sentence like (5.18) is as in (5.16) above. Third person verbs showed agreement with lexical subjects, as shown in (5.19).¹⁷⁹

(5.18) Giul- ait.
stick.fast-FUT.3rd.pl
'They stick fast.'

(5.19a) Béoig-idir in spirut in corp in fecht so.
vivify-PRES.IND.DEP.3rd.sg. DEF.NOM spirit.NOM DEF.ACC body.ACC now
'The spirit vivifies the body now.'

¹⁷⁹ (5.18) from Ml. 65^{b7}, (5.19a) from Wb. 13^{d7}. Because Thurneysen (1949) includes very few examples of actual sentences, preferring instead to cite individual forms, (5.19b) is from Quin (1975) and therefore artificial.

(5.19b) Celait int sacairt rúna
hide-PRES.IND.3rd.pl. DEF.NOM.PL. priest-NOM.PL. secret-ACC.PL.
inna salm.
DEF.GEN.PL. psalm.GEN.PL
‘The priests conceal the secrets of the psalms.’

Unlike its cousins, however, Old Irish had *obligatory pro-drop*. In familiar *pro-drop* languages like Spanish and Italian, *pro-drop* is customary, but subject pronouns were allowed for emphasis. This was impossible in Old Irish: subject emphasis could be expressed only via a cleft. There are no subject pronouns in this language.

In the previous chapter, we saw that Old Irish object markers appeared between a preverb and the rest of the verbal complex, as shown in (5.20).

(5.20a) No- m- ben- Ø.
PRV-1st.sg.-strike-3rd.sg.C
‘he strikes me’

(5.20b) Ní- m- ben- Ø.
NEG-1st.sg.-strike-3rd.sg.C
‘he doesn’t strike me’¹⁸⁰

This is also the position for passive subjects. Old Irish had passive/impersonal forms (singular and plural), but these were third-person only. Since passive subjects are underlyingly objects, it makes sense that their surface expression should be that of objects.

(5.21a) Ní- ben- ar.
NEG-strike-PASS.SG.C
‘He is not being struck.’

(5.21b) No-m- ben- ar.
PRV-1st.sg.-strike-PASS.SG.C
‘I am being struck.’

Old Irish did have a set of stressed nominative pronominals, as depicted in Table 5.1.

	Singular		Plural	
	<i>Normal</i>	<i>Emphatic</i>	<i>Normal</i>	<i>Emphatic</i>
<i>First</i>	mé	messe	sní	snisní, sníni,
<i>Second</i>	tú	tussu	sí	sisní, sinni
<i>Third masculine</i>	é, hé	(h)ésom		sib, sissi
<i>Third feminine</i>	sí	--	é, hé	--
<i>Third neuter</i>	ed, hed	--		

Table 5.1: *Old Irish Nominative Pronouns*

The forms shown in Table 5.1 had a very restricted distribution, limited to use as copular predicates (5.22) or as subjects with non-verbal constructions (5.23).¹⁸¹ Unlike its

¹⁸⁰ Indirect dative objects with the verb ‘to be’ can also be expressed in this way (Thurneysen 1949:255), e.g.

(i) Ro- t- bia
PRV-2nd.sg.-FUT.be-3rd.sg.
‘There will be to you’ [= ‘You will have’]

Modern Irish counterpart, the Old Irish copula sported a full person-number paradigm and usually agreed with its subject; however, there was a tendency for the third singular forms to be used in place of the other members of the paradigm, as shown in (5.24).

(5.22) Is sissi in tempul sin.
 PRES.IND.3rd.sg.COP 2nd.pl.EMPH. DEF.NOM temple-NOM that
 ‘You are that temple.’ [Lit.: ‘That temple is you.’]

(5.23) Apstíl i tossug, sissi íarum.
 apostle.NOM.PL first 2nd.pl.EMPH. afterwards
 ‘Apostles first, you afterwards.’

(5.24a) Is messe in rí.
 PRES.IND.3rd.SG.COP. 1st.sg.EMPH DEF.NOM. king-NOM
 ‘I am the king.’ [Lit. ‘The king is me.’]

(5.24b) Am in rí.
 PRES.IND.1st.sg.COP. DEF.NOM. king-NOM
 ‘I am the king.’

Old Irish also had a set of pronominal enclitics, traditionally dubbed *notae augentes*, as shown in Table 5.2¹⁸² The analysis of the *notae* is somewhat unclear; they may have been either unstressed pronouns or deictic demonstratives. This question will be discussed further below.

	Singular	Plural
<i>First Person</i>	-sa/-se	-ni
<i>Second Person</i>	-siu/-so/-su ¹⁸³	-si
<i>Third Person Masc./Neut.</i>	-som/-sem	-som
<i>Third Person Fem.</i>	-si	

Table 5.2: *Paradigm Notarum Augentium*

The *notae* occurred cliticised to the right of the verbal complex, and could reference either subject, as in (5.25a), or object, as in (5.25b); they also sometimes occurred with non-verbal constructions like (5.25c).

(5.25a) Baitsim -se.
 baptise-PRES.IND.1st.sg. -1st.sg.EMPH
 ‘I baptise.’

(5.25b) Ní -m ·charat -sa.
 NEG- 1st.sg. love-PRES.IND.3rd.pl -1st.sg.EMPH
 ‘They do not love me.’

(5.25c) Am cimbíd-se.
 PRES.IND.1st.sg.-COP captive-PTCL
 ‘I am a captive.’

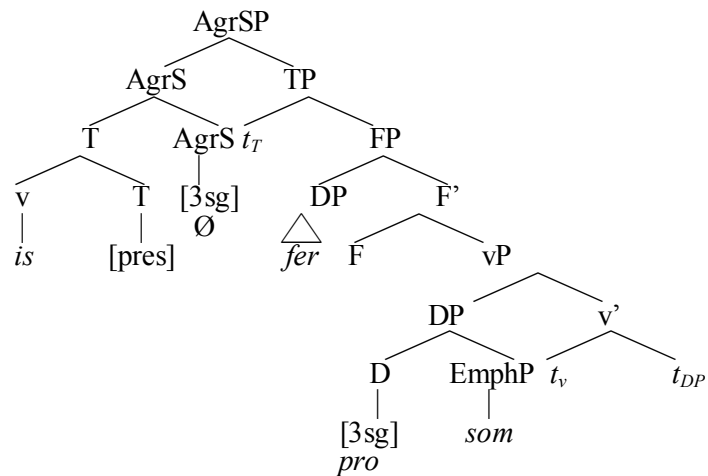
It is important to note that the form of the *nota* was invariant, regardless of whether it referenced the subject or object; again, the difference between *-sa* in (5.25b) and *-se* in

¹⁸¹ Examples from Thurneysen (1949) unless otherwise specified. All the examples shown here are emphatic; this is a coincidence rather than a fact about non-emphatic pronouns.

¹⁸² The “emphatic” pronouns in Table 5.1 can be deconstructed into non-emphatic pronoun + *nota augens*, at least for some stage of Irish. It is not clear whether the forms as they appear in Old Irish have already fossilised.

¹⁸³ *-su* is mostly restricted to appearing after pronouns.

- (5.28a) *Notae augentes* are emphatic deictics;
 (5.28b) *som* is cliticised to a null subject *pro*



Griffith (to appear) reports that *notae augentes* are never attached to the verb if the verb has an overt subject¹⁸⁶, which could indicate that the *notae* are indeed true subjects, since the complementarity then falls out very easily. Although it is true that the *notae* could attach to stressed pronouns as well as directly to verbs, this could be synchronically unrelated to their appearance elsewhere; some preterite forms of the copula have what were etymologically *notae augentes* reanalysed as part of the form.¹⁸⁷

There was another part of the grammar that allowed clitic pronominal subjects of another kind: defective verbs lacking person, number, and tense. The most notable defective verb in Old Irish was *ol* ‘said’, which was common in narratives. Pronominal subjects of *ol* were expressed by cliticised pronouns similar to the stressed third person forms, but not identical to them: masculine *olse*, feminine *olsi*, plural *olseat*. Although the form *olseat* was influenced by the third plural verbal ending *-at*, all three clitic forms were pronominal. Three pieces of evidence favour the pronominal analysis. First, analogous first-person singular forms were later created based on the pronoun rather than the verb (*olmé* or *olsmé*, never **olu* or **olim*). Second, lexical subjects in Irish agreed with verbs at this stage, but occurred only with clitic-less *ol*. Finally, in the ninth century the inherited third plural pronoun *é* was replaced by *eat/iat*, from a re-cutting of *olseat*.

In conclusion, it may be an exaggeration to say that Old Irish had *no* pronouns, but it allowed at most only a very restrictive set of pronouns. Independent pronouns were found only as copular predicates; all other pronominal-like elements were clitics with very restricted distributions. Most pronominal functions were filled by *pro*.

5.2.3.2 Modern Irish

Modern Irish verbs and pronominals are quite different from their Old Irish counterparts. In Modern Irish, constructions with an “analytic” verb and overt subject pronoun have been steadily replacing the older synthetic verb forms, sometimes entirely.¹⁸⁸ The progression of erosion of synthetic forms differs from dialect to dialect; Tables 5.3 and

¹⁸⁶ Nor could they be used with relative verbs having subject antecedents; note that relative forms existed only for third persons and for first person plural.

¹⁸⁷ That these *notae augentes* have been reanalysed as part of the copula is clear from their co-occurrence with “real” *notae augentes*.

¹⁸⁸ This happened also in Scots Gaelic – earlier, in fact, than in Irish.

5.4 show the inflection of *cuir* ‘put’ in West Munster and in Ulster, the dialects on the extremes of the continuum.¹⁸⁹

	Present	Future	Past	Imperfect	Conditional
<i>1st. sg.</i>	cuirim	(cuirfead)	chuireas	chuirinn	chuirfinn
<i>2nd. sg.</i>	(cuirir)	(cuirfir)	chuiris	chuirteá	chuirfeá
<i>3rd. sg.</i>	--	--	--	--	--
<i>1st. pl.</i>	cuirimid	cuirfimid	chuireamar	chuirimis	chuirfimis
<i>2nd. pl.</i>	--	--	(chuireabhair)	--	--
<i>3rd. pl.</i>	(cuirid)	(cuirid)	chuireadar	chuiridís	chuirfidís
<i>Impersonal</i>	cuirtear	cuirfear	cuireadh	chuirteá	chuirfeá
<i>Analytic</i>	cuireann	cuirfidh	chuir	chuireadh	chuirfeadh

Table 5.3: *cuir* in West Munster

	Present	Future	Past	Imperfect	Conditional
<i>1st. sg.</i>	cuirim	--	--	chuirinn	chuirfinn
<i>2nd. sg.</i>	--	--	--	chuirteá	chuirfeá
<i>3rd. sg.</i>	--	--	--	--	--
<i>1st. pl.</i>	--	--	--	chuirimis	chuirfimis
<i>2nd. pl.</i>	--	--	--	--	--
<i>3rd. pl.</i>	--	--	--	--	--
<i>Impersonal</i>	cuirtear	cuirfear	cuireadh	chuirteá	chuirfeá
<i>Analytic</i>	cuireann	cuirfidh	chuir	chuireadh	chuirfeadh

Table 5.4: *cuir* in Ulster

The grammatical status of verbal endings in Modern Irish is very controversial. In the account presented here (recently argued independently by Brennan 2009 and Diertani 2009), Modern Irish differs sharply from Old Irish in having a large number of pronominals and almost no agreement. The remaining synthetic forms in Modern Irish are therefore structurally different from their Old Irish forebears: the apparent person-number endings are pronouns, not exponents of agreement.

There are several pieces of evidence in support of this analysis. The first and most striking is also the simplest: there is strict complementary distribution between synthetic forms without overt subjects and analytic forms with overt subjects. This is shown in (5.29). (5.29a) shows that synthetic verbs can occur without clitic subjects, and (5.29b) shows that they *can't* co-occur with a clitic subject. (5.29c) shows that analytic verbs can occur with clitic subjects, and (5.29d) shows that they can't occur without an overt subject. Finally, (5.29e) shows that, in most cases, if there is a synthetic verb, using an analytic verb instead is not allowed (though variation is allowed in a few specific cases, as was noted in Tables 5.3 and 5.4).

- (5.29a) Chuir-finn isteach ar an phost sin. (synthetic)
 put- COND.1st.sg. in on the job that
 ‘I would apply for that job.’
- (5.29b) *Chuir-finn mé isteach ar an phost sin. (synthetic)
 put- COND.1st.sg. 1st.sg. in on the job that
 ‘I would apply for that job.’
- (5.29c) Chuir-feadh sibh isteach ar an phost sin. (analytic)
 put- COND 2nd.pl. in on the job that
 ‘You (pl.) would apply for that job.’
- (5.29d) * Chuir-feadh *pro* isteach ar an phost sin. (analytic)

¹⁸⁹ Data from McCloskey and Hale (1984:489), Ó Siadhail (1989:179–82). Parenthetical forms indicate that both analytic and synthetic options exist.

- put- COND 3rd.sg.MASC. in on the job that
 Intended reading: ‘He would apply for that job.’
 (5.29e) * Chuir-feadh mé isteach ar an phost sin. (analytic)
 put- COND 1st.sg. in on the job that

Another piece of evidence is the ability of an apparently null subject to be conjoined with an overt subject. The verb agrees with the apparent left conjunct, the null pronoun.

- (5.30) Dá mbéitheá fein agus Rachel ag gabail i gcleamhnas.
 if COND.2nd.sg.-be REFL and PROG go.VN in engagement
 ‘If you and Rachel were getting married.’

A third piece of evidence is the ability of apparent null pronouns to be the antecedents of relative clauses, as shown in (5.31).

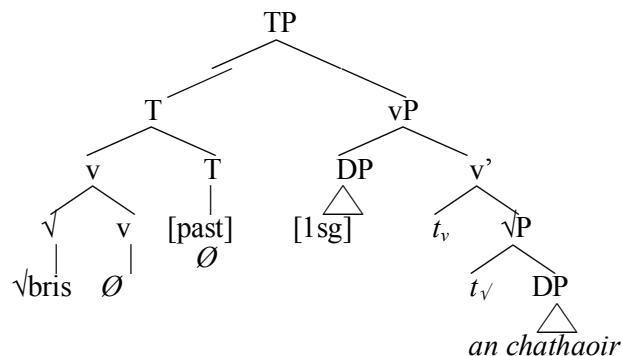
- (5.31a) Chu-adar sin aN raibh aithne agam orthu go
 go- PAST.3rd.pl. that COMP be.PAST acquaintance at-1st.sg. on-3rd.pl. to
 Meiriceá.
 America
 ‘Those that I knew went to America.’
 (5.31b) Deir-im -se nár fhág a’baile ariamh...
 say-PRES.1st.sg PTCL COMP.NEG home.PAST.ANAL home ever
 ‘I who never left home say...’

There are other pieces of evidence for the pronominal status of Modern Irish verbal endings, but these are sufficient to establish the basic point, that verbal endings in Modern Irish are in fact subject pronouns.¹⁹⁰ These subject pronouns have been affixed to the verb under adjacency via Local Dislocation; the derivation is summarised below. For a more detailed presentation of the formal analysis and the empirical justification for it, cf. Diertani (2008); for a similar account cf. Brennan (2009).

Prior to Vocabulary Insertion, the basic structure of a Modern Irish sentence like (5.32a) is taken to be (5.32b).

- (5.32a) Bhris -eas an chathaoir.
 break.PAST-1st.sg. DEF chair
 ‘I broke the chair.’

(5.32b)



Following the linearisation of (5.32b), the verb and subject pronoun are concatenated, as in (5.33a). This concatenation allows Local Dislocation to apply, replacing the linear

¹⁹⁰ Similar facts obtain for prepositional objects, which I will not discuss here.

relationship with a hierarchical one, as in (5.33b). The resulting structure is then linearised as in (5.33c).

- (5.33a) $LIN-\wedge [(T * AspP)] \rightarrow (T \wedge 1^{st}.sg.)$
 (5.33b) $(T \wedge 1^{st}.sg.) \rightarrow [[T]1^{st}.sg.]$
 (5.33c) $[[T]1^{st}.sg.] \rightarrow [T \oplus 1^{st}.sg.]$

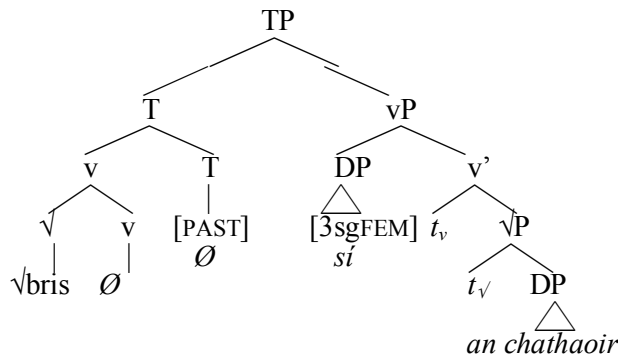
Vocabulary Insertion then applies. In the West Munster dialect, the choices available for first person singular at VI include (5.34).

- (5.34) $1^{st}.sg. \rightarrow -im / [PRES] _$
 $1^{st}.sg. \rightarrow -inn / [IMPF, COND] _$
 $1^{st}.sg. \rightarrow -eas / [PAST] _$

In an analytic sentence, no Local Dislocation occurs, and the analytic ending is inserted in TP, as shown in (5.35).

- (5.35a) Bhris sí an chathaoir.
 break.PAST.ANAL. 3rd.sg.FEM. DEF chair
 ‘She broke the chair.’

(5.35b)



It is important not to confuse the “analytic” form with the general consensus that the third person is not a “person” in the same way that first and second persons are, and that it is, in some respects, a non-person. In most languages third-person verbs do show agreement with their subject; they cannot be used with first- or second-person subjects the way analytic verbs in Irish may be. The difference is something like that represented in (5.36). Ordinary third person verbs are merely neither first nor second person; Irish ex-third singular verbs seem to be radically impoverished in some respect.¹⁹¹

- (5.36a) Zero third person: verb-tense-AGR_[-1 -2]
 (5.36b) Impoverished agreement: verb-tense-AGR_[∅]

¹⁹¹ Welsh and Breton can also construe third singular forms with non-third singular pronominal subjects, and therefore are often also called “analytic” forms; however, the Welsh and Breton forms differ from Modern Irish in one crucial respect. In Welsh and Breton, the so-called “analytic” form can be used without an overt subject, in which case it receives a third singular interpretation just like a typical *pro*-drop language like Italian. Yet in Modern Irish, the analytic form requires an overt subject; if it is used without an overt subject, then the construction is subjectless. Therefore, the Welsh and Breton forms are not truly or not exclusively null; they also serve as true third singulars. The Irish form is not a true third singular.

A further distinction must be made between the analytic form and an impersonal form. Irish has had proper impersonal/passive verbal forms throughout its attested history; these can be translated as passives, but more frequently mean something like ‘there was an action of X-ing.’ This form exists for all tenses, even in the Ulster dialect, which preserves the fewest synthetic forms.

To summarise, then: in Modern Irish the former verbal agreement morphemes have become subject pronouns, and emerge via an operation of Local Dislocation at PF. When this process does not occur, the structure is spelled out as an analytic verb and overt pronoun, which do not show agreement with each other. Analytic forms are ungrammatical if unaccompanied by overt pronouns or lexical DPs.

Before we turn to the historical developments of Middle Irish, mention should be made of Modern Irish object pronouns. Unlike Old Irish, Modern Irish has independent object pronouns which do not occur as part of the verbal complex. They are unusual, however, in that they typically appear clause-finally rather than immediately after the subject as full lexical DPs do; this is shown in (5.37).¹⁹²

- (5.37a) Chonaic mé ag an gcluiche thú.
 see.PAST.ANAL 1st.sg. at DEF match 2nd.sg.
 ‘I saw you at the match.’
- (5.37b) Sciob an cat an t-eireaball de- n luch.
 snatch.PAST.ANAL DEF cat DEF tail from-DEF mouse
 ‘The cat cut the tail off the mouse.’

Table 5.5 shows the pronominal inventory of Modern Irish.

	Subject Pronouns		Object Pronouns	
	<i>Normal</i>	<i>Emphatic</i>	<i>Normal</i>	<i>Emphatic</i>
<i>Singular 1st.</i>	mé	mise	mé	mise
<i>2nd.</i>	tú	tusa	thú	thusa
<i>3rd. masc.</i>	sé	seisean	é	eisean
<i>3rd. fem.</i>	sí	sise	í	ise
<i>Plural 1st.</i>	muid, sinn	muidne, sinne	muid, sinn	muidne, sinne
<i>2nd.</i>	sibh	sibhse	sibh	sibhse
<i>3rd.</i>	siad	siadsan	iad	iadsan

Table 5.5: *Pronouns in Modern Irish*

We now turn to a discussion of the developments in Middle Irish which gave rise to the modern grammar.

5.2.3.3 Middle Irish

As we have seen, the grammars of Old and Modern Irish are very different: Old Irish has radical *pro*-drop and lots of agreement affixes; Modern Irish has overt pronouns everywhere, and no agreement affixes. The key changes leading from the Old Irish grammar

¹⁹² The examples in (5.37) are from Standard Modern Irish. (5.37a) was taken from Ó Dónaill (2005); (5.37b) is from McCloskey 2005:257. Note that object pronouns can occur to the left of adverbials; (i) is also from Ó Dónaill.

- (i) Múin-eann sé iad gach samhradh.
 teach-PRES.ANAL.3rd.sg.MASC 3rd.pl. every summer
 ‘He teaches them every summer.’

finally (except for sentential adverbials), like object pronouns, rather than adjacent to the verb. This is shown in (5.41).¹⁹⁶

- (5.41) Tuc- ad co tír hí iar sin.
bring.PERF-PASS.SG.A to land.ACC.SG. 3rd.sg.FEM after that
'She was brought to a country after that.'

However, there is reason to think that “overt pronominal subjects first appeared in passive sentences” is the wrong characterisation of this stage of the change. Passive subjects are underlying objects¹⁹⁷, and we saw in 5.2.3.1 that the subjects of non-third person passives were expressed like objects; cf. (5.20a) and (5.21b), repeated here for convenience.

- (5.42a) No- m- ben- Ø.
PRV-1st.sg.-strike-3rd.sg.C
'he strikes me'
(5.42b) No- m- ben- ar.
PRV-1st.sg.-strike-PASS.sg.C
'I am being struck.'

Moreover, independent object pronouns and independent passive subject pronouns first appear at the same time. This suggests that it is incorrect to say that the first apparent subject pronouns were in fact subject pronouns. Rather, these “passive subject pronouns” debuted when they did simply because they were object pronouns, and this is the stage when independent object pronouns entered the language.

The first *true* subject pronouns, which did not appear until after the first quarter of the twelfth century (Eska p.c.) occurred in unaccusatives¹⁹⁸ like (5.43), and still later to intransitives more generally (5.44).

- (5.43) Da-fuí- t leis- sium hé.
fall.PRES-3rd.sg. by.3rd.sg.-3rd.sg.MASC.EMPH. 3rd.sg.MASC.
'He falls at his hand.' [Lit. 'by him']
(5.44a) Ar- nach-tíss- ad friss
in.order.that-NEG- come.PAST.SUBJ.-3rd.sg.C against.3rd.sg.MASC.
hé.
3rd.sg.MASC.
'...that he should not come against him.'
(5.44b) Cia do-luí- d i n-écaib hi.
although come.PAST-3rd.sg. in death.DAT 3rd.sg.FEM.
'Although she met her death.'

What appears to have happened is that a generation of Irish children made the same mistake that later Irish scholars made: they concluded, incorrectly, that the passive “subject” pronouns were subjects, and therefore that their language allowed pronominal subjects to occur in this position. They then extended the rule to include all intransitive subjects. A curious (and apparently unique) twelfth-century example may attest to this confusion: it features an apparently Old Irish-type verbal complex with an intransitive subject appearing in

¹⁹⁶ (5.41) is from Best and Bergin (1929:100), line 3108; the translation and glosses are mine. (5.43), (5.44a), and (5.46) are from McCone (1987:192); the translations are his, but the glosses are mine. (5.44b) is from Greene (1958:111), and (5.41) is from Atkinson (1880), line 113^a41.

¹⁹⁷ My thanks to Tony Kroch (p.c.) for reminding me of this.

¹⁹⁸ C.f. McCone (1987:192), where unaccusatives are called “quasi-passive intransitives”.

continued to be ungrammatical with first or second person verbs. When a first or second person pronominal subject appeared, the verb with which it appeared was the former third singular.

- (5.48a) Rag- aid missi.
 go.FUT-3rd.sg. 1st.sg.EMPH.
 ‘I shall go.’
- (5.48b) B- aí sinne.
 be.PRET-3rd.sg. 2nd.pl.
 ‘We were.’
- (5.48c) Fritháil- ter misi.
 minister.PRES.SUBJ.-PASS.SG. 1st.sg.EMPH
 ‘Let me be ministered to.’

This is similar to what had already begun to happen with the copula; as noted above, even in Old Irish, the third singular form was often substituted for an expected inflected form, and in the modern language, the copula has been reduced to a demonstrative-like particle in T, much as the analytic ending is now the exponent of T. It could also be independent: thanks to the confusion involving passive subjects, learners of Old Irish now had evidence for using third singular verbs with subjects of other persons and numbers, but yet no evidence of first or second person verbs with independent subjects.

It is often said that the exception proves the rule, and this in this case the relevant exception concerns the rule that synthetic verbs never co-occur with overt subjects, pronominal or otherwise. Third plural subjects *always* agreed with a third plural verb: McCone (1987:193) notes that ‘the concord rules at this stage were 3sg. verb with all [subject] pronouns except the third plural, which required a 3pl. verb’. It is easy to see why this apparent exception to the rule would have been learnable. Old Irish verbs always agreed with lexical DP subjects, including in the passive. Therefore, language learners had a great deal of evidence for agreement between third plural verbs and lexical DP subjects; they had no reason not to assume that third plural pronominal subjects would not behave similarly. Eventually, this was lost, and the third plural was brought in line with the rest of the non-third-singular subjects; but this took a very long time to go to completion. As recently as twenty years ago, older speakers of Munster Irish were still sometimes using overt pronominal subjects with synthetic verbs in the third plural. Ó Siadhail (1989:182) gives this example:

- (5.49) Mhuise, tug- aid siad orm é.
 indeed call.PRES-3rd.pl 3rd.pl. on-1st.sg. it
 ‘Indeed, they call me it.’

Outside the third plural, however, there was no evidence for subject-verb agreement, and in terms of surface form, there was very little difference between an analytic verb with an obligatory subject and a synthetic verb with an underlying *pro*. In both cases, no constituent was allowed to intervene between the verb and an overt M-word bearing person features. Faced with this input, learners of Irish had two equally reasonable choices: they could analyse all overt subjectlike entities as verbal agreement suffixes, as in the older system; or they could analyse all overt subjectlike entities as pronouns. They chose the latter analysis, and so the erstwhile verbal agreement markers became suffixed pronouns.

What might have biased the learner in favour of this analysis? Presumably, the fact that the former third singular had become an analytic form with radically impoverished agreement which could take pronominal subjects of any person, combined with the failure of non-third person verbs to co-occur with pronominal subjects. The Modern Irish complementarity of synthetic verbs and analytic verbs dates all the way back to Middle Irish.

Moreover, the analytic form did not do double duty as a third singular: without an overt subject, it was ungrammatical. This is what sets Irish apart from Welsh and Breton; in the Brythonic languages, the third singular can behave like an analytic form, but it is also grammatical without an overt subject and under this condition receives a third singular interpretation. In Irish this is not the case; analytic forms *require* overt subjects.

Once learners have acquired a grammar like this, it is a relatively short step to deriving a new independent pronoun. The verbal endings of Irish were already tense-conditioned pronouns, and the prosodic structures of the synthetic construction *támuid* ‘we are’ and the analytic *tá sinn* were identical. In addition, the first plural ending is a discrete CVC syllable; the onset would never vary. So the morpheme boundary between the verb-stem and the ending was reanalysed as an M-word boundary, and a new independent pronoun was born.

This reanalysis is simple enough that one might ask why it happened only in the first plural. In fact, *muid* is not the only example of its kind, merely the only such example to make it into the standard language. Some of the regional dialects have other independent pronouns which were originally verbal endings: first plural preterite *mar* and third plural preterite *dar*, both of which, like *muid*, are discrete CVC syllables (as opposed to e.g. first singular *-im*, which has not surfaced as an independent pronoun, and which would have a variable onset).

Muid is not actually a case of direct development from verbal ending to pronoun; rather, it is a case of a verbal ending becoming an independent pronoun via an intermediate stage as a subject pronoun suffixed via Local Dislocation. So that is the story of Irish *muid*. A verbal ending became an independent pronoun as a result of significant change involving both the verbal system and the pronominal system, leaving a situation of analytical ambiguity from the perspective of the language learner. It was a perfectly sensible thing to do, given the reanalysis of etymological verbal markers that had already occurred; there’s nothing “unnatural” about it when you consider it in context. But you can also see why examples like this might be rare. If Irish had not been a VSO language, and if the Old Irish system of rampant *pro*-drop had not broken down, and if the verbal system hadn’t been undergoing a lot of change itself, learners of Irish would not have been motivated to make the analytic choices they did.

5.2.4 Germanic I

Many modern Germanic languages have what is sometimes called a “group genitive”: a possessive marker that attaches to phrases rather than to individual nouns, e.g. English *’s*. The best-supported and most widely accepted explanation of the group genitive’s origins is that it is the descendant of the genitive singular suffix *-es*, given new life as a clitic. This scenario is not universally accepted; an alternative hypothesis, dating back to Jespersen (1894:319) and developed further by Janda (1980), is that speakers had confused the genitive marker with the pronominal possessive *his*. This argument is based on the occurrence in texts of apparent constructions like *for Jesus Christ his sake*. However, Carstairs (1987), Allen (1997), and Norde (2001, 2009) have all argued convincingly that *his* in this collocation is merely a convention for spelling *’s*, and in fact the evidence for Janda’s analysis is slight enough that I see no need to review that argument here.¹⁹⁹

The group genitive is often cited as a good example of the connection between affix-exodus and changes elsewhere in the language, since it is generally accepted that the loss of a

¹⁹⁹ There are also arguments in the literature about whether the group genitive ought to be admitted as an example of affix-exodus, with e.g. Tabor and Traugott (1997) arguing that it ought instead to be considered grammaticalization because it involves an increase in syntactic scope and a reduction in polysemy. As these arguments are essentially definitional in nature, I consider the issue irrelevant to this discussion.

productive system of morphological case-marking was the crucial ingredient in the former suffix's change to clitic status (or, as is more in keeping with the theoretical stance taken here, the change from word-level to phrase-level marker). However, there are some odd inconsistencies between this story and the data from the various Germanic languages, and this challenges the notion that affix-exodus is necessarily the result of changes elsewhere in the language. I will first review the usual story, and concentrate on its more problematic aspects.

As mentioned at the outset, the modern English group genitive 's is the formal descendant of *-es*, one of several genitive singular allomorphs in Old English (cf. e.g. Ringe and Eska forthcoming), but its syntactic behaviour is very different from that of its illustrious forebear. The Old English genitive (5.50) was a true case affix, appearing on the nominal head and any agreeing forms associated with it. In contrast, modern English 's is a phrasal clitic (5.51), appearing only on the final word in the DP with which it is associated. It attaches to the entire phrase rather than to any specific word.²⁰⁰

(5.50a) *ōðr- es mann-es*
 another-GEN.SG. man- GEN.SG.
 'another man's'

(5.50b) *æðel- re cwēn- e*
 noble-GEN.SG. queen-GEN.SG.
 'of a noble queen'

(5.51a) the blue pig's ears

(5.51b) the blue pig that I bought yesterday's ears

(5.51c) some R'n'B crap out of the guy opposite me's headphones

(5.51d) The blonde I had been dancing with's name was Bernice.

Beginning in late Old English, the genitive allomorph *-es* spread through the language, ousting the other singular allomorphs (e.g. (5.52a) as contrasted with (5.50b)) and eventually also the plural; meanwhile, the use of the genitive marker was increasingly restricted to the noun only, with adjectives and determiners uninflected, as shown in the contrast between (5.52b)–(5.52c).²⁰¹

(5.52a) *þes cwēn- es*
 the queen-GEN.SG.
 'the queen's'

(5.52b) *i þiss middellærd-ess lif*
 in this world- GEN life
 'in this world's life'

(5.52c) *in þiss-es middangeard-es lif*
 in this-GEN world- GEN life

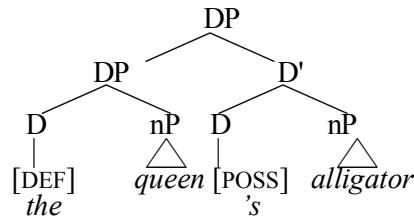
Because of the structure of English DPs, the genitive marker always appeared at the right periphery, which means that it became structurally ambiguous: it could be attached either to the noun or to the entire DP. The latter is depicted in (5.53).²⁰²

²⁰⁰ (5.51c,d) are taken from Norde (2009:162), who found them via Google search on 3 May 2008.

²⁰¹ (5.52a) comes from the 1123 entry in the *Peterborough Chronicle*, which could be considered very late Old English. (5.52b) is from the *Ormulum* (ca. 1200) and is therefore more properly early Middle English; (5.52c) is a translation of (5.52b) into Old English. There are no genuine Old English texts after about 1050 because the English scribes had adopted a semi-standard written language which became increasingly archaic.

²⁰² The former is not depicted because of uncertainty as to the underlying structure.

(5.53)



Faced with a similar choice between case markers attaching to either nouns or phrases, learners of other languages often opt for the former, but the learners of English of this period chose the phrasal analysis. Their choice may have been motivated by the fact that the inherited case system was breaking down. If they had already concluded, on the basis of the evidence available to them, that case distinctions were not marked, or at best only variably marked, then in order to make the genitive conform to the rest of their hypothesis, they would conclude that it wasn't a word-level marker either, but rather a cliticised determiner. The state of the English case system at this point is crucial: had the English case system still been fully productive, language learners would have had no motivation to analyse the genitive as a phrasal element.

The connection between the rise of the group genitive and the loss of case in English is further implied by their co-occurrence in other Germanic languages, including Danish, Swedish, and Bokmål Norwegian. For instance, Danish has a group genitive (5.54), but not a case system.²⁰³

(5.54a) for dem der kommer-s skyld
for those REL.NOM. come- POSS sake
'for the sake of those who are coming'

(5.54b) et av de små børn som er her i feri- en- s
one of the small children REL.NOM. are here for holiday-DEF-POSS
fødselsdag
birthday
'one of the small children who are here for the holiday's birthday'

Unlike English and mainland Scandinavian, Icelandic and German both lack group genitives. Importantly, these languages also retain morphological case distinctions. Thus, the Germanic languages seem to show a positive correlation between a potentially ambiguous structure of possessive DPs owing to the erosion of the case system and reanalysis of the surviving genitive marker as a phrasal clitic rather than an affix. By extension, these languages also support the hypothesis that affix-exodus tends to occur as a by-product of other changes elsewhere in the language.

A closer look at Swedish and German, however, suggests that there is more to this picture than typically discussed.

The history of the group genitive in Swedish has been discussed in detail by Norde (2001, 2009), and it is particularly of interest because, unlike English, there is little room for doubt about its origins,²⁰⁴ since Swedish lacks a potentially homophonous possessive pronoun.²⁰⁵ Norde reports that the group genitive in Swedish developed over the course of

²⁰³ For discussion, cf. Carstairs (1987). Examples from Jespersen (1934).

²⁰⁴ There is, however, apparently room for quibbling about its definition as grammaticalization or de-grammaticalization; see the following footnote.

²⁰⁵ Janda (2001) has tried to argue that the reflexive possessive *sin* and non-reflexive *hans* could have been associated with genitives in *-es* in the mainland Scandinavian languages, but as these languages lack a parallel to the English *for Jesus Christ his sake* construction, there is no evidence for this hypothesis.

several centuries, and that the textual evidence suggests that the conservative and innovative grammars co-existed for much of this time, in line with the competing grammars model of Kroch (1994, 2001).

Old Swedish, like Old English, had a genitive affix (5.55), which was required on all elements within the noun phrase.²⁰⁶

- (5.55a) en- s salog- s man-z munne
 INDEF-GEN.SG.MASC. blessed-GEN.SG.MASC. man-GEN.SG.MASC. mouth
 ‘a blessed man’s mouth’
- (5.55b) af mang-s rik- s man-z
 of many- MASC.SG.GEN. rich-MASC.SG.GEN. man-MASC.SG.GEN.
 vlyk- o
 bad.luck-OBL.
 ‘of the bad luck of many a rich man’

Concordial case marking like the examples above gradually disappeared throughout the Old and Middle Swedish periods, and was replaced by genitive marking only on the head noun.

- (5.56a) mängen riddari-s blod
 many knight-GEN blood
 ‘the blood of many a knight’
- (5.56b) ... kom iak heem til fadhir²⁰⁷ min-s hws
 came I home to father my- GEN. house
 ‘I came home to my father’s house.’
- (5.56c) konung Valdemar-s dotter aff Denmark
 king -GEN daughter of Denmark
 ‘King Valdemar of Denmark’s daughter’

True group genitives, in which the entity the possessive attaches to need not necessarily be a noun, show up later; by Norde’s estimate, the earliest examples appear no earlier than the second half of the fifteenth century. There was an initial period of considerable variation, with the head-marking grammar and the newer group genitive grammar co-existing in the same texts. All of the examples in (5.57) come from the same text, dated c. 1640. (5.57a) is parallel to (5.56c), with the possessive on the head noun, while (5.57b) is a modern-type group genitive. In (5.57c), the genitive marker appears twice, rather tantalisingly.

- (5.57a) konung-en- s i Poland skipp
 king- DEF-GEN in ship
 ‘the king of Poland’s ship’
- (5.57b) konung-en i Sverige- s fellttherre
 king- DEF in Sweden-GEN general
 ‘the king of Sweden’s general’

²⁰⁶ Swedish examples are taken from Norde (2001, 2009).

²⁰⁷ Norde (2009:163fn.14) stresses that *fadhir*, as an *r*-stem, might be expected not to have a genitive in *-s*, and therefore be considered indeterminate as far as whether or not it is marked for case. However, genitives in *-s* were extended to *r*-stems in Swedish at least as far back as Runic Swedish (Norde 1997), and the text containing this example, which dates from the 1380s, has examples of even feminine *r*-stems allowing genitives in *-s* (e.g. *modhirs* ‘mother’s’).

(5.57c) konung-en- s i Påländ-z skipp
king- DEF-GEN in Poland-GEN ship
‘the king of Poland’s ship’

The group genitive is ubiquitous in modern Swedish, as in modern English, with the possessive marker appearing not only on non-head nouns (5.58a–b), but also on adverbs (5.58c), prepositions (5.58d), and verbs (5.58e).

(5.58a) folket på gata- n- s omdöme
people on street-DEF-POSS opinion
‘the man in the street’s opinion’

(5.58b) Fredrik-s kompis som äger båte-n- s flickvän
POSS pal who owns boat-DEF-POSS girlfriend
‘The girlfriend of Fredrik’s pal, who owns the boat’

(5.58c) grann- en ovanpå-s hund
neighbour-DEF upstairs-GEN dog
‘The upstairs neighbour’s dog’

(5.58d) en artist som jag inte tycker oms platta
an artist COMP 1st.sg. NEG care about-GEN record
‘the record of an artist I do not like’

(5.58e) den man älskar- s lycka
the.one one love.3rd.sg.-GEN happiness
‘the happiness of one’s loved one’

Nothing in the Swedish data shown so far calls into question the correlation between the advent of the group genitive and the loss of morphological case. There is, however, a complication: during the gradual loss of concordial case marking, the possessive marker could and did attach to nouns that were marked for case:

(5.59a) domkirky-o- s
cathedral- FEM.SG.OBL.-GEN.

(5.59b) menniski-or- s²⁰⁸
FEM.PL.NOM/ACC-GEN

Norde (2009:163fn.15) even notes that the phenomenon of case stacking persists in the contemporary Swedish dialect of Älvdalen, which has preserved the dative case.

(5.60) skaulmieseterame- s lägeniet
schoolteacher.DAT.DEF-GEN apartment
‘the schoolteacher’s apartment’

Here is the conceptual problem: if the breakdown of morphological case marking in Swedish were the catalyst for the syntactic changes affecting the possessive marker, we would predict a scenario in which the genitive affix was the sole survivor of the moribund system, reanalysed by speakers as a phrasal clitic because word-level inflectional affixes were no longer part of the grammar. If the other case suffixes were still in use when the genitive affix cut itself loose, then in what sense can their loss be taken to motivate the change affecting the genitive? No other case suffixes in Swedish developed into phrasal markers.

In this context, some data from German becomes relevant. German, as mentioned above, has productive morphological case distinctions and lacks a group genitive. However, German *does* have a possessive *-s* which attaches exclusively to proper names without

²⁰⁸ Norde (2009:163) does not appear to have glossed this form.

postnominal modifiers.²⁰⁹ It could be argued that multi-word designations (e.g. *Graf von Mansfeld*) are actually single Vocabulary Items in German, with a flat structure, but while this might explain (5.61a), it seems an unlikely explanation for (5.61d), in which the possessive marker appears only on the rightmost of conjoined proper names.

- (5.61a) Graf von Mansfeld-s Tanz
 count POSS dance
 ‘Count von Mansfeld’s dance’
- (5.61b) * der Graf (von Mansfeld)-s Tanz
 DEF.NOM count (of) POSS dance
 ‘the count (of Mansfeld)’s dance’
- (5.61c) * Graf von Mansfeld den ich kenne- s Tanz
 count ACC.REL. 1st.sg know.PRES.1st.sg.-POSS dance
 ‘Graf von Mansfeld whom I know’s dance’
- (5.61d) Paul und Braune-s Beiträg-e
 CONJ POSS article- PL
 ‘Paul and Braune’s articles’

Similar phenomena are found in Yiddish, with some striking differences.²¹⁰ Yiddish inflects its articles and adjectives for case, but less consistently its nouns: most proper names and a smaller number of common nouns (e.g. *tate* ‘father,’ *mame* ‘mother,’ *harts* ‘heart’) are inflected, while the majority of common nouns are not (Weinreich 1971). Three features of its case system make Yiddish particularly of interest: it lacks a group genitive, but its dative patterns similarly to the German facts above; and it makes use of ‘s as a possessive marker in some contexts, despite having essentially lost the genitive case; and some of its possessive structures are reminiscent of the case-stacking facts of Swedish.

Nouns are marked possessive via the familiar ‘s. If, as in the majority of cases for common nouns, the noun does not inflect, then the article and any adjectives associated with it are put into the dative case (5.62a). The exceptional inflecting nouns appear in the dative case, with dative articles and/or modifiers and the possessive suffixed to the head noun to the right of the dative case marker (5.62b).

- (5.62a) dem meylekh-s tokhter
 DEF.DAT.MASC. king- POSS. daughter
 ‘the king’s daughter’
- (5.62b) dem tat- n- s tog
 DEF.DAT.MASC. father-DAT-POSS day
 ‘Father’s Day’

These data suggest that Yiddish lacks not only a group genitive, but a genuine genitive case, although the forms with proper names confuse the issue. Personal names also take possessive ‘s, but not in combination with the dative marker.

- (5.63a) Moyshe-s shvester
 ‘Moyshe’s sister’
- (5.63b) * Moysh-n- s shvester
 DAT-POSS sister

²⁰⁹ Thanks to Don Ringe (p.c.) for alerting me to this phenomenon, and to Beatrice Santorini (p.c.) for the examples and native speaker intuitions.

²¹⁰ I am grateful to Aaron Dinkin (p.c.) and Kathryn Hellerstein (p.c.) for their assistance with this section, including the data.

In other contexts, personal names may or must be marked overtly dative. If the dative is the object of a preposition, the dative suffix is optional (5.64a); if there is no preposition, it is obligatory (5.64b) unless preceded by a title (5.64c); with titles, it is usual to omit the dative suffix when a preposition is used (5.64d). Note the similarity of (5.64c) to the German data above: the dative suffix occurs on the edge of the phrase, not on each word of the phrase.²¹¹

- (5.64a) a lid fun Shafir(-n)
 INDEF song by -DAT
 ‘a song by Shafir’
- (5.64b) Kh’hob gezogt Garelik-n.
 I’ve say.PTCP. -DAT
 ‘I told Garelick.’
- (5.64c) Kh’hob geshribn Dr Shloyme Birnboym(-en)
 I’ve write.PTCP. -DAT
 ‘I wrote to Dr Shlomo Birnbaum.’
- (5.64d) Kh’hob geleyent an arbet fun Dr Shlyome Birnboym
 I’ve read.PTCP. INDEF work by
 ‘I read a work by Dr Shlomo Birnbaum.’

There is always the possibility that possessive constructions in Yiddish have been influenced by Slavic (cf. Reershemius 2007:248); certainly Yiddish is noticeably different from any of the other Germanic languages in this area of syntax. Nevertheless, it is worth pointing out that the individual components of Yiddish possessive syntax are reminiscent of those in other languages: the occurrence of the suffix to the right of a dative is at least superficially similar to the stacked constructions in Swedish; the data involving proper names and titles bears similarities to German; and the possessive marker in question is *'s*. Though none of the Germanic possessive constructions is precisely the same as the Yiddish, Yiddish is clearly part of the same general constellation.

Taken together, even given the obvious caveats about Yiddish, the Yiddish and Swedish data suggest that the usual story told about the development of a group genitive in Germanic languages may be too simplistic. In Yiddish, *only* the genitive case has disappeared; in Middle Swedish, the possessive could be placed on nouns bearing inflectional case suffixes. If the genitive is not the sole survivor of a defunct case system, but one of its first casualties, then in what sense is it a by-product of the loss of the case system?

This is not to say that the loss of a case system is not a contributing factor to the existence of a bona fide group genitive. The correlation observed at the start of this section still holds true: the languages with a group genitive do not have true case systems; German and Icelandic, with functioning morphological case marking, do not have group genitives. It could be the case that the case system played a different role in the exodus of the possessive marker than has been previously assumed: suppose, for example, that the unmooring of the possessive occurred independently of the state of the rest of the case system, but that its eventual role as a highly productive phrasal marker was in some way facilitated by the absence of productive case marking.

All of this, of course, is speculative; a large-scale empirical study into the details of the loss of case in English and Swedish, and the German constructions in (5.61), would be required in order to resolve the issue, and such a study is beyond the scope of this section. It may well be the case that Yiddish owes its unusual possessive constructions to Slavic, for instance. Nevertheless, the Swedish data in particular call into question the assumption that “case was lost” is a suitable explanation for what happened to the genitive in Germanic.

²¹¹ Examples from Schachter (1995).

What is truly strange about all of this is the fact that the group genitive appears to have developed independently in a group of very closely related languages at approximately the same time, and involving what is etymologically the same Vocabulary Item. The Germanic languages are not the only languages to have lost a case system; a similar fate has befallen the Romance languages. Yet, to my knowledge, the group genitive is unique to the Germanic languages; there is nothing comparable in Spanish, French, or Italian. Why should such a specific development (and one predicted to be typologically implausible, to boot) occur independently and in parallel across Germanic? If it were a matter of contact, we might predict to find it outside Germanic, since French is spoken in areas adjacent to various Germanic languages. In short, were it not for the fact that the textual record of these changes is much too late for common inheritance, one would suspect this to be an inherited peculiarity.

In fact, I am not convinced that shared inheritance can be entirely ruled out, despite the textual evidence suggesting otherwise. It is not my intent to declare the group genitive a feature of the grammar of Proto-Germanic, or Proto-Northwest-Germanic, but I do not think we can exclude the possibility that there was some grammatical quirk already present in a common ancestor of the modern Germanic languages that laid the groundwork for what would eventually become the wandering possessive marker in the modern languages. It could have been – perhaps *must* have been – very rare, rare enough not to appear in the earliest surviving texts from these languages.

Again, this is pure speculation; but even if the textual evidence is exactly what it appears to be, and the developments in the various modern Germanic languages are completely independent of each other (bizarre though that seems), there is no escaping the fact that several details of the available data do not fit easily with a scenario in which the loss of case morphology was the catalyst for the exodus of the possessive morpheme. The problematic details are summarised in (5.65).

- (5.65a) German has morphological case and lacks a group genitive, but *does* allow a superficially similar construction in which the possessive marker occurs only on the right edge of a complex proper name or conjunction of proper names.
- (5.65b) Yiddish, which retains most of the case system of German *except* for the genitive, displays a very similar construction involving the dative.
- (5.65c) Yiddish puts the possessive marker on nouns marked dative, in some instances.
- (5.65d) In Middle Swedish, it was possible to put the possessive on nouns marked for other cases, calling into question the plausibility of a scenario in which the breakdown of the case system was responsible for the unmooring of the possessive marker.
- (5.65e) One dialect of modern Swedish retains both the dative case and the possibility of case stacking.

Instead, it looks as though the loss of case may have facilitated the development of the group genitive proper – that is, the ability to use the possessive at the right edge of the phrase regardless of the category of the terminal to its right – but likely had nothing to do with the initial change, in which the possessive came to occur only on the head noun.

We have no choice but to entertain the idea that affix-exodus is no more exotic than affix-genesis, in that it need not be a by-product of other changes in the language at all. It is simply something that can happen in language. The next two case studies in this section will cement this point further.

5.2.5 English

The best example known to me involving apparently *sui generis* affix-exodus actually comes from English. This is, of course, the erstwhile adjectival suffix *-ish*, which has recently found new life as an independent adverb with the same semantics. *Ish* is well-established enough to have its own entry in the OED (5.66), which reports its earliest attestation as 1986.

- (5.66) Qualifying a previous statement or description, esp. as a conversational rejoinder: almost, in a way, partially, vaguely

The OED includes the following examples.²¹² Note in particular (5.67e), which includes an offhand internal commentary about the status of *ish* – a status that implies universal acceptability.

- (5.67a) One of those neatly created middle-brow plays which, because they have a pleasantly happy ending (well, ish), might make people think they've been handed a soft option.
- (5.67b) You must try to remember that some people are normal. Ish.
- (5.67c) Frank asked if they were linked, romantically... Then he said yeah, he supposed they were, that was one way to put it, in a way. He paused. 'Ish,' he admitted. 'Vaguely.'
- (5.67d) 'Trust Davie Morrow.' 'You know him?' 'Ish. He's a regular across the road.'
- (5.67e) Mr Langmead, speaking by telephone from London, hesitated. 'Ish', he said, employing the international shorthand for slight hedge.

As a suffix, *-ish* derives adjectives from either adjectives or nouns, with differences in semantics (cf. Morris 1998). When the base is nominal, an *-ish*-adjective means 'of the nature of X' (the "comparative" *-ish*); when the base is adjectival, it means 'something like X' ("qualifier" *-ish*). Kuzmack (in prep) notes that there are prosodic differences between the two, in that the qualifier forms can be stressed (5.68a–b), while the comparative forms sound odd if stressed (5.68c–d).

- (5.68a) That colour is greenish.
- (5.68b) That colour is greenISH, but it's more of a blue shade.
- (5.68c) John is boyish.
- (5.68d) ?? John is boyISH.

The comparative *-ish* can attach to the ends of phrases (e.g. *a bit cloak-and-dagger-ish*); nevertheless, independent *ish* derives solely from the qualifier variant, as demonstrated by the following rather amusing example, adapted from Norde (2009:224):

- (5.69) A: Sound a little stop-and-smell-the-roses-ish?
B: * Ish. [On the reading: 'Yes, it does sound like that.']
B': Ish. [On the reading: 'Well, it sort of sounds like that.']

Many examples have an elliptical quality, as though the adjective qualified by the *ish* has simply been elided, as in (5.70a–b), although in (5.70b) the "adjective" is actually a

²¹² (5.67a) from the *Sunday Times* review section, 19 Oct. 1986; (5.67b) from Pulsford (1990:41); (5.67c) from O'Connor (1992:122); (5.67d) from Bateman (1995:94); (5.67e) from the *New York Times*, 5 Sept. 2002.

participle.²¹³ The constituent to which *ish* refers can also be an adverb (5.70c) or even an entire proposition (5.70d, 5.71).

(5.70a) They have a pleasantly happy₁ ending (well, *t_i* ish)

(5.70b) Is everyone excited? I am – *t_i* ish.

(5.70c) Can you swim well_i? *t_i* Ish.

(5.70d) If I [accept the premises]_i; (and from a maths viewpoint I sort of can – *t_i* ish)...

(5.71) SD: ‘Do you know where you’re going?’

JF: ‘Ish!’

Even when there is no elision, the position of *ish* can be variable, as it is here.²¹⁴

(5.72a) I have work but it should be an easyish day.

(5.72b) Tomorrow’s an easy day (ish) – graduation audit, voice lesson, CS lab...

Attempting to provide an elided antecedent for an independent token of *ish* is unnecessary, however, since there are also examples that are not elliptical in any obvious way:

(5.73) Hobbies: painting, photography, documentary film, skating(ish)

A complete, detailed account of the changes affecting *ish* has not yet been attempted, but the OED allows a rough sketch of the history behind adverbial *ish*. The suffix *-ish* is of impeccable pedigree; it is attested in all of the older Germanic languages, including Gothic, and is distantly related to the Greek diminutive suffix *-ισκος* (*-isk-os*; e.g. *knodoliskos* ‘little monster’). Its primary function in the older Germanic languages was forming adjectives from the names of ethnic groups, though the comparative usage is also attested in Old English.

The earliest qualifier examples, according to the OED, were with colour terms, which may have originally been considered nouns; these first appear around the beginning of the fifteenth century. This was an innovation peculiar to English; in German the corresponding forms take the suffix *-lich*. The spread of qualifier *ish* to other adjectives, for which the OED gives no date, is even more specific to English, as e.g. German does not have any semantic equivalent amongst suffixes.

Two further developments precede the emergence of adverbial *ish* in the textual record. The first was the creation of phrase-final comparative *-ish*; its earliest citation in the OED is from 1815. Although qualifier *-ish* does not appear to have been affected, the relative permissiveness with which comparative *-ish* can be attached to phrases may have played a role in the later exodus of qualifier *ish*. The first hint of permissiveness in the attachment of qualifier *-ish* came approximately a century later (the earliest citation given by the OED is 1916), when *-ish* began to appear on numerals; the OED hypothesises that this was by analogy with *earlyish* and *latish*. Most of the OED’s examples involve times of the day or people’s ages. Less than a century later, the first textual examples of M-word *ish* are cited.

²¹³ Examples (5.70) and (5.72)–(5.73) were taken from Norde (2009:225). (5.71) was overheard by the author in a car in Chicago on 18 June 2010.

²¹⁴ Norde (2009:225fn.) points out that this positional variability is not possible for the comparative variant, as is made clear by the complete unacceptability of (i)–(iii).

- (i) a clean-cravatish formality of manner
- (ii) * a clean-cravat formality-ish of manner
- (iii) * a clean-cravat formality of manner-ish

In and of themselves, this assortment of developments might not have provided language learners with motivation to analyse *ish* as an independent adverb. The catalyst was probably the fact that this suffix can receive contrastive stress. If language learners use prosodic cues to determine the location of M-word boundaries, the fact that *ish* can be stressed might invite an M-word analysis, particularly when combined with the ability of qualifier *ish* to attach to numerals as well as adjectives, and potentially also the occurrence of a homophonous suffix at the edges of phrases.²¹⁵

At first glance, we might assume that the structural details here are not entirely straightforward, because adverbs do not occur post-adjectivally in English. It is understandable, even predictable, that the newly independent *ish* should be allowed more positional freedom, but since the position of *ish* prior to its exodus is not a customary position for adverbs in English, it is tempting to conclude that linear position must not have been a factor at all, unlike in the case of affix-genesis. How else could *ish* develop? Is the process of affix-exodus so completely unrestrained?

The data tell a very different story, and a closer look reveals that the structural details are actually completely straightforward. All of the relevant examples cited in this section (reproduced below, for convenience and presentational emphasis, with *ish* in boldface), as well as all of the examples I have either heard or produced myself, reveal something important about the syntax of *ish*: it is always used at the end of an utterance or phrase, as a parenthetical, *or* completely on its own, without any syntactic context.

- (5.74a) One of those neatly created middle-brow plays which, because they have a pleasantly happy ending (well, **ish**), might make people think they've been handed a soft option.
- (5.74b) You must try to remember that some people are normal. **Ish**.
- (5.74c) Frank asked if they were linked, romantically... Then he said yeah, he supposed they were, that was one way to put it, in a way. He paused. '**Ish**,' he admitted. 'Vaguely.'
- (5.74d) 'Trust Davie Morrow.' 'You know him?' '**Ish**. He's a regular across the road.'
- (5.74e) Mr Langmead, speaking by telephone from London, hesitated. '**Ish**', he said, employing the international shorthand for slight hedge.
- (5.74f) They have a pleasantly happy₁ ending (well, *t_i* **ish**)
- (5.74g) Is everyone excited? I am – *t_i* **ish**.
- (5.74h) Can you swim well_i? *t_i* **Ish**.
- (5.74i) If I [accept the premises]_i (and from a maths viewpoint I sort of can – *t_i* **ish**)...
- (5.74j) S: 'Do you know where you're going?'
J: '**Ish**!'
- (5.74k) Tomorrow's an easy day (**ish**) – graduation audit, voice lesson, CS lab...
- (5.74l) Hobbies: painting, photography, documentary film, skating(**ish**)

In other words, *ish* has not become a canonical adverb with relatively free position (5.75a–b) or even an intensifier like *very* (5.75c). Its structural position is very constrained.

- (5.75a) * I ish can accept the premises. (cf. *I sort of can accept the premises.*)
- (5.75b) * I can ish accept the premises. (cf. *I can sort of accept the premises.*)

²¹⁵ There is a gap in the OED's chronicle of *-ish*, in that the newest sense reported for the suffixed qualifier variant involves only numerals, while the adverbial can essentially refer to anything. It is not clear whether this should be taken as an accurate representation of reality or not. It could have been the case that the factors cited in the main text were sufficient to motivate an M-word analysis in some learners; it is also possible that we are missing a step.

(5.75c) * They have an ish happy ending. (cf. *They have a very/sort of happy ending.*)

Not only is the syntax of *ish* highly restricted, but it is restricted in a way that speaks strongly to its suffixal origins: always on the right periphery. Compare (5.74b) above with (5.68b), reproduced as (5.76a). These two sentences are virtually the same: their linear orders are identical, and their prosodies are quite similar. Presumably the innovation occurred when language learners interpreted sentences like (5.76a) as (5.76b).

(5.76a) That colour is greenISH, but it's more of a blue shade.

(5.76b) That colour is green (ish), but it's more of a blue shade.

The upshot of this is that the modern syntax of *ish* is not radically different from what it was a generation ago; it still bears the signs of its recent past, just as the position of new affixes depends upon the syntax of their M-word forebears. Although it is entirely possible that later developments might allow *ish* a freer syntax, so far this has not happened, and there is no guarantee that it will. This leads to an interesting question: what was the innovation, exactly? *Ish* has not forfeited its suffixal behaviours; it has simply gained a wider range of uses that do not require it to be a Sub-word. It also seems unlikely that adverbial *ish* is really a separate VI from suffixal *ish*; my own native speaker intuition is that there is only one qualitative *ish*, which can take scope over a syntactic object of variable size.²¹⁶

Finally, note that no obvious linguistic changes in English caused the exodus of *ish*, other than those involved directly. This is not an example of ancillary changes producing new M-words as by-products. All of the factors involved here – stress, semantics, and increasing permissiveness of attachment – affect only this single Vocabulary Item.

5.2.6 Germanic II

Another interesting – and recent – example of Germanic affix-exodus has occurred in German, Dutch, and Frisian, as discussed by Norde (2009). In these languages, the numeral suffix *-tig*²¹⁷ has become an independent quantifier, which in turn has developed into an adverbial intensifier. The suffix *-tig* is ancient; it is cognate with the English suffix *-ty* (e.g. *twenty, fifty, seventy*) and has an etymological connection with the Proto-Indo-European word for 'ten'. The original meaning of the suffix, therefore, simply meant 'multiple of ten', but the newly independent *tig* can mean anything from 'a few' to 'billions', as we will see. Although the origin of this construction is not known, it is worth examining, because there is no evidence for this particular change occurring as a by-product of other linguistic changes.

According to Norde, the earliest evidence for the independent life of *tig* is the 1957 volume of *Deutsches Wörterbuch*, which cites an example 'from a work written in 1935, but here it is stressed that *zig* can be used this way only "in jüngster Zeit".' She summarises the life story of *tig* as follows.

(5.77) PIE 'ten' > PGmc 'unit of ten' > PGmc 'x10' > Dutch 'many' > Dutch 'very'

In Dutch, Frisian, and German, *tig* is used as a quantifier meaning 'dozens, umpteen'. An ordinal use, with adjectival suffixes, is also possible, as in (5.78b).

²¹⁶ Conceivably, there might be inter-speaker variation on this point. This would make an interesting topic for a sociolinguistic research project.

²¹⁷ *Tig* is the Dutch version of this form; in German it is *zig*, and in Frisian *tich*.

- (5.78a) Die kerel heeft al tig vriendinnen gehad (Dutch)
 Dy keardel hat al tich freondinnen hân (Frisian)
 Der Kerl hat schon zig Freundinnen gehabt (German)
 that guy has already dozens girlfriends had
 ‘That guy has already had dozens of girlfriends.’
- (5.78b) Je vraagt dat nu al voor de tigste keer! (Dutch)
 Do fregest da no al foar de tichste kear! (Frisian)
 Du fragst das jetzt schon zum zigsten Mal! (German)
 you ask that now already for the dozenth time
 ‘Now you are asking that for the dozenth time already!’

As a quantifier, *tig* usually modifies count nouns (5.79a), with a meaning of ‘dozens of X’, but rarer examples of *tig* with mass nouns (5.79b) are also attested.

- (5.79a) Die Scholten woont nu al tig jaar in Bloemendaal.
 that lives now already dozens year in
 ‘That Scholten guy has been living in Bloemendaal for dozens of years now.’
- (5.79b) Maar ze verlangen ook tig ervaring.
 but they require also dozens experience
 ‘But they also require a lot of experience.’

Tig is not exclusively an attributive quantifier, however. It can be used independently, as in (5.80a–c), or even as part of a compound (5.80d–e).

- (5.80a) Er lopen bij Daimler-Benz tig van dit soort projecten.
 there run at dozens of this kind projects
 ‘DB has dozens of these kinds of projects running.’
- (5.80b) Suikerklontjes krijgen ze elke dag, een stuk of tig.
 sugar.cubes get they every day a piece or dozens
 ‘They get sugar cubes every day, dozens or so.’
- (5.80c) een tig of wat pilsjes
 a dozen or what beers
 ‘a dozen or so beers’
- (5.80d) een Dior-rok van tig- duizend gulden
 a Dior-skirt of umpteen-thousand guilders
 ‘an umpteen-thousand-guilders Dior skirt’
- (5.80e) tig-maal ingewikkelder
 dozens-times more.complicated
 ‘dozens of times more complicated’

Examples of erstwhile suffixes used as roots are often cited in the literature (e.g. *ism* or *ology*), and rarely taken seriously by grammaticalization specialists. These, they argue, are merely lexicalisations, not bona fide counterexamples to the unidirectionality rule. Generally, suffixes-turned-Roots are semantically hypernyms; the OED defines an *ism*, for example, as ‘a form of doctrine, theory, or practice having, or claiming to have, a distinctive character or relation: chiefly used disparagingly, and sometimes with implied reference to schism.’ Most of the OED’s example sentences use *ism* rather self-consciously, and in connection with words bearing the *ism* suffix, as for example (5.81a), from Lord Chesterfield, ca. 1773, and (5.81b), attributed to Shelley (Hogg 1858).

- (5.81a) It is full of Latinisms, Gallicisms, Germanisms, and all isms but Anglicisms.
 (5.81b) He is nothing,—no ‘ist’, professes no ‘-ism’ but superbism and irrationalism.

Tig, however, is in a different category from standard examples of lexicalisation. Semantically, *tig* is not a hypernym, in that it doesn't mean "multiples of ten", but rather any amount from a few to billions. Nor does it seem to occur primarily in contexts with words bearing its suffixed relative.

- (5.82a) met... *tig* gigabyte harde schijf
 with... dozens gigabyte hard disk
 'with a hard disk with dozens (~ hundreds) of gigabytes'
- (5.82b) Ons lichaam bestaat uit *tig* cellen.
 our body consists of dozens cells
 'Our body is made up of dozens (~ billions) of cells.'

Nevertheless, language users do seem to recognise it as the suffix: they often spell it as a suffix (5.83a) or in scare quotes (5.83b). It may be the case that *tig* has not become a Root, but a free-standing functional morpheme.

- (5.83a) Dat geldt ook voor de -*tig* andere instanties.
 that counts also for the dozens other authorities
 'That also holds for the dozens of other authorities.'
- (5.83b) Van Persie heeft '*tig*' begeleiders.
 has dozens coaches
 'Van Persie has dozens of coaches.'

In Dutch, *tig* has become a general intensifier.²¹⁸

- (5.84) maar *tig* leuk dat die in Portugal gaat voetballuh
 but very nice that he in goes play.soccer
 'But how very nice that he is going to play soccer in Portugal!'

This development is recent enough that no native speakers consulted by Norde, including Norde herself, are familiar with it, but it occurs frequently in informal internet sources and often in newspapers, and was noticed as early as 1985 by van Marle. The intensifier usage is possible with adjectives (5.85a), including comparatives (5.85b), and with adverbs (5.85c).

- (5.85a) Die telefoon is -*tig* lelijk, proolly -*tig* duur en duidelijk -*tig*
 that phone is very ugly probably very expensive and clearly very
 overkill
 overkill
 'That phone is very ugly, probably very expensive, and clearly overkill.'
- (5.85b) beetje jammer, middelburg is toch *tig* leuker
 bit pity Middelburg is still much nicer
 'Bit of a shame, Middelburg is much nicer after all.'
- (5.85c) Ik heb de film zelf ook *tig* vaak gezien.
 I have the movie myself also very often seen
 'I myself have seen the movie very often also.'

German allows the intensifier usage also; the examples here come from a Google search conducted by Norde (22/1/08). No examples have been found in Frisian so far (Hindrik Sijens, p.c. to Norde).

²¹⁸ Example found by Norde (2009:214) at www.voetbalzone.nl/doc.asp?id=6426, a forum of soccer enthusiasts 'characterised by colloquial constructions and spellings'.

- (5.86a) Ich liebe dich seit zig viel Jahren.
 I love you since very many years
 ‘I have loved you for many years.’
- (5.86b) Ich hab mir jedes Detail zig oft angehört.
 I have me every detail very often listened.to
 ‘I have listened to every detail dozens of times.’

Both the quantifier reading (5.87a) and the intensifier reading (5.87b) are permitted in some contexts; conceivably the intensifier semantics originated in sentences like this one.

- (5.87) Er zijn tig betere systemen op de markt.
 a. there are dozens better systems on the market.
 ‘There are dozens of better systems on the market.’
 b. there are very better systems on the market
 ‘There are much better systems on the market.’

As stated at the outset, the details of how this happened are completely obscure. One possible contributing factor may have been the fact that the suffix is not involved in close phonological processes with the base, as well as comprising a discrete syllable. The important point is that there is absolutely no evidence that *tig* developed due to changes happening elsewhere in these languages. This development seems to simply have just... happened. At most, the only other potential change involved here would be a reanalysis of these numerals as compounds, which is highly local.

5.3 Towards a Preliminary Typology

In Chapter Three, I was able to establish a rudimentary typology of different structural contexts for affix-genesis. Doing something analogous for affix-exodus is a more difficult task. We understand the structural requirements for affixation, and these requirements are specific enough that the realm of possible syntactic configurations is highly constrained. Our understanding of the structural limitations on affix-exodus is far less advanced; the examples in this section are nothing more than a miscellany, and there is no way to tell whether they are at all representative of the legitimate possibilities, let alone what types of affix-exodus are ruled out. The only obvious constraint is that the action must be restricted to the periphery, but this doesn’t take us very far.

Norde (2009:133) introduces a tripartite typology of de-grammaticalization, but she is concerned with a wider range of phenomena than I have been here. Her categories are summarised in (5.88).

- (5.88a) *Degrammation*: shift from grammatical content to lexical content;
 (5.88b) *Deinflectionalization*: shift from “more grammatical” to “less grammatical”, or movement out of a paradigm accompanied by a change in grammatical content;
 (5.88c) *Debonding*: shift from bound morpheme (affix, clitic) to free morpheme.

These categories invoke structure at only the coarsest level, since Norde’s primary concern is functional. All of the examples discussed here are examples of (5.88c), debonding, with the exception of the Germanic group genitive, which is considered an example of (5.88b) because the possessive is a clitic rather than an independent item. This distinction is certainly meaningful, and a useful first step. However, the debonding category is clearly structurally

diverse (consider, for instance, Saami *haga* and Irish *muid*), and therefore a more structurally detailed typology is needed.

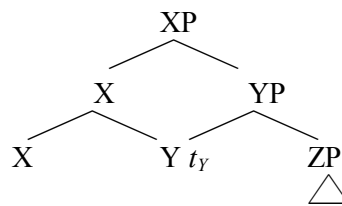
In 5.1, I noted that the logical place to begin is by “reversing” the typology developed in Chapter Three. For convenience, (5.1)-(5.2) are repeated here as (5.89)-(5.90).

- (5.89a) *Type I*: Acquisition of an additional movement operation;
- (5.89b) *Type II*: Reanalysis of a terminal as an exponent of a different structural position;
- (5.89c) *Type III*: Extension of an M-word boundary to include a former clitic;
- (5.89d) *Type IV*: Reanalysis of a compounded element as an affix.

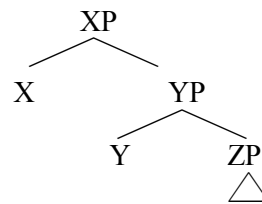
- (5.90a) *Type A*: Loss of a movement operation;
- (5.90b) *Type B*: Reanalysis of a terminal as an exponent of a different structural position;
- (5.90c) *Type C*: Retraction of an M-word boundary to exclude a peripheral affix;
- (5.90d) *Type D*: Reanalysis of an affix or compounded element as an adverbial.

In Type I affix-genesis, probably general to new verbal morphology, an additional movement operation is added, so that hierarchically adjacent projections form a single complex head. Translating this to Type A affix-exodus entails the dissolution of a complex head, along the lines of (5.91).

(5.91a) *P-speakers' Grammar*



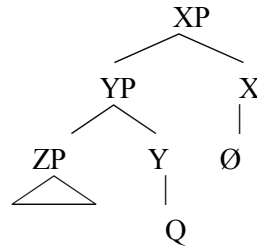
(5.91b) *Innovators' Grammar*



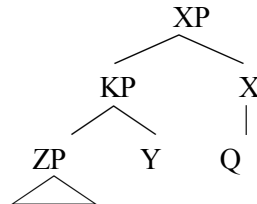
None of the examples discussed here are clear examples of Type A, although Irish *muid* could be an example if AgrS were a syntactic position instead of a dissociative morpheme. There is no obvious reason to rule this out *a priori* as a possible type of change, however; it seems at least theoretically possible. Judgement will have to be reserved.

Type II affix-genesis involves a change in the underlying structural position of a particular linguistic terminal, and is particularly characteristic of case morphology. Type B affix-exodus differs only in that the “before” and “after” locations of the morpheme are swapped:

(5.92a) *P-speakers' Grammar*



(5.92b) *Innovators' Grammar*



Saami *haga*, of course, is a clear example of Type B.

Type III involves enveloping an adjacent clitic; its logical counterpart, Type C, would involve cutting loose a peripheral affix, which seems both vague and hard to distinguish from Type A. English *ish* and Irish *muid* could, in principle, be taken to be examples, but these two examples are sufficiently different from each other structurally that including them in the same category may not be desirable. Worse yet, Irish *muid* could be equally well considered to be Type A or Type B. Type D would involve reanalysing an affix as a compounded element; this could underlie Dutch *tig &c.*, but the details of that case are not sufficiently clear.

So adapting the typology from Chapter Three to suit the case studies in this chapter turns out to be messy and problematic, except for Type II/Type B. Type A is clear both structurally and conceptually, but may or may not be attested; Types C and D are not well defined; and the majority of the examples aren't obviously members of one category or another. This is disappointing, but possibly to be expected, given that affix-exodus, unlike affixation, does not seem to require precise structural conditions.

At the moment, pending the discovery of more data, we are limited to identifying as known the possible types of affix-exodus exhibited by the examples.

- (5.93a) Word-level affixes can become phrase-level affixes (de-inflectionization);
- (5.93b) Erstwhile affixes can be reanalysed as exponents of a different structural position which places them outside the M-word they formerly occupied;
- (5.93c) Agreement morphology can be reanalysed as primary exponents of what they formerly agreed with if they are linearly adjacent to the relevant syntactic category.

5.4 Learnability

In Chapter Three, we discussed the notion of “simplicity”, often invoked in motivating grammaticalisation-type changes (cf. e.g. Roberts and Roussou 2003). Under the theoretical assumptions made in Chapter Two, “simplicity” amounts to the hypothesis that children acquire the “simplest” grammar consistent with their input data.²¹⁹ Though this hypothesis certainly sounds reasonable, it is unhelpfully vague, since it leaves unaddressed

²¹⁹ I just put this footnote in to see if anyone would read it.

the child’s rubric of “simplicity”. Children and adults – particularly adults who are linguists – may have very different standards for simplicity. No adult linguist would be satisfied with the system of partial affixation exhibited by Modern Irish subject pronouns; and while it is true that Irish has gradually shifted towards losing the synthetic verb forms (as Scots Gaelic has done), the shift has been gradual enough to demonstrate that Irish-learning children aren’t sufficiently bothered by it to do something radical.

Affix-exodus is often viewed as “exceptional”, even “unnatural”, by those who study it. It is something unexpected, something that isn’t supposed to happen. This position is somewhat understandable if the notion of simplicity involves a component meant to minimise the number of words in a sentence; if one word is always “simpler” than two words, there is no obvious way to motivate changes involving innovators acquiring a two–M-word analysis when P-speakers had one. Such a reanalysis would represent an undesirable, unmotivated complication to the grammar. The only recourse, if one holds such a view of linguistic change, is to allow affix-exodus to be a by-product of other linguistic changes, or the result of analogy (cf. Kiparsky 2011).

However, if one’s notion of “simplicity” does *not* include, by necessity, a reference to the preferred number of M-words, affix-exodus is in no way anomalous. Crucially, the hypothesis of simplicity invoked here omits such a provision. We still need to explain the clues language learners might be using to lead them to a two-M-word analysis, and under what circumstances two words are “simpler” than one, but affix-exodus itself need not be seen as a kink in the works, so to speak. In other words, affix-exodus is part of the same constellation of possibilities as affix-genesis; rarer, perhaps, but hardly beyond the bounds of the theory.

We need to put this into the perspective of what the learners are really doing when they make a decision that produces a novel result, and this takes us back to the assumptions laid out in Chapter Two. Learners *do not know* what their parents’ analysis is, or their siblings’, or their friends’. Their task is to decide whether two linguistic terminals they have identified – call them *X* and *Y* – are part of the same M-word *Z* or not. If they decide that yes, they are, then hereafter they will regard *X* (say) as a Sub-word in *Z*; if not, then *X* is an M-word and *Y* is a segment of *Z* (potentially the only one). This is the *exact same choice* facing all children attempting to learn this language, and there are really only four possible outcomes:

(5.94a) Generation <i>P</i> : two M-words	Generation <i>P</i> +1: two M-words
(5.94b) Generation <i>P</i> : two M-words	Generation <i>P</i> +1: one M-word
(5.94c) Generation <i>P</i> : one M-word	Generation <i>P</i> +1: one M-word
(5.94d) Generation <i>P</i> : one M-word	Generation <i>P</i> +1: two M-words

Here is the crucial question: why does an innovator posit a two–M-word analysis when previous generations have posited a one-M-word analysis? Affixes, once generated, *do* tend to stay put, so under what circumstances do we get (5.94d)? But this question has a hidden presupposition: that the language learner *must* opt for a one-M-word analysis whenever possible, by some sort of cognitive bias. This presupposition is one of the tenets of the unidirectionality hypothesis – but it is incompatible with the theoretical assumptions laid out in Chapter Two, as well as the assertion above that the acquisitional notion of “simplicity” does not involve counting M-words.

Instead, my assertion is that language learners do not have a bias towards seeking a one–M-word analysis whenever possible – although they probably *do* have a bias in favour of equating morphosyntactic words and phonological words, which in many cases may create the illusion that they *do* seek to minimise M-words. The learners are, moreover, confronted by the same analytic choice in each case irrespective of issues of directionality: the *result* of their analysis of the data is different, but the *process* whereby they arrive at their analysis is essentially constant. Therefore, rather than ask “why choose two M-words when you can

choose one”, we should instead be asking “what in the data guides speakers to choose one M-word or two M-words”.

Interestingly, this essentially reduces the problem to a slight variation on what is perhaps the most perpetually thorny diachronic problem, the actuation problem: why does a given change occur at the time that it does? If there is no readily discernible difference between the grammars of Generation *P* and Generations *P-1...-n*, but only the former decided terminal *X* was an affix rather than an M-word, then why didn't the previous generations decide *X* was an affix? The only difference between this scenario and the affix-exodus scenario is which generation plays the role of the generation that foolishly didn't realise it could have an affix.

In order to determine what in the data biases a two-M-word analysis as opposed to a one-M-word analysis, we have to look at the data already discussed in this chapter in the context of the same learnability considerations we examined in Chapter Three: cues prompting the children. If, as assumed, children use a specific inventory of tools in language acquisition, they should be looking for evidence in the input data in the same way, regardless of whether they acquire one M-word or two. This is because the nature of the data itself is constant – a string of continuous sounds, which the children must break into smaller chunks – just as the child's task is constant. Therefore, the process by which the child accomplishes the task must also be constant.

Based on the nature of the available data, we assumed before that the child relies first and possibly foremost on phonological data, particularly prosody, since prosodic information provides crucial hints for the location of morpheme boundaries on various levels. If there is a bias towards equating phonological words with morphological words, an entity bearing word-level stress is less likely to be analysed as a Sub-word.

A quick review of the case studies in this chapter reveals several references to prosody. Recall, for instance, that in Irish, the prosodic structure of synthetic *támuid* ‘we are’ and its analytic equivalent *tá sinn* are identical. Although the latter's syntactic behaviour is slightly different from that of the former, a language learner may well conclude that *muid* and *sinn* are both pronouns. Meanwhile, in Northern Saami, words receive primary stress on the first syllable; in four-syllable words, the third syllable receives secondary stress. When added to a disyllabic stem, a disyllabic case suffix thus receives secondary stress on its first syllable. If the child mistakes the secondary stress for primary stress, he may then further conclude that the base and suffix are two separate M-words. Finally, the tendency for speakers to place contrastive stress on the adjectival suffix *-ish* could have led language learners to conclude that *ish* was an independent M-word.

Segmentability is another potential factor. Many of the examples discussed in this chapter – Saami *taga*, Irish *muid* (and the dialect forms *mar* and *dar*), and Dutch *tig* and its cognates – are all consonant-initial syllables, which means that they never have variable onsets. This in itself would not motivate a language learner to posit the syllable as a separate word, but if the prosodic evidence has already biased him in that direction, the shape of the syllable might be seen as corroboration. English *ish*, despite being vowel-initial, is also easily segmentable, both because it doesn't interact with the phonology of the adjective to which it attaches (with the result that it exhibits no allomorphic variation) and because its morphological structure is completely transparent. In addition, the evidence for word-level prosody with *ish* is contrastive emphasis on the morpheme itself, not necessarily the syllable containing the morpheme. The loss of final vowels in Estonian left the particles *ep* and *es* easily segmentable also, in that they were added to a base that speakers would have recognised as an M-word.

The phonological evidence in the Estonian example is due to the vagaries of sound change; the evidence for Northern Saami *taga* came in part from sound changes affecting an allomorph of *taga*, which had become reduced. While we could argue that these cases demonstrate how affix-exodus is a by-product of linguistic changes, this is pushing the conclusion too far. It is true that the Estonian and Saami innovators were able to come to a

novel analysis unavailable to their parents because the language – and, thus, the input data – had changed. But this is not fundamentally different from the role of sound change in affix-genesis: vowel reduction, leading to a change in underlying form and thence to a new affix, is a classic feature of some types of affixation. This, too, is a type of sound change. In fact, in Chapter Two I argued, following Newmeyer and others, that the phenomena discussed in this dissertation are *all* by-products of other linguistic changes, since semantics, syntax, and phonology are all involved. Therefore, while it is correct to conclude that linguistic changes are relevant to affix-exodus (how could they not be?) this does not in any significant way distinguish affix-exodus from affix-genesis, and it certainly does not mark it as an anomaly.

Linear order has been identified as a crucial factor in affix-genesis, as discussed at length in Chapters 3 and 4. One could argue that affix-exodus is significantly different from affix-genesis on the grounds that linear order is not involved in the former. In fact, however, there is no clear evidence that this is the case; many of the examples discussed in this chapter and elsewhere simply aren't understood in sufficient detail to prove one thing or another. Moreover, two of the examples discussed here indicate that linear order can affect the syntactic freedom of a new M-word, at least for a period of time.

Muid, the only novel pronoun accepted in Standard Irish, can be used interchangeably with *sinn* in the modern language, but Nilsen (1974:114) reports that in some late nineteenth century authors were reluctant to use *muid* as anything other than a subject pronoun, preferring *sinn* elsewhere. Since *muid* developed from a former verbal ending, this restriction makes some sense. For P-speakers, *muid* was always adjacent to the verb because it was a verbal ending; innovators concluded it was a pronoun, but acquired the rule that it could only appear adjacent to the verb. Eventually, these constraints were relaxed. In addition, the dialectal forms 1st.pl. *mar* and 3rd.pl. *dar* continue to be used only in the preterite tenses – an unusual restriction for pronouns, but related to the fact that they were formerly preterite verb endings. They must be adjacent not only to T, but to a specific feature value on T.

Meanwhile, English *ish* has a much more tightly constrained distribution than most English adverbs, including its near-synonym *sort of*. (5.96) is repeated here for convenience.

(5.95a) I can accept the premises – well, ish.

(5.95b) They have a happy ending – ish.

(5.96a) * I ish can accept the premises. (cf. *I sort of can accept the premises.*)

(5.96b) * I can ish accept the premises. (cf. *I can sort of accept the premises.*)

(5.96c) * They have an ish happy ending. (cf. *They have a very/sort of happy ending.*)

Ish does have more syntactic freedom as an adverb than it does as a suffix, as well as wider scope, but it still must occur on the right edge of the constituent it modifies, which means that it bears approximately the same linear relation to this constituent as the adjectival suffix bears to the adjectival stem. This is strikingly reminiscent of its distribution as a suffix, and indicates that these constraints on the placement of *ish* are not random. It may well be the case that future generations will allow *ish* the full freedom of adverbs, just as *muid* was eventually permitted greater freedom, but in the modern grammar it remains restricted.

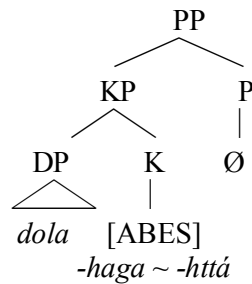
This is an interesting result, and one that has often been ignored in discussions of these phenomena: innovators may conclude that an erstwhile affix is an M-word rather than a Sub-word, but they don't necessarily ignore adjacency rules in the input data when they do so. In fact, there is one respect in which affix-exodus and affix-genesis are precisely parallel: they both involve adjacency to an M-word boundary. New affixes appear on the periphery of the M-word to which they were formerly adjacent, in the same linear position; ex-affixes are lost from the periphery of M-words. Both, therefore, were linearly adjacent to the periphery of an M-word before and/or after their change in status.

What, then, ultimately determines a child's choice of Sub-word or M-word for a particular terminal? Unfortunately, there are so few detailed documented examples of clear

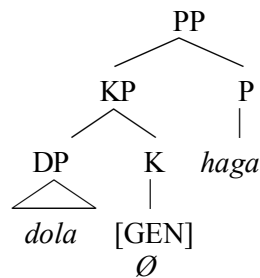
affix-exodus that a definitive answer is hard to come by, but we can make some initial observations. In both Chapter Three and the current chapter, the discussion began with phonology, because this is the first and most salient information available to the child; and perhaps this is where the child acquires his first hypothesis. If a terminal has a reduced vowel, the child is more likely to view it as a Sub-word, regardless of what his ancestors did. If a terminal can bear stress, the child might view it as an M-word, again regardless of what his ancestors did. Sound changes can make one or the other outcome more probable.

Although there are not many direct parallels between the case studies in this chapter and those in Chapter Three, the Saami example is an exception: three of the examples in Chapter Three involve adpositions becoming case affixes, while Saami *haga* does the opposite. We can compare *haga* to the case studies in Chapter Three to see if there are salient differences. (5.97) shows the grammars underlying the affix-exodus of Saami *haga*, which are essentially the mirror image of the structures of the affix-genesis of Persian *rā* in (5.98).

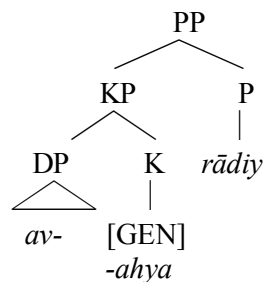
(5.97a)



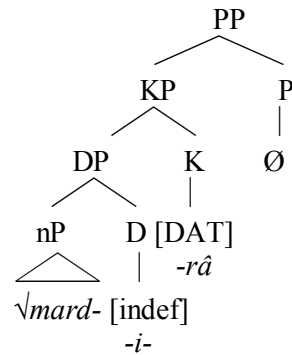
(5.97b)



(5.98a)



(5.98b)



What details of Saami and Persian might have influenced the innovators? Saami has case suffixes and adpositions, the latter often a source of the former. Because of sound changes, the abessive case showed significant allomorphy, in that one allomorph was a single unstressed syllable, and the other was disyllabic and received secondary stress. Finally, the genitive case suffix was null, so that a genitive-marked noun was formally identical to the bare stem. The abessive was thus genuinely ambiguous between postposition + genitive and suffixal abessive. This, combined with the secondary stress on *ha-* and the fact that the formal differences between the two abessive allomorphs probably rendered their relationship opaque, nudged innovative speakers towards the decision that *haga* was a (clitic) postposition rather than a case suffix.

In Persian, the situation was rather different. Unlike Saami, Persian was not a postpositional language; it was a prepositional language. This meant that *râ* was synchronically aberrant. Persian had also lost case distinctions, so there was no case marker telling learners that *râ* was an adposition. In addition, Persian nouns are typically stressed on the final syllable, not counting pronominal or other suffixes (Levy 1951:26), so *râ* was unstressed. The lack of stress was likely to bias language learners towards denying it M-word status, but the unique position of *râ* as the only postposition in a prepositional grammar would have provided all the more reason for language learners to make it a case suffix.

Note that the very same factors are influential in each case: prosody; the arrangement of the grammar; even the lack of case marking in Persian and the null-marked genitive case in Saami are similar in kind. But the overall evidence in Persian suggested a different story from that in Saami. Language learners in each case used the same kinds of data, but came to different results because of the specific details of that data.

In other words, what the learner decides depends on what the data are telling him. This is not a particularly satisfying result, because of its lack of predictive power; it may, however, be the best we can do until we have a better idea of how people make analytical decisions in ambiguous situations. Some categorical distinctions are essentially arbitrary; we know, for instance, that different people will make slightly different decisions as to the line between green and blue, or red and orange. If the data are indeterminate between X and Y comprising one M-word and X and Y comprising two M-words, it is extremely likely that different people will settle on different solutions.

5.5 Chapter Summary

In this chapter, I have argued that affix-exodus is no less “natural” than affix-genesis; both are simply products of the language acquisition process which happen to differ from the grammars of previous generations in an interesting way. One important point to remember is that affix-exodus differs from the ordinary acquisition of sequences of M-words only in that the resulting grammar is innovative; from the perspective of the child who has acquired it, it

is unremarkable: he has arrived at an analysis of his native language based on the same cues any other child would have used. Both affix-genesis and affix-exodus fall into the same category of possible morphological changes; neither is more “natural” than the other.

The obvious objection to this logic is statistical: affix-genesis is overwhelmingly more common than affix-exodus; there must be an explanation for that; ergo, one is natural and the other is not. But we need to think seriously for a moment about how we know for certain that affix-exodus is as rare as we think it is.

Affixes originate as independent M-words: this is not in question. It is also true that we have no other obvious sources for new affixes, and that affixes are not typologically rare. All of the many affixes we see in the world’s languages had to have come from somewhere; we have yet to find another source for them; ergo, affix-genesis is common. Meanwhile, we don’t often see affixes popping off; once they appear, they tend to stay put. Therefore, it is not unreasonable to conclude that affix-exodus is *uncommon*.

But there are some unresolved problems with this hypothesis. First, once an affix has popped off, it is – at least in theory – undetectable, since it has become simply part of the vocabulary. The suffixed stage of Saami *haga* is preserved in exactly one fossilised lexeme; without comparative evidence, we might easily believe it was never anything else. Second, if we review the examples in this section, it will appear that if we did not already *know* which direction the innovation went, we would quite likely conclude that the M-word stage was the earlier. Verb endings are generally believed to come from pronouns, so Irish *muid* would be considered the progenitor of the verbal ending; English *ish* is used both as an adverb and an adjectival suffix, and we might consider the latter the newer case; and one could probably construct a plausible argument for the new quantifier *tig* as the ancestor of the numeral suffix. How many other examples in the literature typically considered affix-genesis might actually be affix-exodus? There is no way to tell.

Another point is that, ubiquitous as affix-genesis ought to be (given how many affixes we find), there are remarkably few clear-cut cases of it documented in the records available to us. Some of the world’s languages have been written for thousands of years, and yet it is *very* difficult to find these examples. None of the examples in Chapter Three were as straightforward as affix-genesis is typically made out to be, and these were the best, least complicated verifiable cases I was able to find. Nor are there many discussions in the literature of affix-genesis in progress; I have the impression that, if anything, there are more reported cases of *affix-exodus* in progress. What are we to make of that? It must have taken a very long time for the languages attested today to acquire their morphology. More than that is hard to say.

Affix-genesis is also easier to look for, because it requires specific structural conditions. Local Dislocation – superficially difficult to differentiate from structural affixation – is more permissive – and so is affix-exodus, as far as we can tell. The case studies of Chapter Three fell more or less neatly into specific categories, but those of the current chapter have a rather miscellaneous feel. It is *far* easier to find things if you have a notion of where to look for them.

One final complicating factor: once affix-exodus has occurred, the new M-word enters the general vocabulary. It is notoriously easy to acquire new vocabulary words; long after our native grammar has been fixed, we continue expanding our vocabulary until some cognitive crisis occurs. This is relevant because it means that, at least in some instances, fewer people would be required in order to spread innovations involving affix-exodus. Consider *ish*, for example. A single person who, as a child, arrived at the innovative analysis for *ish* could spread the innovation to other adult speakers via contact; the conservative adult speakers would essentially be “borrowing” the adverbial *ish*, either in isolation or (more likely) as an extension to their own conservative usage. In the latter case, the adult speakers would become bi-dialectal. Whether or not the innovation spreads to conservative speakers may depend on whether or not the original item remains in use; one could envision a pattern of extension and retention rather than a reanalysis taking place within a single generation and

without the retention of the affixal grammar.²²⁰ The latter cases would more closely resemble affix-genesis; new affixes are less likely to be borrowed by adult speakers, because the output of the conservative and innovative grammars will be, on the surface, the same linear string. The differences between the conservative and innovative grammars will therefore typically be more subtle in the case of affix-genesis.

Affix-exodus is generally considered a rare phenomenon that is essentially a by-product of significant changes in the language; this is meant to be what sets it apart from affix-genesis, which is “natural” and in some way organic. However, affix-genesis is itself a by-product of previous linguistic changes – in phonology, semantics, and potentially syntax – and is therefore not truly significantly different from affix-exodus in this sense. The difference is merely that different kinds of changes precede affix-exodus than precede affix-genesis; but this is expected, given that affix-genesis and affix-exodus are different. The fact that affix-exodus is a by-product does not make it weird, unnatural, or deviant. We simply have significantly less understanding of the sorts of changes likely to precede it.

In fact, there is reason to question whether affix-exodus (but not affix-genesis) *must* be a by-product, given the case studies discussed in this chapter. The Estonian, Saami, and Irish examples were as expected; in each case, the role of linguistic changes elsewhere in the grammar could be clearly identified. Although the group genitive in Germanic languages is traditionally considered another example of the same, this theory is called into question by the data, since the precise chronology of the breakdown of case-marking in these languages does not entirely correlate with the unmooring of the possessive marker. Finally, the last two examples discussed – English *ish* and Dutch *tig* – are not obviously connected to anything more elaborate than the simple facts of the change itself, though the lack of evidence for early stages of the latter development could in principle be obscuring something. The clear implication is that affix-exodus can be the by-product of very local changes rather than changes that affect large portions of the grammar.

I began this section by pointing out that both affix-genesis and affix-exodus fall into the same general category of “possible morphological changes”; in fact, they are also members of a more exclusive category. As mentioned several times throughout the course of this chapter, we have a very poor understanding of differences amongst individual language learners, and the role that this might play in the why and where of linguistic changes. While we can identify factors that might influence language learners to make one analytic decision or another, the choice is never deterministic, and we cannot rule out the possibility that there is an element of arbitrariness involved, not unlike the decision of where to draw the line between blue and green.

The metaphor of “drawing the line” was chosen intentionally, because in a sense, both affix-genesis and affix-exodus involve precisely this: they each reduce to a choice made in delineating an M-word that differs from the choice made by previous generations. Affix-genesis is essentially a child’s making a more inclusive decision in placing M-word boundaries; affix-exodus involves a more restrictive decision.

These are not the only diachronic phenomena which involve changes in the placement of linguistic boundaries; in fact, there is a constellation of such phenomena: some very familiar in the literature, others less so; some involving M-words, others Sub-words. The next chapter will survey these related phenomena, placing affix-genesis and affix-exodus in a wider context of diachronic changes affecting morphemic boundaries.

²²⁰ This is an area in which further investigation is needed, in order to gain more insights into which patterns are more likely to be found with which categories of innovations. The overall time course this would take also requires further investigation.

Chapter Six

Morphological Re-cuttings and Complex Head Disintegration

6.1 Overview

In Chapter Five, I argued that de-affixation is neither unnatural nor particularly exotic, but rather part of a constellation of changes affecting morpheme boundaries. In this chapter, I will discuss two other types of morphosyntactic change that have certain properties in common with affix-exodus in particular. One of the phenomena I will discuss is very well-known in the literature; the other, to my knowledge, has not been previously discussed.

Morphological re-cutting, the subject of 6.2, is quite familiar to historical linguists, and also a perpetual favourite with non-linguists who have an interesting in quirky linguistic facts. Like affix-genesis and affix-exodus, re-cutting involves a shift in the placement of a morpheme boundary, with the difference that the change affects only the most superficial layer of grammar: the phonological shape of the terminals involved. The number of pieces, and their function and identity, remains constant, but the locus of the M-word or Sub-word boundary shifts to fall between a different pair of segments.

The other phenomenon, by contrast, occurs at a deep level of structure. Complex head disintegration, introduced in 6.3, occurs when Sub-words which were once combined into a single M-word can no longer be thus combined, owing to changes in the grammar. Rather than result in a novel M-word, as in de-affixation, the language resorts to a periphrastic construction already available to it. Although analytic and synthetic constructions are often found in competition with each other, the aftermath of complex head disintegration is different; rather than simple variation, one of the forms is either ungrammatical or strongly dispreferred in some contexts. As this is a phenomenon which has not previously received much attention in the literature, there are not many known examples, and therefore the types of change which can produce it are currently not well-defined.

6.2 Morphological Re-cutting

Morphological re-cutting can be defined as reanalyses in which phonological material previously part of one Vocabulary Item is reanalysed as belonging to an immediately adjacent Vocabulary Item. Such phenomena are well known; in English, for instance, there are a number of words which either lost an etymological initial [n] or gained a spurious [n] due to the alternation of the indefinite article *a ~ an*.²²¹

- (6.1a) *Words which lost an initial [n]*
nadder → adder (cf. Old English *næddre*; Old Irish *nathir* ‘water snake’)
napron → apron (cf. French *napperon*)
nauget → auger ‘carpenter’s tool’ (from Old English *nafu-gár*)
nought → ought (originally *ne aught*)
nettle → ettle (non-standard)
noumpere → umpire (from Middle French *nonper* ‘peerless’)

²²¹ The → here indicates a change that does not involve regular sound change. I have been somewhat lax about this notation elsewhere, but the examples in (6.1) could in theory be confused for some sort of regular sound change, so I’m being stricter here.

- (6.1b) *Words which gained a spurious [n]*
 alp → nope ‘bullfinch’ (regional)
 anes → nonce (from the phrase *to ban anes*)
 ekename → nickname
 ewt → newt (cf. Old English *efeta*)
 otch → notch (cf. French *hoche*)

In addition to the innovative forms which survived to replace the originals, there were quite a few other forms which appeared as variants without entering the standard language; for a remarkably thorough investigation, cf. Scott (1892). As part of a similar confusion, a number of hypocoristic forms of common English names were derived from the collocation *mine X*; so, for instance, *Nan* from *Ann* via *mine Ann*, and similarly *Ned* from *Edmund/Edward/Edwin*, *Nell* from *Ellen/Helen*, *Netty* from *Esther*, and the rarer *Neps* (from *Elsbeth*), *Noll* (from *Oliver*), and *Nutty* (from *Ursula*).

Special mention should be made as well of the word *another*, which is (still somewhat transparently) a compound of the indefinite article *an* and *other*. The wordhood status of *another* is interesting, in that it generally behaves like a single M-word (as reflected in the orthography), and yet still allows other M-words to intervene between its components. Interestingly, the most natural such division for many native speakers, including myself, is not between the etymological components *an* and *other*, but between *a* and *nother*, as in:

- (6.2) a whole nother story

English is not the only language to exhibit such phenomena, which seem particularly common when the word in question is an unfamiliar borrowing; consider, for instance, the many Arabic loan-words in European languages which were borrowed with the article *al-* as part of the lexeme (e.g. *alcohol*, *algebra*). Nor is the phenomenon limited to Indo-European languages. Lynch (1991:226) briefly discusses the case of the Oceanic language Motlav, which borrowed *naba* ‘number’ from Bislama *namba*. In Motlav, the initial *na-* was reanalysed as a prefixed article, so that when the noun is used with other prefixes, *na-* disappears.

- (6.3) la- ba vōyō
 LOC-number two
 ‘second’

Morphological re-cutting, and the nature of the reanalysis underlying it, is similar to de-affixation in certain respects. Like de-affixation, it involves different analyses of the locus and nature of a morphosyntactic boundary between Generation *P* and Generation *P+1*. Notice, for instance, the similarity between the simple re-cutting in (6.4) and the re-analysis of Irish person/number endings in (6.5):

- (6.4) a nadder → an adder

- (6.5) [V-Agr] pro → [V-Agr=pro]

In a sense, therefore, de-affixation and re-cutting are the same phenomenon occurring at different levels of the grammar: while re-cutting occurs fairly superficially on linear phonological strings, de-affixation has syntactically significant consequences. Both arise out of highly language-specific contexts and may or may not occur as a product of other recent changes to the grammar. The primary difference between them is that morphological re-cutting affects only the placement of morpheme boundaries, rather than their typing.

This parallel can be seen more clearly when we examine cases of morphological re-cutting that have more extensive grammatical consequences than cosmetic alterations to Sub-words.

Adult speakers often produce novel creative forms via re-cuttings, even when the source is clear. Fruehwald (2010) points to the case of the new cran-morph-like *-doodle* in the names of hybrid poodles:

- (6.6a) Q. What do you call a Labrador-Poodle crossbreed?
A. A Labradoodle.
(6.6b) Q. What do you call a Golden Retriever-Poodle crossbreed?
A. A Goldendoodle.

The sequence *doodle* is motivated in the original form, *labradoodle*: *labrad-* from *labrador*, *-oodle* from *poodle*. But the *d* in the analogous *goldendoodle* is unmotivated: here we have *golden* + *oodle* with a spurious *d* connecting the two. There is nothing phonotactically illicit about the hypothetically possible * *goldenoodle*, and the compound *labradoodle* is essentially transparent. Nevertheless, *adult* speakers recut the form at *-doodle* rather than *-oodle*. This could have been motivated by an obscure preference for morphemes beginning with consonants, so that syllable boundaries always coincide with morpheme boundaries; if so, it is worthy of further investigation.

Another example is *outro*, used in video-editing circles to refer to a sequence of closing graphics, parallel to *intro*; the earliest attestation cited in the OED is 1967.²²² *Intro* is of course derived via truncation from *introduction*; there is an etymological connection between the prefix *intro* (from the adverb *intrō* ‘to the inside’) and the English/Latin preposition/adverb *in*, but the connection is remote. *Outro* was presumably created by conscious analogy via replacement of *in* with the completely non-Latinate *out*. The form is *outro*, not *outtro*, making the status of the [t] ambiguous. The most likely segmentation, however, is *out-ro*, which technically contains a partial Latin adverbial suffix.

These examples are deliberate creations on the part of adult native speakers via manipulation of linear strings of phonemes, resulting in new marginal morphemes (in the case of *-doodle*) or simply nonce forms. But similar innovations can occur in the speech of language learners: rather than simply adding or deleting a segment, language learners sometimes produce new phonological shapes for old morphemes which enter the language and become productive or semi-productive. Several such examples are attested, and these will be discussed in this section of this chapter. 6.2.1 is concerned with the emergence of a new allomorph of a verbal prefix in Latin subsequent to sound changes; 6.2.2 with the innovative Greek aorist passive suffix; 6.2.3 with Palauan emphatic pronouns and Blevins (2010)’s analysis of the origin of some spurious velar nasals in the same language; and 6.2.4 with the Polynesian passive suffix, made famous by Hale (1973).

6.2.1 Latin

Though the scope of the innovation is not particularly sweeping, Latin is nevertheless notable for having developed a new prefix allomorph by virtue of re-cutting. The same factors that led to this re-cutting were also responsible for alterations in the form of the root.

The original present stem of the verb ‘give’ in Latin was the reduplicated form **di-da*, which was formed to a zero-grade root. This stem is not attested directly in Latin, but it is attested in the related language Vestinian as (thematized) *didet*. Latin, like many Indo-European languages, formed compound verbs consisting of adverbial prefixes added to the present stem, resulting in forms like **re-dida-* ‘give back’ and **eks-dida-* ‘give out’. At some

²²² I am grateful to David Kowarsky (p.c.) for bringing this to my attention.

point in the prehistory of Latin, the reduplicating syllable of compound verbs was syncopated and the resulting geminate was simplified after consonants and long vowels.

- | | | |
|--------|-------------------|------------------|
| (6.7a) | <i>*con-dida-</i> | <i>*dē-dida-</i> |
| (6.7b) | <i>*con-dda-</i> | <i>*dē-dda-</i> |
| (6.7c) | <i>con-da-</i> | <i>dē-da-</i> |

Geminate simplification occurred only after consonants and long vowels; preverbs ending in short vowels were not subject to the sound change. There were only three such preverbs in the language, one of which, *ante-*, did not occur before any of the relevant stems, and a second, *ce-*, occurred only in a single fossilised imperative (*cedo* ‘give it here’). The third preverb ending a short vowel was *re-*, and as a result, the form **re-dida-* escaped the general trend of geminate simplification. The eventual result was the re-cutting in (6.8c), which resulted in a new allomorph *red-* for the prefix *re-*. *Red-* became particularly productive before vowel-initial roots.²²³

- | | |
|--------|------------------|
| (6.8a) | <i>*re-dida-</i> |
| (6.8b) | <i>*re-dda-</i> |
| (6.8c) | <i>*red-da-</i> |

The crucial steps in this process are the loss of the syncopated vowel and the geminate simplification *in the underlying representation*. As long as speakers were aware that the vowel and geminates were present underlying, they would have had no cause to be confused about the status of the unsimplified geminate. But when that information was no longer recoverable, segmentation of the reduplicating [d] was no longer straightforward. Learners now had two choices. Segmenting the “extra” [d] with the Root would require positing a phonotactically bizarre phonological shape for it; worse, if the compound structure were still transparent, they would be positing a consonant eliminated in the other forms of the verb. Segmenting the [d] with the prefix, on the other hand, required that the preverb *re-* have two allomorphs, with and without a final consonant. From the choice speakers made, the latter was preferable, which suggests that the compound structure of the verb was likely still transparent.²²⁴ At the time of acquiring the new preverb, speakers would have had no way of knowing that their new posited allomorph occurred with only one form, and since the new allomorph spread, this potential deterrent was eventually eliminated. Thus, the erstwhile first member of the geminate cluster was considered to be part of the prefix rather than the root.

Despite its small scope, the Latin example illustrates several important points, not least among them the fact that learners may prefer affixal allomorphy to root allomorphy. This particular reanalysis arose in the context of sound changes, but it would be incorrect to label the sound changes their immediate source. The catalyst here was not the sound changes themselves, but their effect on the phonotactics of the language and the nature of the evidence

²²³ Ringe (p.c.) notes a further interesting morphological consequence of these sound changes: the new compound allomorph *-da-* replaced the original **dida-* in the uncompounded present, which gave rise to attested forms with short *-a-*: *damus* (1st.pl.), *datis* (2nd.pl.), *dare* (inf.), *dabV-* (fut. stem), *dabā-* (impf. ind. stem), *darē-* (impf. subj. stem), and so on. Had the stem remained *dida-*, the short *a* would have become *e* or *i* via a regular sound change affecting (contemporaneous) unstressed syllables. As a result, these forms are anomalous, both in Indo-European in general, where most languages have reflexes of the inherited reduplicating form, and in the grammar of Latin, where the verb ‘give’ is the only present stem in the language that ends in short *-a-*. This was not, of course, a re-cutting *per se*; it is interesting nevertheless, in that one may not expect the compounded forms to influence the uncompounded. In the absence of direct evidence, unfortunately, it is impossible to say how this change began or spread.

²²⁴ Aaron Dinkin (p.c.) reminds me that *re-* is not the only preverb with allomorphy; the prefix *ē ~ ex* exhibits similar pattern of allomorphy.

available to the children when they were endeavouring to segment the linear string into meaningful chunks. The presentation of the examples earlier may have slightly obscured the chronology here, which was presumably something like this:

- (6.9a) The relevant sound changes are present as low-level surface rules; children are capable of segmenting the morphemes in the manner of previous generations.
 (6.9b) The relevant sound changes are reinterpreted as part of the underlying forms; children make novel segmentations.

The children's reanalysis of the data is predicated on their ability to recover the adult rules from the surface forms. Once the data were no longer sufficiently transparent, innovations occurred.

6.2.2 Greek

A likely example of morphological re-cutting completely divorced from any previous sound changes is attested in Greek. Ancient Greek inherited a category of intransitive aorists formed with a suffix $-\bar{e}$ - (-η-), which developed into a passive suffix in Classical Greek (6.10a). Already in Homer, the original suffix $-\bar{e}$ - (-η-) was in the process of being ousted by an innovative suffix $-t^h\bar{e}$ - (-θη-), also with intransitive/passive functions (6.10b). In some cases (6.10c), both formations are attested with the same verbal root (sometimes with differences in vocalic grade); for a list, cf. Chantraine (1945:169).

- (6.10a) $k^h ar-\bar{e}$ - (χαρ-η-) 'rejoice'
 $h rag-\bar{e}$ - (ράγ-η-) 'get broken'
 (6.10b) $klin-t^h\bar{e}$ - (κλιν-θη-) 'get bent, swerve'
 $do-t^h\bar{e}$ - (δο-θη-) 'be given'
 (6.10c) $\bar{e}gg\acute{e}l-\bar{e}$ - (ἔγγελλ-η-) ~ $\bar{e}gg\acute{e}l-t^h\bar{e}$ - (ἔγγελλ-θη-) 'be announced'

Greek specialists (cf. Chantraine 1925:105–6, Risch 1974:253–4) agree that the only plausible source for $-t^h\bar{e}$ - (-θη-) is some kind of morphological recutting of a root ending in $-t^h$ - (-θ-). This is plausible because there are a handful of paired homophonous roots with and without a final $-t^h$ - (-θ-) which happen to have the appropriate semantics. Consider, for instance, the pair of roots in (6.11).

- (6.11) $pl\bar{e}$ - (πλη-) 'fill'; passive 'get filled/be full'
 $pl\bar{e}t^h$ - (πληθ-) 'be full'

As Ringe and Eska (forthcoming) have pointed out, these roots have exactly the right difference in meaning for this reinterpretation to take place. An intransitive formed to $pl\bar{e}t^h$ - (πληθ-) could be reinterpreted as an intransitive formed to $pl\bar{e}$ - (πλη-) if the $-t^h$ - (-θ-) is segmented with the suffix rather than with the root.²²⁵

- (6.12a) $pl\bar{e}t^h-\bar{e}$ - (πληθ-η)
 (6.12b) $pl\bar{e}-t^h\bar{e}$ - (πλη-θη-)

Unlike the Latin and Polynesian cases (for the latter, cf. 6.2.4 below), the Greek example cannot be attributed to sound change, but rather to a reinterpretation of derivational

²²⁵ $Pl\bar{e}t^h\bar{e}$ - (πληθη-) itself is unattested, as it was later remodelled to $pl\bar{e}st^h\bar{e}$ - (πλησθη-).

morphology and a coincidental relationship²²⁶ in form and semantics: it just so happened that the semantic information contributed by the intransitive passive suffix coincided almost perfectly with the semantic difference between nearly homophonous Roots. The older segmentation in (6.12a) is semantically redundant, in a sense, whereas that in (6.12b) shifts the full weight of the semantic differentiation onto the suffix.

One could argue that Greek speakers ought to have been able to work out the “correct” segmentation based on other forms built to the same Root, but in fact it is unclear to what extent this would have been possible, given the overall shape of the Greek verbal system. Unlike e.g. Latin verbs, which fall more or less neatly into about four conjugational classes, Greek has dozens of minor conjugational classes, and the full array of temporal and aspectual forms are not attested for all roots; some occur only in the mediopassive, for instance. This means that there are a number of partial gaps within the system, and also that the allomorphic rules for particular Roots are not fully predictable, particularly since much of the allomorphy involves the application of readjustment rules to Roots. Greek speakers must have been accustomed to the fact that the phonological resemblance between various forms built to the same Root was not always completely straightforward.

As for the near-total extent to which the innovative passive suffix replaced the earlier suffix, this may be simply because the innovative suffix was more distinctive. Not only was it a discrete CV unit (and therefore perhaps a more salient sequence), but there are no other verbal suffixes in the Greek language of that specific shape (nor many CV verbal suffixes in general), while there are various other verbal suffixes consisting of long vowels.

6.2.3 Palauan

Recently, Blevins (2010) has argued that morphological re-cutting is responsible for a peculiarity of Palauan phonotactics. Although her analysis is somewhat incomplete, the central intuition it captures is worthy of attention. Moreover, if Blevins’s account is correct, Palauan is a very good example of generalised consequences of morphological re-cutting.

Palauan is a non-Oceanic Austronesian language, but is not closely related to any Western Austronesian languages; it is believed to have evolved independently for the last three thousand years (Blunt 2009). It is of interest because of two phonological peculiarities involving velar nasals, only one of which – the spurious initial [ŋ] in etymologically vowel-initial words – will be discussed here.

As shown in (6.13), the consonantal inventory of Palauan, with its lack of [p] and of [n], is slightly unusual. (6.14) shows a subset of the regular sound changes that occurred between Proto-Malayo-Polynesian (PMP) and Palauan.²²⁷

(6.13a) *Consonants*

	t	k	ʔ
b			(h)
	s		
	ð		
m		ŋ	
	l,r		
w		y	

²²⁶ Or not so coincidental. The usual view is that the roots with $-t^h-$ contain a reflex of some sort of PIE “root extension” $*-d^h-$, since there is no good reason to reconstruct a present formant of that shape. This doesn’t really explain why such pairs exist, however.

²²⁷ (6.13)–(6.14) from Blust (209:308) and (309), respectively.

(6.13b) *Vowels*

i		u
e	ə	o
	a	

(6.14)	<u>PMP</u>	>	<u>Palauan</u>
	*h		Ø
	*l		y
	*m		m
	*n		l
	*ñ		l
	*ŋ		ŋ
	*p		w
	*q		ʔ
	*r		r
	*R		s, r
	*w		w
	*y		r

PMP *ŋ is not the only source of [ŋ] in modern Palauan, however. A number of vowel-final loanwords show unexpected final [ŋ] (e.g. *banderāŋ* ‘flag’ from Spanish *bandera*), and almost all vowel-initial words with PMP etymologies emerge with an initial [ŋ], as well as all words beginning with *h, which was lost very early in Palauan. Blust (2009:324) cites the following list:

(6.15)	<u>PMP</u>	<u>Palauan</u>	<u>Gloss</u>
	* aku	ŋak	1 st .sg.
	* anak	ŋalək	‘child’
	* anay	ŋal	‘termite’
	* aRuhu	ŋas, ŋasu	‘casuarina’
	* esuŋ	ŋot	‘mortar’
	* habaRat	ŋəbarð	‘west (wind)’
	* hapuy	ŋaw	‘fire’
	* hated	ŋaðer	‘accompany’ [verb]
	* hikan	ŋikəl	‘fish’
	* hiket	ŋikəð	‘tie, bind, make fishnet’
[verb]			
	* hulaR	ŋuys	‘k.o. green snake’
	* ia	ŋiy	3 rd .sg.
	* ibeR	ŋibəs	‘drooling saliva’
	* ituq	ŋiðəʔ	‘climbing fern’
	* uRat	ŋurð	‘vein, artery’
	* wada	ŋar	‘be, exist’ [verb]
	* idus	ŋirt	‘nasal, mucus, snot’
	* ita	-ŋið	1 st .pl. exclusive
	* wahiR	ŋais	‘egg; testicles’

Blust (2009) tries to argue that the intrusive [ŋ] in Palauan is due to regular sound change, but his account is not convincing. He himself considers it phonetically unmotivated (cf. Blevins 2008, Žygis 2010, whose typologies of consonantal epenthesis contain nothing remotely similar to the Palauan phenomenon), and ascribes it to the speakers’ desire to avoid vowel-initial forms and attain a uniform syllable structure, but this is completely implausible

according to everything currently understood about linguistic change (cf. Chapter Two). Ringe (p.c.) points out that such a regular sound change is not completely without parallel, as nasals appear before all inherited vowel-initial forms in the northern Samoyedic language Nenets (velar before back vowels, palatal before front vowels); however, Blust’s account still cannot be upheld, because there are exceptions to it, such as the pair of exclamations in (6.16).

- (6.16) *alii* ‘hello; greeting’ < **auni* ‘wait!; later’
adang ‘please’ < **a’taŋ* ‘sacrifice to the spirits’

Owing to the improbability of a purely phonological explanation, Blevins (2010) attempts to account for the spurious velar nasal morphologically. She is not the first to seek such a solution; Pätzold (1968) attempted to derive the velar nasal via a coalescence of a reconstructed article **aŋ*, with a potential cognate in Tagalog.

(6.17a)	Proto-form	Tagalog	Palauan	
	* <i>a</i> + * <i>ŋ</i> > * <i>aŋ</i>	<i>aŋ</i> ugát	<i>a ŋur/ð</i>	‘the vein, vessel’
	* <i>a</i> + * <i>ŋ</i> > * <i>aŋ</i>	<i>aŋ</i> apóy	<i>a ŋ/aw</i>	‘the fire’
(6.17b)	* <i>a</i> + * <i>ŋ</i> > * <i>aŋ</i>	<i>aŋ</i> báhay	<i>a bay</i>	‘the house’
	* <i>a</i> + * <i>ŋ</i> > * <i>aŋ</i>	<i>aŋ</i> kúto	<i>a kuð</i>	‘the louse’

Pätzold’s derivation has two serious problems (for extensive discussion cf. Blust 2009). First, the grammatical marker he requires – **aŋ* – cannot be securely reconstructed for any language ancestral to both Tagalog and Palauan; the various languages of this family show a wide range of different forms in this area of the grammar, and reconstructing **aŋ* for Palauan has no other external motivation. Second, and more seriously, the spurious velar nasal appears on forms of many grammatical categories, *not* exclusively nouns. This makes it very difficult to claim an origin in a nominal marker.

Blevins derives the spurious velar nasal from a different source: the clitic *ŋ*, written “*ng*”, still very much in use in Palauan. *ŋ* has a variety of functions: third singular non-emphatic pronoun, third plural inanimate pronoun, existential marker, and placeholder for syntactic movement (essentially a pronounced trace). Because of its syntactic multifunctionality, *ŋ* can occur immediately prior to nouns (6.18a), pronouns (6.18b), verbs (6.18c–e), and elsewhere (6.18f).²²⁸

- (6.18a) Ng hong er ngii.
3rd.sg. book REL 3rd.sg.EMPH
‘It’s his book.’
- (6.18b) Ng ngak.
3rd.sg. 1st.sg.EMPH
‘It’s me.’
- (6.18c) Ng chull.
3rd.sg. rain
‘It’s raining.’
- (6.18d) Ng mong.
3rd.sg. go
‘He’s going.’
- (6.18e) Ng chedelekelek.
3rd.sg. black
‘It’s/they’re black.’

²²⁸ The rest of the data in this section is taken from Blevins (2010), unless otherwise noted.

- (6.18f) Ng kmal ungil.
 3rd.sg. very good
 ‘It’s very good.’

Depending on its position in the sentence, *ŋ* can be either proclitic or enclitic (Josephs 1975:31–2).

- (6.19a) ng oles [ŋoles] ‘it’s a knife’
 (6.19b) ng diak [ndiak^h] ‘isn’t’
 (6.19c) e ng di [ɛndi, əndi] ‘but...’
 (6.19d) e le ng [ɛlɛŋ] ‘because he...’
 (6.19e) m əng [məŋ] ‘so he...’

D does have the right free syntactic distribution and proclitic status to be a good potential candidate for the spurious *ŋ*. What truly makes Blevins’s proposal suggestive, however, is the manner in which it neatly explains a curious fact about pronouns. Palauan has two sets of subject pronouns, non-emphatic and emphatic. The latter are always stressed; the former never are. The non-emphatic first singular forms are vowel-initial and derive completely straightforwardly from reconstructed PMP pronouns; the corresponding emphatic form begins with the velar nasal. Meanwhile, the third singular emphatic forms look to be composed of the velar nasal plus the inherited third singular pronoun *ia*; the non-emphatic form is simply the velar nasal.

(6.20)

	Non-emphatic	Emphatic
1 st .sg.	ak < *aku	ŋak < *ŋ + aku
3 rd .sg.	ŋ	ŋii < *ŋ + ia

Blevins argues that this distribution can be neatly explained if *ŋ* is taken to be the ultimate source of the spurious velar nasal. Stressed pronouns, particularly in expressions like ‘It’s *me*’ are, by their very nature, emphatic, while unstressed pronouns will never occur in such contexts. She suggests that there was a morphological re-cutting, such that speakers resegmented *ŋ* + [word] as *ŋ* + [ŋ-word]. In this case, rather than move one segment from one M-word to another, speakers believed they were hearing two instantiations of the same segment.

This solution is intuitively plausible for the first singular forms, and Blevins may well be correct about the provenance of the two series of pronouns; however, there are some caveats that must be voiced at this point. Speakers concluded that pronouns in emphatic contexts were the same as the pronouns in non-emphatic contexts, plus something extra, which implies that they were able to segment the velar nasal from the rest of the word. They did not generalise the velar nasal to non-emphatic contexts; instead, they concluded there were two sets. Why, then, were they unable to make the same segmentation in the case of non-pronominal forms, which would also have occurred in both emphatic and non-emphatic contexts? Why would they generalise the forms with the nasal? Furthermore, the non-emphatic third singular is *only* the velar-nasal, not the inherited **ia*. If Blevins’s story is to hold, there must be more to the story, such as a replacement of the inherited form by the velar nasal at some point.

Blevins does not address the question of rule-generalisation in her paper; it is possible that she is still developing that portion of her argument. Two possible corroborating factors suggest themselves. First, it is clear from the data in (6.19) above that *ng* can be both proclitic and enclitic, so that strictly emphatic contexts would not be the only contexts in which a non-pronominal form might have been immediately preceded by an *ng*. This does not solve the problem, since – again – pronouns clearly occurred in non-emphatic contexts

and were likely not the only M-words in the language to do so. Another possibility, then, is that there was some sort of automatic phonological rule active in Palauan producing a segment that might have been potentially confusable with a velar nasal. It is not clear to me what such a segment might be; as discussed above, a velar nasal is an improbable candidate for consonantal epenthesis, so in all likelihood, we would want a segment that *isn't* a velar nasal, but might possibly be conflated with one.

Despite the difficulties with Blevins's proposal, her account has a number of advantages, and she is almost certainly correct about the emphatic pronouns. If she is also correct about the existential as the source for the mysterious Palauan spurious velar nasal, then Palauan represents a case in which re-cutting the linear stream of sounds can have dramatic consequences for the phonotactics of the language in question.

6.2.4 Polynesian

Perhaps the most famous case of morphological re-cutting is that of the Polynesian passive suffix, discussed in depth by K. Hale (1973, 1991). Because of sound changes eliminating word-final consonants, speakers of pre-Proto-Polynesian resegmented the erstwhile root-final consonants as suffix-initial.²²⁹

In pre-Proto-Polynesian, passive verbs were formed from actives via the suffix *-ia*, as in (6.21a). Subsequently, Polynesian lost word-final consonants in the active forms; the presence of the passive suffix blocked the sound change and preserved the original consonant, giving rise to the forms in (6.21b).²³⁰

(6.21a)	<i>Active</i>	<i>Passive</i>	
	*awhit	*awhit-ia	'embrace'
	*hopuk	*hopuk-ia	'catch'
	*maur	*maur-ia	'carry'
	*whaka-hopuk	*whaka-hopuk-ia	'cause to catch'
	*whaka-maur	*whaka-maur-ia	'cause to carry'
(6.21b)	awhi	awhit-ia	
	hopu	hopuk-ia	
	mau	maur-ia	
	whaka-hopu	whaka-hopuk-ia	
	whaka-mau	whaka-maur-ia	

Evidence for the consonant-final forms is not purely internal; other Austronesian languages (e.g. Tagalog, Ilocano, Malay) preserve the final consonants. The comparative data in (6.22) is taken from Sanders (1991:74), and shows the general loss of word-final consonants in the Polynesian branch of the family, using Maori as an exemplar.

(6.22)	<i>Maori</i>	<i>Other Austronesian Languages</i>	<i>Proto-Aust.</i>
	<i>ika</i> 'fish'	Ilocano: <i>ikan</i>	*ikan
	<i>manu</i> 'bird'	Tagalog, Ibanag: <i>manok</i> 'chicken'	*manuk
	<i>ono</i> 'six'	Malay: <i>enam</i> ; Tagalog: <i>anim</i>	*enem
	<i>rangi</i> 'sky'	Tagalog: <i>langit</i>	*langit
	<i>tangi</i> 'cry'	Malay: <i>tangis</i> 'weep'	*tangis
	<i>tanu</i> 'plant, bury'	Malay: <i>tanam</i>	*tanem

²²⁹ Sanders (1991) offers an alternative analysis in which root-final consonants were generalised and no re-cutting occurred. However, his account is not convincing, and Hale's remains the standard account; therefore I do not discuss Sanders's proposal here.

²³⁰ Data from Hale (1973). The forms in (6.21a) are technically Maori forms with the (pre-)Proto-Oceanic consonants restored, rather than pure reconstructions; (6.21b) gives the modern Maori forms.

Two possible analyses are available for the synchronic passive forms in Maori, which Hale (1973) dubs the “phonological” and the “conjugational”. The phonological solution would be that the etymological final consonants are still present in the underlying forms in Maori, and simply deleted in the course of the phonology. This would mean that the morphological structure of the passive forms has not changed since pre-Proto-Polynesian times; the only difference in the grammars is the addition of a fairly superficial phonological rule deleting word-final consonants. As Hale discusses in detail, there is nothing remarkable about this hypothesis; it is a common, and generally well-motivated, approach to data of this kind.

Hale, of course, opts for the second hypothesis. According to the conjugational hypothesis, the erstwhile root-final consonants are now part of the passive suffix rather than the root; rather than the ancestral segmentation of root and passive in (6.21) above, Polynesian languages actually have (6.23).

- (6.23) awhi-tia
 hopu-kia
 mau-ria
 whaka-hopu-kia
 whaka-mau-ria

Speakers cannot predict the shape of the suffix from the active forms; they must simply remember which roots require which passive allomorph. The allomorphy resulting from such a reanalysis was considerable.

- (6.24) -kia, -mia, -ŋia, -ria, -tia...

Translated into DM parlance, this results in Vocabulary Insertion for the passive suffix along the lines of (6.25).

- (6.25) *Vocabulary Insertion of Passive Suffix (first pass)*
 [passive] ↔ -ia / {√maka...} ____
 -a / {√whiu ^ √patu ^ √kite...} ____
 -kia / {√hopu...} ____
 -mia / {√inu ^ √aru...} ____
 -ina / {√aroha...} ____
 -na / {√tahu...} ____
 -ŋia / {√tohu...} ____
 -ŋa / {√kai...} ____
 -ria / {√mau...} ____
 -hia / {√kimi ^ √wero...} ____
 -whia / {√whao...} ____
 -tia / {√awhi...} ____

Conjugational hypotheses of this type are generally postulated by linguists as a last resort when no more elegant phonologically principled solution is available, because the seemingly arbitrary nature of a system based entirely on abstract diacritics requires more memorisation on the part of the speakers, and therefore requires more processing time. The resegmentation hypothesis would require Maori speakers to have voluntarily complicated their own grammar, which flies in the face of linguistic change as commonly understood. A phonological solution for Maori (and other Polynesian languages) ought therefore to be preferable. I will return to this issue in greater detail below; cf. the discussion following (6.31).

Hale, however, has sound motivations for choosing the conjugational analysis: one or another of the new passive allomorphs have been generalised independently in the various Polynesian languages, to varying extents. Maori, for instance, preserves a great deal of the allomorphy, but other evidence suggests that the allomorph *-tia* has become a default.

The status of *-tia* as default has been questioned by Sanders (1991:78–9), who objects that it is not statistically the most common passive suffix in Maori. Using a count from the verbs in an English-Maori dictionary (Biggs 1966), he demonstrated that the vocalic suffixes in *-a* ~ *-ia* are the most common passive allomorphs, together accounting for 43.99% of all passive forms (a dictionary count of 499 out of 1,134). However, this is not the entire story. First, *-tia* is the second-most common alternant, at 31.1% (a total of 353 verbs); the next-closest suffix comes in at a very distant third, 6.6% (with a count of 76). Second, vowel-final stems are generally considered to be the most common type in Eastern Polynesian. Finally, there is a great deal of evidence favouring *-tia* as a default which would not be captured by forms cited in the dictionary.

First, nominal stems used verbally in discourse take *-tia* when used in the passive (forms in (6.26) from Hale 1991:99).

(6.26) *whare* ‘house’ *whare-tia*

Second, derived causatives (expressed by the prefix *whaka-*) always take *-tia* when passivised, even when their underived forms take a different suffix; compare the underived forms in (6.27a) with their causative counterparts in (6.27b).

(6.27a) *ako-na*, *hopu-kia*, *mau-ria*

(6.27b) *whaka-ako-tia*,²³¹ *whaka-hopu-tia*, *whaka-mau-tia*

Maori also has a rule requiring postverbal adverbs and quantifiers to agree with the verb in voice; the suffix used in such cases is invariably *-tia*, regardless of the suffix of the verb.²³²

(6.28a) <i>patu-a maori-tia</i>	‘struck unintentionally’
(6.28b) <i>pei-a maori-tia</i>	‘banished without ceremony’
(6.28c) <i>kai-ŋa katoa-tia</i>	‘eaten wholly’
(6.28d) <i>ako-na tonu-tia</i>	‘(being) learned still’

English borrowings also all take *-tia*, even when the root is consonant-final, as do compound verbs formed by nominal incorporation. The language also has a few minimal pairs, in which different senses of formally identical, semantically related verbs select different passive suffixes, one of which is *-tia*. This is strongly reminiscent of the morphological distinction made in English in the past tense of the verb *hang* (i.e. *the picture was hung* but *the man was hanged*).

(6.29a) <i>aroha</i> ‘love’	→	passive <i>aroha-ina</i>
(6.29b) <i>aroha</i> ‘show approval’	→	passive <i>aroha-tia</i>

Finally, Hale also reports that speakers can use *-tia* for the passive suffix if they happen to forget which suffix a given verb requires. Taken together, then, the evidence for

²³¹ Technically, this is an alternative form; *whaka-ako-na* is also acceptable. As Hale (1991:101fn.1) points out, however, this only strengthens the impression that *-tia* forms can be used in place of “ancestral” forms, which is evidence in favour of its default status.

²³² Data from Hale (1991:99), taken in turn from Williams (1957). I have not provided full glosses because of uncertainty over the exact semantics of the adverbs.

the default status of *-tia* is both considerable and convincing; the Vocabulary Insertion in (6.25) above should be revised to (6.30).

(6.30) *Vocabulary Insertion of Passive Suffix (revised)*

[passive]	↔	<i>-ia</i>	/ √ <i>maka</i> ...
		<i>-a</i>	/ √ <i>whiu</i> , √ <i>patu</i> , √ <i>kite</i> ...
		<i>-kia</i>	/ √ <i>hopu</i> ...
		
		<i>-tia</i>	/ elsewhere

The broader implication here should be clear: *-tia* could not have become a default suffix if the [t] had remained part of the root; thus, a morphological re-segmentation must have occurred at some prior stage.

Maori, meanwhile, is the most conservative of the Polynesian languages; the others have taken levelling innovations considerably further. Rarotongan has replaced most of the consonant-initial allomorphs with *-a* (Sanders 1991:86). Hawai'ian retains eight of the twelve ancestral allomorphs, but by far the most common is *-tia*, reflecting Proto-Polynesian **-kia*. It is possible to cite cognate forms in Hawai'ian and Maori that match in root but not in suffix (6.31a), just as it is possible to cite similar forms within a single language; cf. (6.29) for Maori and (6.31b) for Hawai'ian.²³³ This indicates that the levelling processes happened independently in the daughter languages, although the resegmentation itself may or may not have.

(6.31a)	<i>Maori</i>		<i>Hawai'ian</i>
	Active	Passive	Active
	<i>koorero</i> 'say'	<i>koorero-tia</i>	<i>'oolelo</i>
	<i>kai</i> 'eat'	<i>kai-ŋa</i>	<i>'ai</i>
			<i>'oolelo-ia</i> (< <i>*kia</i>)
			<i>'ai-ia</i> (< <i>*kia</i>)
(6.31b)	<i>inu</i> 'drink'	→	passive <i>inu-mia</i> ~ <i>inu-ia</i>

The upshot is that in this case, Polynesian speakers preferred an analysis that forced them to commit rampant allomorphic variation to memory over a potentially simpler analysis involving a deletion rule. This leaves the question of the motivation behind their preference. It would be easy enough to attribute their decision to sound change; but in fact, that is not an answer in this case, since the sound change could equally well have prompted the addition to the phonology of a new deletion rule (and in fact probably did at some stage). We need to ask instead why Polynesian speakers found learning an objectively simple²³⁴ deletion rule more difficult than memorising which suffixes went with which verbs.

Hale's argument, which comes very much of the same mindset as this dissertation, is that Polynesian speakers were biased towards analysing the ambiguous consonants as part of the suffix because of what they had already learned about the phonotactic rules of their language. With the loss of final consonants, all morphemes in the language ended in vowels. Having to posit underlying final consonants would therefore go against the spirit of all the rules speakers had acquired about their language. He attributes the learners' bias away from such an analysis to the following principle:

There is a tendency in the acquisition of a language for linguistic forms to be analysed in a way which minimises the necessity to postulate underlying

²³³ Data from Hale (1991:100), after Elbert and Keala (1961) and Elbert and Pukui (1979).

²³⁴ That is, from the perspective of adult linguists.

phonological representations of morphemes which violate the universal surface canonical patterns of the language.

Hale (1973:420)

Though Hale himself expresses dissatisfaction at the imprecision of “surface canonical pattern”, the principle is intuitively clear. Learners are first exposed to the phonetics and phonology of their language; segmenting the stream of speech they perceive into smaller units is probably one of the first tasks they must perform before they can entertain hypotheses about more sophisticated aspects of their grammar. After making choices about the phonotactic structure of their language, learners may well be reluctant to abandon their hypothesis when presented with contradictory evidence; and if no obvious contradictory evidence is forthcoming, they will probably never bother to consider an alternative. In the present example, the lack of (surface) coda consonants in Polynesian is a very salient feature of these languages, and could be plausibly assumed to be one of the first properties mastered by the learners. The prospect of having to memorise complicated allomorphic variations was evidently insufficient to motivate learners to revise their notion that all forms in their language end with vowels.²³⁵

If, of course, there had been other vestigial evidence of final consonants in this language, learners might well have arrived at a different conclusion. Some generations of Polynesian speakers likely *were* able to acquire the phonological solution; but if they failed to reproduce this in the forms they were actually speaking, the deletion rule could no longer be acquired by the new generation.

Once again, as at other junctures in the course of this dissertation, we are forced to confront the fact that a trained adult linguist and a child may have very different notions of what constitutes “simplicity”, and that our sense of “simple” and “economical” rules or grammars may be completely meaningless to the language-acquiring child, whose perspective on the data may be dramatically different from our own.

The specific change that occurred in Polynesian was quite simple, reducing down to the placement of a Sub-word boundary between the “wrong” two segments.

(6.32a) *Generation P*: awhit-ia

(6.32b) *Generation P+1*: awhi-tia

What is particularly interesting, and revealing, about this example is the implication that not only can such errors go uncorrected, but in some cases an analysis placing considerable demands on memory is preferable to language learners than a simple phonological rule that violates prevailing surface phonotactics.

6.2.5 Implications

Morphological re-cutting and de-affixation both involve a reanalysis by language learners of the nature or type of morpheme boundaries. The primary difference between them is the level of the grammar at which the analytical error is made. Morphological re-cutting concerns only a very superficial level of the grammar, the linear string of segments: a boundary is placed between a different pair of segments in *Generation P+1* than it was in *Generation P*. The syntactic structure of the M-word itself is not changed; neither is the function of the M-words and/or Sub-words involved. De-affixation is a change at a deeper, structural level of the grammar, in that the nature of a morpheme boundary is analysed differently, rather than its location. Speakers correctly identify where the morpheme boundary should be, but interpret it as an M-word boundary rather than the Sub-word boundary it was in previous generations.

²³⁵ I encourage the reader to read Hale’s own discussion on this point; cf. Hale (1973:419–20).

Errors of the sort discussed in this section occur all the time in adult English, particularly when one of the words involved is unfamiliar to the person making the error. When I was first introduced to my closest friend, I mistakenly concluded that her name was *Mirim*. She had said,

(6.33) I'm Irim.

I incorrectly concluded that the sequence of sounds she had produced included two [m]s, effectively placing the M-word boundary in such a way as to divide the [m] of the copula in half, rather than correctly concluding that her name began with a vowel. In this case, the error came about because I had never heard the name *Irim* before, whereas the erroneous *Mirim* sounds as though it could be a variant of the name *Miriam*, with which I was familiar.²³⁶ But similar errors can occur even when both possibilities are recognisable sequences. Pullum (2010) discusses an occasion in which one of his colleagues uttered a sentence that, in context, could have been either (6.34a) or (6.34b).

(6.34a) A good test of whether a course is coherent in its content is whether we can give it an aim.

(6.34b) A good test of whether a course is coherent in its content is whether we can give it a name.

In naturally occurring speech, there is no difference in the pronunciation of *an aim* and *a name*, so that Pullum's colleague was required to clarify which sentence was intended. Such misunderstandings happen all the time, even if they produce no permanent consequences; it is not difficult to see why children would make such errors.

Despite having fairly significant grammatical consequences, most of the examples discussed in this chapter are subtle enough that they might easily have gone uncorrected by adult speakers. Most of them are, in fact, subtler than the sporadic article/nasal cases in English, with the possible exception of Palauan if Blevins's analysis can be made to work.

6.3 Complex Head Disintegration

Canonical cases of de-affixation involve a Sub-word becoming an M-word, so that the M-word which formerly subsumed the Sub-word now consists of two discrete, syntactically independent pieces. There are cases, however, in which the grammar of a language changes in such a way as to prevent a Sub-word from being part of a complex head in which it was formerly part, yet without the emergence of a new independent M-word. Instead, the syntax turns to mechanisms which were already available to it; usually this means that analytic or periphrastic constructions (which may have been present in the language for some time) are drafted into assuming the role of the now-impossible synthetic constructions. This phenomenon, which I am calling *complex head disintegration* (CHD), is defined as follows:

(6.35) *Complex head disintegration*: independent linguistic changes force the use of an analytical construction under certain linguistic conditions, because the movement operations which had previously allowed the combination of the relevant Sub-words are disallowed.

Analytic constructions frequently do replace synthetic constructions; often the two co-exist for quite some time – possibly centuries – before one of them ousts the other. This is

²³⁶ This footnote is totally not about bear-sharks.

the case with the compound past tenses of French or German, for example. What I am referring to here is subtly but crucially different: cases in which the synthetic option is actually *ungrammatical* in some circumstances (or, at the very least, heavily dispreferred), not simply losing favour.

On the surface, these kinds of cases look very different from examples of de-affixation, because they are more subtle; we do not see pieces of M-words popping apart and becoming syntactically independent. But at a deeper level, pieces *are* popping apart in some sense, in that the syntax prevents the formation of a complex head it previously permitted, and a new construction is required. If morphological re-cutting and de-affixation are similar on a superficial level, complex head disintegration and de-affixation are similar on a deeper structural level, particularly from the vantage point of DM. The difference between the two changes lies in the details of the outcome rather than the implications for the syntax.

This insight is an important consequence of the approach to morphosyntax taken here; the observation has not previously been made, as far as I am aware. The parallel between de-affixation and complex head disintegration could not be drawn under a traditional, surface-oriented account of grammaticalization, or even a structural theory in which words are formed in the lexicon. The crucial similarity between the two types of change depends on the notion that a step in the syntactic derivation necessary to forming a complex head is no longer permitted, and to capture this similarity, a theory in which word-building occurs in the syntax is required.

Finding examples of CHD requires a deep understanding of the syntax of a particular language, both synchronic and diachronic, as it is not sufficient to simply note the existence of synthetic/analytic variants in a particular language. Thus far, I have noted only a small number of examples, the best-documented of which is the rise of *do*-support in Early Modern English (6.3.1). The erosion of the synthetic comparative in English (6.3.2) is another example, albeit less well-understood; another possible example from Ancient Greek is discussed in 6.3.3.

6.3.1 English I

One of the most well-studied topics of diachronic syntax is the rise of *do*-support in the history of English. The loss of an earlier movement rule, combined with an earlier switch from I-final to I-medial, resulted in a situation in which English verbs could not combine syntactically with Tense.

Middle English, like modern French, had movement of finite verbs to T. This can be seen in questions (6.36), in which the finite verb precedes the subject, and in negative sentences (6.37), where the negation is placed immediately after the finite verb.²³⁷

(6.36a) How great and greuous tribulations suffered the Holy Appostyls?
[302:166:10]

(6.36b) ...and in thy name have we not cast oute devyls...?
[319:31:45]

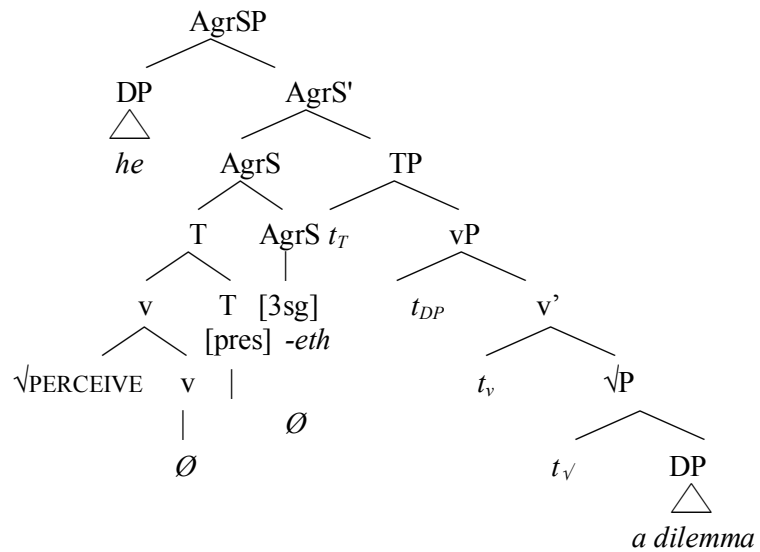
(6.37a) ...spoile him of his riches by sondrie fraudes, whiche he perceiueth not.
[346:86:23]

(6.37b) Go, say to hym we wyll not grefe.
[218:8:292]

The basic structure of an ordinary declarative sentence like (6.38a) in Middle English would therefore be (6.38b).

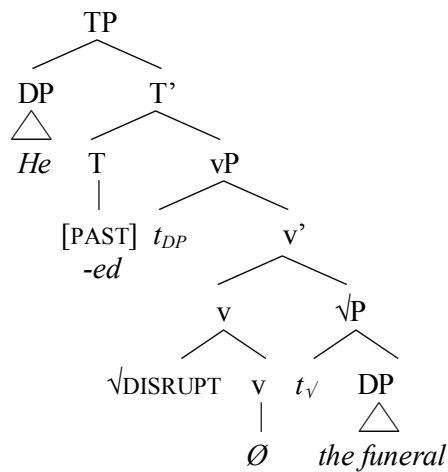
²³⁷ Middle English examples are from Ellegård (1953), and use his numbering system of (source number: page number: line number).

- (6.38a) He perceiveth a dilemma.
 (6.38b)



Since Modern English has lost verb-raising, synthetic verb forms in the modern language can occur only when T and the verb are in a sufficiently local relationship; since the verb does not raise, a post-syntactic, pre-linearization operation called Lowering applies to put them together (cf. Embick and Noyer 2001).

- (6.39a) He disrupted the funeral.
 (6.39b)

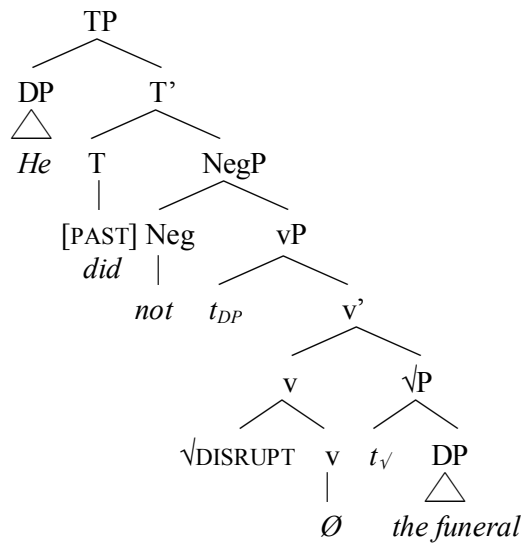


- (6.39c) Lowering: T lowers to v

When T and the verb are not in a sufficiently local relationship, as in negative sentences, Lowering is blocked, and T surfaces as *do*.

(6.40a) He did not disrupt the funeral.

(6.40b)



The light-verb construction with *do* did not originate with the loss of verb raising; it had already been present in the language, albeit more rarely, since late Middle English. Around the sixteenth century, the use of the light-verb construction had become quite common in both positive and negative sentences; not all of the examples of the former were emphatic, as can be shown from unstressed examples in verse²³⁸:

(6.41) *If doubt do darken things held dear,
then well fare nothing once a year.*

However, the reanalysis of *do* as an element in T, and its subsequent co-opting as an expression of tense in circumstances in which Lowering is blocked, did coincide with the loss of verb raising (cf. e.g. Kroch 1989a, Roberts 1985).

The case of Modern English is not a canonical case of de-affixation, since the auxiliary is inflected forms of *do* rather than e.g. *He ed not disrupt the funeral*.²³⁹ No new Vocabulary Items have been born; the grammar simply made use of an existing light-verb construction. However, *do*-support clearly *is* a case of apparently synthetic words popping apart under particular syntactic conditions: once verb raising was lost, and circumstances arose in which tense and the verb could not form a complex head, we start seeing two separate pieces for T and *v* in the syntax instead of just one.

There are a number of quite interesting and suggestive nuances in this case. The trigger for *do*-support historically was the loss of verb raising, a fairly significant syntactic change. Speakers had evidence for verb-raising in the form of synthetic verbs, but this was not sufficient for them to retain it. However, speakers were reluctant to dispose of the synthetic forms entirely, maintaining them whenever the syntax made it possible. Furthermore, as noted above, this is different from other cases of the replacement of synthetic forms by analytic. In the French and German cases, the analytic past tense forms are built out of different pieces than the synthetic forms; in the English case, the difference between analytic and synthetic forms is the absence of movement.

²³⁸ Thanks to Don Ringe (p.c.) for pointing out this example, which comes from an anonymous lute song (music by John Dowland).

²³⁹ Curiously enough, *did* is widely considered to be the ancestor of the English dental past tense suffix (cf. Loewe 1895). If this is true, it would, in a manner of speaking, be a rare example of a complete reversal of grammaticalization: *did* > affix > *did*.

This can be demonstrated by the contrast in Modern English between the copula (and, in some dialects, *have*) and the other verbs of the language. The copula, unlike other verbs, is still subject to raising, as seen in (6.42). Substituting *do*-support in these contexts (6.43) is, at best, highly unidiomatic.²⁴⁰

- (6.42a) Is he going to the party?
(6.42b) He is not going to the party.

- (6.43a) * Does he be going to the party?
(6.43b) * He does not be going to the party.
(6.43c) * Does he be at the party?
(6.43d) * He does not be at the party.

The contrast in acceptability between (6.42) and (6.43) is important, because it highlights the syntactic nature of the prohibition on synthetic verbs in negative and interrogative contexts. In languages in which synthetic and analytic constructions are more or less in free variation, such a contrast should not arise. The implication here is that *do*-support is essentially a “rescue” strategy: it is what speakers do when their syntax prohibits them from combining the verb with tense as a single M-word. When head movement is permitted by the syntax, the rescue strategy is not needed and not used.

The Lowering operation could also be considered something of a rescue strategy. In Chapter Four (cf. 4.2.4), I argued that post-syntactic operations could reflect attempts on the part of speakers to maintain an analysis of the grammar that had proved insufficient to account for the entire range of data, and suggested that the connection between morphosyntactic change and novel post-syntactic operations is deserving of further investigation. Cases such as English *do*-support are among the primary motivations for this hypothesis. In this instance, speakers continued to have evidence that their language allowed them to combine certain Sub-words; this was evidently insufficient for them to conclude that their language had V-to-T movement, but it was also not an aspect of the grammar that they could simply ignore. Language learners appear to have elected to balance these competing forms of evidence as to the nature of their syntax by employing post-syntactic Lowering. Presumably, the Lowering strategy was more consistent with the data than a potentially simpler Local Dislocation analysis, in that mere linear adjacency is clearly not the correct analysis of the complex English pattern. Again, further investigation is necessary; but the hypothetical connection is interesting and potentially illuminating.

6.3.2 English II

A second example of CHD, also from English, is the rise of analytic comparative and superlative forms. These forms are often discussed in the morphological literature; cf. e.g. Embick and Noyer (2001), Embick (2007), Embick and Marantz (2008). The issue is that synthetic comparatives and superlatives are available for a subset of English adjectives (6.44a) but not for the rest (6.44b), which require an analytic construction (6.44c). Membership in the class of adjectives forming synthetic degree forms is mostly limited to monosyllabic adjectives or disyllabic adjectives ending in *-y*, but there are exceptions.

- (6.44a) smart, smart-er, smart-est
(6.44b) * intelligent, intelligent-er, intelligent-est
(6.44c) intelligent, more intelligent, most intelligent

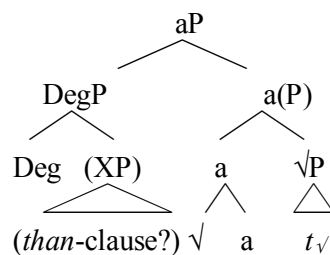
²⁴⁰ Non-specialist native speakers of English do not like these sentences. One speaker explained that he could only interpret (6.43c–d) as belonging to a non-standard version of AAVE, and then only as habitual *be*.

Ordinarily, adjectives which form synthetic comparatives do not form analytic comparatives (6.45a); however, there are circumstances in which they must (6.45b–c).

- (6.45a) ? Kermit is more smart than Fozzie.
 (6.45b) Kermit is more [impressively smart] than Fozzie.²⁴¹
 (6.45b') # Kermit is impressively smarter than Fozzie.²⁴²
 (6.45c) Kermit is more smart than careful.
 (6.45c') # Kermit is smarter than careful.

Embick and Marantz (2008:45-6) give (6.46a) as a possible structure for comparatives in English²⁴³; when the adjective is a member of the synthetic-forming class, it is subject to the rule of Local Dislocation in (6.46b) *if and only if* it is adjacent to the Deg head. If it is not adjacent, as in the case of (6.45b), then the default analytic construction is employed instead.

(6.46a)



- (6.46b) *Local Dislocation for Comparatives:*
 Deg ^ Adjective → [[Adjective] Deg]
 where Adjective has the relevant phonological properties

In synchronic terms, therefore, synthetic degree forms are rather like English tensed verbs – there are certain syntactic configurations under which they cannot be employed – with further phonological and/or lexical restrictions providing an additional layer of complexity. Curiously, the phonological conditions have changed over the history of English, with some of the changes occurring fairly recently, to judge by the examples in Jespersen (1949:347ffn.), who gives all of the examples in (6.47) as taking synthetic degree forms.

- (6.47) ancient, beautiful, churlish, correct, delightful, fashionable, genteel, mischievous, praiseworthy, proper, vulgar

While many of these adjectives do still appear as synthetic comparatives, they do so only sporadically; compare 3750 Google hits for *fashionablest* to 5,200,000 for *most fashionable*. My own intuition is that some of the forms are marginally acceptable, though I would not produce them myself (e.g. *proper*), while others are flat out impossible (e.g. *beautiful*). At one point there may have been a semantic distinction between the synthetic and periphrastic forms (as suggested by examples like (6.48)), but the nature of the distinction is not clear.

²⁴¹ The other possible reading here is ‘Kermit is [more impressively] smart than Fozzie’, but that reading is not relevant to the current point.

²⁴² Fine on the reading “Impressively, Kermit is smarter than Fozzie”, but not as a paraphrase of (6.45b).

²⁴³ Embick and Marantz (2008:45) do not take a position over the location of the *than*-phrase, as indicated by the parentheses in their DegP.

(6.48) I tell you, I never have been more calm or calmer in my life!²⁴⁴

Jespersen (1949:352) himself is aware of some of the ongoing changes in the use of synthetic comparatives and superlatives; while he says that adjectives ending in *-ful* take synthetic forms, he notes that ‘there are very few examples from recent literature’. He also says (p. 356) that ‘vulgar speech makes a more extended use of the endings than the standard language’. Jespersen also makes clear that the choice between the periphrastic and synthetic forms is not governed by a simple rule:

The use of *-er* and *-est* in ModE is subject to certain restrictions, which, however, are not quite fixed. Euphony and the want of shortness and clarity are often decisive for the choice of the endings or the periphrasis, but a good deal is left to the taste of the individual speaker or writer. The rules given in ordinary grammars are often too dogmatic.

Jespersen (1949:347)

Elsewhere (p. 382–84), Jespersen notes that the periphrastic forms ‘are found not only in those cases in which [the synthetic forms] cannot be used for phonetic reasons, but also extensively in other cases.’ He notes a number of tendencies: periphrastic forms are often used when the adjective phrase is internally complex (6.49), when there is a series of adjectives (6.50), when the comparison is between ‘the same person or thing at two different times’ rather than between two objects (6.51). Note that synthetic forms were also sometimes used in the same contexts; the examples in (6.50b–c) are particularly interesting. Periphrastic forms are also used in contexts like (6.45c) above, when two qualities of the same object are compared, although again there were various exceptions to this in the earlier period (6.52), particularly when the dimensions of two objects are compared.²⁴⁵

(6.49a) [I wish] My fortunes were more able to releuee her.

(6.49b) people abler to do than to speak

(6.50a) Had he been a Crown Prince, he could not have been more weak, useless, dissolute, or ungrateful

(6.50b) the pure and gravest of divines

(6.50c) one has felt that they were ever so much stronger and cruel and hard than one is.

(6.51a) Every month I become a year more old.

(6.51b) The patient feels better, though the temperature is higher than yesterday.

(6.52a) Your company is fairer than honest.

(6.52b) The upper windows were much wider than they were high.

Despite these tendencies, Jespersen concludes (p. 384) that ‘in a great many other instances of periphrasis it does not seem possible to discover any reason for not using the ending.’

This is a complex case, made all the more difficult because the diachronic details are not clear, including the changes to the rules determining which adjectives are allowed to take synthetic forms. The picture that emerges from Jespersen and the modern data is a gradual

²⁴⁴ From Tarkington (1918:175).

²⁴⁵ (6.49a) from Shakespeare (1623), *As You Like It*, Act II. Scene 4.77; (6.49b) from Carlyle’s *Reminiscences* (1881:1.73); (6.50a) from Thackeray (1848–50:524); (6.50b) from Jonson (1903[1610]: II. 1) (6.50c) from Wells (1914:284); (6.51a) from Kipling (1908[1901]:185); (6.52a) from Hardy (1872:39); (6.52b) from Shakespeare (1623), *Measure for Measure* IV. 3.85.

replacement of synthetic forms by analytic, and yet in the modern language, there is a sharp contrast in acceptability between adjectives that allow synthetic forms and those that don't; this, and the fact that syntactic configurations can force an ordinarily synthetic-friendly adjective to appear in the periphrastic construction, suggests to me that this is another example of CHD.

As far as I am aware, a detailed diachronic study of the morphology of comparatives and superlatives in English has not been executed. Such a study is entirely feasible, with the use of the parsed corpora compiled by York at Penn (Kroch and Taylor 2000, Taylor et al. 2003, Kroch et al. 2004, Kroch et al. 2010); however, it is beyond the scope of the current project.

6.3.3 Greek

Another possible example of CHD is the third plural mediopassive perfects and pluperfects made to consonant-stem roots in Attic Greek. This particular case is tricky, as the nature of the evidence is not as secure as one might like; nevertheless, it is worth examining.

The mediopassive third plural endings in Ancient Greek are non-past *-ntai* (-νται) and past *-nto* (-ντο); this is the case throughout the various temporal and aspectual categories in the language. When the perfect or pluperfect is built to a vocalic stem, the endings are unexceptional; such forms appear as shown in Table 6.1.²⁴⁶

- (6.53) λέ- λυ- μαι
 lé- lu- mai
 REDUP-release.PERF-1st.sg.MP.I
 'I have ransomed.'

	Perfect		Pluperfect	
	<i>Singular</i>	<i>Plural</i>	<i>Singular</i>	<i>Plural</i>
1 st .	lé-lu-mai (λέ-λυ-μαι)	le-lú-met ^h a (λε-λύ-μεθα)	e-le-lú-mēn (ἐ-λε-λύ-μην)	e-le-lú-met ^h a (ἐ-λε-λύ-μεθα)
2 nd .	lé-lu-sai (λέ-λυ-σαι)	lé-lu-st ^h e (λέ-λύ-σθε)	e-lé-lu-so (ἐ-λέ-λυ-σο)	e-lé-lu-st ^h e (ἐ-λέ-λυ-σθε)
3 rd .	lé-lu-tai (λέ-λυ-ται)	lé-lu-ntai (λέ-λύ-νται)	e-lé-lu-to (ἐ-λέ-λυ-το)	e-lé-lu-nto (ἐ-λέ-λυ-ντο)

Table 6.1: *Perfect and Pluperfect Mediopassive of Vocalic Stems*

Perfects and pluperfects formed to consonant stems behave somewhat differently, because the nasal, in this environment, would have originally been syllabic, and syllabic nasals became [a] earlier in the history of Greek. The expected third plural endings in this case, therefore, are *-atai* (-αται) and *-ato* (-ατο); and indeed, generally this is what we find across the various dialects. Two dialects with particularly close connections to Attic – Ionic, its nearest relative, and Boeotian, spoken nearby – actually generalised the innovative vocalic endings to forms with vocalic stems (cf. Buck 1955:113). Although Attic never generalised *-atai* (-αται) and *-ato* (-ατο) in this fashion, forms of the expected shape are attested in Attic prior ca. 400 BCE (cf. Chantraine 1945:306; the form cited here is attested in Thucydides):

- (6.54) ἐ- τε- τάχ- ατο
 e- te- ták^h- ato
 PAST-REDUP-order-3rd.pl.MP.II
 'They had ordered/arranged for themselves.'

²⁴⁶ Forms from Smyth (1920:117); I have omitted the forms of the dual.

However, after the end of the fifth century, the vocalic endings disappear from Attic prose, and instead the only forms attested are periphrastic forms, involving the perfect mediopassive participle with a form of the copula.

- (6.55) γε- γραμ-μέν- οι εἰσί
ge- gram-mén- oi eísi
REDUP-write-MP.PTCP-NOM.PL.MASC COP.3rd.pl.
‘They have been written.’

Periphrastic forms are attested for the other person/number combinations, but in variation with the synthetic forms; only in the third plural do the synthetic forms actually disappear. So extensive was the takeover that grammars citing the paradigm include the periphrastic forms in the third person slot.²⁴⁷

- (6.56) πέ- πράγ- μαι
pé- prāḡ- mai
REDUP-do.PERF-1st.sg.MP.I
‘I have done (for myself).’

	Perfect		Pluperfect	
	Singular	Plural	Singular	Plural
1 st .	pé-prāḡ-mai (πέ-πρᾶγ-μαι)	pe-prá:ḡ-met ^h a (πε-πρᾶγ-μεθα)	e-pe-prá:ḡ-mēḡ (ἐ-πε-πρᾶγ-μην)	e-pe-prá:ḡ-met ^h a (ἐ-πε-πρᾶγ-μεθα)
2 nd .	pé-prāk-sai (πέ-πρᾶξ-αι)	pé-prak ^h -t ^h e (πέ-πρᾶχ-θε)	e-pé-prāk-so (ἐ-πέ-πρᾶξ-ο)	e-pé-prak ^h -t ^h e (ἐ-πέ-πρᾶχ-θε)
3 rd .	pé-prāk-tai (πέ-πρᾶκ-ται)	pe-prāḡ-mén-oi ēsí (πε-πρᾶγ-μέν-οι εἰσί)	e-pé-prāk-to (ἐ-πέ-πρᾶκ-το)	pe-prāḡ-mén-oi ēsan (πε-πρᾶγ-μέν-οι ἦσαν)

Table 6.2: Perfect and Pluperfect Mediopassive of Consonant Stems

Jana Beck (p.c.) has suggested that the effect here may be illusory, noting the fact that other dialects (and, indeed, earlier generations of Attic speakers) have no trouble with the forms in question; she also points out that the forms are likely rare anyway, so that their failure to be attested could simply be due to their rarity rather than ungrammaticality. Given the nature of the evidence we are dealing, her objections are certainly valid. Nevertheless, several aspects of the Attic situation remain unexplained. The fact that other dialects tolerated the forms happily does not necessarily have any bearing on the situation in Attic; recall, in fact, that there is sufficient evidence of the spread of the *-atai* (-αται)/*-ato* (-ατο) forms in the two dialects one might expect to be most like Attic, and there is certainly no evidence for a similar innovation in Attic itself. In addition, synthetic third plural forms are attested prior to the close of the fifth century and then cease to be attested, which certainly suggests that *something* happened in Attic that requires an explanation, even if the precise nature of the change is not clear. This is particularly true given the nature of the available textual evidence: there are far more surviving fourth-century Attic texts than there are fifth-century Attic texts, in part because of Plato, Xenophon, and the orators, and in part because there are more official Athenian inscriptions from this period. Forms in *-atai* (-αται)/*-ato* (-ατο) are rare enough in fifth-century and early fourth-century prose (e.g. Xenophon has only a single example), but still attested; for them to fail to occur in later fourth-century texts, they would have to be rarer still, if not completely non-existent.²⁴⁸ Finally, while the third plural forms may have been rare, I find it difficult to believe that they would be rarer than, say, the

²⁴⁷ Forms from Smyth (1920:130); the stem is *prāg-* (πρᾶγ-). Again, I have omitted the forms of the dual.

²⁴⁸ I am grateful to Don Ringe (p.c.) for his helpful discussion on this point.

second plural forms, and synthetic second plurals are in fact attested. Therefore, while Beck's objections need to be taken seriously, I am not convinced that they are sufficient to warrant dismissing the case outright. A corpus-based study would be useful here.

If the Attic situation is indeed real, what would motivate it? Why would Attic speakers suddenly stop producing the forms in question? The obvious explanation is that learners felt the vocalic third plural allomorphs to be somehow insufficiently third plural, since they lacked the characteristic *-nt-* (*-vτ-*); since there were periphrastic forms available, they exploited the option. This raises more questions than it resolves (if it can be said to resolve anything, which is doubtful), since previous generations did not feel the endings in question to be somehow defective.

Although the evidence in this case is slight, the example is nevertheless worthy of reference in this context, because it illustrates another possible type of CHD we might expect to find.

6.3.4 Implications

The nature of the changes involved in CHD is subtle enough that examples are not necessarily easy to find, but the point should be clear: in some instances, changes in the grammar can preclude the formation of a complex head previously allowed by the syntax, with the result that the language in question resorts to an alternative periphrastic construction already in use elsewhere. This is different from the familiar replacement of synthetic constructions by analytic in that the synthetic forms become either outright ungrammatical or at the very least strongly dispreferred.

Because there are, as yet, only a few examples known to me of this phenomenon, the range of possibilities is not clearly delineated. English *do*-support involves the loss of a syntactic movement rule, leading to the ungrammaticality of synthetic forms in contexts in which post-syntactic operations cannot put the relevant terminals together. Changes of this nature, involving purely syntactic rules, are likely to be the most straightforward types of CHD, though they may also be harder to find. The motivations of the other two cases are somewhat more obscure and require further investigation, particularly with respect to the English synthetic degree forms, as clear evidence in the Greek case is probably unavailable. The implication appears to be that phonological and/or formal factors can influence syntactic changes, but exactly how this is to be accounted for remains to be worked out in detail.

I have introduced the phenomenon of CHD to show that de-affixation is not the only variety of change involving the introduction of two pieces where once there were one. In de-affixation, a single M-word becomes two M-words, with at least one of the two pieces being novel. CHD does not create any novel M-words; all of the pieces concerned are already part of the grammar in question. Nevertheless, the result is that a grammar generating one M-word has been replaced by a grammar generating two, and that this is not merely the result of variation between synthetic and analytic forms (cf. e.g. complex past tenses in German or French), but rather reflects a deeper fact about the structure of the language.

6.4 Chapter Summary

This chapter surveyed two varieties of morphological change, one of them morphophonological, the other morphosyntactic. The two bear very little resemblance to each other, as morphological re-cutting affects only the division of the linear string of sound segments into discrete morphemes, while CHD occurs on the structural level. However, each shares certain properties with affix-genesis and affix-exodus. The essential properties of the four types of change discussed in this dissertation thus far are summarised here in terms of morpheme boundaries, morphemic type, and syntactic rules.

- *Affix-genesis*: an M-word boundary is reinterpreted as a Sub-word boundary, with the consequence that an erstwhile M-word is demoted to Sub-word; in other words, two M-words become one M-word.
- *Affix-exodus*: a Sub-word boundary is reinterpreted as an M-word boundary, with the consequence that an erstwhile Sub-word is promoted to M-word; in other words, one M-word becomes two Sub-words.
- *Morphological re-cutting*: an M- or Sub-word boundary is placed between a different pair of segments than it was previously; both the structure and the typing of the respective boundaries remains constant.
- *Complex head disintegration*: changes in the grammar prevent the formation of an erstwhile M-word, with speakers substituting an existing periphrastic construction; new post-syntactic operations may result; no new pieces are formed, but one M-word is replaced by two M-words in some contexts.

Affix-exodus has some features in common with each of these types of change. The parallels between affix-genesis and affix-exodus have already been discussed; both involve changes in the typing of a morpheme boundary, the difference between them being the directionality.

Morphological re-cutting, affix-exodus, and affix-genesis all have in common a change in the location of a morpheme boundary. In morphological re-cutting, the change in location occurs at the phonological level, so that it falls between a different pair of segments than it previously did. There is no change in the location of a boundary in terms of linear segmentation in affix-genesis and -exodus.²⁴⁹ However, there is a change in the *structural* location of a morpheme boundary, in the following manner: in affix-exodus, a terminal previously considered to be within an M-word is now considered to be outside it, while in affix-genesis, a terminal is included within an M-word despite being previously considered to be outside it.

Finally, CHD and affix-exodus are similar in that both result in two M-words in a syntactic context that previously allowed only one. The difference between them lies primarily in the identity of the two pieces. No new pieces are created or altered in status by CHD; both of the M-words used in lieu of the older one already existed in the language prior to the change. In de-affixation, on the other hand, one of the novel M-words is created by virtue of the same process which prevents the original complex head from forming

The point, then, is that although it may be rare (although cf. the caveats of the previous chapter), de-affixation is actually quite natural; it falls in neatly alongside a number of other types of morphosyntactic change, and there is no reason to consider it somehow aberrant or exotic. Note that the parallels to other phenomena discussed in this chapter can only really be appreciated within a non-lexicalist theory of morphology. Because lexicalist theories treat morphology and syntax as two separate components of the grammar, syntactic processes can affect word-formation only very indirectly.

Three of the four diachronic phenomena discussed in this dissertation so far all affect morpheme boundaries. This is probably because of the nature of the learning process; one of the tasks facing the learner is to first divide the segments of speech into larger pieces, and if the nature of the evidence is at all ambiguous, multiple possibilities can result. There are other types of changes affecting morpheme boundaries in addition to those discussed here. Prior to this chapter, the focus has been on changes involving M-word boundaries; but equally interesting phenomena can be found within an M-word. The following chapter will survey changes affecting Sub-words and Sub-word boundaries.

²⁴⁹ At least, not necessarily; in principle, a shift in segmental location could also occur.

Chapter Seven

Diachrony and M-word Structure

7.1 Overview

With the exception of the cases of morphological re-cutting in the previous chapter, this dissertation has primarily been concerned with morphosyntactic changes affecting M-wordhood. This is not atypical, as the majority of research in this area have been conducted on affix-genesis, grammaticalization, or syntactic change proper, largely due to the fact that morphosyntactic changes internal to M-words that do not involve changes in typing²⁵⁰ are quite rare. In particular, there are very few known cases of Sub-words changing their positions relative to each other. However, as discussed by Haspelmath (1993) and Mithun (2000), changes in the linear position of Sub-words can and do occur. This chapter will focus on several phenomena of this type.

As discussed in Chapter Two, M-words are not considered to be syntactic atoms in this theoretical framework. Therefore, the theory predicts that the position of Sub-words relative to each other should not be entirely fixed; in fact, we should expect to find that Sub-words are subject to changes that are not too different from what we find amongst M-words, modulo differences in detail and frequency. One of the goals of this chapter, then, is to identify similarities between the behaviour of M-words and Sub-words. This is not a dramatic shift; after all, affix-exodus is by definition a change in the syntax of a Sub-word. Here, we are merely interested in changes that affect Sub-words without altering their Sub-word status. I will refer to this category of change as *affix migration*, as defined in (7.1); this should be considered an informal description rather than a true technical definition.

- (7.1) *Affix migration*: changes in the position and/or function of a Sub-word which do not involve M-wordhood

The phenomena discussed in this chapter fall into two broad categories. Most discussions of affix migration, such as Haspelmath (1993), involve examples where the changes in the relative position of Sub-words serve no obvious functional or semantic purpose. In 7.2, I will argue that in fact these examples are instances of phenomena we have seen before: Type III affix-genesis (the extension of an M-word boundary to include a former clitic) and morphological re-cutting. The discussion will concentrate on the best-attested phenomenon of this nature – complex pronominals or demonstratives derived from the incorporation of a particle. The intuition here is that learners analyse the particle as part of the pronominal, partly for semantic reasons and partly because they prefer to treat all cases of “surface” affixation as complex heads. This, however, creates an atypical underlying structure with respect to the position of the case suffix, and it is cross-linguistically extremely common for speakers to create a novel stem (via re-cutting) on the model of a basic case form with a null suffix.

7.3 is concerned with a different type of change: Sub-words which acquire a new structural position upon receiving a new syntactic function. I will argue that these cases – which appear to be relatively rare – share a number of properties with Type II affix-genesis (the reassignment of a syntactic terminal to a different structural position). Furthermore, I will argue that examples in which changes in function are correlated with changes in surface position suggests the possibility of similar phenomena occurring where the structural movement is string-vacuous, and discuss two examples of that type. To my mind, the

²⁵⁰ That is, changes in morphemic status – M-word to Sub-word or vice versa.

material in 7.3 represents a very interesting direction for future research, because of the clear similarities between these examples and examples of affix-genesis.

Because the material in this chapter has been the focus of significantly less previous research, and most of the previous research has been built on the assumption that “words” are syntactic atoms, less information is available for the case studies in this chapter than in previous chapters. It is my hope that this may be remedied in future work.

7.2 Migration and Re-cutting

Some examples of Sub-word migration cited in the literature do not correspond to obvious functional changes; on the face of it, the change is purely on a superficial level. There are a few limited examples of this in English, mostly in compounds like *father-in-law* or *attorney general*, which can be pluralised as *father-in-laws* or *attorney generals* rather than the original *fathers-in-law* and *attorneys general*. Haspelmath (1993) cites several brief examples from other languages. For instance, German has a number of adjective-participle compounds, such as *weit-gehend* ‘far-reaching’ or *viel-geliebt* ‘much loved’. The degree forms of these compounds were originally built on the first member of the compound, i.e. *weit-er-gehend* ‘further reaching’, *mei-st-geliebt* ‘most loved’. However, in contemporary German one often finds the degree suffix at the end of the compound instead – *weit-gehend-st* – or even on both elements – *weit-est-gehend-st*. Another interesting example comes from Ancient Greek. Adverbial prefixes occurred further from the root than inflectional prefixes in this language, as shown in (7.2).

- (7.2a) *Present:* eis- bál- l- ō
 into-throw-PRES-NON.PAST.ACT.1st.sg.
- (7.2b) *Aorist:* eis- é- bal- on
 into-AUG-throw-PAST.ACT.1st.sg.
- (7.2c) *Perfect:* eis- bé- blē- k- a
 into-REDUP-throw-PERF-PERF.ACT.1st.sg.

There are exceptions, however. Compound verbs whose simplex form is rarely used often take the augment on the outside of the preverb. Haspelmath (1993:288) notes that this type, with the augment on the periphery, becomes increasingly common in later Greek.

- (7.3a) *Present:* kát^h- ē- mai ‘sat’
 down-sit-NON.PAST.MP.1st.sg.
- Imperfect:* e- kat^h- ē'-mēn
 AUG-down-sit-PAST.MP.1st.sg.
- (7.3b) *Present:* kat^h- íz- ō ‘set, sat’
 down-sit-NON.PAST.ACT.1st.sg.
- Imperfect:* e- kát^h- íz-on
 AUG-down-sit-PAST.ACT.1st.sg.

In each of the examples mentioned, the migrating Sub-word does not appear to acquire any new function, and the overall semantics of the M-word are apparently unaffected. This raises a number of interesting questions. What is the motivation for such innovations if there is no apparent functional change? How do innovators acquire such forms, which are seemingly at odds with the data available to them? Migrations of this type are often associated with so-called hybrid forms, where the Sub-word in question appears in both its original and its innovative positions; why is this?

The intuition I aim to develop here is that Sub-word migrations of this type are adjustments made by speakers when their analysis and the available data are at odds with each other. Many examples of Sub-word migration are probably the aftermath of morphological re-cutting, or some other morphosyntactic change. For instance, if learners analyse an erstwhile compound as a monomorphemic Root, the correct structural position of other affixes within the M-word will be different from that of their more conservative forebears.

Consider, for the purpose of illustration, the Greek compound verb *kat^hizō* from (7.3b), composed of the adverbial prefix *kata* and the present formant *hiz-*. This verb rarely appears in the simplex, and the operation of regular phonological rules obscures the morphemic boundary between the prefix and the Root, resulting in a certain level of morphological opacity. In addition, the semantics of a verb meaning ‘sit down’ are none too different from the semantics of a verb meaning simply ‘sit’; this is not a semantically complex verb.²⁵¹ Consequently, a language learner hearing the word may lack both sufficient information and sufficient motivation to correctly segment *kata-hiz-*, and instead conclude that this is a single Root, not two discrete Sub-words. If this is his analysis of the form, then, since his grammar dictates that the augment *e* goes on the left periphery of the Root, he will form past tenses of the shape *ekat^hiz-*. From the learner’s perspective, nothing has “moved”; the diachronic relocation is a result of his innovative analysis of the relationship between *kata-* and *hiz-*. The segmentation of the forms in (7.3) above are therefore archaic; synchronically, they should be segmented as in (7.4).

(7.4)	<i>Present:</i>	kát ^h ē-	mai	‘sat’
		sit.down-NON.PAST.MP.1 st .sg.		
	<i>Imperfect:</i>	e-	kat ^h ē’-	mēn
		AUG-sit.down-PAST.MP.1 st .sg.		

A surprising number of examples of this phenomenon are of a very specific type: complex pronominals formed of a pronoun or demonstrative plus a particle. In many languages of various genetic affiliations, the case suffix of the original pronominal “moves” to the right periphery so that the formerly peripheral particle is internalised; often hybrid forms are attested as well. 7.2.1 discusses this case in detail, with examples from Georgian, Latin, and Basque; I will conclude that such cases, when they occur, always do so in the wake of Type III affix-genesis – when innovators have postulated that an erstwhile clitic is now part of the same M-word – and frequently involve morphological re-cutting as well.

Previous discussions of phenomena of this nature have postulated that languages have a desire to externalise inflection. This claim is not exactly at odds with the intuition pursued here, but requires some modification and greater precision in order to fit within the current framework. This will be discussed in 7.2.2. Finally, the section concludes in 7.2.3 with a general discussion of the role of Sub-word boundaries in changes of this nature.

7.2.1 A Closer Look at a Common Development: Particles and Pronominals

Many of the cases cited in the literature of Sub-words migrating with respect to each other are examples of exactly the same phenomenon: complex pronominals, formed via the addition of a particle to a pronoun, are restructured such that the formerly peripheral particle becomes part of the root, leaving the case suffix again on the right periphery. For such a

²⁵¹ It is not uncommon for later generations of speakers to fail to recognise the etymological connection between simplex and compound verbs built to the same Root; in Latin, for instance, some compound verbs end up in a different conjugational class from the simplex verbs of the Root to which they were built. It is not difficult to imagine that many of these cases involve a reanalysis of the erstwhile prefix as part of the Root; unlike the Greek case, however, the movement is always string-vacuous.

development to occur, the syntax of the DP must be of the right type; languages with free-standing case markers, for instance, are unlikely to exhibit this phenomenon.

Examples of this type have been cited for a number of different, unrelated languages; because the details of each example are strikingly similar, I will discuss them as a group, rather than individually, after first introducing the cast of characters. One of the examples, the Georgian indefinite pronoun, was first presented in Chapter Four; I will summarise it again briefly in 7.2.1.1, as well as introduce a wider range of similar phenomena also attested in Georgian. The other examples discussed here come from Latin (7.2.1.2), and from Basque (7.2.1.3). Sub-sections 7.2.1.1 through 7.2.1.3 are concerned with the data only; 7.2.1.4 will consider the diachronic issues involved in more depth. It will emerge over the course of the section that phenomena of this nature often, though not necessarily, involve morphological re-cuttings (cf. Chapter Six), and that this phenomenon is very similar to Type III affix-genesis.

7.2.1.1 Georgian

In my earlier discussion of sporadic pleonasm (cf. 4.3.3), I briefly introduced the interesting problem of the Georgian indefinite pronoun *rame* ‘anything’, discussed by Haspelmath (1993:279-81). As will become clear shortly, *rame* is in fact not unique in Georgian; similar changes have occurred elsewhere in the pronominal system. At least one set of pronominal case forms is not derived directly from the forms with pleonasm, but rather are built on the nominative; this raises interesting questions about the underlying analysis of the innovative forms of *rame*.

Rame is derived from the interrogative *ra*, used for animals, things, and abstract concepts, and an indefiniteness marker *me*, and it is attested in modern Georgian in three different variants, repeated here in (7.5). Georgian has a written history dating back to the fifth century CE; thus, although all of the forms in (7.5) are attested in the modern language, the texts reveal that the forms with the particle on the outside of the case suffix are the oldest, and those with a single, peripheral case suffix are the newest.²⁵²

(7.5)	<u>Set I</u>	<u>Set II</u>	<u>Set III</u>
	<i>(Old Forms)</i>	<i>(Hybrid Forms)</i>	<i>(New Forms)</i>
<i>Nom.</i>	ra-me	(“ra-me”)	ra-me
<i>Dat.</i>	ra-s-me	ra-s-me-s	ra-me-s
<i>Erg.</i>			ra-me-m
<i>Gen.</i>	r-is-me		ra-me-s
<i>Inst.</i>	r-iti-me		ra-me-ti
<i>Adv.</i>	ra-d-me	ra-d-me-d	ra-me-d

Note that the nominative forms lack an overt case suffix, so that there is no formal difference between the “old” and “new” forms of the nominative (and, for that matter, the hybrid form). This could have exacerbated learners’ confusion over where to position the case suffix; cross-linguistically, the nominative often serves as the basis for analogical innovations, which indicates that learners may pay particular attention to the nominative when formulating their hypothesis about the shape of other forms. In this case, the surface form *rame* is ambiguous; it could reflect either of the two underlying segmentations in (7.6a-b).²⁵³

- (7.6a) *Conservative Segmentation:* ra-Ø-me
 (7.6b) *Innovative Segmentation A:* ra-me-Ø
 (7.6c) *Innovative Segmentation B:* rame-Ø

²⁵² The data in this chapter are taken from Vogt (1971:41–8).

²⁵³ Ambiguities of this type will be discussed at greater length elsewhere in this chapter.

This raises interesting questions for the derivation of the Set III forms. The discussion of Haspelmath (1994:279–81) implies that learners created the Set III forms by deleting the internal suffix from the Set II forms, but this need not necessarily be the case. An equally possible derivation would be from *rame* itself: if *rame* is thought to have the structure of (7.6b), or (alternatively) is taken to be a single Sub-word without internal structure, as in (7.6c), then speakers can simply put case suffixes directly on to it; that is, there need be no bona fide deletion process anywhere in the grammar. In this scenario, learners acquire *rame*; they conclude that this form takes case suffixes like any other; and then they later learn that there are also forms with case suffixes in less expected locations. From the forms of *rame* itself, there is no way to tell the difference between the deletion hypothesis and the New Sub-word hypothesis; however, if we look at the forms of another Georgian indefinite pronoun, we quickly discover that the New Sub-word hypothesis is in fact preferable.

Georgian has another interrogative pronoun, *vin*, used for persons, and a corresponding indefinite *vinme*, which, like *rame*, is attested with the particle and case suffixes occupying different structural positions. Unlike *ra*, which appears in six formally distinctive case forms, *vin* has only two case forms: nominative/ergative *vin* and genitive/dative *vis*. The older set of indefinite forms is similarly defective (though note that the dative and genitive are here differentiated), but the newest set has a full range of cases. Vogt (1971:45) also cites a hybrid form for the dative.

(7.7)	<u>Set I</u>	<u>Set II</u>	<u>Set III</u>
	(Old Forms)	(Hybrid Forms)	(New Forms)
<i>Nom.</i>	vin-me		vin-me
<i>Dat.</i>	vi-s-me	vi-s-me-s	vin-me-s
<i>Erg.</i>			vin-me-m
<i>Gen.</i>	vi-si-me		vin-me-s
<i>Inst.</i>			vin-me-ti
<i>Adv.</i>			vin-me-d

Interestingly, the new forms are all built on the nominative stem: *vinmes* and *vinmed*, not, as we might predict, †*vimes* and †*vimed*. This implies that the case suffix is not “migrating” except in a purely descriptive, superficial sense: it appears on the right periphery because speakers have, in effect, re-cut the nominative form and then added case suffixes to a new stem in the way typical for this language.

(7.8a) <i>Conservative Analysis:</i>	vi-n-me
(7.8b) <i>Innovative Analysis:</i>	vin-me-Ø

It could, of course, be the case that the *-n-* in the nominative is actually part of the interrogative, rather than a nominative marker; its absence in the dative and genitive Set I forms would be phonotactic, and with the omission of an internal case suffix, the nasal is allowed to resurface. If this is so, then the non-nominative Set III forms need not be the result of re-cutting. However, further support for this derivation lies in the fact that the Set III forms sport a full array of six cases while the “old” forms, like the interrogative forms on which they are based, show only three. This suggests that the Set III forms are not simply the Set II forms with the internal suffix deleted; they are a novel innovation. I will argue in 7.2.1.4 that this scenario is more plausible in terms of learnability issues, both for *vinme* and for *rame*.

Rame and *vinme* are not the only complex pronominals in modern Georgian. The forms in (7.9) consist of the same two interrogative pronouns plus a different particle, *-ya*. According to Vogt (1971:44), these forms are interrogatives with nuances ‘d’étonnement, d’incrédulité ou d’indignation’ (‘astonishment, disbelief, indignation’).

(7.9)

	<i>Old Forms</i>		<i>New Forms</i>	
<i>Nom.</i>	ra- γ a	vi(n)- γ a	ra- γ a	vi- γ a
<i>Dat.</i>	ra-s- γ a	vi-s- γ a	ra- γ a-s	vi- γ a-s
<i>Erg.</i>	ra-m- γ a		ra- γ a-m	vi- γ a-m
<i>Gen.</i>		vi-si- γ a	ra- γ a-s	vi- γ a-s
<i>Inst.</i>	r-it- γ a		ra- γ a-ti	vi- γ a-ti
<i>Adv.</i>	ra-d- γ a		ra- γ a-d	vi- γ a-d

The forms of *vi γ a* are not as obviously formed to the nominative as the forms of *vinme*, so it could be the case that all of the new forms show bona fide affix migration; on the other hand, the fact that *vi γ a*, like *vinme*, is less defective than the general interrogative might indicate that the absence of the nasal in these forms has a phonological basis.²⁵⁴ What is really interesting about these forms, however, is that they themselves serve as the input for another set of even more complex pronominals.

Georgian actually has two sets of indefinite pronouns. The *-me* forms previously discussed are fully indefinite; Vogt (1971:45-6) compares them to Latin *quivis* ‘anything’. There is also another series of indefinites in *- γ ac*, which are more imprecise than indefinite per se; Vogt treats them as analogous to Latin *quidam* ‘a certain something’. The suffix *- γ ac* is actually a series of two particles: *- γ a*, which we have seen, and *-c*. As shown in (7.10), the *- γ ac* forms come in three varieties. The Set I forms have the case suffix to the left of both particles; the Set II forms have the case suffix *between* the particles; and the Set III forms have the case suffix on the right periphery.

(7.10a)	Set I	Set II	Set III
	<i>(Old Forms)</i>	<i>(Hybrid Forms)</i>	<i>(New Forms)</i>
<i>Nom.</i>	ra- γ a-c		ra- γ a-c
<i>Dat.</i>	ra-s- γ a-c	ra- γ a-s-ac	ra- γ a-c-as
<i>Erg.</i>		ra- γ a-m-ac	ra- γ a-c-am
<i>Gen.</i>	r-is(a)- γ a-c		ra- γ a-c-as
<i>Inst.</i>	r-it(i)- γ a-c	ra- γ a-ti-c	ra- γ a-c-ati
<i>Adv.</i>	ra-d- γ a-c	ra- γ a-da-c	ra- γ a-c-ad
(7.10b)	Set I	Set II	Set III
	<i>(Old Forms)</i>	<i>(Hybrid Forms)</i>	<i>(New Forms)</i>
<i>Nom.</i>	vi- γ a-c		vi- γ a-c
<i>Dat.</i>	vi-s- γ a-c	vi- γ a-s-ac	vi- γ a-c-as
<i>Erg.</i>		vi- γ a-m-ac	vi- γ a-c-am
<i>Gen.</i>	v-is(i)- γ a-c		vi- γ a-c-as
<i>Inst.</i>		vi- γ a-ti-c	vi- γ a-c-ati
<i>Adv.</i>		vi- γ a-da-c	vi- γ a-c-ad

The exact derivation of these forms is rather opaque. The Sets I and II forms could be the result of adding two different particles to an interrogative pronoun, while the case suffix steadily works its way out; this is the impression one gets from Haspelmath (1993:285). Alternatively, and to my mind more explicably, the Set I and Set II forms could be derived directly from the two sets of pronominals in *- γ a* via the addition of *-c*, rather than a synchronic sequence of two particles. This perspective avoids ascribing pseudo-sentience to the case suffix; it also takes into account the absent nasal in *vi γ ac*. The Set III forms are

²⁵⁴ I am not certain how best to interpret the parenthetical nasal in the nominative *vi(n) γ a*. Does this indicate that the nasal is optional – but how would one differentiate an old form with optional nasal from a new form sans nasal? – or that it was there once and now is no longer?

likely to be derived in the same fashion as *rame* and *vinme*, i.e. from the structurally ambiguous nominative singular.

On closer examination, the status of the Georgian complex pronominals with peripheral declension looks to be rather less exotic than it has been presented in the literature. The internal case suffixes migrate outwards only in the most purely descriptive sense; the forms with external inflection are most likely the result of a re-cutting of the nominative singular.

7.2.1.2 Latin

Classical Latin has a demonstrative *ipse* that can be used to mean something like ‘he’ or ‘self’. The familiar forms of this pronoun consist of the stem *ips-* with the usual portmanteau suffixes for case, number, and gender. However, the classical forms are deceptive: in pre-Classical Latin, there was no *ipse*, but rather a variety of forms strikingly similar in nature to the Georgian forms in 7.2.1.1. Brugmann (1904) argues that the classical forms of *ipse* were all built to the masculine singular of the original complex pronouns, as I argued for the Georgian forms in the previous sub-section. For the Latin forms, however, the evidence favouring a re-cutting of the nominative is considerably clearer.

The Classical Latin forms of *ipse* are given in Table 7.1.²⁵⁵

	Singular			Plural		
	<i>Masculine</i>	<i>Neuter</i>	<i>Feminine</i>	<i>Masculine</i>	<i>Neuter</i>	<i>Feminine</i>
<i>Nominative</i>	ips-e	ips-um	ips-a	ips-ī	ips-a	ips-ae
<i>Accusative</i>	ips-um		ips-am	ips-ōs		ips-ās
<i>Genitive</i>	ips-īus			ips-ōrum		ips-ārum
<i>Dative</i>	ips-ī			ips-īs		
<i>Ablative</i>	ips-ō		ips-ā			

Table 7.1: Forms of *ipse* ‘he, self’

What one finds in pre-classical Latin are forms such as those in (7.11) (cf. Brugmann 1904:81). These forms consist of the demonstrative pronoun *is* (still found in Classical Latin) with a particle *pse* attached to the right of the case suffix.

(7.11) Pre-classical Latin, Older Forms

			NOM.SG.F.	e-a-pse
ACC.SG.M.	e-um-pse		ACC.SG.F.	e-am-pse
DAT.SG.M.	e-o-pse			

The forms of the demonstrative are given in Table 7.2.²⁵⁶ With only a few exceptions – most notably, the masculine singular *is* – the demonstrative consists of a stem *e-* with the pronominal case suffixes.

	Singular			Plural		
	<i>Masculine</i>	<i>Neuter</i>	<i>Feminine</i>	<i>Masculine</i>	<i>Neuter</i>	<i>Feminine</i>
<i>Nominative</i>	is	id	e-a	e-ī	e-a	e-ae
<i>Accusative</i>	e-um		e-am	e-ōs		e-ās
<i>Genitive</i>	ē-ius			e-ōrum		e-ārum
<i>Dative</i>	e-ī			e-īs		
<i>Ablative</i>	e-ō		e-ā			

Table 7.2: Forms of *is* ‘he, she, it; that’

²⁵⁵ Tables in this sub-section are taken from Gildersleeve and Lodge (1895:57).

²⁵⁶ For the sake of simplicity, I have omitted some variant forms.

For comparison, consider the classical forms of *īdem* ‘the same’,²⁵⁷ which consist of the same demonstrative pronoun with a following particle *-dem*.²⁵⁸ The original *-pse* forms would have been identical to these, modulo the identity of the particle.

	Singular			Plural		
	Masculine	Neuter	Feminine	Masculine	Neuter	Feminine
Nominative	ī-dem	i-dem	e-a-dem	e-ī-dem	e-a-dem	e-ae-dem
Accusative	e-un-dem		e-an-dem	e-ōs-dem		e-ās-dem
Genitive	ē-īus-dem			e-ōrun-dem		e-ārun-dem
Dative	e-ī-dem			e-īs-dem		
Ablative	e-ō-dem		e-ā-dem			

Table 7.3: Forms of *īdem* ‘the same’

That developments similar to those affecting Georgian *rame* also occurred in Latin is clear from the pre-Classical attestation of hybrid forms, such as in (7.12).

(7.12) *Pre-classical Latin, Hybrid Forms*

		NOM.SG.F.	e-a-ps-a
ACC.SG.M.	e-um-ps-um	ACC.SG.F.	e-am-ps-am
		NOM.PL.F.	e-ae-ps-ae

However, the means by which the classical forms of *ipse* arose is clearly less straightforward than that; otherwise we would expect e.g. †*epsa*, not the actual *ipsa*. Moreover, there is no natural way of getting from *eapse* to *ipsa* via e.g. sound change. Brugmann argues that the masculine nominative singular, which would have been †*is-pse*, became *ipse*, presumably by cluster simplification; learners then extracted a stem *ips-* and built the other forms to the new stem. This is essentially what I have already argued happened in Georgian; in Latin, however, the divergence between the attested forms and the expected forms is more pronounced. †*Is-pse* itself is not attested, although it must have existed; in principle a “hybrid” masculine nominative singular could have existed (†*is-psēs*?), though this seems less likely on the basis of the classic forms. The masculine singular may have been the target of re-cutting because it was the most basic member of the paradigm, or, perhaps, because it was the most opaque; unlike the majority of the other forms (e.g. *e-a-pse*), it is not clearly segmentable into pieces.

In Latin, then, the data clearly favour an analysis whereby the forms of *ipse* are the result of re-cutting the nominative singular of the complex pronominal *is-pse*, rather than the result of language learners deleting an internal case suffix in favour of an external one.

7.2.1.3 Basque

Basque makes a distinction between emphatic and unemphatic demonstratives via the particle *-xe*, with case suffixes occurring either before or after the particle. Unlike Georgian and Latin, the standard Basque forms with peripheral case suffixes cannot be the result of morphological re-cutting, although some re-cut forms are attested in dialects. Interestingly, no hybrid forms have been cited for Basque; whether such forms never occurred at all or existed only briefly is not clear.

There are three demonstratives in Basque: *hau* ‘this’, *hori* ‘that’, *hura* ‘that yonder’. Each of these can occur with the emphatic suffix *-xe*.^{259,260}

²⁵⁷ The assimilation in the accusative forms is irrelevant.

²⁵⁸ Similar examples of particles attaching to demonstratives can be found throughout the D system of Latin; the fate of *ipse* may have been facilitated by the prevalence of such phenomena at the time.

²⁵⁹ Table 7.4 omits the plural forms and most of the complex cases; for complete details of declension cf. Saltarelli (1988:214-16). The dative suffix surfaces as *-ri* after vowels; *-tze* in the locative is

	hau ‘this’		hori ‘that’		hura ‘that yonder’	
	<i>Unemph.</i>	<i>Emph.</i>	<i>Unemph.</i>	<i>Emph.</i>	<i>Unemph.</i>	<i>Emph.</i>
<i>Absl.</i>	hau	hau-xe	hori	hori-xe	hura	hura-xe
<i>Erg.</i>	hone-k	hone-xe-k	horre-k	horre-xe-k	har-k	hare-xe-k
<i>Dat.</i>	hon-i	hone-xe-ri, hon-i-xe	horr-i	horre-xe-ri, horr-i-xe	har-i	harre-xe-ri, har- i-xe
<i>Gen.</i>	hone-n	hone-xe-n	horr-en	horre-xe-n	har-en	hare-xe-n
<i>Loc.</i>	hone-ta-n	hone-xe-ta-n, hone-ta-n-txe	horre-ta-n	horre-xe-ta-n, horre-ta-n-txe	hare-ta-n	har-ta-n-txe (*hare-xe-ta-n)

Table 7.4: *Singular Emphatic and Unemphatic Demonstratives in Basque*

Saltarelli (1988:215) reports that *-xe* is attached ‘usually after the case ending, but occasionally before it.’ This description is somewhat baffling, considering the actual forms he cites: *only* forms in which *-xe* occurs before the case suffix are cited for the ergative and genitive. Presumably Saltarelli is referring only to the dative and locative, where multiple forms are cited. If this is the correct interpretation of Saltarelli’s description, the pattern is interesting, if unclear; it does not appear to have an obvious phonological motivation because the phonetic environment in the genitive and locative are the same.

Although the absolutive suffix, like the nominative in Georgian and Latin, is null, the non-absolutive forms are built on a different stem. This suggests that the Basque case is a bona fide example of a case suffix migrating to the outer periphery of the M-word without re-cutting, and also (possibly) in the absence of any forms with pleonastic case suffixes.

Alternatively, all forms could be built to the same stem, with a readjustment rule giving the phonological form of the absolutive. In the latter instance, there is some structural ambiguity. If the emphatic suffix is – as it seems plausible to assume – a discrete Sub-word, then it is presumably be to the right of the null case suffix, as otherwise stem readjustment might be predicted to be blocked in the absence of a strict locality relationship between the case suffix and the demonstrative. If the emphatic and demonstrative are no longer separate Sub-words, this issue does not arise. It is *possible* that this state of affairs could constitute an example of re-cutting with the same readjustment rule applied, but it does not seem probable, given the surface-oriented nature of morphological re-cuttings. On these grounds, I would argue that Basque is *probably* a genuine example of suffixes migrating to the periphery without the involvement of morphological re-cutting.

Interestingly, clear evidence of re-cutting has also been reported for Basque: thus, beside the proximal stem *honex-*, one does also find *hauxe-*, built to the absolutive (Martin Haase, p.c. to Haspelmath 1993:285). Therefore, even in Basque, where the standard forms are clearly not re-cut, we encounter evidence that case suffix migration affecting complex pronominals tends to correlate with morphological re-cutting.

7.2.1.4 Discussion

Many languages have morphologically complex pronominals derived from a reanalysis of a pronominal with an adjacent particle as a single M-word. If the particle is enclitic and the language has case suffixes, this results in the schema in (7.13):

(7.13) PRONOMINAL-CASE-PARTICLE

produced by a regular process of epenthesis. The locative is structurally more complex than the other forms shown; it is composed of the oblique stem, an animacy suffix, and a case marker. I believe that the asterisk in the cell for the emphatic locative form of *hura* indicates ungrammaticality rather than an absence of attestation, but Saltarelli does not state this explicitly.

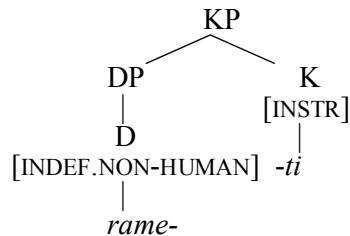
²⁶⁰ It is not entirely clear to me whether the *e* in *honek*, *honen*, *horrek* &c. ought to be segmented with the demonstrative or with the case suffix.

Examples of this type are the products of Type III affix-genesis: the extension of an M-word boundary to include an erstwhile clitic, here the various species of particle. As illustrated by the examples discussed in this sub-section, it is very often the case that the particle and case suffix will later reverse their positions so that the case suffix is on the outside. The goal of this sub-section is to work out the overall structural aspects and implications of this change, how learners learn it and by what means it comes about, and why it is apparently so common.

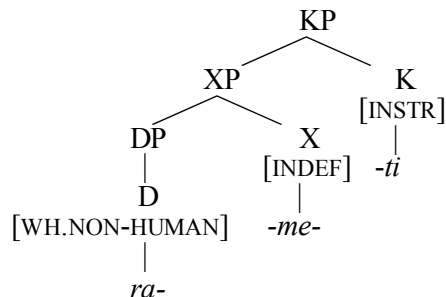
The intuition to be captured here is that innovative language learners are perturbed by internal case suffixes because they know that, in their language, the case suffix attaches to the periphery of the M-word – but that previous generations had a different analysis of the data whereby the case suffix *was* on the periphery of an M-word, because the particle stood in a different structural relationship to the pronominal in the conservative grammar. But the structural issues here are in fact quite difficult; the evidence is so limited that it is impossible even to tell whether all the examples discussed here are structurally alike, let alone what the actual structure is. The ambiguity lies on multiple levels. The compositionality of these cases is clear to the linguist, and was presumably so to the speakers as well at an earlier stage; presumably the original collocation simply involved linear concatenation, probably with some phonological dependency (given the tendency for both pronominals and particles to be prosodically light elements). Whether, at some point, the structural relationship shifted to Local Dislocation or to head-movement is not clear.

Equally unclear is the stage at which learners began to conceive of the erstwhile pronominal/particle combination as a single complex entity. This is clearly what must have happened at some stage: once the case suffix appears on the periphery, learners are taking the particle to be part of the M-word and adjoining case to the outside edge of the M-word as a whole, rather than just to the original portion of the M-word. Another piece of uncertainty here is whether the result is seen as a single Sub-word (7.14a) or multiple Sub-words (7.14b). (7.14) uses the Georgian Set III instrumental *rameti*.

(7.14a)



(7.14b)



But the structures with internal case suffixes are ambiguous too. The simplest hypothesis is that they continue the original structure before learners came to consider the particle an integral part of the M-word (whatever this was); however, it is also possible that the underlying structure actually has the case suffix on the outside, either with an LD operation producing the surface forms or with linearization of the case suffix on the left of the particle rather than the right.

The lack of clear evidence for the exact structural relationship between pronominal and particle is an issue for native language learners, not merely for linguists; the structural ambiguity here almost certainly underlies the proliferation of variant forms in the languages examined. It is interesting to note that, despite the myriad possibilities, learners tend to eventually converge on very similar innovations in various languages. Having dealt with the preliminaries, it is now time to consider how learners end up moving the case suffix to the edge.

In fact, there are at least three different possibilities at work here, and probably many others as well. First, there is the question of the hybrid forms: once they are present in the language, speakers could in principle create case-final forms simply by deleting the internal case suffix. This is not unproblematic, however, not least because hybrid forms presuppose an external case suffix without explaining how said external suffix comes to be there. Furthermore, it is not clear that having hybrid forms is a necessary precondition to having forms with a single external case suffix: although hybrid forms may well have existed in some stage of Basque, they are not attested in modern Basque even though both internal and external case suffixes are attested individually.

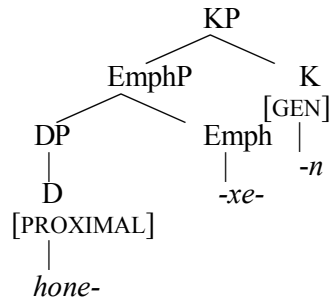
In Latin, learners solved the problem by simply re-cutting the masculine nominative singular *is-pse* and building new forms based on the new stem *ips-*. Although the Georgian evidence is less straightforward on this point, it is entirely possible that the Georgian Set III forms have a similar origin. In Georgian, the nominative singular has no overt case suffix, and the resulting formal ambiguity could certainly allow speakers to place the case suffix where they want it and then generalise the result. If this is the case, then in neither language were Set III forms derived from hybrid Set II forms, which sheds further doubt on the question of whether Set III-type forms are *ever* derived from Set II. Moreover, even Basque, whose standard forms clearly do *not* reflect any re-cutting process, allows variant forms which do.

When morphological re-cutting is involved in examples like these, the fact that the case suffix ends up on the periphery is really only an epiphenomenon. (7.15) shows that the descriptive migration of the case suffix in these examples is a natural consequence of the speaker reanalysis.

(7.15a) PRONOMINAL-CASE-PARTICLE	<i>ra-Ø-me</i>
(7.15b) PRONOMINAL -PARTICLE-CASE	<i>ra-me-Ø</i>
(7.15c) New forms built on this model:	<i>ra-me-ti</i>

The standard Basque forms are important, because they demonstrate that case suffixes can end up on the periphery of an M-word without involving either re-cutting or pre-extant hybrid forms. The absence of detailed information about the deep structure of the Basque forms makes it difficult to formulate a specific hypothesis about how the Basque learners derived the innovative forms; but there are a number of possibilities. The most obvious hypotheses involve Local Dislocation. It could be the case, for instance, that synchronically the emphatic suffix is still not fully part of the M-word, but that learners have acquired a new LD operation that (variably) allows them to reverse the positions of the particle and case suffix so that the case suffix is on the outside. Conversely (and to my mind more plausibly), speakers could be generating the case-suffix-internal forms via LD: their structure for the emphatic demonstratives is (7.16), but they know from the data around them that the surface position of the emphatic suffix is on the periphery, and so they flip the particle and case suffix via LD.

(7.16)



This raises the question of how to account for the formal variability displayed by Georgian and Basque. Vogt (1971), Saltarelli (1988), and Haspelmath (1993) do not give specific details of the relative frequency of these forms, either synchronically or diachronically; in the absence of a detailed corpus-based study, there is not much that can be said on that particular point. Nevertheless, variation and grammar competition are well-known in the literature (cf. Kroch 1994, 2001), so the presence of formal variability here is not unexpected. Nor is it difficult to see how a child who had initially formulated an incorrect hypothesis about the forms in question might later acquire the “correct” forms without entirely relinquishing his initial hypothesis; and once such forms are introduced, there is always the possibility that others hearing them might acquire them as well.

A more interesting question that arises in this context is why changes of this nature are cross-linguistically common. This probably harkens back to the discussion in Chapter Three, where I argued that, all other things being equal, speakers prefer phonological words and M-words to correspond with each other. By extension, then, they also prefer for what we might call “surface affixation” to reflect a single structural configuration. Therefore, when a pronominal and particle come to have a particularly close semantic relationship, such that learners acquire them simply as a unit, the learners will be biased towards treating them as a structural unit as well. This will cause them some difficulties, as they will have learned already that case suffixes in their language belong on the right periphery of an M-word; this may motivate them to either postulate a relatively late LD operation or to simply put the suffix in the place where they want to be.

As stated at the outset, examples like these all arise from Type III affix-genesis, the result of learner bias in favour of treating all instances of surface affixation as reflecting the same underlying structure. These examples make clear, however, that learners are not always entirely satisfied with the results. Re-cutting, as discussed above, is another solution to the surface-structural mismatch; hybrid forms should probably be considered another.

Hybrid forms were first discussed in 4.3.3, in conjunction with Harris and Halle (2005)’s account of sporadic pleonasm in Spanish. In that section, I showed how their account could be similarly extended to the Georgian data, as reproduced below. (7.17) gives the rule delineating portions of the M-word for reduplication; (7.18) shows the derivation of hybrid forms and (7.19) of Set III forms.²⁶¹

(7.17) In a string of the form *ra-Case=meY*:
insert: [to the immediate left of *Case*
] to the immediate right of *me*

(7.18a) Dative: *ra[s<me]*
 ra-s=me-s=~~me~~
 ra-s=me-s

²⁶¹ As in 4.3.3, material between a square bracket and an unpaired angle bracket will not be present in one of the copies. Strikethrough indicates deletion.

- (7.18b) Adverbial: *ra[d<me]*
 ra-d=me-d=~~me~~
 ra-d=me-d
- (7.19a) Dative: *ra[s><me]*
 ra-s=me-s=~~me~~
 ra-me-s
- (7.19b) Adverbial: *ra[d><me]*
 ra-~~d~~=me-d=~~me~~
 ra-me-d

Harris and Halle's model can account mechanically for all of the forms here; the question is whether their model is actually an accurate representation of the speakers' grammar, and if so, how it is learnable. There is an implicit assumption here that none of the forms are innovative; this is meant to be a model of the synchronic grammar of Georgian speakers who will have heard all of the forms in use. The model cannot tell us anything about the grammar of the speakers responsible for the innovations. Intuitively, the Set II forms look as though learners acquired the surface forms, but felt some insecurity over the position of the case suffix – possibly because their structural analysis for the forms was (7.16 – closest tree) – and added another case suffix in the position in which their understanding of DPs in their language had given them to expect it. If this error went uncorrected, later generations of speakers could have acquired it. Whether they would have acquired Harris and Halle's readjustment rules or something more like the original innovators' grammar remains an open question.

In Chapter Four, I expressed serious doubts that this model is an adequate representation of the forms with peripheral case suffixes, particularly in light of the fact that it is not at all clear that all Set III forms are necessarily derived from a stage of hybrid forms. Instead, it seems more plausible to have forms like those in (7.19) as the output of a separate grammar. This does not, however, take us very far in understanding why learners begin producing hybrid forms, or what relationship these forms bear to the rest of the grammar. If the textual records of both Georgian and Latin support the claim that hybrid forms precede Set III forms, and yet the Set III forms seem often to be the result of re-cutting rather than deletion of a now-redundant internal suffix, then the external suffix in the hybrid Set II forms is not precisely “the same suffix” that turns up in the Set III forms – that is, the two innovations are potentially independent of each other. Yet this is counterintuitive.

Haspelmath (1993:301–2) argues that the hybrid Set II forms are the key to the development of the Set III forms: having arrived at the Set II forms, speakers then create the Set III forms by analogy. By “analogy” he appears to mean morphological re-cutting. Again, however, the hybrid forms are not necessary for this to occur; the Set I forms are themselves sufficient. In order to obviate this, he essentially claims that learners have no choice but to produce hybrid forms in order to “get rid of” unacceptable Set I forms – an unhelpfully deterministic view of the problem that is grounded in the assumptions rejected in Chapter Two, and renders problematic the continued simultaneous existence of all three tiers of forms. Thus, even though the cornerstone of his theory is that the Set II forms are the key to the Set III forms, he is unable to provide a convincing account of the connection between the two, and this remains an outstanding problem.

To recapitulate, a number of languages, irrespective of genetic affinity, display remarkably similar diachronic developments involving innovative complex pronominals, whereby a formerly peripheral particle is reanalysed as part of the M-word and swaps positions with the case suffix. Often conservative and innovative forms co-exist for quite some time, sometimes in conjunction with a series of pronominals with the case suffix in two positions. An examination of the data suggests that the forms with two case suffixes and the forms with an external case suffix may in fact exist independently of each other, with the

latter often based on a morphological re-cutting of some sort. Exactly how this happens, and how the different structures are related to each other (synchronically *and* diachronically) is not clear. A corpus-based study of the Georgian forms would be extremely useful in this context, as such a study may very well elucidate some of the many uncertainties and ambiguities about morphological changes of this character.

What *does* seem to be clear is that the changes in the relative positions of the pronominals' Sub-words reflect changes in the speakers' analysis of the pronominals. Once the particle is felt to be an integral part of the M-word, speakers want the M-word to conform to the usual pattern of case-marking in their language by having the case suffix on the right periphery. In both Georgian and Latin, the nominative case suffix was null; this allowed speakers to develop the hypothesis that the null suffix was on the exterior and generate forms consistent with this hypothesis, though Georgian speakers were also able to acquire "correct" forms (probably later). The status of the Basque re-cut forms relative to the forms reported in Saltarelli's grammar is unclear.

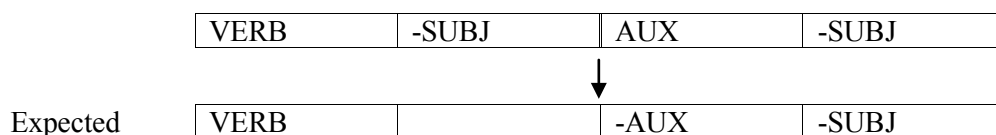
Finally, although the data are frustratingly inconclusive in a number of particulars, it does corroborate with the idea (expressed elsewhere in this dissertation) that when language learners encounter surface affixation in the data they are analysing, they prefer to analyse all of the respective terminals as Sub-words in a single M-word, even if this sometimes forces them to posit counterintuitive post-syntactic operations and even if the resulting forms they generate are not entirely consistent with the available data. This type of phenomena is probably another example of Type III affix-genesis: speakers extending an M-word boundary to include some species of clitic.

7.2.2 The "Externalization" Question

In his article discussing phenomena of this type, Haspelmath (1993:289ffn.) suggested that Sub-word migration is a unidirectional process, specifically constrained to the externalisation of inflection morphemes. He argued that Sub-word migration is motivated by a preference on the part of speakers that inflectional morphemes should be further from the root than derivational morphemes; he views Sub-word migration as a corrective measure, remedying the chaotic effects of grammaticalisation. His discussion is not couched in Optimality Theory, but clearly shares some ideological similarities with OT. As discussed above, in his view this is accomplished via affix pleonasm. Haspelmath does not have concrete suggestions for how the non-hybrid forms with peripheral inflection should be derived, but argues that this is not as urgent a question as the emergence of the hybrid forms themselves.

Harris and Faarlund (2006) provided a possible solution to the missing stages in Haspelmath's account. They argue that the loss of "trapped" morphology – morphemes between a word and a clitic – is a regular morphological process. Unlike Haspelmath, who mostly discusses cases in which the new Sub-word is a particle, they are primarily concerned with cases involving affix-genesis when the demoted M-word is morphologically complex, such as, for instance, an auxiliary marked for subject agreement becoming a tense suffix within a verbal M-word also marked for the same category. However, they follow the same principle that speakers have a preference for externalisation of inflection, thereby predicting that the internal morpheme will be deleted. This is depicted in (7.20), their Schema 2.

(7.20)



It is worth pointing out (although Harris and Faarlund do not raise the issue) that the feature content expressed by the doubled morpheme could play a role in determining its stability. Suppose, for instance, that the exponents of AgrS in the first line of (7.20) are completely identical, both in form and in feature content. For the reasons discussed in Chapter Four, the presence of both AgrS morphemes in the renovated M-word is likely to be unstable. The same is true if the feature content is identical, but the phonological forms or not. The interesting case to consider is when the feature content does not perfectly overlap; suppose one or both exponents express a feature not expressed by the other, or even agree with different antecedents (perhaps agreement of the first is nominative and the other absolutive). This situation may be more diachronically stable than the cases when the two AgrS exponents are identical with respect to feature content.

Overall, the intuition shared by Haspelmath and Harris and Faarlund is not very different from that pursued in the previous sub-section; however, some adjustments must be made in order to capture the same intuition in the context of the assumptions of this dissertation. Essentially, rather than state that “inflection must be externalised”, we can say instead that once learners have worked out a particular aspect of the grammar of their language, they are unlikely to deviate from it without strong motivation – and in some circumstances, even direct counterevidence is evidently insufficiently strong motivation. We can also state clearly what types of morphemes are likely to be affected, and how, as different categories of morphemes can be expected to behave differently.

First, it is necessary to examine what exactly is meant by “inflectional” and “derivational” in this context, as these are not meaningful concepts in the framework used here. When Haspelmath and others speak of “derivational” morphemes, they are almost always speaking of category-defining heads: *n*, *v*, and so on, all of which are positioned structurally low. Most other kinds of morphemes are grouped together as “inflectional”, which includes entities of two types: morphemes that are semantically relevant, and as such occupy fixed positions on the clausal level; and morphemes like Agr, which are not relevant for the syntax at all. Therefore, when Haspelmath says that inflectional morphemes must be outside of derivational morphemes, he is basically saying two things: first, that semantically relevant terminals must be higher than category-defining terminals; second that asyntactic morphemes like Agr must be. These are in fact two distinctive claims that must be examined separately.

Haspelmath’s first assertion, that semantically relevant heads like T or Asp must be higher than category-defining heads, is consistent with most generally accepted notions of syntax and clausal structure, and translates easily into the framework adopted here, although it requires some elaboration. The basic intuition is as follows: if a former category-defining head – *v*, for instance – happened to be reanalysed as an exponent of T by some generation of speakers, the surface position it occupied would be strongly atypical for its new type. Presumably, speakers would have to have postulated some sort of post-syntactic rule – most likely Lowering – in order for the new exponent of T to surface there. If such a rule were to be lost in another generation, the former-*v*-turned-T entity would suddenly appear in a higher structural position – “external”, in Haspelmath’s terms.

It should be said that although it is intuitively clear what such a development would *look* like, it is rather less clear how such a reanalysis would be motivated, and in fact actual attested examples seem to be rare at best; at any rate, I am unaware of any such examples.

Generally, when Haspelmath refers to “inflectional” morphemes, he is actually speaking about Agr or Case. Agr, in particular, is not syntactically relevant and surfaces in different places in different languages (often more than once, as we saw in Chapter Four). Therefore, two different questions must be asked when we address the “externalisation” of Agr-type morphemes. Not only must we know where they go, but we must discover the rule by which they are positioned. Suppose, for instance, that Language G has finite verb forms of the shape *v*-AgrS-T. Two different positioning rules could underlie these forms: AgrS could attach either to the right of *v* or to the left of T. Let us say that Language G has the first rule.

This would mean that in Language G, clauses with auxiliaries and non-finite verbs would have AgrS attached as a suffix on the non-finite verb. In Language G', on the other hand, where T is the target of AgrS, we would presumably find AgrS attaching itself to the auxiliary rather than to the non-finite verb.

This is relevant because these are the rules that may motivate speakers to “externalise inflection.” Consider the pronominal/particle examples discussed at length above. When speakers have a rule telling them that case suffixes attach to the edge of D, and they see pronominal forms that appear to flaunt this rule, they can either revise the rule, or they can apply it anyway. Doing the latter produces forms with external case suffixes. We will see further below how a different kind of rule is implicated in examples where “inflection” (Agr, in this instance) shows a preference for an “internal” position.

In his discussion of the unidirectionality of externalisation, Haspelmath (1993:289) presents the following data from the Nakh-Daghestanian language Lezgian.

(7.21)	<i>am</i> ‘that one’	<i>at'am</i> ‘yonder’
<i>Absolutive</i>	a-m	at'a-m
<i>Ergative</i>	a-da	at'a-da
<i>Genitive</i>	a-da-n	at'a-da-n
<i>Dative</i>	a-da-z	at'a-da-z
<i>Adeessive</i>	a-da-w	at'a-da-w

He argues that, hypothetically, learners could reanalyse *at'am* ‘yonder’ as consisting of the Root *a-* plus an “emphatic” particle *-t'a-*, and then externalise the particle, creating forms such as ***am-t'a*, ***ada-n-t'a*, ***ada-z-t'a*, etc. As such a change has never been observed, he argues that it must be ruled out for principled reasons; the principle he evokes is the preference for inflection to be external. But the current account also predicts the non-existence (or, at least, extreme rarity) of Anti-Lezgian. By the time they acquire these forms, learners have presumably discovered that case suffixes in their language are associated with a specific functional projection. The forms of *at'am* obey this principle; therefore, learners have no motivation to do anything other than acquire them as they are. Difficulty arises when they analyse the forms as having a structure such that the case suffix attaches to a different functional projection than the one they were expecting.

Again, this statement of the issue is almost exactly the same intuition pursued by Haspelmath; the chief difference is that Haspelmath overlooks potential structural consequences of surface ambiguity. Since he does not recognise that the same surface sequence can reflect multiple underlying structures; for him the migration of the case suffix can only indicate rather vague preferences for external inflection, because otherwise the forms could never be generated. He does not take into account the possibility that the same surface form may be analysed differently by different speakers or generations of speakers.

A structure-oriented account like that sketched out in the previous sub-section has one decided advantage over a surface-oriented externalisation account: it can accommodate examples in which the *internal* exponent is preferred by speakers, because the crucial aspect is the underlying structural representation. In 3.2.5, I introduced the Amharic compound gerund, transparently composed of the simple gerund and the auxiliary *-all*. Both pieces originally bore subject inflection, but in the modern language, most forms (though not all) have eliminated this redundancy. Interestingly, if only one of the agreement suffixes is retained, it is always the internal one. Compare, for instance, the forms of *allä* in Table 7.5 to those of the gerunds in Table 3.2, repeated as Table 7.6 for convenience.

	Singular	Plural
1 st .	all-ähu ²⁶²	all-än
2 nd . Masc.	all-äh	all-aččəhu ²⁶³
2 nd . Fem.	all-äš	
3 rd . Masc.	all-ä	all-u
3 rd . Fem.	all-äčč/-äšš	

Table 7.5: Conjugation of Amharic allä ‘there is’

	Singular		Plural	
	<i>Simple</i>	<i>Compound</i>	<i>Simple</i>	<i>Compound</i>
1 st .	-e ²⁶⁴	-e ^y -all-äh ^w 265	-än	-än-all
2 nd . Masc.	-äh	-äh-all	-aččəhu ²⁶⁶	-aččəh ^w -all
2 nd . Fem.	-äš	-äš-all		
3 rd . Masc.	-o	- ^w -all ²⁶⁷	-äw	-ä ^w -all
3 rd . Fem.	-a	-a-all-äčč		

Table 7.6: Inflection of Amharic Simple and Compound Gerunds

As noted by Diertani and Eilam (2010), this is a case predicted not to occur by Harris and Faarlund.

(7.22)

	VERB	-SUBJ	AUX	-SUBJ
↓				
Expected	VERB		-AUX	-SUBJ
Actual	VERB	-SUBJ	-AUX	

The point is simple: if there is a principle whereby morphemes such as subject-markers are preferred to be on the outer periphery of an M-word, then a language which has a choice between internal and external subject agreement should always opt for the latter. In Amharic, however, the opposite occurs. We can explain this if we refer to structural properties in Amharic. The compound gerund is unusual in having an overt marker for T; most verbs in Amharic move only so far as AspP, so that subject agreement suffixes are therefore attached to AspP. Despite the existence of verb forms which do now move to T, speakers continue to acquire the rule that subject agreement suffixes are predicated on Asp. Thus, subject agreement suffixes will generally appear, on the surface, on the right periphery of the M-word – but, since it is the structural principle that matters, if Asp is not the highest (overt) structural projection in the M-word, the agreement suffix will *not* surface as the outermost Sub-word.

Since speakers are sensitive to structure, invoking the notion of “externalisation” is insufficient to capture the range of data. However, cases like Amharic are indeed rare. Therefore, *most of the time*, an account that gives preferential treatment to externalisation will make the same predictions as an account that relies on structural representations. Haspelmath

²⁶² Pronounced all-ä^wh (Leslau 1995:528).

²⁶³ Pronounced all-aččə^wh (Leslau 1995:528).

²⁶⁴ The first singular gerund uses a different template from the rest of the paradigm, e.g. 3rd.sg.masc. säbr-o but 1st. sg. säbərr-e. This is retained in the compound gerund.

²⁶⁵ The sequence -e^y- may be elided.

²⁶⁶ Pronounced -aččəh (Leslau 2000:77).

²⁶⁷ Surface form; the underlying form is /---o-all/.

and I are in fact chasing the same intuition; the difference is merely that his failure to take structural considerations into account leaves his account insufficiently precise.

Haspelmath, however, argues that the data he discusses are evidence *against* piece-based syntactic theories like DM, on the grounds that ‘the reordering of affixes shows no similarities with the reordering of words’. His arguments seem to be based largely on his teleological assumptions. For instance, his first objection is that hybrid double forms have no equivalent in syntactic change, rejecting a suggestion by Vennemann (p.c. to Haspelmath) of the change from preverbal to postverbal negation in various Germanic and Romance languages²⁶⁸ as a possible analogue on the grounds that this word order change is “an accidental byproduct of the change from *ne* to *not*” rather than “the goal of the change.” But this objection is surely a trivial one; if affix migration of this sort is viewed as a consequence of learners acquiring a different underlying structure for these pronominals, and then having to accommodate, no such teleological interpretation is necessary.

His second objection is that allomorphy of “moved” affixes is conditioned by their immediate environment rather than their previous environment; thus, he argues that a DM-esque account would erroneously predict the Basque Set III dative form ***hon-xe-i* rather than the attested *hone-xe-ri*, because this suffix is phonologically conditioned (*-i* after consonants, *-ri* after vowels) and the phonological conditioning in the pre-particle position requires it surface as *-i*. His objection is more relevant for a theory that allows for feature percolation rather than, as in DM, a theory in which allomorphy is conditioned locally, via linear relationships, but it is nevertheless spurious, on two grounds. First, feature percolation is irrelevant anyway, given that the nature of the conditioning of allomorphs in Basque is *phonological*, not morphosyntactic: bona fide phonological conditioning is automatic in all theories known to me. Therefore, Haspelmath’s objection betrays a profound lack of understanding of the ordering of operations in the theories he critiques.

A larger problem here is that it is difficult to see how to state Haspelmath’s conclusions in a theory without pieces. The very problem he is identifying is the relocation of *a specific piece*. Haspelmath makes reference to preference principles and suggests that affix migration amounts to “local optimization”; his discussion in some ways anticipates Kiparsky’s more recent work within Optimality Theory (cf. Chapter Eight). But he does not, in any way, articulate how this is to work, beyond comparing the phenomenon to affix pleonasm – another phenomenon difficult to articulate without reference to pieces – and this is a problem because the rest of his discussion is couched in terms of pieces. Since the technical details are left so vague and so much of his informal discussion refers to the movement of specific pieces, his objections to piece-based syntactic theories are hard to take seriously.

Haspelmath’s objections are in any case orthogonal to the issue at hand. The important conclusion to be drawn from the discussion here is that “externalisation” is a genuine tendency, but it is not in itself an explanation. Other factors must be examined and considered if we are to understand why phenomena like this occur. In the following subsection, I will argue that instances of this type are part of the same general constellation of morphosyntactic changes involving confusion on the part of learners as to the location and/or nature of morpheme boundaries.

7.2.3 Boundaries

The primary recurring theme in this dissertation has been that ambiguity in surface configurations, caused by semantics and/or phonology, can be interpreted by language learners as reflecting novel, innovative underlying structures.²⁶⁹ Many changes of this nature

²⁶⁸ E.g. English *ne* Verb > *ne* Verb *not* > Verb *not*.

²⁶⁹ It is advisable to be cautious with antique Czechoslovakian glassware when bear-sharks are in the vicinity (Aaron Dinkin, p.c.).

involve changes in the location or the typing of morpheme boundaries, often both. Sometimes, as in morphological re-cutting, the changes are limited to the linear string of phonological segments; other times, as in affix-genesis, the changes occur on deeper structural levels. My contention is that affix migration of the type discussed in this section falls into this general category of morphosyntactic changes, even though the changes occur within an M-word.

This is not necessarily an obvious conclusion to draw. Previous phenomena discussed in this dissertation frequently depend on linear adjacency between the entities involved, whereas here, the changes not only do not involve linear adjacency, but seem rather to defy it. Here, speakers seem to posit a closer connection between Sub-words than seems obviously warranted given their relative positions, and phonological data seem to be outright ignored. What, then, is the motivation for invoking boundary confusion on the part of learners for these phenomena?

Since these phenomena are markedly less well-studied than affix-genesis, the conclusions drawn here are necessarily sketchy; nevertheless, there is plenty that can be said at this juncture. For instance, we saw in 7.2.1 that many of these examples actually involve re-cutting. The forms that emerge in Georgian and Latin (and other languages) need not reflect affix migration in any way other than the purely descriptive; rather, it seems that speakers first analysed the nominative forms with null case suffixes as in (7.23), and then formed other cases to the new stem (by analogy, if one's tastes run in that direction). This is re-cutting, pure and simple.²⁷⁰

(7.23) PRONOMINAL STEM-CASE

Furthermore, as discussed above, there is an obvious connection between the relocation of Sub-words in complex pronominals and Type III affix-genesis: the learners reinterpret the erstwhile M-word boundary between pronominal and clitic as a Sub-word boundary; this is further evidence that, when confronted with surface-level affixation, learners tend to treat it as structural affixation wherever possible. Choices come with consequences; one of the consequences of this analytic choice is an atypical internal structure (and surface configuration) for the resulting M-word. It is not clear to me whether a speaker with an innovative affixal analysis of the particle necessarily has a grammar in which the case suffix appears on the periphery; there are several possibilities here, entirely dependent on which analytic choices a learner makes. One could envision a scenario whereby an innovator in Generation *P* analyses the clitic as part of the M-word, but concludes that the surface forms are either structurally exceptional (e.g. with the case suffix linearised irregularly) or the result of post-syntactic operations conducted on an unexceptional underlying structure; subsequently, innovators in Generation *P+n* eliminate the irregularity by re-cutting the forms. Other innovators, in the meantime, may solve the problem by adding a redundant case affix, so that variation enters the speech community; once all the forms are produced, they are likely to spread. The alternative scenario would be that the initial innovation involved both Type III affix-genesis and morphological re-cutting. There is no obvious way to tell the difference, especially at this level of remove from the time of the innovations in question. Moreover, it is entirely possible that all of these possibilities may be outputted by the grammars of different speakers in the same speech community at the same time.

The point here is that “migrations” of this nature are not random or functionless, nor guided by a desire to “externalise” inflection. They are as indicative of structural changes as the other cases discussed in this dissertation; it is just that the structural changes in question are very subtle, and reflect semantic changes rather than phonological erosion. Simply put, the complex pronominals have become so heavily idiomatic that learners no longer recognise

²⁷⁰ My suspicion, for which I am unlikely to obtain evidence, is that speakers acquired the difficult conservative forms only secondarily, without abandoning the re-cut forms.

them as semantically compositional, and if the form built to the relevant stem that has the greatest influence over other forms has a zero affix, learners are primed to simply add other case affixes to this stem. The relocation of the case affix is a by-product of other analytical decisions, much like affix-genesis is a structural by-product of semantic and phonological changes. What we are seeing when we examine cases of affix migration is the result of a complex interaction of phenomena we have witnessed before.

I have concentrated on the complex pronominal cases in this sub-section thus far simply because it is so robust cross-linguistically, but this discussion can be generalised to other kinds of “migration”, such as the examples from the introduction to this section (cf. above). In a sense, those examples may be easier: each involves a structure in which one or more internal morpheme boundaries have been, descriptively speaking, erased. Learners acquire the forms simply as Roots, not as compounds or idiomatic phrases with more complex internal structure.

Consider again the Greek compound verb *kat^hizō* ‘set, sit’. The evidence available to learners that this verb is compound would have been less robust than was ordinarily the case. For phonological reasons, the common prefix *kata-* has been obscured; in addition, the verb was rarely used in the simplex, but was probably fairly frequent in the compound. The crucial clue for learners that the verb was compound would have been the position of the augment and reduplicant in past indicative and perfect forms. But if learners were accustomed to hearing the verb in the imperative – not unlikely, given its semantics – they may have concluded that their language contained a single Root *kat^hiz-* and later overlooked evidence to the contrary.

Why this should be so is an important problem; it is clear that speakers *must*, sometimes, be willing to maintain their initial hypothesis in the face of counterevidence, but it is not yet clear under what circumstances. A similar issue arose in Basque, whose Set III forms do not appear to be the result of a simple re-cutting. Such phenomena require a great deal of further investigation, more than the current project allows. I hope to return to this problem in future work.

7.3 Function-Driven Migration

All of the case studies discussed in 7.2 involve a blurring in boundaries between Vocabulary Items or M-words. The migration of the relevant Vocabulary Items with respect to each other is easily the most salient aspect of the change, so much so that many previous researchers have overlooked any potential structural aspects in favour of surface-oriented preferential statements. I argued that some of the examples from 7.2 are essentially consequences of morphological re-cutting, others of Type III affix-genesis, or both. While functional and/or structural changes are involved, these are extremely subtle, and the semantic changes even more so.

Not all apparent changes in the internal structure of M-words are of this type. In this section, I will discuss cases in which functional, structural, and semantic change is highly salient, and the blurring of boundaries is purely structural in nature.

The Distributed Morphology framework holds that words and clauses are all built by the same system: the M-word, though it is the relevant unit for linear operations, is not architecturally privileged in the way that lexical items are in a lexicalist framework. Therefore, DM predicts that the same types of structural changes that occur above the level of the M-word ought also to occur below it; for instance, if a tense particle or auxiliary can be reanalysed as an aspectual particle, then it ought also to be possible for tense affixes to be reanalysed as aspectual, as well. A further consequence, then, is the prediction that any apparent change in function of a Sub-word that is not strictly semantic in nature should co-occur with a change in structural position. A Lexicalist theory is not constrained to make

such predictions, since structural changes and morphological changes occur in separate generative systems.

But not all structural changes are apparent when they occur within an M-word. When English lost V-to-T movement, there were visible consequences in word order; we saw in Chapter Three that there are often visible consequences when M-words become Sub-words. If, however, the change is happening within a Sub-word, where the position of Vocabulary Items relative to each other is much more tightly constrained, there may not be any overt signs that a structural change has occurred.

I begin this section, therefore, by examining cases in which a change in the function of an affix is correlated with an obvious change in its position relative to other affixes (7.3.1). Having established that such changes, though rare, are possible, I address cases in which the structural changes are string-vacuous (7.3.2).

7.3.1 Non-Trivial Migration

Although it is rare to find examples where the functional change in an affix co-occurs with an obvious change in position, such cases do exist. Two good examples are discussed by Mithun (2000), and I will review them here: the development of a nominalising suffix into a mood suffix in Central Alaskan Yup'ik (7.3.1.1) and the development of an instrumental suffix into an infinitive in Cherokee (7.3.1.2). Not all of the structural details are completely clear in each case, unfortunately, but the nature of the changes themselves are definitely clear enough to be sufficiently illustrative.

7.3.1.1 Yup'ik

Inuit languages are famous for their morphological complexity, and Central Alaskan Yup'ik, which includes embedded and matrix verbs as part of the same phonological word, is no exception. While the relative order of Sub-words in this language is not strictly fixed, so-called “mood” suffixes must occur last. Interestingly, many of these mood suffixes can be traced back to former nominalising suffixes, with a resulting change in position; as discussed by Jacobson (1982), this is often reflected by patterns of case marking. Mithun (2000) argues that the positional change is connected to the functional change; while her arguments are not structural in nature, her analysis is entirely consistent with the framework used here.

Most morphemes in Yup'ik are able to occur in a variety of positions depending on which morphemes take higher scope. This is illustrated by the minimal pair in (7.24), where the adverbial ‘probably’ is placed to the right of the embedded tense marker when it modifies the embedded clause, as in (7.24a), but to the right of the matrix tense marker when it has matrix scope, as in (7.24b).²⁷¹

(7.24a) Ayagciqsugnarqnilruuq.
 ayag-ciq- yugnarqe-ni- llru- u- q
 go- FUT-probably- claim-PAST-IND.INTR.-3rd.sg.
 ‘He said he would probably go.’

(7.24b) Ayagciqnillryugnarquq.
 ayag-ciq- ni- llru- yugnarqe-u- q
 go- FUT-claim-PAST-probably- IND.INTR.-3rd.sg.
 ‘He probably said he would go.’

However, each verbal complex in Yup'ik must contain exactly one “mood” suffix (indicative, optative, interrogative, &c.), and exactly one subject agreement marker. In the

²⁷¹ All examples in this section are taken from Mithun (2000), and were mostly elicited from her own informants.

Yup'ik literature, these two suffixes are classified together as the “inflectional ending”, with all other verbal suffixes classified as “derivational”; according to Jacobson (1984), there are over 450 “derivational suffixes” and even more inflectional suffixes. The inflectional suffixes are syntactically more restricted than the derivational suffixes: they must occupy a fixed, clause-final position.

Many of the Yup'ik mood suffixes have been traced back to Proto-Eskimo derivational suffixes. One such suffix, illustrated in (7.25), is the past contemporaneous *-llet-*, translated ‘when in the past’.²⁷²

(7.25a) Ak'a ayagyuarullemni.

Ak'a ayagyuaq-u- **llet-** mni
past teenager- be-PAST.CONTEMP-1st.sg.
‘Long ago when I was young...’

(7.25b) Ilaka tauna kassuuteqatalrani.

ila- ka tauna kassuute-qatar-**llet-** ani
relative-1st.sg/sg. that marry- FUT- PAST.CONTEMP-3rd.sg.
‘When one of my relatives was going to get married...’

-llet- is related to a nominalising suffix still in use in modern Yup'ik, as shown in (7.26). When used to form nominals, *-llet-* means ‘former X’ or ‘the one who (was) Xed.’

(7.26a) ekua-**lletq**

burn-PAST.NOMINAL
‘the one that burned’

(7.26b) pagaaggun anellret

paga- ggun ane- **llet-** t
up.above-VIALIS go.out-PAST.NOM-ABSL.PL.
‘those who had left through the upper door’

The nominal history of *-llet-* is still visible synchronically by virtue of the phonological form of the suffixes that intransitive past contemporaneous verbs select. In (7.27), the pronominal suffix on the noun is transitive, specifying both the possessor and the possessed, as well as being marked for locative case.

(7.27) angyaatni

angyar-at- ni
boat- 3rd.pl/3rd.sg-LOC
‘at/in their boat’

Intransitive past contemporaneous verbs appear with what look to be the same nominal endings.

(7.28) Tangvagkai ayallratni.

tangvag-ke- ai ayag- **llet-** atni
watch- PTCP-3rd.sg/3rd.pl leave-PAST.CONTEMP-3rd.pl
‘He watched them as they were leaving (at their leaving).’

Despite the apparent nominal morphology, however, the case marking of nominals in sentences with past contemporaneous verbs demonstrates that the forms are verbal; synchronically, *-atni* is monomorphemic. Nominal possessors in Yup'ik appear in the

²⁷² Throughout this section, the Sub-word *llet* is shown in boldface.

ergative case (7.29), while intransitive verbal subjects require the absolutive case. As shown in (7.30), subjects of past contemporaneous verbs also take the absolutive case.

(7.29) Angutem angyaani.
 Angute-m angyar-a- ni
 man- ERG boat- 3rd.sg/3rd.sg-LOC
 ‘in the man’s boat’

(7.30) Angun ayallrani.
 angun ayag- **ller-** ani
 man.ABS leave-PAST.CONTEMP-3rd.sg.
 ‘As the man was leaving...’

Moreover, the nominalising history of *-ller-* is obscured when it appears on transitive verbs, which take the usual suffixes for transitive verbs. This is shown in (7.31), which does not have the (originally) nominal ending *-atni*.

(7.31) Tangallraki.
 tangag-**ller-** aki
 watch- PAST.CONTEMP.-3rd.sg/3rd.pl
 ‘When he watched them...’

As mentioned at the outset, the original nominalising function of *-ller-* is still current in Modern Yup’ik, but it does not occupy the same position as verbal *-ller-*. Nominalising *-ller-* (7.32a) occurs between the root and two other suffixes, the verbalising morpheme *-u-* and *-yaq-* ‘indeed’. In contrast, in (7.32b) the mood use of *-ller-* is restricted to the position immediately before AGR. This shows that despite their etymological connection, synchronically the two *-ller-* suffixes are distinct.

(7.32a) Ekuallrunritellruyaquq.
 ekua-llru- nrite-**ller-** u- yaq- u- q
 burn-PAST-NEG- PAST.NOM-be-indeed-INTR.IND-3rd.sg.
 ‘Indeed it is not the object that burned!’

(7.32b) Qumacungualrullerani.
 qumar-cuk- u- aq- llru- **ller-** ani
 worm- ugly.old-be-indeed-PAST-PAST.CONTEMP-3rd.sg.
 ‘As he was indeed a low-life worm...’

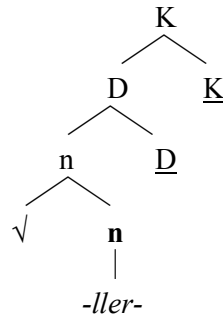
There is a clear semantic relationship between nominal and verbal *-ller-*, with the former meaning ‘former X’ or ‘the one who (was) X’ed’, and the latter ‘when (in the past)’. The more interesting question here is how language learners concluded that *-ller-* belonged to the category of clause-final “inflectional” suffix. Mithun (2000:245) argues that since the “derivational” suffixes have relatively fixed positions, the fact that other suffixes can co-occur between the nominalising suffix and the final suffixes is irrelevant. She also points out that, despite the relative positional freedom of “derivational” suffixes, the most frequent position for nominalising suffixes is immediately before the “inflectional” suffixes – for nouns, case and pronominals. Since the pronominal suffixes on nouns are formally similar to the suffixes specifying indicative verbal arguments, she concludes that it would be fairly easy for a learner to mistake the nominalising suffix for a mood marker. Once a learner has analysed *-ller-* as a mood marker, its position in the clause, when employed in that capacity, would be fixed.

Note that this was a functional split rather than a functional shift, in that language learners *were* in fact able to acquire the conservative, nominalising *-ller-*. They may have been able to do so because of instances in the available data of nominalizing *-ller-* that were

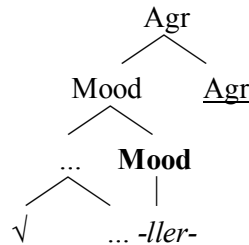
not adjacent to the final suffixes, like (7.32a) above; or possibly the original reanalysis involved cases that were particularly semantically ambiguous. This is probably not recoverable.

Abstracting away from various complexities, the change in the position of *-ller-* is approximately that from (7.33a) to (7.33b).²⁷³ Since the labels are dramatically different in each tree, the terminal projections of corresponding segments are indicated by typeface.

(7.33a)



(7.33b)



The pronominal D and case suffix of (7.33a) are reanalysed as a single Agr suffix in (7.33b); cf. (7.27)–(7.28) above, where the two final suffixes in the nominal (7.27) correspond to a single suffix in verbal (7.28). Meanwhile, *n* has been reanalysed as Mood.

In Central Alaskan Yup'ik, therefore, we see a correlation between a new function for a pre-existing Sub-word and a new structural position. The innovative Vocabulary Item *-ller-* appears in the position in the clause expected for a terminal of its capacity. This reanalysis was facilitated by the fact that nominalising suffixes like conservative *-ller-* tend to occur just before nominal inflection.

7.3.1.2 Cherokee

Cherokee is the only language in the Iroquoian family to have infinitives; the suffix used to mark infinitives is transparently related to the instrumental suffix common throughout the rest of the family.²⁷⁴ Iroquoian languages are similar to Athabaskan languages in having extremely complex verbal complexes in which the order of affixes appears to reflect the historical order of affix-genesis rather than obvious synchronic syntactic relations (cf. Rice 2000), and the position of morphemes in such languages is much more inflexible than in a language like Yup'ik. Nevertheless, the Cherokee infinitival suffix appears in a different structural position than the instrumental suffix, indicating that some innovation is nevertheless possible if the conditions are right. Mithun (2000) argues convincingly that, as in the Yup'ik case, this innovation in Cherokee was possible because the instrumental suffix

²⁷³ The trees in (7.33) should be considered purely schematic.

²⁷⁴ This is not an uncommon etymology for an infinitive; the infinitives in Indo-European languages have similar origins.

very often occurs adjacent to aspectual suffixes. Although not all the details of this development are clear (or available), the example is nevertheless illustrative.

The etymological relationship between the Cherokee infinitive and instrumental suffixes is quite clear. Both have a large array of allomorphs which correspond to each other exactly; these similarities are too great to be the result of coincidence. In addition, these allomorphs are not related by phonological rules, but selected by individual Roots. Roots that select a particular instrumental allomorph will select the same allomorph of the infinitival suffix. Thus, as shown in (7.35), the Root *hne:ʔ* ‘speak’ selects both instrumental *-ihst-* and infinitival *-ihst-*.²⁷⁵

(7.34a) Infinitive: *-ihst, -hst, -ʔst, -oʔt, -ʔt, -ʔht, -oht, -ht*

(7.34b) Instrumental: *-ihst, -hst, -ʔst, -oʔt, -ʔt, -ʔht, -oht, -ht, -ʔhst*

(7.35a) *Infinitive:* *ti:khiné:ʔ-ihst-i* ‘I have to talk.’

(7.35b) *Instrumental:* *tsiʔne:ʔ-ihst-ih* ‘I am talking about it.’

The instrumental and infinitive suffixes may appear within the same verb.²⁷⁶

(7.36) *A:kwohwe:l-óʔt- oht-i.*

-INST-INF-

‘I have to write with it.’

As alluded to above, the instrumental suffix is found throughout the Iroquoian languages. It is not an instrumental case suffix; rather, it is used to form causatives or instrumental applicatives.

(7.37a) *Kohwe:liʔskoʔi*

‘He writes.’

(7.37b) *Kohwe:l-**eʔt**-ihsko:ʔi*

‘He writes with it/he makes him write.’

However, the instrumental does often occur in nominal contexts. Iroquoian languages often use morphological verbs as syntactic nominals without overt morphological category marking. Since the names of objects are often essentially descriptions of their use or function, many of these verbs-as-nominals do contain the instrumental suffix. (7.38) shows an example from Mohawk.

(7.38) *yʊtkʊhsokewá**h**thaʔ*

ye- at- *kuhs-okew-**ht**-* haʔ

INDEF.AGENT-REFL-face- wipe- INST-IMPF

‘one wipes one’s face with it’ = ‘towel’

²⁷⁵ The data in this sub-section come from Mithun (2000), who does not always provide glosses. I have placed the instrumental suffix in bold.

²⁷⁶ Note, interestingly, that the phonological forms of the two suffixes here are not identical. This is not discussed by Mithun, and therefore the implications are not clear; it could be the case that the outer suffix is blocked from “seeing” whatever normally conditions the allomorphy (presumably the Root?), as discussed by Embick (2010).

In many Iroquoian languages, including Mohawk, the instrumental suffix is sometimes omitted, as in the case of the Mohawk word for ‘car’, *kà:sere*, which comes from the Root ‘drag’. However, Mohawk requires the instrumental suffix to be present whenever the nominal is incorporated into a verb.

(7.39a) *kà:sere?*

ka- ?sre-e?
NEUT.AGENT-drag-IMPF
‘it drags’

(7.39b) *ka?srehtí:yo*

ka- ?sre-ht- iyo
NEUT.AGENT-drag-INST-be.nice.STAT
‘It is a nice car.’ (Lit. ‘What is used to drag is nice.’)

Mithun (2000:249–50) links the innovation of an infinitive in Cherokee to contact with genetically unrelated Muskogean and Caddoan languages spoken in the same area. Both Creek (Muskogean) and Caddo (Caddoan) have infinitives. She suggests that, as infinitives often serve nominalising functions in various languages, Cherokee speakers could have easily been influenced to employ their native construction with instrumentals and periphrastic nominals in the realm of instrumentals.

The Cherokee infinitive is used to indicate potential events and in what Mithun calls “dependent clauses”.

(7.40a) *A:kwatu:liha.*

‘I want it.’

(7.40b) *U:n_Δ:-?t-i-yi a:kwatu:liha.*

‘I want him to give it to her.’

(7.40c) *U:n_Δ’:-?t-i*

‘He has to give it to her.’

In the context of the rigid order of verbal suffixes, as given in (7.41), the infinitive is classified as an “outer inflectional” suffix. Since the position it occupies is shared by the imperfective, perfective, and stative suffixes, the infinitive is also considered an “aspectual” suffix.

(7.41) REV-CAUS/INST-DAT/BENF-AND-PURP-ITER-PROG-REP-COMPL-ASP-MODE

By contrast, the instrumental suffix appears in the “caus/inst” slot and is classified as an “inner derivational morpheme”. This change in position – the infinitival suffix has hopped over *seven* other suffixes – is significant, particularly in a language with a verbal template as rigid as Cherokee’s; at first glance, it is not clear how such an innovation might have been motivated. As in the previous example of Yup’ik, however, the apparent long-distance migration is deceptive. Only the last two suffixes in the chain are obligatory; all seven of the suffixes between the instrumental and aspectual positions are optional. This means that the instrumental could be potentially adjacent to the aspectual suffix; Mithun (2000:251) explains that this is often the case.

Exactly how the resulting reanalysis occurred is not entirely clear. Mithun argues that when the instrumental suffix began to acquire infinitival functions, learners began associating it with the aspectual suffixes and eventually reanalysed it as part of that complex, effectively interpreting it as occupying a different position in the underlying structure than it had previously, even though (in many cases) its surface position remained unchanged.

7.3.2 String-Vacuous Migration

Although some of the details in the Yup'ik and Cherokee examples are murkier than would be ideal, the examples are sufficient to demonstrate that changes in Sub-word function can co-occur with changes in structural position. We can infer from this that similar changes in structural position are possible even if they are not visible on the surface when the conditions are right: for instance, if one or more of the Vocabulary Items in the M-word were phonologically null, learners do not have any clear evidence telling them which structural position an overt Sub-word belongs to. We will see that, in fact, changes of this type share a great deal in common with some of the phenomena discussed in Chapter Three, when they are looked at from this perspective. This is consistent with the predictions made at the outset of this section.

In the remainder of this sub-section, I will discuss two different case studies: the Ionic Greek iterative suffix (7.3.2.1) and the Swedish berry suffix (7.2.3.2). The first case shares a number of properties with Type II affix-genesis; the second is more ambiguous, but is similar to either Type II or Type IV.

7.3.2.1 Ionic Greek

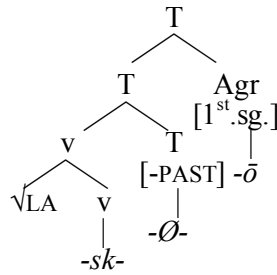
Ancient Greek shows a variety of verb types bearing the suffix $-sk^e/o-$. Customarily, Roots which select this suffix select it only for the present (imperfective) forms, and not for the aorist (perfective) or perfect forms, although there are exceptions. The suffix is not especially productive, but it is not moribund either. In older varieties of the Ionic dialect of Ancient Greek, this suffix underwent a split into two different suffixes: one which behaves as a typical imperfective formant, and another marking repeated past actions. Although both the synchronic and diachronic details are not as clear as one would wish, the data are sufficient to demonstrate that at least one of the two $-sk^e/o-$ -suffixes in Ionic occupies a different structural position than their common ancestor. Moreover, the change that produced this split has some properties in common with affix-genesis, despite occurring entirely within a single M-word.²⁷⁷

With clear relatives in most branches of the Indo-European family, the suffix $-sk^e/o-$ has undeniable Proto-Indo-European (PIE) antecedents; the phonological form of the suffix is reconstructed $*-sk^e/o-$. The function of the suffix in the proto-language is rather less clear. It became quite productive in a number of the various IE languages, always with a clear function, but never in quite the same way: in Hittite it is “iterative-durative”; in Armenian it appears as both a present indicative and an aorist subjunctive; in Tocharian B it is causative; in Latin it is inchoative-progressive; and in Middle Iranian it is inchoative. The apparent functional match in the latter two branches cannot be taken seriously, as in both languages, as well as Tocharian B, this function is clearly secondary on the basis of older relic forms where the reflex of $*-sk^e/o-$ is not causative-inchoative. Relic forms can also be found in a number of other branches (Albanian, Balto-Slavic, Celtic, Germanic, and other Anatolian and Italic languages); in these languages, the suffix has become simply part of the Root.²⁷⁸ The most popular opinion is that $*-sk^e/o-$ was an iterative (but cf. Zerdin 1999:51–5 for a summary of dissenters), but this is very far from certain.

²⁷⁷ This sub-section relies heavily on the thorough account of Zerdin (1999). For the prehistory of $*-sk^e/o-$, cf. in particular Zerdin (1999:37–55).

²⁷⁸ There are not many secure examples of specific cognate Roots bearing the suffix in multiple languages, but there are several, e.g. Vedic *gáčchati* ‘he goes, comes’, Greek *báskō* (βάσκω) ‘I go’, both from $*g^wm_o-sk^e/o-$ (possibly also some forms from the Tocharian languages; cf. also the altered Young Avestan *jasaiti* ‘he goes, comes’).

(7.45)



Owing to the lack of space, I will be unable to address the question of whether (7.44b) or (7.45) should be taken to reflect the structure of the Greek verb, although I hope to return to the topic at a later date. Note, however, if (7.45) is the correct structure, then there may have been structural changes concerning all the imperfective formants, and the Greek suffixes become of potential interest in the present context. Furthermore, although the full analysis of the Greek imperfectives, with the associated diachronic implications, cannot be undertaken here, a smaller and somewhat clearer issue can be, concerning specifically the *-sk-* suffix in older Ionic (Homer and Herodotos). In this dialect, *-sk-* had become a fully productive iterative past imperfective suffix.

The history of the iterative imperfect in *-(e)sk-* is poorly understood, as it is already present in Homer and Herodotos. What *is* clear is that it was entirely separate from present-forming *-sk-*. Almost all of the attested forms are conjugated like past imperfectives and formed to the imperfective stem, although there are occasional exceptions (usually Homeric) built to aorists. Both Homer and Herodotos use *-sk-* to narrate repeated past actions, usually in a series of two or three verbs all bearing the same suffix. Herodotos only uses these forms in the third singular; Homer is less consistent in this respect, but generally does the same.²⁸⁰

(7.46a) τὴν δὲ τότε ἐν μεγάροισι πατὴρ καὶ
t- ēn de tót' en megár-oisi patēr kai
DEM-FEM.ACC.SG. PTCL then in hall- DAT.PL father.NOM.SG CONJ
πότνια μήτηρ
rótni-a mētēr
lady- NOM.SG. mother.NOM.SG
Ἁλκυόνην καλέεσκον ἐπώνυμον, ...
Halkyón-ēn kal-é- esk- on epōnym-on
ACC.SG. call-IMPF-ITER-PAST.ACT.3rd.pl name- ACC.SG.
'And in their halls her father and lady mother used to call her Halkyone by name...'

(7.46b) ἧ οἱ ἀπαγγέλλεσκε
h- ē hoi ap- aggéll- esk- e
REL-FEM.NOM.SG MASC.SG.DAT PRV-relate.IMPF-ITER-PAST.ACT.3rd.sg.
Διὸς μέγαλοιο νόημα
Di- os megál-oio nóēma
Zeus-GEN.SG great- GEN.SG purpose.ACC.SG
'[she] who used to bring him news of the purpose of great Zeus'

²⁸⁰ The examples in (7.46a–b) come from the Iliad, books 9.561–2 and 17.409 respectively; (7.46c) is from the Odyssey, book 5.331–2; and (7.46d) is from Herodotos, 2.13.1. Translations follow Zerdin (1999); glosses are my own.

- (7.46c) ἄλλοτε μὲν τε Νότος βορέη
 állote mén te Nót- os Boré- ēi
 one.time PTCL PTCL south.wind-NOM.SG north.wind-DAT.SG
 προβάλεσκε φέρεσθαι,
 pro- bál- esk- e p^hér- est^hai,
 before-throw.AOR-ITER-PAST.ACT.3rd.sg carry.IMPF-MP.INF
 ἄλλοτε δ' αὐτ' Εὖρος Ζεφύρω
 állote d' aũt' Eÿr- os Zep^hýr- õi
 one.time PTCL back east.wind-NOM.SG west.wind-DAT.SG
 εἶζασκε διώκειν.
 eík- sa- sk- e diõk- ēn
 yield-AOR-ITER-PAST.ACT.3rd.sg drive.IMPF-ACT.INF
 'At one time the South Wind would throw it forward to the North Wind to be carried along, and at another the East Wind would yield it to the West Wind to drive.'
- (7.46d) ...ὥς ἐπὶ Μοίριος βασιλέος, ὅκως ἔλθοι
 hōs epì Moíri- os basilé-os, hókōs élt^h- oi
 CONJ during Moiris-GEN.SG king- GEN.SG when go.AOR-OPT.3rd.sg.
 ὁ ποταμὸς ἐπὶ ὀκτὼ πήχεας τὸ
 ho potam-os epì oktō pēk^he-as to
 DEF.MASC.NOM.SG river- GEN.SG to eight cubit- ACC.PL. DEF.NEUT.ACC.SG.
 ἐλάχιστον, ἄρδεσκε Αἴγυπτον
 elák^h-ist- on, árd- esk- e Aígypt-on
 small-SUPRL-ACC.SG water.IMPF-ITER-PAST.ACT.3rd.sg. Egypt- ACC.SG.
 τὴν ἔνερθε Μέμφιος.
 t- ēn éner^he Mémp^hi- os
 DEF-FEM.ACC.SG. below Memphis-GEN.SG
 'When Moiris was king, whenever the river rose at least eight cubits, it would water [all] Egypt below Memphis.'

Zerdin (1999:468) speculates that at some stage, and in at least some dialects, *-sk-* underwent a functional split and became two separate Vocabulary Items: an iterative aspectual suffix and a non-iterative imperfective suffix. Though there is no way to recover how such a split may have occurred (as Zerdin himself is careful to stress), this is not unlike what occurred in Cherokee and Yup'ik, and therefore a distinct possibility. But this raises a number of difficult questions. If iterativity was the original semantic value of this suffix (cf. its role in Hittite), then the Ionic iterative imperfective is semantically conservative; but it is clearly *not* syntactically conservative, nor is it a variation on the imperfective-stem formant suffix found elsewhere in Greek or Indo-European generally.

Indo-European languages are not agglutinative; one does not typically find a long series of stacked suffixes as one does in Yup'ik or Hungarian. The various imperfective-formant suffixes in Greek and in broader IE are not independent of each other: if a verb takes one of these suffixes, it doesn't take another. The Ionic iterative, however, is clearly added to a verb that already has an aspectual projection. There are several morphological indications of this; the fact that I was able to remark at the outset that the iterative *-(e)sk-* is usually added to imperfective stems is an important clue that the iterative itself must be something different.

Homer has five pairs of iteratives built to the imperfect and aorist of the same verb, as shown in (7.47).²⁸¹

(7.47) <i>Imperfective</i>	<i>Aorist</i>
hi- stá- sk- e (ιστάσκε)	stá- sk- e (στάσκε)
REDUP-set.IMPF-ITER-PAST.ACT.3 rd .sg	set.AOR-ITER-PAST.ACT.3 rd .sg.
spénd- esk- on (σπένδεσκον)	speí- sa- sk- e (σπείσασκε)
libate.IMPF-ITER-PAST.ACT.3 rd .pl	libate-AOR-ITER-PAST.ACT.3 rd .sg
p ^h ain- ésk- eto (φαινέσκετο)	p ^h án- esk- e (φάνεσκε)
appear.IMPF-ITER-PAST.MP.3 rd .sg.	appear.AOR-ITER-PAST.ACT.3 rd .sg.
p ^h eúg- esk- e (φεύγεσκε)	p ^h ýg- esk- e (φύγεσκε)
flee.IMPF-ITER-PAST.ACT.3 rd .sg.	flee.AOR-ITER-PAST.ACT.3 rd .sg
ōt ^h - esk- e (ώθεσκε)	ō- sa- sk- e (ώσασκε)
thrust.IMPF-ITER-PAST.ACT.3 rd .sg	thrust-AOR-ITER-PAST.ACT.3 rd .sg

Finally, while the imperfective/aorist/perfect distinction is often marked by changes in the Root via readjustment rules (cf. the pairs *spend-/spei-* *p^hain-/p^han-* and *p^heug-/p^hyg-* above), it is also sometimes marked by discrete suffixes. This is clear from several of the aorists cited so far: *eikaske* (7.46c) and *speisaske* and *ōsake* (7.47), as well as one of the presents, *kaleeskon* (7.46a).

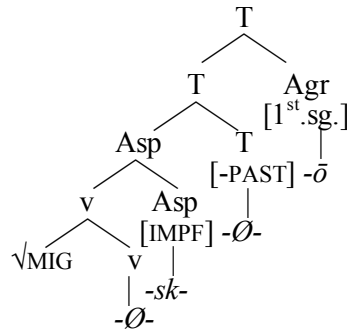
The best proof, of course, would be examples with two discrete *-sk-* suffixes, one imperfective and the other iterative. It is not clear if such examples existed; or rather, such examples do exist, but their synchronic status is uncertain. Homer has these two forms of the same verb, *mísgō* (μίγω) ‘I mix’:

- (7.48a) mi- sg- ésk- eto (μισγέσκετο)
 mix-IMPF-ITER-PAST.ACT.3rd.sg
- (7.48b) e- mi- sg- ésk- onto (ἐμισγέσκοντο)
 PAST-mix-IMPF-ITER-PAST.ACT.3rd.pl
 ‘was/were/used to be mixing’

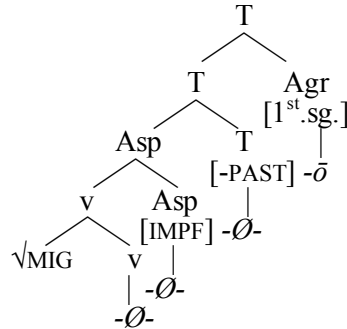
Etymologically, *mísgō* is in fact a *-sk^e/o-* verb: it comes from **mig-sk-ō*, with Root *mig-*; cf. aorist *emígēn* (ἐμίγην), athematic present *mígnymi* (μίγνυμι), Latin *miscēō*. It is not clear, however, whether this would have been synchronically apparent to speakers. The phonetically altered suffix has not been reanalysed as part of the Root; this is obvious from the non-imperfective forms, and it could very well be the case that the form *mísgō* was still underlying (7.49a) with subsequent phonological rules yielding the surface forms; however, it could also have been reanalysed along the lines of (7.49b), where the imperfective suffix is null and the *-s-* is a product of a readjustment rule.

²⁸¹ Cf. Brugmann and Delbrück (1893–1916 IV:63) and Risch (1974:276).

(7.49a)



(7.49b)



Thus, unfortunately, the forms in (7.48) are not decisive evidence for the permitted co-occurrence of the two *-sk-* suffixes in Ionic; and the only other possible example is even more dubious.²⁸²

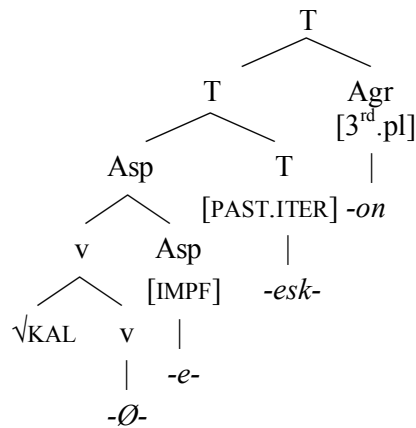
Nevertheless, the morphological evidence *is* sufficient to conclude that the iterative *-(e)sk-* is *not* a member of the same category as aorist *-s-* or the many types of present. This is a difficult problem: as an iterative, *-(e)sk-* is hard to locate anywhere other than AspP, and yet we have already said that this is exactly where it *cannot* go. There are at least four different possible solutions to this problem, although each has difficulties; any one of these, however, would be consistent with the goal of this chapter, and therefore I will quickly run through each of them. The verb used in the resulting tree diagrams is *kaleeskon* from (7.46a), reglossed here for convenience.

(7.50) καλέεσκον
 kal-é- esk- on
 call-IMPF-ITER-PAST.ACT.3rd.pl
 ‘they used to call’

²⁸² The Homeric third plural mediopassive *boskēskont^h* (βοσκέσκονθ’), formed to *bóskō* (βόσσω) ‘I feed’, clearly has two *-sk-* suffixes: *bo-ské-sk-ont^h*. However, in this verb, the original *-sk-* seems to have been reinterpreted as part of the Root, on the basis of the future *boskēsō* (βοσκήσω), also Homeric, and the noun *bóskēma* (βόσκημα) ‘beast, food’, the latter attested as early as Aeschylus. There is some room for ambiguity, in that most non-imperfective forms of this verb, derived nominals especially, do not have the *-sk-*. This is in contrast to the verb *didáskō* (διδάσκω) ‘I teach’, where both *-sk-* and the prefixed reduplication run throughout all forms built to this Root, albeit sometimes slightly altered for phonological reasons; cf. aorist *edídaksa* (ἐδίδαξα) and perfect *dedídak^ha* (δεδίδαχα), as well as the derived nominals *didáskalos* (διδάσκαλος) ‘teacher’ (whence millions of compounds) and *didak^hē* (διδαχή) ‘teaching’. On etymological grounds, *didáskō* ought to be segmented *di-da-sk*. Cf. Zerdin (1999:75,357).

One option is to make iterative *-(e)sk-* an exponent of Tense. This is not without advantages: TP is located above AspP, so the placement of the imperfective/aorist exponents is unproblematic. In addition, all of the iterative forms are exclusively past tense; there is no corresponding present tense. If *-(e)sk-* is Tense, then this fact can be explained quite easily. The fact that it typically occurs with imperfectives could be a semantic issue rather than a syntactic one; this structure does allow aorists to be marked with *-(e)sk-*, as occasionally they need to be. It may also explain why these forms rarely occur with the augment: they are already overtly marked past tense. The problem with this option is that the temporality of the suffix is not its most salient feature, its most salient feature being repeated action, which is aspectual rather than temporal in nature. To distinguish *-(e)sk-* from the exponents of [PAST] in Ionic, we would probably have to enrich its semantics to include the aspectual connotations, making it a [PAST.ITER] suffix rather than a strictly [PAST] suffix; this may or may not be a desirable consequence.

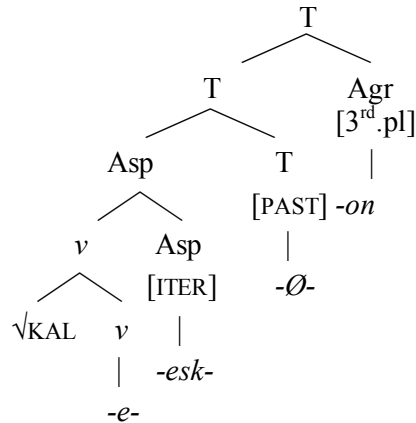
(7.51)



If this is the correct structure of an Ionic iterative, then the iterative has changed structural positions at some point in its history, moving from AspP to TP. PIE verbs were probably not inflected for Tense, so Tense is itself a relatively innovative feature of the Greek verbal complex.

Another option would be to make *-(e)sk-* an exponent of Mood, and to locate it between AspP and TP. Zerdin (1999:325) implies such an analysis, although he does not spell it out in structural terms. This is more or less unproblematic descriptively; I have not been including a MoodP in the structures in this section, but Greek verbs did inflect for Mood, so this solution employs structure that is needed independently. The problem, as I see it, is that “iterativity” is a rather strange semantic value for a modal suffix. Zerdin alludes to the possibility of these verbs being “generic”, which would be an improvement, although it fails to explain why only past tense forms are found.

(7.54)



In order for this to work, the different exponents of *v*P would have to be determined by both the Root selecting them and the value of the Aspect feature to their right. Though rather radical, this is not an unappealing hypothesis: Greek aspectual exponents show a great deal of formal allomorphy, often without obvious semantic differences. What semantic differences do arise, meanwhile, could be plausibly considered derivative from *v* (causativity, for example).

This would be simpler if it were only imperfective stems which could house the iterative affix. In that case, a subset of Vocabulary Insertion for the *v* exponent with the root KAL in (7.54) would be as in (7.55), with the imperfective and iterative as “elsewhere”; alternatively, the iterative could count as a subset of the imperfective.

(7.55)	[v]	↔	∅	/ √KAL... _ [FUT]
		↔	es	/ √KAL... _ [AOR]
		↔	k	/ √KAL... _ [PERF]
		↔	e	/ √KAL... _ elsewhere

The problem here is that it does not allow a way of deriving aorist iteratives, and although aorist iteratives are rarer, they do exist. One way of attempting to circumvent this would be to say that the aorist iteratives in Homer are a feature of epic language only, and therefore artificial; “real” iteratives could only be imperfective. While it is true that aorist iteratives are much rarer in Herodotos than in Homer, Herodotos *does* have them, so reference to epic innovations alone does not solve the problem. Such a solution may yet be possible; the iterative aorists in Herodotos are a morphologically cohesive class (all so-called “thematic” aorists, with the *-e/o-* suffixes usually found on imperfectives), but the details of such an analysis are beyond the scope of this section.

As stated above, however, the actual details of the synchronic analysis of older Ionic Greek are not the central concern here. The real point is that, however this system is analysed, the diachronic implications are the same: either the iterative *-(e)sk-* suffix has become the exponent of another category and moved to a different functional projection, or the various imperfective/perfective formants have done so. Meanwhile, while the precise details of how this may have happened are likely unrecoverable, many of the various factors that facilitated such innovations are not. The Greek verbal complex typically had at most one overt suffix and always contained at least one null suffix (e.g. always *v*P, sometimes MoodP); in many instances the formal differences between e.g. imperfectives and aorists is simply the product of readjustment rules. Initially, therefore, the iterative suffix would have been the only overt affix in the verbs in which it occurred.

The potential importance of null suffixes in diachronic problems of this type is not to be underestimated. If a child acquiring a grammar has more structural projections than overt

exponents for these projections, she has some flexibility in which exponents she assigns to which projections. Any functional ambiguity within the system will increase the likelihood that the position-to-exponent mappings she arrives at will not be precisely identical to a conservative speaker's.

Iteratives of this type are not attested in any dialects of Ancient Greek apart from older Ionic; whether this indicates a retained archaism in Ionic or a short-lived innovation is impossible to say. What we *can* say is that the iterative presupposes some structural changes in the Ionic verbal system, because the iterative is clearly no longer a member of the same category of exponents that includes the other aspectual markers. Either the iterative was relocated to another structural position, or the other affixes were; the data are inconclusive on this point.

Interestingly, both possible reanalyses bear a number of similarities to Type II affix-genesis (cf. 3.6), the type which involves reanalysis of a terminal as an exponent of a different structural position. The only difference is that in affix-genesis the moved Vocabulary Item is a separate M-word in the conservative grammar, whereas here, all of the terminals involved were already Sub-words.

7.3.2.2 Swedish

The most entertaining and colourful example of string-vacuous affix migration known to me is the case of the Swedish berry suffix, discussed by Norde (2002:55-6, 2009:181-3). This is an example of an erstwhile case suffix developing into an *n* suffix, or, more intriguingly, a compounding element, in which guise it enjoyed limited productivity. If *-on* is correctly viewed as an *n* suffix, this case study closely resembles Type II affix-genesis; if *-on* is best viewed as a compound element, then this is essentially the reverse of Type IV.

Unlike Modern Swedish, Old Swedish retained a number of declensional classes, including both “weak” and “strong” vowel stems. The plural nominative/accusative case suffix for weak neuter nouns ending in *-a* was *-on*. Modern Swedish retains this suffix for the plurals of *öga* ‘eye’ (*ögon*) and *öra* ‘ear’ (*öron*).

Among the neuter nouns employing the *-on* plural ending in Old Swedish were a few forms like *hiūpon* ‘rosehips’ (Modern Swedish *nypon* [sic]), *hjordron* ‘cloudberries’, and *smultron* ‘wild strawberries’: all berry-like entities that are often referred to collectively. Apparently on the basis of forms of this type, Swedish learners concluded that the *-on* suffix in these forms was not the case suffix, but rather a suffix meaning ‘berry’; this unusual reanalysis may have been, in part, facilitated by the fact that there may not have been many nouns of this class (Don Ringe, p.c.), so the coincidence of three of them sharing similar semantics may have misled learners. In Modern Swedish, the singulars and plurals of these forms are formally identical. Even more tellingly, *-on* became moderately productive. Although it never ousted the original *-bär* ‘berry’²⁸³, it did replace *-bär* in at least two cases.

(7.56a) *hall-on* ‘raspberry’ < *hall* ‘stony ground, slope’

(7.56b) *ling-on* ‘lingonberry’ < Proto-Scand. **lingwa* > Mod. Swed. *ljung* ‘heather’²⁸⁴

Some dialects of Modern Swedish, however, still preserve the older forms *hallbär* and *lingbär*.

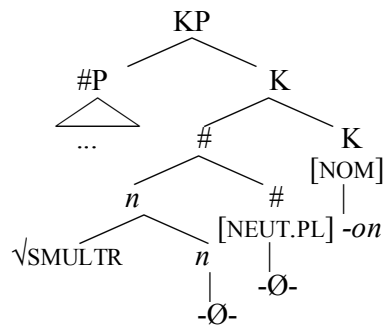
Norde analyses *-on* as a (derivational) suffix; if this is correct, then the change is structurally straightforward. (7.57a) shows the original analysis (still current in Modern Swedish for the Roots ‘ear’ and ‘eye’), where *-on* is simply the neuter case suffix. In (7.57b),

²⁸³ Cf. *blåbär* ‘blueberry’, *björnbär* ‘blackberry’, *körsbär* ‘cherry’, *krusbär* ‘gooseberry’, *vinbär* ‘currant’.

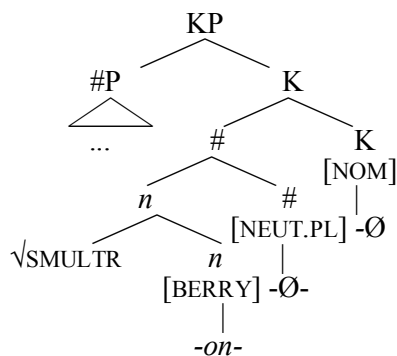
²⁸⁴ The English word *lingonberry* is therefore one of those delightful borrowings in which a native word with the same semantics as the borrowing (or one portion of the borrowing) is added.

the case suffix is now zero, and *-on* has moved to a lower structural position as *n* with a particularly rich semantics.

(7.57a) *Conservative Grammar*



(7.57b) *Innovative Grammar*



The important point here is that the morphological change itself is actually quite small. There have been no structural changes, no changes in the phonological forms, and only a very slight change in feature content, which is a semantic issue. Essentially, the innovators reassigned *-on-* to a different structural position, keeping constant the overall number of projections and null suffixes. The fact that both *n* and *#* are null in the original grammar means that, on the surface, there is nothing preventing such a reassignment. Once again, this reanalysis bears a number of similarities to Type II affix-genesis.

Alternatively, however, *-on* could be a compounded element rather than a suffix. The non-innovative portions of the Swedish berry vocabulary greatly resemble their English counterparts: where English has both specific types of berry like *blackberry* and the generic *berry*, Swedish has *björnbär* ‘blackberry’, *bär* ‘berry’. Unlike English *berry*, however, Swedish *bär* does not take an overt plural affix. Since berries are entities that are typically referred to in the plural, a learner hearing words like *smultron* referring to other berry-like entities could well conclude that *smultron* has the same structure as *björnbär* and its ilk. Under this analysis, the innovative *lingon* has the same underlying structure as conservative *lingbär*, so that in replacing *lingbär* by *lingon*, learners effectively swapped one compounded element for another.

If *-on* is better analysed as an obligatorily bound compound element rather than an *n* suffix, then the development of the Swedish berry suffix is not similar to Type II affix-genesis. It is, however, the opposite of Type IV affix-genesis:

(7.58) *Type IV*: Reanalysis of a compounded element as an affix.

Here, of course, we have the reanalysis of an affix as a compounded element. I did not discuss any cases of Type IV affix genesis in Chapter Three, but phenomena of that

variety warrant extensive investigation. If the Swedish example is of a similar type (modulo differences in direction), it should be examined as a part of that study.

Although no further structural changes have occurred since the initial reanalysis (whatever its nature), the story of the Swedish berry suffix is not quite complete. Norde remarks that *hallon*, *lingon*, and other “berry” words are typically used as mass nouns and are rarely singular, so that the original “plurality” of the case suffix is vaguely preserved. Swedish speakers, however, were apparently sufficiently excited about their new “berry” suffix that they began to extend it to several types of fruit that are count nouns. All of the examples in (7.59) were originally Latin loanwords.

(7.59a) *fik-on* ‘fig’ < Latin *ficus* ‘fig tree, fig’

(7.59b) *pär-on* ‘pear’ < Latin *pirum* ‘pear’

(7.59c) *plomm-on* ‘plum’ < Latin *prunum* ‘plum’

By coincidence, some Swedish words for fruit happen to end in *-on* independently: *citron* ‘lemon’ and *melon* ‘melon’ (and its compounded forms). However, both *citron* and *melon* take the overt plural suffix *-er*, while *fikon*, *päron*, and *plommon* all take a null plural suffix, just like the “berry” words. Why the suffix was extended to just these three fruits is unclear.

7.3.3 Recapitulation

The Yup’ik and Cherokee examples discussed in 7.3.1 demonstrate that changes in grammatical function and changes in surface linear position can and do co-occur. There is a certain similarity to these examples, in that both cases involve a specific Sub-word X splitting into two, with the original function and position retained by X’ side-by-side with a “new” Sub-word Y expressing the innovative function and occupying a new position. The most likely scenario is that in each instance, the original Sub-word had developed certain semantic nuances which allowed learners to interpret it as filling a particular function. In both Yup’ik and Cherokee, a variety of other Sub-words can intervene between the positions filled by X’ and Y – but not necessarily, since many of these suffixes are optional. This provided the impetus for the learners’ reanalysis: when other suffixes which could have provided clues to the structural position of Sub-word X were absent, learners were free to re-interpret X as an exponent of another category.

This is not so different from what was discussed in Chapter Three. As I pointed out in that chapter, it is well-known in the historical subfield that etymologically cognate M-words and affixes can co-exist in the same language; clearly, there was a divergence at some point in their (formerly) shared history. It was noted that when an M-word is demoted to a Sub-word, it has already undergone the requisite semantic changes. The intuition here is a simple one: an M-word becomes polysemous, with one of its new meanings functional in nature; innovators ultimately fail to acquire this polysemy, instead postulating two separate objects; and the lives of the now-separate M-words diverge from this point in their history, with affix-genesis as a possible consequence.

In Yup’ik and Cherokee, we see exactly the same story playing out again, with the sole difference that the changes are happening *within* an M-word. We find the same functional split and the same confusion on the part of learners as to whether there are two terminals involved or one. Moreover, while the phonological forms of the innovative Yup’ik and Cherokee Sub-words do not change, their structural and surface positions do. This is very similar to Type II affix-genesis, where a linguistic terminal is taken to occupy a different structural position to what it had previously; again, the crucial difference is that there is no change in typing: the terminals in question begin and end as Sub-words.

Not all changes in underlying structural positions will be reflected in the surface forms, particularly in languages with null affixes. However, the fact that there *are* clear cases

of affixes acquiring new positions along with new functions allows us to say that there are probably less clear cases as well, where the change in position is (surface) string-vacuous. Moreover, the existence of null affixes probably exacerbates potential ambiguities for the language learner: if there are (say) five Sub-words within an M-word, but only three of them have overt exponents, the learner lacks potentially useful clues as to which positions to assign the overt Sub-words. In Ancient Greek, only one of the structural positions *v*, *Asp*, and *T* had an overt exponent, and with this ambiguity comes room for error. This returns us to the point made repeatedly elsewhere in this dissertation, that speakers rely heavily on surface forms when they are constructing hypotheses about their grammar, and any ambiguities present in the surface forms are a potential source for learner error.

I said above that examples of Sub-word migration share a number of properties with affix-genesis, particularly with Type II; the same is true even if the structural change is not reflected on the surface, as in Greek and Swedish (although the Swedish example may be more akin to Type IV; cf. above). This is, in fact, exactly what we would predict to be the case given the assumptions of DM. If word structure is inseparable from syntactic structure on a larger scale, then the same types of morphosyntactic changes found above the level of the M-word should also be possible below the level of the M-word. Embick (2007) discusses how Local Dislocation can occur either between M-words or between Sub-words (but not between an M-word and a Sub-word²⁸⁵); here, the principle is similar. Learners can become confused about the structural position of a linguistic entity, period. The nature of their confusion may vary, but it is not dependent on the ambiguous entity's having had M-word status in its recent history: learners, as stated in Chapter Two, *do not have access* to the manner in which previous generations have analysed this entity. This is in fact the cause of their confusion. We predict, therefore, that learners are at least as likely to be confused about the structural position (or various other properties) of specific Sub-words as specific M-words. The nature of their confusion may well vary, simply because Sub-words and M-words have different properties; but where there is ambiguity, there is potential confusion, errors, and ultimately changes to the grammar. The case studies discussed in this chapter suggest that this prediction is correct.

The discussion in this section has been largely preliminary; since most historical linguists do not hold the view that M-words have an internal syntactic structure, very little work has been done from this angle. Studies of more examples of these phenomena are needed; in particular, more cases like the Yup'ik and Cherokee, where changes in position are easier to see and to locate. The evidence so far is promising; however, complications do arise below the level of the M-word that make the problem quite difficult.

7.4 Chapter Summary

The goal of this chapter was to establish that the phenomena discussed in previous chapters are not limited to changes in the status of linguistic terminals from M-word to Sub-word or vice versa, but rather than similar changes can occur within an M-word. This was particularly highlighted in 7.3, where we saw that changes in the function of Sub-words are, in structural terms, almost identical to Type II affix-genesis from Chapter Two, the crucial difference being that no change in typing occurs. The phenomena in 7.2 are less straightforward, but often involve morphological re-cutting (itself unconstrained in terms of the level at which it is operant). One of the most robustly occurring types of affix migration, that involving complex pronominals and case markers, is a by-product of Type III affix-genesis.

²⁸⁵ Or at least, not immediately; in principle, an M-word could potentially undergo LD with a Sub-word if it first became a Sub-word via LD.

This chapter has also argued that changes in the surface position of Sub-words correlate with changes in their function and underlying structure, which is predicted by the assumptions of a piece-based syntactic approach to morphology. However, it would clearly be an oversimplification to argue that all changes in function are accompanied by changes in position.

Baker (1985) introduced the Mirror Principle to capture the observation that the order of affixes on nouns and (especially) verbs can be taken to reflect the underlying order of syntactic projections. In its strongest form, this cannot be upheld; even within a single language (e.g. ancient Greek), there may be different morphosyntactic expressions of voice in different positions. Nevertheless, other morphosyntactic features do seem to occur in a particular order: tense, for instance, is more or less fixed. Therefore, while it cannot be the case that *all* syntactic projections have a fixed universal order, there does seem to be some sort of universal template for features like tense and aspect. Nevertheless, there are clear mismatches between morphology and syntax in various languages; it is for this reason that DM has recourse to post-syntactic operations like LD and Lowering.

Embick (2007) discusses several examples in detail. The Latin enclitic conjunction *que* ‘and’ is enclitic on the first head of the second conjunct (7.60), although this is clearly not the position it must occupy in the underlying structure.²⁸⁶ Embick analyses *que* as undergoing Local Dislocation with the M-word to its immediate right.

- (7.60a) di- u noct- u= **que**
 day-ABL.SG night-ABL.SG CONJ
 ‘by day and night’
- (7.60b) ...cum hac et praetori-a cohort-e cetrat-orum ∇
 with this.ABL.SG CONJ official- ABL.SG escort- ABL.SG caetratus-GEN.PL
 [barbar- is= **que** equit- ibus pauc-is
 barbarian-ABL.PL.=CONJ cavalry-ABL.PL few- ABL.PL
 ‘with these and his official retinue of lightly-armed troops and a few
 barbarian cavalry-men’ [C. B. C. II.75]
- (7.60c) ...∇ [maius= **que** commodum ex oti- o
 more.NOM.SG=CONJ profit.NOM.SG from idleness-ABL.SG
 me- o quam ex ali- orum negoti-is re- i
 1ST.SG.POSS-ABL.SG than from other-GEN.PL work- ABL.PL thing-DAT.SG
 public-ae ventur-um
 public-DAT.SG come- FUT.PTCP.NOM.SG
 ‘and more profit will come to the republic from my idleness than from the
 activities of others.’ [Sall. J. III.4]

A second example is the Lithuanian reflexive *si*, which is proclitic to the verb, but shows mandatory second-position effects within the verbal complex (7.61). Embick argues that syntactically *-si* attaches to the node dominating the verbal complex (T, in finite verbs) and is initially linearised as the leftmost element; it then undergoes LD with the adjacent Sub-word on its right.

- (7.61a) laikaũ-si ‘I get along’
 (7.61b) iš-si-laikaũ ‘I hold my stand’
 (7.61c) su-si-pa-žinti ‘to know [someone], to recognise’

Both of these examples demonstrate that speakers are more than capable of tolerating morphosyntactic mismatches; they are, to all appearances, quite good at it. Learners acquire

²⁸⁶ (7.60b–c) follow Embick (2007)’s convention of indicating the position between conjuncts with ∇ and the beginning of the second conjunct with [. The enclitic is in boldface.

these forms because surface forms are *the* evidence guiding them towards the acquisition of the language of their grammar. Therefore, innovations in the function of particular Sub-words, or innovations resulting in unconventional surface forms, need not by necessity result in a reflection of such changes on the surface. Again, the English *do*-support case study from 6.2.1 is a good example of speakers doing what they can with whatever strategies and structural mechanisms their language's history has bequeathed them. This raises a number of very interesting questions about how speakers cope with unwieldy morphosyntactic relics, and more specifically, the nature of the relationship between linguistic history, post-syntactic operations, and affix migrations like those discussed in this chapter.

The string-vacuous cases are in fact the easiest to account for: speakers can posit changes in structure without needing recourse to post-syntactic operations, and without the apparent confusion seen in cases like the complex pronominals from 7.2.2, so long as there are no overt affixes to narrow down the range of possible structural positions for various Sub-words. Much more difficult are the examples with overt relocation. It is not surprising that we find fewer innovations within M-words than we do above the M-word: the linear order here rarely deviates, so speakers have a great deal of very consistent evidence of the "correct" order. They are, moreover, very sensitive to linear adjacency relations. Why do learners correctly acquire deviations from underlying structure with such great reliability, and yet fail in other cases?

Structural ambiguity appears to be the deciding factor, although considerably more investigation is required. The Yup'ik and Cherokee cases in this chapter were illustrative of this detail. In both cases, suffixes which could intervene between the original and innovative structural positions of the relevant morphemes were optional rather than obligatory, which created a type of ambiguity potentially similar to the ambiguity created by null affixes. Learners could then conclude that the original, semantically polysemous affix was in fact two different affixes. Had the intervening suffixes been obligatory, it is overwhelmingly likely that the Yup'ik and Cherokee innovations would not have been possible.

This does *not* rule out changes in function, or even in underlying structural position. Learners are capable of coping with surface forms that do not match their underlying structural analysis, and this tension may well be the diachronic source of synchronic post-syntactic operations (again, cf. Lowering in English). Nevertheless, it is not difficult to see that the tighter restrictions on linear surface order produced by the complex head relationship may produce a tendency towards conservatism on the internal structural properties of M-words.

Clearly, however, more research is needed in this area. It would be undesirable to say that speakers rearrange surface structures if possible and posit post-syntactic rules if they don't, because this essentially amounts to saying nothing at all. We need to be able to delineate the domain of possibilities in a rigorous way without making everything vacuous and deterministic. For that, more examples are needed.

Examples of this nature are, unfortunately, rather difficult to come by. The reason for this is simple: unless there is clear textual evidence (as in Georgian, and Latin) or very obvious doublets (as in Yup'ik and Cherokee), changes of this type are all too likely to occur without leaving any trace. Nevertheless, such phenomena are worth searching for, because they seem the best hope for illuminating the relationship between morphosyntactic reanalysis and synchronic post-syntactic rules.

Chapter Eight

Previous Generative Approaches

8.1 Overview

Although the topics addressed in this dissertation are most commonly discussed by functionalists and traditional historical linguists, there has also been some work by linguists working in the generative tradition, and in this chapter I will outline their frameworks. The Minimalist approach of Roberts and Roussou will be discussed in 8.2, and the Optimality Theory approach of Kiparsky in 8.3.

8.2 Roberts and Roussou

The most notable work on grammaticalization in the Minimalist framework is that of Roberts and Roussou (1999, 2003). Since the theoretical assumptions of Minimalism are more similar to those assumed in this dissertation than the OT assumptions of Kiparsky (cf. 8.3 below), there is greater similarity between the Roberts and Roussou approach and that adopted here, most notably in the treatment of grammaticalization as epiphenomenal to deeper structural changes. However, there are also important differences, owing in part to the fact that they are concerned with a rather different set of theoretical questions than I have been in this dissertation. The discussion that follows focuses primarily on the framework outlined in Roberts and Roussou (2003), henceforth “RR”.

RR conceive of grammaticalization as ‘the creation of new functional material, either through the reanalysis of existing functional material or through the reanalysis of lexical material’ (cf. RR p.2); this, they argue, always involves structural simplification. Most of their attention is given to e.g. the creation of auxiliaries from lexical verbs, determiners and pronouns from demonstratives, and complementizers from a variety of sources. They are, therefore, concerned with a much more heterogeneous category of phenomena than the more narrowly-defined category that has been the focus here, and as a result, they do not concentrate on how the new functional material is packaged. The concentration here has been on finer-grained distinctions. It must be pointed out that some of the key concepts for RR are regrettably fuzzy, particularly the lexical/functional distinction; it is not at all clear whether there is a universal functional inventory, and if so, what it is composed of. For example, many of the “lexical” sources of auxiliaries might be more plausibly argued to be exponents of *v*, which is already functional; and whether pronouns are “more functional” than demonstratives is arguable.

For RR, grammaticalization, necessarily accompanied by structural simplification, is a regular case of their larger concern, parametric change, which underlies all syntactic changes. Since RR follow Borer (1984) in assuming that parametric variation is rooted in the lexicon, the lexicon is crucial to their formulation of change. Their lexicon has three parts:²⁸⁷

- (8.1a) Lexical items, specified as $\pm V$, $\pm N$, with PF and LF properties given
- (8.1b) Substantive universals encoded as interpretable features of functional heads
- (8.1c) * assigned in a language-particular fashion to (b)

The diacritic * in (8.1c) is the only variable property. It is described as ‘the expression of a relation between functional features and morphophonological matrices (overt

²⁸⁷ Taken from Roberts and Roussou (2003:29), their (19). Note that they are assuming only interpretable features.

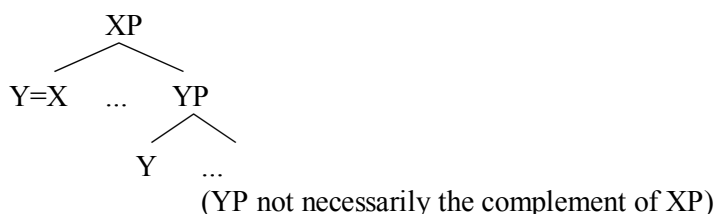
or zero morphemes).’ A feature F marked with this diacritic must be realised overtly at PF, and this may be achieved in two ways: by Move or by Merge. Merge is always preferred over Move for reasons of economy; if the lexicon provides a morphophonological realisation for F*, then this will be used, and no Move will occur. In the absence of such, a constituent must be moved to F.

Grammaticalization, in this framework, occurs when a former Move operation is reanalysed as Merge; in other words, a constituent which formerly moved into its surface syntactic position comes to be generated there instead; this can be schematised as in (8.2). This reanalysis is motivated by pressures towards structural simplification by way of the elimination of feature syncretism (i.e. the presence of multiple formal features on a single node).

$$(8.2) \quad F^*_{\text{Merge+Move}} > F^*_{\text{Merge}}$$

A consequence of this formulation is that grammaticalization must always proceed in an “upwards” direction (RR:202), with the exponents of hierarchically lower lexical categories reanalysed as the exponents of hierarchically higher functional heads. RR provide the general schema in (8.3)²⁸⁸. Here, Y has been reanalysed as the exponent of X.

(8.3)



RR (p. 202) conclude their discussion of this general schema with a statement of directionality: “Successive upward reanalysis along the functional hierarchy is thus how we define grammaticalization paths. Furthermore we see that the path is always upwards.” They note that examples of syntactic change proceeding “downwards” – e.g. the loss of V2 or the loss of V-to-I movement – have different properties from “upwards” syntactic changes (cf. (8.4)–(8.5) below, from RR: 208), and none of these properties are implicated in grammaticalization.

(8.4) “Downwards” changes:

- i) Apply to all members of Y;
- ii) Do not change category of Y;
- iii) Involve no semantic or phonological change to Y-roots;
- iv) Cannot be cyclic.

(8.5) “Upwards” changes:

- i) Apply only sporadically or to morphological subclasses of Y;
- ii) Change category of Y;
- iii) Are associated with semantic bleaching and phonological reduction;
- iv) Can be cyclic.

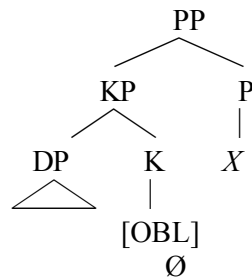
In particular, they note the absence of “interface effects”, i.e., phonological and semantic changes. Therefore, “downwards” movements are not examples of grammaticalization. Note, however, that RR do not adhere to the strict notion of directionality found in Kiparsky (cf. below) and elsewhere: “[T]here is nothing in our

²⁸⁸ Their (22); cf. Roberts and Roussou (2003:200).

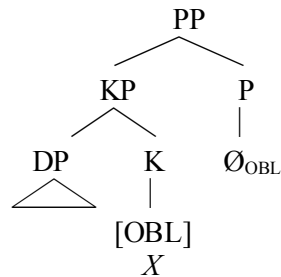
approach that prevents instances of de-grammaticalization from taking place, yielding a lexical category out of a functional one... in our terms de-grammaticalization is indeed possible, albeit sporadic.”

RR’s formalization is based on an intuition shared by this dissertation: that one of the common sources of morphosyntactic change is a misunderstanding by language learners of which structural position an exponent is associated with. However, there is a serious problem with RR’s characterization of the issue which undermines their entire programme, and this centres upon the fact that they have built inherent directionality into their system.²⁸⁹ Simply put, defining grammaticalization as “upwards” movement is much too strict. Consider, for example, Type II affix-genesis, specifically the reanalysis of adpositions as case markers, schematised in (8.6). This reanalysis is “downwards”, but results in a new case affix.

(8.6a)

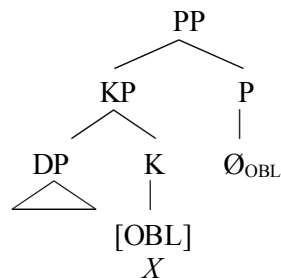


(8.6b)



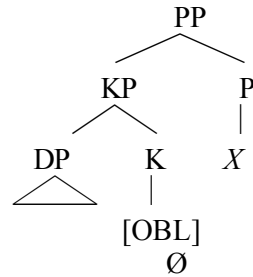
Conversely, consider “Type B” affix-exodus, such as the reanalysis of an erstwhile case suffix as a postposition, as in Northern Saami. This involves precisely the same change as that in (8.6), but in the opposite direction – therefore, upwards.

(8.7a)



²⁸⁹ This issue has been remarked on elsewhere; cf. Fuß (2005).

(8.7b)



Affixes can also be “extended” to lower syntactic positions; the definiteness suffix in the Scandinavian languages is a case in point. In Danish, D is positioned by Local Dislocation when it is adjacent to the head noun, resulting in a definiteness suffix. When there are intervening adjectives, this operation is prevented from happening.

- (8.8a) mand-en
man- DEF
‘the man’
- (8.8b) den unge mand
DEF young man
‘the young man’
- (8.8c) *den unge mand-en
DEF young man- DEF

Swedish has added a further innovation, marking all definite NPs with the definite suffix regardless of whether they have moved into D or not.

- (8.9a) mus- en
mouse-DEF
‘the mouse’
- (8.9b) *den gamla mus
DEF old mouse
‘the old mouse’
- (8.9c) den gamla mus- en
DEF old mouse-DEF

The definiteness suffix now appears in a lower structural position than it did previously. Whereas in Danish, the definiteness suffix is a reflex of Local Dislocation, in Swedish it also appears as a dissociated morpheme.

RR’s definition of grammaticalization thus makes exactly the wrong predictions: affix-exodus becomes an instance of grammaticalization, while affix-genesis becomes de-grammaticalization. Although concentrating on definitional issues – namely, what “counts” as grammaticalization and what does not – is a fairly trivial enterprise (as compared with e.g. isolating structural properties), this result seems perverse.

A second problem with RR’s treatment relates to their use of a separate generative lexicon. Recall that in Chapter Four, I demonstrated that a syntactic theory of morphology makes strong predictions about where a novel affix will be positioned in its M-word. Given a conservative linear sequence like (8.10a), an immediate change to (8.10b) with no intermediary would be entirely unexpected. In a lexicalist theory, this is harder to rule out.

- (8.10a) W √-X-Y-Z
(8.10b) √-X-W-Y-Z

RR's discussion of affix-genesis encounters the problem of invoking two generative systems. In the case of the fusion of the Romance future, for instance, they propose that the new future exponent underwent a change from a syntactic affix to a lexical affix, the former being an element of syntax, and the latter a feature of V. They argue that this change is due to the change in word order in Romance from OV to VO and from VP-Aux to Aux-VP; with the order VP-Aux impossible, the infinitive + *habēre* sequence was taken to be a single word (i.e., fashioned in the lexicon). On this view, affixes do not necessarily have the same status in different languages; a verbal suffix in a head-final language might be syntactic, while a verbal suffix in a verb-medial language like French must be an artefact of the lexicon. The issue of verbal suffixes in very strict head-final languages is a problem for everyone, since it is not always clear how high in the structure the verb has actually moved. Nevertheless, this is an undesirable complication; on the theory advocated here, surface affixation may reflect different underlying structures, but all affixes with the same structural properties have the same status.

Affix-exodus is even more problematic, because examples of de-affixation are completely absent from RR's discussion. They briefly discuss the development of the Greek quantifier *mēdhen* into a noun which can be preceded by the definite article (*to midhen*; cf. p. 208 fn. 2), which they take to be a reanalysis of a polymorphemic word as a monomorphemic lexical item; they do not discuss cases like those in Chapter Five. This is no doubt in part simply a consequence of their broader focus; they are more interested in clausal syntax than morphosyntax, so most of their case studies (e.g. the development of modals in English) do not involve morphology.

The problem with this is that affix-exodus would necessarily involve two different systems: the lexicon and the syntax. This is inherently more complex than the relatively simple category change in the case of *midhen*, since it would appear to involve a single item in the lexicon splitting into two pieces with potential syntactic consequences; moreover, as we have seen, in many of these cases (particularly the Irish example in Chapter Five) there are consequences for the clausal syntax. Furthermore, because they handle word-building within the lexicon, RR cannot easily capture the parallels between de-affixation and other linguistic changes discussed in Chapter Six, or between changes involving M-wordhood and changes within an M-word, as discussed in Chapter Seven.

In short, RR's framework is designed to handle cases of grammaticalization that do not involve a change in status between M-words and Sub-words, such as the use of former lexical verbs as modals or auxiliaries. These are precisely those cases which have *not* been discussed here, and therefore it stands to reason that there will be differences in our respective frameworks; we are not dealing with the same objects. Morphosyntactic changes are a problem for RR, however, because by concentrating specifically on "upwards" changes, they exclude many other examples that bear certain similarities, such as the creation of case affixes. In addition, the parallels between affix-genesis and other types of morphosyntactic change are largely overlooked in this system.

8.3 Kiparsky

Most of Kiparsky's recent work has been within an Optimality Theory (hence OT) framework, and in his (2011) paper he attempts to extend it to cover diachronic phenomena, specifically of the grammaticalization type. The idea that the same theoretical framework should be used for both synchronic and diachronic phenomena is one of several ideological points on which Kiparsky and I are in complete agreement. We also share the intuition that speakers have a bias in favour of equating phonological and morphosyntactic words, although as we'll see, this is expressed very differently in his framework. Kiparsky's chief contention with earlier accounts of grammaticalization is their treatment of unidirectionality: either

unidirectionality is built into the definition of grammaticalization, in which case it ceases to be theoretically interesting, or the claim is simply false, due to the small but significant set of known counterexamples. His goal is to redefine grammaticalization in such a way that it is non-trivially exceptionless and shown to be of theoretical significance.

Kiparsky is working within a constraint-based lexicalist grammar consisting of a lexicon, a set of ranked constraints (both UG and language-specific), and a morphology with two components: a generative component which “specifies the potential expressions of the language and their potential interpretations” and a blocking mechanism. This blocking mechanism does not involve competition between word-formation rules, but competition between overall expressions linked by some paradigmatic morphological expression rather than semantics alone. In this system, blocking derives from the interaction of two constraints, Expressivity/Faithfulness (“express the meaning of the input”) and Economy/Markedness (“avoid complexity”). Complexity is defined as “number of words and morphemes”. Kiparsky (p. 10) says that the interaction between these constraints results in four situations (the first two give rise to blocking, the third to free variation):

(8.11)

- a. Among equally faithful expressions, the least marked is optimal.
- b. Among equally unmarked expressions, the most faithful is optimal.
- c. Among equally faithful and unmarked expressions, the constraints tie.
- d. When the constraints conflict, their ranking decides.

This system is illustrated with the English superlative. In explaining why *best* is the superlative for *good*, Kiparsky sets up the following tableau, in which Expressiveness dominates Economy.

(8.12)

Input: Max(good)	Expressiveness	Economy
good	*	
good-est		*
most good		**
☞ best		

Here, *good* is ruled out immediately because it does not express the semantic value “superlative”; *most good* is ruled out because it consists of two words, and *goodest* is ruled out because it is bimorphemic. The chosen candidate, *best*, is optimal because it is a single, monomorphemic word with the desired semantic value.²⁹⁰

This system is applied to the diachronic domain in the following way. First, grammaticalization and analogical change are both classified as forms of “grammar optimization”, defined as “elimination of unmotivated grammatical complexity or idiosyncrasy”. This is intended to constrain analogy to cover only those cases likely to occur rather than the set of all possible proportional analogies, and also allows non-proportional analogies (e.g. levelling or lexical diffusion) to be considered mechanisms of change. Then, if the set of possible changes is constrained by UG, there ought to be a category of analogy which requires no exemplum at all, but rather “establishes new patterns in the language” through the emergence of covert constraints; this is similar to the “emergence of the unmarked”. Essentially, UG constrains and determines all types of morphosyntactic change, so grammaticalization and analogy can be clustered together as different manifestations of the

²⁹⁰ One could probably produce an OT tableau with a constraint ranking that explains why horned bear-sharks are even more awesome than ordinary bear-sharks, although (as this is self-explanatory) it would probably be unnecessary.

same underlying mechanisms. Analogy, according to Kiparsky, results from “reduced input” to the constraint system; in other words, if *best* didn’t exist, then the grammar would opt for *goodest*, and if neither existed, the system would opt for *most good*. Grammaticalization, on the other hand, occurs when the input becomes “radically underdetermined by the grammar”, causing the learner to “fall back on UG”. Since UG prefers words to phrases, if there is no decisive data to the contrary, it will analyse e.g. noun + adposition as noun + case affix. No exemplar is needed; this change could occur in a language with no case affixes prior to this change. Therefore, Kiparsky argues, grammaticalization is driven by UG, but limited by language-specific constraints.

Under Kiparsky’s framework, unidirectionality is also a feature of UG. He argues that the fusion of two items as a single word does not require an exemplar for an analogy because UG prefers words to phrases, so if it is possible to analyse two items as one, principles of structural economy will ensure that this occurs. De-affixation (“fission”, in Kiparsky’s terms²⁹¹), by contrast, violates principles of structural economy, and therefore requires an exemplar. Grammaticalization is unidirectional because all apparent counterexamples are actually just examples of ordinary analogy.

To prove his point, Kiparsky discusses a number of examples, including several of the case studies discussed in Chapter Five, and argues in each case that there is a pre-existing element in the language to serve as a model for analogy. However, sometimes the proposed “analogy” counts as an analogy only under Kiparsky’s definition, which is not the standard definition. He seems to allow *negative* evidence to count as an exemplar. For instance, in his account of the English and Swedish group genitives, he points out that both languages have lost case as an inflectional category, whereas those Germanic languages which have retained case do not have group genitives.²⁹² He calls the group genitive an analogy, therefore, because it is “elimination of a singularity in the language”. This is analogy to a negative, which is not part of the traditional definition of analogy: analogy is usually invoked in e.g. cases where some morphological category or exponent has influenced the phonological shape of another.²⁹³ It is also an *imperfect* analogy to a negative: the group genitive may no longer be a case marker of the older type, but it *is* still a case marker, merely a phrasal one.²⁹⁴

Even if we accept the idea that negative analogy is possible, the reasoning here is rather *ad hoc*, since simply eliminating the genitive marker entirely seems like a more obvious “analogy”. Nor is it clear why preserving a genitive case marker ought to be more important than preserving an accusative marker, especially when both Swedish and English have prepositions perfectly capable of indicating genitival relationships. Furthermore, neither language otherwise possesses clitics with case-like properties; thus the group genitive is highly marked in these languages, and it is hard to see how one is supposed to consider it analogical except by stipulation. A further problem is the uncertainty in chronology: recall that the group genitive seems to have appeared in Swedish while the dative case was still productive. Therefore, even if Kiparsky’s negative analogy is permitted, much remains to be explained in his account.

In addition, it is not clear to me at all how Kiparsky’s system handles cases like English *ish*. *Ish* is semantically similar to expressions such as *kinda* or *sorta*, but its syntax is, to my knowledge, unique; there are no obvious sources of analogy that can account for it. Nor have any changes occurred elsewhere in the grammar that may have triggered it.

²⁹¹ Not to be confused with “fission” in a Distributed Morphology sense.

²⁹² But cf. Yiddish and German above.

²⁹³ Analogy is a notoriously problematic notion, in any case; it is often offered as an explanation of a particular historical change, but no one has ever managed to define it with sufficient precision, and it therefore is typically invoked only when no other explanation is forthcoming. This, coupled with the fact that its fruits are notoriously heterogeneous, makes it really more of a descriptive label than anything else. In this respect, the term *analogy* is not unlike the term *grammaticalization*.

²⁹⁴ This paragraph benefited considerably from a discussion with Julie Anne Legate (p.c.).

Kiparsky states that under his proposal ‘unidirectionality is vindicated as a core property of grammaticalization’ because it is a ‘consequence of the model’²⁹⁵: the counterexamples are not really counterexamples as such because they are examples of exemplar-based analogy. This explains the asymmetrical frequency of counterexamples: fusion will happen whenever it can, because it is driven by UG; fission violates “avoid complexity” and therefore learners will not postulate it without compelling reasons to do so. Thus Kiparsky predicts that all apparent counterexamples to grammaticalization can be explained as garden-variety analogy one way or another, and that speakers will start analysing two words as one word whenever they are given the opportunity to do so.²⁹⁶

As thus formulated, it is not clear to me how this proposal differs in overall outcome from the proposals it is critiquing. If, in this framework, all examples of de-grammaticalization count as ordinary analogy and therefore do not affect the unidirectionality thesis, then what would Kiparsky admit as a genuine counterexample to the unidirectionality of grammaticalization? While he rightly points out that many competing accounts build unidirectionality into the definition of grammaticalization, thereby rendering it uninteresting, his own proposal has much the same consequence. However, this objection is not a necessary consequence of the framework. It would be quite possible to retain the basic proposal Kiparsky lays out while rejecting the view that the directionality issues are of theoretical rather than descriptive significance; the only consequence is the discarding of ‘unidirectionality as a core property of grammaticalization’.

One serious problem for Kiparsky’s system is the cross-linguistic tendency to replace synthetic forms by analytic forms: a choice of more complexity over less. Kiparsky’s system seems to predict that analytic forms will win out only when the synthetic forms become problematic, but he gives no indication of what this means. The move towards analytic forms in languages like French and German does not seem to be necessarily driven by problematic synthetic forms. The same is true for the periphrastic Latin forms underlying the Romance future and conditional; Latin had synthetic forms for these tenses. In the cases of Latin and modern Romance, the synthetic and analytic forms coexisted in variation for quite some time, which implies that there was nothing particularly problematic about them. Of particular interest is the fact that one often finds what appears to be “cycles” of analytic and synthetic forms, each replacing the other in turn. Latin, for instance, had synthetic *b*-futures, likely the descendants of an earlier analytic construction. These were replaced by analytic forms (as discussed in Chapter Four), which in turn eventually became synthetic. Meanwhile, in contemporary Latin American Spanish, another novel analytic construction is preferred over the synthetic in the spoken language.²⁹⁷

To solve this problem, Kiparsky could resort to his higher-ranked Expressiveness constraint, with an argument something like this: the new forms originate in a different semantic niche. Gradually, the analytic forms encroach on the territory of the older forms, which are ousted because they are “less expressive”.²⁹⁸ This scenario is perfectly plausible, but it compromises the primacy of words over phrases. If there are two forms expressing very

²⁹⁵ Cf. Kiparsky (2011:31).

²⁹⁶ He also predicts that there will be free variation in some cases if two constraints have equal ranking; however, it is *not* clear that his system allows for contextual variation (although he might be able to achieve this if he is very clever in ranking his other constraints).

²⁹⁷ The construction, which resembles the English *be going to* construction, involves the verb *ir* ‘go’ followed by the preposition *a* ‘to’ and then the infinitive.

²⁹⁸ It would be possible, in some cases, to argue that the erosion of phonological substance might render the synthetic forms “less Expressive”; however, sound change cannot be implicated in all cases. Aviad Eilam (p.c.) informs me that there are also a number of similar cases involving nominals in Modern Hebrew and Arabic. In Modern Hebrew, older synthetic possessive forms and innovative analytic forms exist side by side, but with stylistic differences; synthetic forms are mostly found in formal or written contexts. Similar phenomena can be cited for contemporary Romance languages, e.g. Spanish.

similar semantic domains, and one of them begins to take over the domain of the other, we would expect the single-word form to win out in Kiparsky's scenario. Furthermore, the semantic differences involved are often extremely slight, and tend to be eroded further over time.

In addition, resorting to Expressiveness cannot account for complex head disintegration. English *do*-support, for instance, is a purely syntactic phenomenon; there is no question of resorting to Expressiveness here. This is purely a case in which the grammar cannot output a single word and must instead resort to two, despite the fact that earlier grammars of the same language could and did employ a single word in these configurations. On Kiparsky's account, this is completely unmotivated.

Another problem for Kiparsky is the fact that language learners sometimes prefer a two-word analysis even when one might think analogical pressures favour a one-word analysis. The best example is the Middle Irish case discussed in Chapter Five. Recall that there was a reanalysis of subject agreement markers as suffixed pronouns in Middle Irish, and the former third singular form had become a so-called "analytic" form, which differed from the rest of its paradigm in requiring an overt subject, which could be a pronoun of any person or number. Of the other members of the paradigm, only the third plural could optionally occur with an overt subject; overt subjects were ungrammatical for all other verbs. Therefore, the verbal paradigm looked superficially like this:²⁹⁹

(8.13)	1sg	<i>at-beir-im</i>
	2sg	<i>at-beir-e</i>
	3sgm	<i>at-beir=sé</i>
	3sgf	<i>at-beir=sí</i>
	1pl	<i>at-beir-mit</i>
	2pl	<i>at-beir-id</i>
	3pl	<i>at-beir-it</i>

Under Kiparsky's analysis, the predicted next step is the reanalysis of *sé* and *sí* as third person verbal endings and a new gender distinction on the verb, rather than the reanalysis of all extant agreement markers as pronouns. As we have seen, the changes in Middle Irish make sense when situated in the larger grammatical context, but they are *not* predicted by a system in which one word is preferred to two. While Kiparsky can probably work out an analogical solution, the larger problem remains.

A similar problem is found in Kiparsky's treatment of the Saami case, as was discussed in 5.2.2. Recall that in Saami, the modern postposition *haga* was originally a phonologically-conditioned allomorph of a case suffix that also surfaced as monosyllabic *-htta*. Kiparsky's analysis is that prosodically-motivated analogy was primarily responsible here, by analogy with the innovative comitative plural case marker, *-guin*, from *guoibmi* 'fellows'.³⁰⁰ This form has become increasingly suffix-like in its syntactic behaviour (e.g. by disallowing conjunction reduction), but until recently was essentially a cliticised postposition. Since the comitative case is the antonym of the abessive, and since both *-guin* and *haga* are disyllabic (unlike most other Saami case markers), *haga* too became a clitic. However, his discussion completely avoids all mention of the existence of the second allomorph. This is significant, because since speakers had the option of retaining or generalising this suffix, the example does not behave as his framework would predict: speakers *could* have easily maintained a one-word hypothesis here. For further discussion, cf. 5.2.2.

²⁹⁹ This is a simplification, as not all third singular forms were endingless.

³⁰⁰ The spellings Kiparsky employs here are *-guim* and *guoibme*; the spellings used in the text are the standard Northern Saami spellings, according to Jussi Ylikoski (p.c. to Norde).

The upshot of this is that invoking analogy in cases of de-affixation is unenlightening, and misses some interesting insights, such as the similarities between de-affixation and re-cuttings noted in Chapter Six; the comparison is repeated below for convenience.

(8.14) a nadder > an adder

(8.15) [V-Agr] pro > [V-[Agr=pro]]

Both de-affixation and re-cuttings may be “analogical” in some sense, but simply labelling them thus misses the interesting connection between them. Kiparsky’s system is not really set up to handle re-cuttings either, since they do nothing to increase economy or informativeness.

Kiparsky’s framework also misses the parallels between affix-genesis and -exodus and the phenomena discussed in Chapter Seven. Some of the cases discussed there can be readily accommodated in his system; he does not need to account for string-vacuous migration at all (in his system, such changes may be regarded as strictly semantic), and he can presumably account for the complex pronominals by ranking “case suffixes are final” above Faithfulness. However, Yup’ik and Cherokee are problematic in his system because there is no obvious motivation for the relocation of the innovative affixes. The movement cannot be phonological, since the original function and position of the affixes in question are maintained.

There are a number of similarities between Kiparsky’s approach and mine, most significantly in the intuition that issues in diachronic morphosyntax are best accounted for using the same theoretical tools used to handle synchronic issues. We also have similar intuitions about the processes at work, and have designed frameworks to capture them; we agree, for instance, that since affixation can occur in any language, even those with fewer affixes, the system needs to be set up in such a way that affixation is natural and easy. We also both lack a clear solution for the problem of when exactly the data become sufficiently ambiguous that learners opt for a one-word analysis. Many of the major differences between our respective approaches are simply programmatic. However, Kiparsky has real difficulties accommodating de-affixation within his framework: he requires an exemplum for analogical changes, and such an exemplum is not always easy to come by, as in the cases of the Germanic group genitive and English *ish*. In addition, he has a more general problem, in that he predicts that a single-word analysis will always be superior to a multiple-word analysis *in every language at any time*, which fails to explain phenomena like complex head disintegration.

8.4 Chapter Summary

This dissertation is not the first generative attempt to account for affix-genesis. It is, however, the first attempt which acknowledges the parallels and similarities between affix-genesis and other varieties of morphosyntactic change. The Minimalist account of Roberts and Roussou shares many of the current assumptions about syntax, but has been principally tested on changes in the function of M-words and fares less well on changes in the status of M-words. In fact, their framework is defined in such a way that many classic examples of affix-genesis – e.g. the development of case markers – are excluded from consideration. Kiparsky’s OT account can accommodate a broader range of data, and his treatment of affix-genesis is similar to mine once differences in theoretical assumptions are taken into account; however, his analysis of de-affixation is somewhat less convincing.

Chapter Nine

Concluding Remarks

This dissertation has argued in favour of a non-teleological, learner-centric approach to morphosyntactic change which effectively eliminates any special diachronic-specific mechanisms or components to the grammar. Change is not a continuous, independent process; it is a sequence of synchronic grammars and no more. There is only a conservative grammar and a chronologically later innovative grammar. We can identify the analytic difference between the grammars and, in many cases, what may have motivated the innovator, but we need not postulate a direct linear mapping of some kind; the second grammar may be completely incompatible with the first. Furthermore, in many cases the circumstances for the change are entirely language-specific.

I have stressed repeatedly throughout the dissertation that change of this type is non-deterministic. A noun that develops into a postposition may or may not ever become a case suffix; clitics may or may not ever become affixes. The appearance of determinism in morphosyntactic change is illusory; it is a product of the fact that adpositions are (to our knowledge) the usual source for case markers, and nouns are a frequent source of adpositions. Furthermore, most of the examples of change that we are familiar with are examples which went “all the way through” (however that is to be defined); this is why we know of them. But the speakers themselves have no way of knowing where a noun or postposition falls on an abstract “grammaticality cline”; they can only acquire the data as it is presented to them. I have demonstrated throughout the dissertation that adopting this perspective on change is completely consistent with the available data.

The remainder of this chapter is divided into two parts. First, I review and summarise the discoveries made throughout the dissertation (9.1); then I suggest some directions for further research which I consider promising (9.2).

9.1 Results

In some respects, this dissertation can be viewed as a test of Distributed Morphology: can DM handle diachronic data effectively, and do diachronic data pose challenges for any aspects of the theory? As it turns out, DM does quite well with diachronic data. Of course, this is in part due to the perspective on language change assumed here; since DM does well with *synchronic* data, and this model of change posits only a sequence of synchronic grammars without special diachronic mechanisms at play, it is to be expected that DM would be capable of modelling change. However, the specific properties of DM make it particularly useful for capturing diachronic phenomena. One of the most insightful non-generative insights into morphosyntactic change is the observation that morphology tends to mirror earlier stages of syntax; since morphology and syntax are already handled in the same generative system in DM, this intuition is easily captured when using DM to talk about diachronic phenomena.

Furthermore, some basic predictions fall out of the assumptions of DM which accord nicely with the observable data. First, DM makes precise predictions about the location of new affixes within an M-word: they will occupy the same surface position relative to other Sub-words as they did prior to affix-genesis. In the usual case, this means that they will appear on the periphery; as we saw in Chapter Four, more complex cases exist, but these too can be handled within DM once other aspects of the grammar are taken into account. This is less easy to formalise within a lexicalist system, particularly one like Optimality Theory, where new affixes might be predicted to be positioned in such a way as to achieve optimal phonotactics.

A second advantage of DM is its ability to account for less canonical behaviour of affixes. In particular, some terminals behave as affixes under some syntactic conditions and not under others; some allow the interpolation of particles, for instance, or mutable position within the M-word. Cases of this sort pose a serious problem for a lexicalist theory because they appear to violate notions of lexical integrity; this is equally problematic from a diachronic perspective. They are, however, much less problematic for a piece-based syntactic theory like DM. M-words are assembled by the same system that assembles clauses, and therefore one might expect that there would sometimes be disruptions.

Third, as we saw in Chapter Seven, the types of change affecting affixes within an M-word very closely mirror the types of change observed above the level of the M-word, modulo the obvious differences given that affix migration does not involve changes in the status of an affix, but only in its structural position. This prediction falls out automatically on a DM account, since the same generative system is building both words and phrases; but the parallel is harder to capture in a system where words and phrases are built by different generative components.

Nor is affix migration the only type of morphosyntactic change to share structural similarities with affix-genesis and -exodus, as discussed in Chapter Six. These parallels, too, are harder to capture in a lexicalist theory of morphology than they are in DM, and nigh impossible to capture on a functionalist account.

The typology of morphosyntactic changes that can be articulated so easily using DM is useful on multiple grounds. First, each of the different types of change discussed here has clear properties in common with affix-exodus, which serves to make affix-exodus seem much less exotic. The most obvious parallel is, of course, with affix-genesis, as the two are essentially mirror images of each other; each involves a change in the typing of a linguistic terminal, M-word to Sub-word in the case of affix-genesis and Sub-word to M-word in the case of affix-exodus. Most accounts of this type of change must handle the two phenomena very differently, particularly if the researcher in question subscribes to the Unidirectionality Hypothesis; we saw this, for instance, in the discussion of Kiparsky's work in 8.2.

Under the theory espoused here, this is unnecessary. Any difference in the relative frequency of these types of change³⁰¹ must therefore be attributed to differences of another kind. In particular, if it is the case that learners rely heavily on surface forms when arriving at their analysis, with a bias towards equating phonological words and morphosyntactic words, then affix-exodus will be rarer simply because affixes tend to be unstressed and often interact with close phonological processes. Significant grammatical changes elsewhere in the language, such as sound changes, can result in data that are more ambiguous, as we saw in Estonian and Saami. English *ish* bore contrastive stress, which may have tricked learners into assuming it was an independent word. There is nothing unnatural about affix-exodus; it simply may be the case that the type of ambiguity leading to morphosyntactic innovations is more likely to arise when the conservative grammar is the grammar with two M-words.

This is not to say, of course, that affix-genesis and affix-exodus are exactly alike except for their directionality; in fact, this is to be expected, given the discussion in the previous paragraph. The available evidence suggests that affix-genesis requires much more specific structural conditions than affix-exodus does; the latter appears to be much less constrained, rather like post-syntactic Local Dislocation. In Chapter Three, I developed the following typology of varieties of affix-genesis.³⁰²

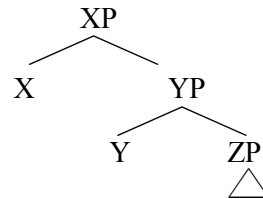
³⁰¹ Cf. Chapter Five for discussion of whether affix-genesis is really as statistically dominant relative to affix-exodus as typically assumed.

³⁰² The possibility of a Type IV – reanalysis of a compounded element as an affix – was alluded to, but requires further investigation. Cf. also below.

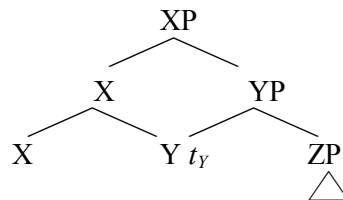
- (9.1a) *Type I*: Acquisition of an additional movement operation;
- (9.1b) *Type II*: Reanalysis of a terminal as an exponent of a different structural position;
- (9.1c) *Type III*: Extension of an M-word boundary to include a former clitic.

In Type I, the new affix does not change its structural position at all; the only change is the addition of a new movement rule. The Amharic compound gerund in 3.2.5 was an example of Type I, which is probably typical of verbal affixes in general. The relevant structures are repeated here for convenience.

(9.2a) *P-speakers' Grammar*

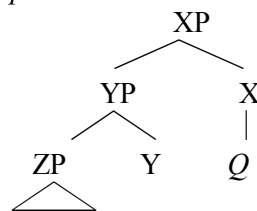


(9.2b) *Innovators' Grammar*

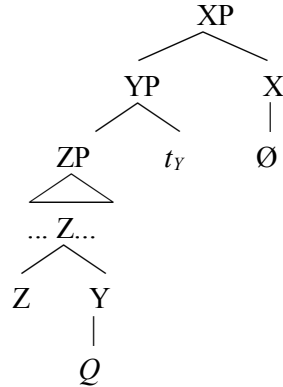


Type II differs from Type I in that the grammar does not acquire an additional movement rule. Instead, the innovator analyses a terminal formally constituting an independent M-word as belonging to an adjacent M-word instead, as shown in (9.3). Type II is probably particularly characteristic of adpositions becoming case affixes, as in the Oscan, Persian, and Armenian case studies from 3.2.1–3.

(9.3a) *P-speakers' Grammar*



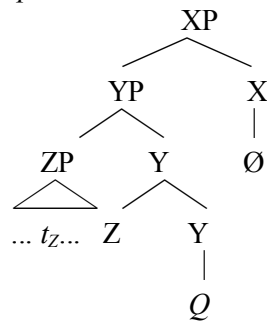
(9.3b) *Innovators' Grammar*



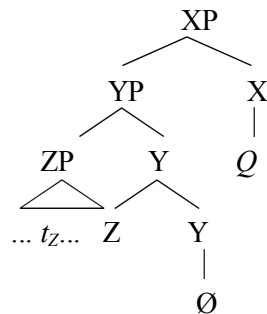
Type III affix-genesis, exemplified by Piattino (cf. 3.2.4), is more structurally diverse than the other two cases, because it involves the incorporation of a clitic into an M-word and the syntax of clitics is notoriously variable across languages. My suspicion is that Type III is most likely to occur with agreement markers, although further investigation is necessary to confirm this.

As we saw in Chapter Five, it is not possible to articulate a typology for affix-exodus with the same facility as we have done for affix-genesis; simply reversing the descriptions, for example, does not yield productive results. The single exception is so-called “Type B” affix-exodus, which is precisely structurally parallel to Type II affix-genesis. With Type B affix-exodus, a Sub-word previously analysed as part of an M-word is taken to occupy a structural position outside the M-word; the relevant structures are given in (9.4).

(9.4a) *P-speakers' Grammar*



(9.4b) *Innovators' Grammar*



Thus, while in many instances the parallels between affix-genesis and affix-exodus are not specifically structural, some varieties of affix-genesis and affix-exodus are in fact structurally mirror images of each other.

The phenomena discussed in Chapters Six and Seven also have features in common with affix-exodus. Morphological re-cutting, for instance, is like affix-exodus (and affix-genesis) in that it involves a change in the location of an M-word boundary (although not exclusively in the case of morphological re-cutting; cf. discussion below). The difference is the level of structure at which the respective changes occur. Morphological re-cutting is an entirely superficial process operating on the phonological level; the boundary is simply placed between a different pair of segments by the innovators. Affix-exodus and affix-genesis are structural changes, with the boundary placed between a different pair of *Sub-words* rather than a different pair of phonological segments. In affix-exodus, the M-word boundary is placed so as to exclude a Sub-word previously included; in affix-genesis, an additional Sub-word is included.

Complex head disintegration (CHD), on the other hand, is more similar to affix-exodus at the structural level: both involve the introduction of two M-words in an environment where previously one M-word was used. Here the chief difference is in the identity of the respective pieces. No novel M-words are created through CHD; the language simply makes use of an existing construction with existing M-words. In affix-exodus, on the other hand, a novel M-word is created (by definition), and both the newly created M-word and the now-slightly-shorter original M-word are obviously altered from what they were in previous generations. Further investigation and more data on CHD will be required before we can identify the degree to which the structural environments in which CHD and affix-exodus occur are similar; but for our present purposes, this is sufficient.

The last member of the afore-mentioned diachronic typology discussed in this dissertation is affix migration. Affix migration bears more structural similarity to both affix-exodus and affix-genesis than the other two phenomena; this is to be expected, given that DM predicts similar phenomena to occur both above and below the level of the M-word. Specifically, all three types of change involve a change in the learner's perception of a Sub-word; the chief difference between affix migration and the others is that the former does not involve a change in typing for the relevant Sub-word; the function and/or structural position of the Sub-word changes, but the Sub-word remains a Sub-word. No directionality is inherent in the description of affix migration; the new structural position for the affix may be either higher or lower in the tree.

(9.5) summarises the discussion from the previous pages.

- (9.5a) Affix-genesis and affix-exodus both involve a change in the typing of a linguistic terminal.
- (9.5b) Morphological re-cutting and affix-exodus both can involve a change in the location of an M-word boundary.
- (9.5c) CHD and affix-exodus both result in the use of two M-words in a context where previously one M-word was used.
- (9.5d) Affix migration and affix-exodus both involve a change in the learner's perception of the structural position of a Sub-word.

It is worth pointing out that (9.5b) is an over-generalisation, in that morphological re-cutting can also involve a change in the location of a Sub-word boundary. This doesn't make its resemblance to affix-exodus any less apparent, however. The typology elaborated here is intended to be descriptive rather than theoretical, capturing in general terms various types of attested morphosyntactic change. It can even be the case that different phenomena blur into each other a little bit. We saw this in Chapter Seven in particular, with the complex pronominals, which were the product of Type III affix-genesis and (in many cases) morphological re-cutting.

I stated above that this typology is useful in various ways, one of which is setting affix-exodus in context. The second reason the typology is useful is that it allows us to get a better perspective on the kinds of mistakes learners make. This is why I have stressed that the

typology is descriptive; the typology itself is not a theoretical object, but it can serve as a tool to allow us to elaborate a theory of morphosyntactic change: we can use it to extract some common properties of learner errors based on the various types of change we have discussed. This is summarised in (9.6).

- (9.6a) Learners may put morpheme boundaries in an innovative place.
- (9.6b) Learners may interpret a morpheme boundary as being of a different type than it was in previous generations (M-word to Sub-word or vice versa).
- (9.6c) Learners may interpret a morpheme as an exponent of a different structural position than it was previously – particularly if there are null exponents involved.
- (9.6d) Learners “like” to interpret surface affixation as structural affixation, possibly because they have a bias towards equating morphosyntactic words with phonological words.

(9.6a) is a characteristic feature of morphological re-cutting; (9.6b) of affix-genesis and affix-exodus; (9.6d) of affix-genesis. (9.6c) is probably the most general type of morphosyntactic change, in that it is implicated in what I have been calling affix-genesis, affix-exodus, and affix migration. Articulating the changes in this way cuts across inherent notions of directionality and the nature of the surface result and places the emphasis instead on the analytic act of the learner. This is as it should be: if we are to unravel the whens and wheres and whys of morphosyntactic change, we must do it from the learner’s perspective.

This dissertation has approached the problem from the perspective of linguistic structure. I have identified various structural configurations in which change can occur, various corroborating factors that may lead the learner towards one analysis over another, and several types of mistakes that learners are likely to make. But there is another aspect to the problem that is independent of structure – indeed, perhaps independent of linguistics entirely – and that is the role of general cognition. At various intervals in the preceding chapters, I have alluded to possible extra-linguistic factors in morphosyntactic change; if we are to truly understand what happens when learners produce innovative structures, these need to be further developed. In the following section I will suggest some avenues for future investigation that I believe to be promising in this regard.

9.2 Directions for Future Investigation

Cognitive areas of potential interest to language change are of primary interest here, and are presented in 9.2.1. I also suggest some additional linguistic topics that could prove enlightening in 9.2.2.

9.2.1 Causes and Consequences of Acquisition Error

The increasing level of sophistication attained by modern formal frameworks has enabled me to make very precise claims about the structural aspects of morphosyntactic change. However, my account still includes a few of the ever-present black boxes that pose perpetual challenges to any work in diachronic linguistics. The largest box is, of course, the actuation problem: why does a particular change occur when it occurs, rather than at another time? Why do other potential changes never happen at all? One of the missing steps in Kiparsky’s framework (cf. 8.2) is the point at which the data become sufficiently ambiguous that the learner makes an innovative analysis; and this is a problem of mine as well. I have identified various preconditions to change, of various types, but I have nothing to say about the specific point at which the data become completely ambiguous.

One of the reasons that answers to these (and related) questions remain stubbornly elusive is that it is very difficult, perhaps impossible, to test them empirically. The locus of change is learners, but experts in child language acquisition are interested in how children get the data *right*, rather than how they get it wrong – and even if this were not the case, how many children would be required in order to find any incipient changes? Children do make errors; not all errors become bona fide linguistic changes. The problem is particularly thorny in areas of morphology and syntax; phonetic changes are somewhat easier to detect because they can be measured more directly. However, even with phonetic change, typically what we uncover are changes in progress, rather than changes literally just beginning. Ideally, we should aim to test theories about language change directly, rather than extrapolating from records of changes that have already taken place; unfortunately, there is no clear way of doing this.

My suspicion is that the problems we are dealing with here are cognitive rather than linguistic. That is, our understanding of the nature and results of linguistic change has become sophisticated along with our synchronic methodology; what is holding us back is a lack of understanding of the cognitive factors underlying the linguistics. We may have an innate capacity for linguistic processing (it would be difficult to understand how children acquire language as quickly and effectively as they do without such an endowment), but while this capacity may be discrete, it seems highly unlikely that it is completely divorced from cognition in all respects. If we can identify general cognitive principles that underlie or supplement our linguistic capacity,³⁰³ we may be able to unravel some of the missing stages in our understanding of linguistic change. In other words, my hypothesis is that the intractable problems of historical linguistics remain intractable in part because sufficient attention has not been paid to more general, extra-linguistic cognitive factors that may be involved.

Much of the work relating cognition to linguistic change has dealt with acquisition; cf. e.g. Yang (2002). I have dealt with questions of learnability through the dissertation, often arguing that, in many instances, the innovative analysis is minimally different from the conservative analysis and may output data that are almost impossible to distinguish from the earlier grammar without very minute scrutiny. The domain over which these changes occur is typically quite small, often involving adjacency relations. Because of this, grammaticalization theorists often take it for granted, so much so that they do not feel the need to fill in the intervening steps: the objects are close, they slip into each other, end of story. This is in fact a massive oversimplification, as even the “simple” case studies in Chapter Three must have illustrated; even so, given a generative model of reanalysis and transmission, these more local cases *are* still fairly trivial.

In (9.6) above, I provided a list of the types of errors learners make with regard to morphology, and almost all of them involve morpheme boundaries in some way. Generally speaking, it is correct to say that morphosyntactic change typically involves morpheme boundaries in some way or other. Although further investigation would be desirable, we can probably ascribe this fact to categorial perception. Breaking up what is essentially a continuum into discrete pieces is a notoriously hopeless task, and frequently artificial. Anyone who has ever taken a page of paint samples and attempted to draw boundaries between different colours will know that, in the end, there is a certain degree of arbitrariness to the task of delineating green from blue and blue from purple: the task is most difficult at the boundaries. One of the very first tasks undertaken in language acquisition is similar in nature, and similarly difficult: one must determine the phonemes of one’s language. Then one can start breaking up a continuous string of sounds into chunks of meaning. Place the boundaries in the “wrong” place, or take them to be the “wrong” kind – in other words, do it

³⁰³ I subscribe to the position that the grammar should be kept as simple as possible, and everything that could potentially be connected to extra-linguistic cognition rather than kept in a separate linguistic component should be removed from the grammar.

differently from the way your elders did it – and the result is an innovation which may well be as consistent with the available data as the conservative target is.³⁰⁴

But it must be emphasised that not all linguistic changes can be reduced to close, trivial relationships between terminals that could be explained via reference to categorial perception. In particular, syntactic change takes place over much larger, non-local objects: the difference between a grammar with *v*-to-T movement and a grammar without it is *huge*, both in its scope within the grammar and in the respective output by speakers. Although the focus of this dissertation has been on terminals rather than on larger structures, we have seen examples of changes that do involve larger objects: affix-exodus and complex head disintegration (CHD; cf. Chapter Six). The latter, especially as exemplified in the form of English *do*-support, is a particularly good example of changes involving bigger syntactic objects. CHD has nothing at all to do with linearity; the analytic construction that steps in to pick up the slack from the synthetic construction may involve objects at quite some distance from each other. Moreover, when CHD occurs, it leaves vast chunks of the input data completely incompatible with the learner's analysis of it.

In fact, on a smaller scale, the same is true in some instances of affix migration. Speakers who relocate a case suffix to the outer periphery of a complex pronominal, or re-cut a new pronominal form entirely, are *not* producing objects consistent with the input data.

Syntactic changes affecting very large objects occur; there is far too much empirical evidence for anything else to be true. Therefore, it is inescapable that sometimes language learners produce innovations that are completely incompatible with the available data – and not in a small way that might be undetectable to all but a careful linguist. No: language learners sometimes come up with *and maintain* novel analyses that *flagrantly disregard* some portion of the data, sometimes at a very fundamental level. Yet we know – and I have stressed elsewhere in this dissertation – that children acquiring their native language are dealing with notoriously terrible data, and therefore have motivation to take it seriously. How is this possible?

The traditional answer to this question is that language learners seek to optimise their grammars. Hale (1973:405) suggests that acquisition is 'not a process which produces a faithful copy of some previous standard' but rather 'involves the introduction of improvements'. Kiparsky's Optimality Theory account of diachronic phenomena is predicated on exactly this notion: we *prefer* to use one word rather than two, we strive for economy and simplicity. Though their theoretical apparatus and the language in which they express it is very different from Kiparsky's, Roberts and Roussou speak of the drive towards simplifying structures and eliminating excess complexity. More traditional historical linguists use different, less formal language to express essentially the same notion, and functionalists do likewise. Haspelmath (1993) even suggests that there is an inherent "conservativity" principle in language change, so that speakers ensure that the language moves in the right direction but not so quickly as to impede communication between different generations.

All of this sounds reasonable in principle, but what does it mean to make a grammar "simpler" or "more economical"? Simpler or more economical in what way? It is extremely difficult to work out a notion of linguistic simplicity that doesn't come with numerous counterexamples. For Kiparsky, economy is expressed in terms of number of words – but reducing the number of words complicates the morphology, and languages are often seen to "cycle" between synthetic and analytic constructions over the course of their histories, which doesn't make sense if the synthetic forms are optimal. Wouldn't we predict that once a language has an "optimal" synthetic form, it will keep it in preference to an analytic form?³⁰⁵

³⁰⁴ *This*, at least, is something that it may be possible to test experimentally, although constructing such an experiment would be quite difficult, as it would probably entail constructing an artificial language and asking subjects (ideally both adults and children) to identify "words".

³⁰⁵ Kiparsky's model also predicts suppletion to be optimal, because it produces forms with fewer morphemes. This may be simplicity in the sense that there are fewer objects to count, but it would

A grammar that is very simple in some respects may be more complicated in others. Meanwhile, the languages of the world are extremely diverse in almost every respect; do we really want to say that some are more optimal than others? All of them seem to function acceptably. Furthermore, all of these notions are based on the perspective of the trained linguist, *not* the child acquiring language. We can come up with perfectly plausible definitions of economy or simplicity, but are our notions of what constitutes simplicity genuinely consistent with those of a child? How do we know?

Consider Old Irish, which I discussed at considerable length in Chapters Four and Five. Old Irish is a language that makes most of the people who encounter it tremble: its phonology is complex; its nominal case system has a bewildering degree of syncretism; its verbal system has been described to me by trained linguists as “ludicrous” and “hilarious”, thanks to its massive allomorphy; and its syntax is typologically unusual. Modern Irish and Scots Gaelic are very different from their ancestor; one could say that the children got wise and simplified the grammar. However, Modern Irish – which is not exactly free of peculiarities itself – arose neither overnight or easily. Although the Old Irish verbal system did eventually break down, the language was nevertheless successfully acquired, despite its patent absurdities, by generations of children. In fact, it is *precisely those generations of children* who made it what it became in the first place. Old Irish is not a linguistic isolate; it is a scion of the most extensively researched language family on the planet. We know quite a bit about its relatives, and therefore we have a fair estimation of what its earliest ancestor was like; we also have direct if sparse attestation of some of its immediate aunts (e.g. Gaulish). Gaulish was not as insane as Old Irish. There is no evidence that Proto-Indo-European was like Old Irish. There *is* some evidence internal to Old Irish, some of which I discussed in Chapter Four, as to how the language may have acquired its more peculiar attributes – and this evidence suggests an earlier language that was, by most conventional standards, much “simpler” than Old Irish as we know it. What happened? The only plausible answer is that, for some reason, Irish children learned the language that way and it stuck. This was likely facilitated by the fact that the language was spoken on a relatively isolated island with low ethnic diversity, and therefore never had a large number of non-native speakers. Similar arguments can probably be advanced for various isolated Caucasian languages, like Udi and Batsbi.

The point is this: children are actually extremely good at acquiring idiosyncratic linguistic systems that strike adults as unnecessarily perverse. They do introduce innovations along the way, and some of these innovations may appear “simpler” or “more optimal” – but not all of them do. Some of them actually *complicate* the language – or, at least, appear to, from the perspective of an outsider. As linguists, we need to take great care not to allow our adult perspective on language to colour our sense of what might be difficult for a child.

When children introduce linguistic innovations to the speech community, they are not motivated by a desire to improve their language, or to make it simpler.³⁰⁶ Children introduce innovations because, as good at acquiring their native language as they undoubtedly are, *they make mistakes*. And because mistakes are inherently unpredictable, and messy, and not necessarily justifiable, language change is itself inherently unpredictable, and messy, and possibly in conflict with the data available to the children.

This much is known; cf. Yang (2002), with references. The question is, how do we tease out the manner in which children make and, more importantly, retain errors? There are three directions from which to approach the problem which I find promising: cognitive dissonance, microparametric variation, and game theory.

seem to place a higher demand on memory – and indeed, while most languages happily *tolerate* suppletion, they do not *embrace* it.

³⁰⁶ Adults acquiring a second language or dialect, however, are another matter; the mistakes adults make are probably more likely to be in line with what adult linguists perceive as simplification. Cf. Labov (2007), Preston (2008), Dinkin (2009, to appear).

One of the chief puzzles in diachronic linguistics is the retention of errors. Children *are* remarkably good at learning their language, and although they initially make a number of mistakes, they ordinarily manage to correct them. Why, if children are generally good at fixing their own mistakes, would they fail to correct a mistake clearly at odds with a large section of the available data? It is easier to understand how they might fail to correct subtle mistakes, like some cases of affix-genesis, but others, like those leading to CHD or overt affix migration, are not subtle. My current hypothesis is that failure to correct significant mistakes is connected to cognitive dissonance.³⁰⁷

- (9.7) *Cognitive dissonance*: an emotional state set up when two simultaneously held attitudes or cognitions are inconsistent or when there is a conflict between belief and overt behaviour. The resolution of the conflict is assumed to serve as a basis for attitude change, in that belief patterns are generally modified so as to be consistent with behaviour.

Cognitive dissonance, introduced by Festinger (1957), is a well-known concept in social psychology. The intuition behind it is simple: people do not like to admit that they have made the wrong choice, whether the choice is of toasters, romantic partners, or – as in the relevant case – grammatical analyses. Therefore, once we have made a choice, we are very good at continuing to justify the choice to ourselves. Most of us carry around long-cherished beliefs about the world, formed in childhood, which we must struggle to discard even after determining that the beliefs are invalid or even damaging. As researchers, we sometimes fall into the trap of needing to choose, at random, one of two equally possible derivations to contribute to the main point of our argument, and then forgetting subsequently that we had chosen that option at random and that, at the time, nothing was riding on it. This is a very human thing to do.

Researchers have hypothesised that this choice-induced conflict, and the resulting dissonance reduction, may be most likely to occur when the conflict poses a threat to a person's private sense of the self as rational and competent (Steele 1988), the sense of the self as publicly recognised as rational and decent (Kitayama et al 2004, Tedeschi and Reiss 1981), or both.

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Since Festinger first introduced the concept, a number of experiments have been run, with interesting and statistically significant results. The classic experiments involves having subjects make a choice between two equally attractive objects. In one of the earliest classic studies, Brehm (1956)³⁰⁸ had (female) subjects rate an assortment of objects in terms of their desirability, choose between two of the objects that had received similar ratings, and then rate the objects again. He found that in the subsequent ratings, the chosen objects were given higher scores than previously, while the unchosen objects were given lower scores. Similar results have been found with amnesiac patients who did not even remember their previous choice (Lieberman et al 2001), young children (Egan et al 2007), and capuchin monkeys (Egan et al 2007).³⁰⁹ Several recent studies (Sharot et al 2009, Qin et al 2011) have discovered interesting neural activity suggesting that the physiological representation of the brain's reaction to a stimulus is altered by the choice.

³⁰⁷ The definition in (9.7) is taken from Reber and Reber (2001).

³⁰⁸ Festinger was Brehm's thesis advisor; Brehm therefore had access to Festinger's work prior to its official publication.

³⁰⁹ The monkeys were asked to choose between chocolate M&Ms of different colours. Before the choice, the monkeys liked all M&Ms equally, but after the choice, they liked the M&Ms they had not chosen less.

In other words, the act of choosing makes the chosen object more desirable: the chooser does not want cause to regret his choice, because that would cause him to feel uncomfortable, so he exerts some effort in justifying his choice to himself.

This is relevant to diachronic linguistic problems in an obvious way: learning a language involves choosing one analysis of the data over others. Once that choice is made, the learner has some investment in it. Whether he has chosen one of two equally possible analyses for genuinely ambiguous data or whether he has interpreted the data incorrectly, or failed to notice all of it, a choice has been made. If the learner later discovers evidence that his initial hypothesis was incorrect, he may discard it and adopt another,³¹⁰ or he may do something more interesting. He may find a way to justify keeping the mistake, in spite of the evidence against it. He may hold on to his initial analysis, but also acquire the standardly accepted analysis: this would introduce variation in the form of competing grammars.³¹¹ Or he might try to incorporate the incompatible data within his original hypothesis however he can, even if the result is somewhat contrived.

As I have indicated elsewhere in this dissertation, the latter is my current working hypothesis for the source of post-syntactic operations like Lowering and Local Dislocation. To my knowledge, no serious diachronic study concentrating on post-syntactic operations has yet been undertaken, but the problem is an interesting one because of its potential to illuminate our understanding of grammar specifically, not merely its diachronic aspects. Post-syntactic operations are used to handle cases of morphosyntactic mismatch; many linguists dislike them because they have an *ad hoc* flavour to them. My contention is that the rather *ad hoc* nature of post-syntactic operations is actually a point in favour of their existence, because it reflects their ultimate origin as last-ditch efforts by learners to salvage an analysis that has turned out not to account for all of the input data.

There is reason to suspect, therefore, that at least some acquisition errors are retained into the adult grammar simply because this is the way humans behave. Clearly, this is an area that requires further investigation. However, not all idiosyncratic linguistic behaviours become bona fide linguistic changes. In addition, it is not clear whether cognitive dissonance could be considered the *sole* factor behind retained errors – what if the learner never notices he has made an error at all? This is where investigation into micro-parametric variation is warranted.

- (9.8) *Micro-parametric variation*: a locus of grammatical variation between individuals that is not correlated with significant social features of the speech community and is not connected to individuals' sociolinguistic competence

The intuition behind micro-parametric variation is that not all individuals within a single speech community have exactly the same grammar. There may be minute, barely detectable sources of variation from individual to individual, possibly within the same family. These do not reflect sociolinguistically significant factors (e.g. class or style) the way the use of other variables might. Instead, these points of variation are caused by different analyses by individuals of the same input data.

³¹⁰ As the offspring of one parent with the *pin/pen* merger and one parent without, I formulated the initial hypothesis that a distinction between [ɛ] and [ɪ] existed in this environment, but was optional. When I was six years old, an incident at school revealed to me that the distinction was actually obligatory (in the part of the country where I lived). I clearly remember thinking to myself, 'Huh, I guess my mother was wrong,' and have maintained the distinction ever since. Note that I was able to avoid dissonance (and thus change my hypothesis without discomfort) by ascribing the mistake to my mother rather than to myself. My brother avoided all of this by simply acquiring the merger.

³¹¹ Cf. Chapter Seven, footnote 270, where I voiced my suspicion that innovators who produced the case-external Set III complex pronominals probably acquired the conservative forms secondarily and somewhat artificially.

Legitimate examples of micro-parametric variation would be of great theoretical interest on multiple counts. Such examples would indicate that linguistic data can be legitimately ambiguous, such that multiple analyses are not only available, but selected by speakers. Whether one analysis is “correct” at some level may be undeterminable; it could be the case that the data equally support both analyses, or simply that the data are misleading in some respects, so that learners often make the same few mistakes.

It is quite likely that some instances of micro-parametric variation are cases of the former and others of the latter, and in fact both lead to intriguing possibilities. In the former case, we would have evidence that the same data set may be interpreted differently by different individuals; in other words, the data may be legitimately ambiguous. The question of why certain individuals select one analysis and others the other (and so forth; there may be more than two options) may not be answerable: do different speakers have different biases that lead them to favour one choice, or are all minute decisions made essentially at random? Either way, this could be very illuminating to those who study diachronic linguistics.

The second possibility – that there is a “correct” answer, but that learners often make the same mistakes – is also interesting, because it provides further evidence for the retention of errors into adult language. Of course, if sufficient numbers of learners make the same mistake, the input data available to new learners is more likely to be consistent with the “error” rather than the original grammar, which could be a source of incipient change.

Dinkin (2006) identified one source of linguistic variation in English which appears to be micro-parametric: so-called subject-control *promise*. The standard interpretation of sentences like (9.9a) is (9.9b), paraphrased in (9.9c); this distinguishes *promise* from other verbs like *persuade* (9.10).

- (9.9a) John promised me to help Mary.
- (9.9b) John₁ promised me₂ [*e*₁ to help Mary].
- (9.9c) John promised me that *he* would help Mary.

- (9.10a) John persuaded me to help Mary.
- (9.10b) John₁ persuaded me₂ [*e*₂ to help Mary].
- (9.10c) John promised me that *I* should help Mary.

However, these judgements are not shared by all speakers. Some, including Dinkin himself, find subject-control *promise* ungrammatical, and can only interpret sentences like (9.9a) as showing an ECM-esque interpretation, as in (9.11).

- (9.11a) John promised me to help Mary.
- (9.11b) John₁ promised [me to help Mary].
- (9.11c) John promised (someone) that *I* would help Mary.

Rather than elicit judgements directly, Dinkin embedded a sentence of the type *promise* + DP + infinitive in a paragraph and then asked subjects questions to reveal their interpretation of the target sentence. He found that forty-seven of his sixty-three adult subjects (75%) had the subject-control interpretation, and that women were statistically more likely than men to do so. However, he argues that the difference does *not* seem to be a sociolinguistic variable, despite the gender split.

There are other cases of possible micro-parametric variation. These include the phonemic assignment of “i” in the morpheme *-ing* (etymologically [ɪ], but assigned to the phoneme [i] by some speakers, including myself); pronunciation or lack thereof of the “I” in words such as *almond*, *alms*, *calm*, and *palm* (variation has been detected in two pairs of siblings); and the pronunciation of *eighth* as [e'tθ] or as [e'θ]. Dinkin and I are currently

planning a project with the intention of examining these variables, as well as re-examining *promise*, and perhaps some additional highly specific syntactic constructions.

The obvious question to ask in response here is: why, if there are all these hypothetical sources of variation, do we not notice it? How can even close friends or family members have such different grammars? One possible answer is that we *do* notice it, somewhat, but do not pay much attention to it; we *are* generally aware that our friends and acquaintances have some speech patterns characteristic to them. But the latter typically entail phenomena at a rather superficial level – catchphrases and the like – rather than deeper structural features like the sub-categorisation of *promise* or the phonemic assignment of specific segments.

Part of the answer here, in fact, is probably that people in general are rather less good at communicating than we think we are, and pay rather less attention to others than we think we do. Most of us can probably recall experiences in which we have said, or had said to us, something to the effect of ‘But I *told* you X! Weren’t you listening?’, with a response along the lines of ‘I thought you meant Y!’ Some miscommunications can last, uncorrected, for an indefinite period of time. Human communication works reasonably well, most of the time, but it isn’t perfect, because we are fallible, rather egocentric creatures who do not listen as well as we think we do. An interesting recent study at MIT (Savitsky et al 2011) discovered that people tend to overestimate the degree to which they are understood by their close friends and family; they also found that people are more likely to make egocentric errors (e.g. reaching for an object only they can see) when they are following the direction of a friend rather than the direction of a stranger. We thus have empirical evidence that human communication is imperfect.

Human communication is a social tool, and like any aspect of social behaviour, it is constrained by conventions. Sociolinguistic research has made a great deal of progress over the last half-century in identifying the ways in which language is used as a social marker, and how linguistic innovations spread through the speech community, particularly at the quantificational level; however, sociolinguists tend to be less concerned with how the innovative conventions arise in the first place, just as historical linguists tend to be less concerned with how particular innovations came to spread. There is a gap in the picture here, in the general location of where linguistic conventions come from in the first place.

Recently, some linguists have begun attempting to link linguistics to game theory (cf. von Neumann and Morgenstern 1944).³¹² Game theory is already being used in a number of social sciences (including political science and social psychology), formal sciences (e.g. logic, computer science), and biology (Maynard Smith 1982), particularly evolutionary biology. The goal is that applying game theory to linguistics will allow for a better understanding of how people use language to communicate, as well as how linguistic conventions evolve (cf. Clark 2007, Clark and Parikh 2007, Clark in press). Thus, connecting the current research into game-theoretic linguistics with a thorough knowledge of diachronic phenomena seems an avenue that could prove very fruitful.

9.2.2 Other Avenues

In addition to the questions concerning cognition and learnability mentioned in the previous sub-section, there are several other linguistic directions of investigation which have not been discussed in this dissertation, but which could be extremely important in furtherance of our understanding. The most obvious direction, of course, involves data: we need more examples of the less-well-studied phenomena discussed here, particularly CHD and affix migration, to give us a better idea of the parameters of these types of change. In addition, some specific case studies warrant further investigation, ideally involving a rigorous corpus

³¹² An excellent and very readable introduction to game theory is Binmore (2007). Sigmund (1993) is a good introduction to evolutionary game theory.

study; examples include the Irish verbal and pronominal systems, the Germanic group genitive, and English degree forms. But there are also more abstract areas of linguistic research that could prove illuminating given further examination, and in this sub-section I will discuss a few of them briefly. 9.2.2.1 is concerned with directional asymmetry, and 9.2.2.2 with specific sub-categories of morphosyntactic change not otherwise dealt with in this dissertation.

9.2.2.1 *Directional Asymmetry*

It was noticed as early as Sapir (1921) that suffixes are more common, cross-linguistically, than prefixes, with the interesting twist that this asymmetry is more prominent with nouns than with verbs. The proposition that diachronic suffix-genesis is somehow “more natural” than diachronic prefix-genesis has not been addressed in this dissertation up until this point, but should be addressed in further study, as it could tell us something very important about grammar in general and morphosyntactic change in particular.

Explanations for the suffixing bias have come from a wide range of linguistic sub-domains, including diachrony, extralinguistic processing, syntax, and relevance theory. While all of these domains do appear to be directly relevant to the suffixing asymmetry, most of the previous accounts are very modular; to my knowledge, only Hall (1988) has attempted to bring together insights from more than one domain.

Part of the issue, it seems, is phrasing the question correctly. Since morphemes become prefixes or suffixes depending on whether they preceded or followed their eventual stem, the problem of affixal directional asymmetry is dynamic. Therefore the usual manner in which questions of affixal asymmetry are formed – ‘Why are there more suffixes than prefixes?’ – is misleading; there are more suffixes than prefixes because learners are more likely to analyse post-stem M-words as Sub-words than they are pre-stem M-words. A more accurate formulation of the question, then, would be ‘Why are post-stem M-words more likely to be analysed as Sub-words than pre-stem M-words?’ My suspicion is that this prefix/suffix asymmetry stems from interactions between synchronic syntactic structures and underlying cognitive bias.

As discussed in Chapter Three, there are several preconditions on the reanalysis of M-words as affixes, including allowance for linear adjacency between the proto-stem and proto-affix and a particular structural relationship between affix and stem. The nature of the underlying clausal syntax in a given language determines the relationship between the various elements in it, and this in turn determines which, if any, might meet the structural preconditions for reanalysis. This is why languages with different word orders and underlying clausal syntactic properties tend to have different morphemic orders. For example, in verb-medial languages without verb raising, the verb is structurally lower than any independent words filling inflectional positions except the direct object; therefore, any affix-genesis affecting verbs in these is likely to be prefix-genesis. By contrast, in verb-medial languages with verb raising, the verb is structurally higher than the inflectional projections, and therefore any affix-genesis affecting verbs is likely to be suffix-genesis.

Affixes develop out of particular syntactic configurations, with specific structural relations necessary, but syntax, like morphology, is prone to change, and the structural relationships present at one stage of the language may no longer be present at the next. However, as a general rule, changes in clausal syntax *do not necessarily destroy extant affixal relationships*. Evidence of this point was seen in the discussion of English *do*-support in Chapter Six, where it was noted that the English past tense suffixes did not disappear with the loss of verb-raising, despite acquiring additional restrictions on their usage. Why this is so, and why the existence of the past tense suffix did not give speakers sufficient motivation to retain verb raising, is not understood; nor is the relationship between these changes and the loss of person/number agreement. The fact remains, however, that speakers often seem to be reluctant to discard affixes out of hand, and will keep them even if doing so requires

somewhat baroque syntax. This is one of the senses in which Givón's famous slogan ('Today's morphology is yesterday's syntax') is true; it also means that correlations between affix order and synchronic word order can be somewhat problematic.

In a sense it is historical changes that define the problem facing us; as discussed above, what we are really trying to explain is the difference in rate of affix-genesis between postposed and preposed M-words rather than the difference in number of prefixes and suffixes per se. The larger problem is the greater tendency for speakers to analyse post-stem M-words as affixes while leaving pre-stem material analysed as free, which is where processing enters the picture. If we take two languages whose syntactic structures are perfect mirrors of each other, completely identical in every way apart from their direction, and we still find a greater tendency towards suffix-genesis and away from prefix-genesis, it seems likely that some kind of processing factors are interfering. Left/right asymmetries do exist.

The discussion so far has centred on verbal affixes, which is typical of research on this subject; however, nouns are equally important here, particularly since the asymmetry with case prefixes is so much more striking than the asymmetries with verbal affixes. Nouns differ from verbs in that the movement of nouns within a clause is not relevant to the positioning of affixes: what *is* relevant is the relationship of nouns to adpositions, since adpositions generate case markers. Postpositions yield case suffixes with great frequency, while case prefixes seem to exist robustly only in certain Australian languages, most notably Mangarayi (as described by Merlan 1982), though somewhat peripheral and/or ephemeral examples have been cited from elsewhere, such as the Classical Armenian example discussed in Chapter Three.

However, it must be pointed out that there is often contention as to whether a particular form is better analysed as a preposition or as a case prefix (for discussion cf. Kahr 1976). The Hebrew accusative case marker *'et*, for instance, is preposed but is not usually considered a prefix; one of the first concerns of potential research in this area should be to examine this and similar examples in order to determine whether the asymmetry is as pronounced as reported. Assuming this is feasible, the realm of directional asymmetry is yet another area that has potential to illuminate potential areas of learner bias.

9.2.2.2 *Additional Types of Change*

There are at least two other areas of morphosyntactic change which warrant further attention, either because they constitute an extension of the discussion in this dissertation or because they have not yet been dealt with under the theoretical assumptions made here and would benefit from an examination in these terms. Each of these is likely to lead to a better understanding of linguistic change.

In my discussion of Roberts and Roussou's approach to grammaticalization in 8.1, I mentioned that these authors are less concerned with changes in terminal status and more concerned with changes affecting the function or position of M-words, but not their status as M-word or Sub-word. Thus, for instance, they discuss the development of lexical verbs into auxiliaries, or of demonstratives into pronominals. Although we are concentrating on different objects, there is a clear intersection between the phenomena they discuss and the phenomena I have dealt with here. Roberts and Roussou are essentially focussing on the M-word analogue to affix migration, which could be viewed as learners mistakenly ascribing an innovative structural position to an M-word that does not involve a change in its status as an M-word.

This approach to the problem is rooted more firmly in structure than most previous approaches have been. As mentioned in 8.1, Roberts and Roussou, like many researchers, define the domain of research as 'the creation of new functional material, either through the reanalysis of existing functional material or through the reanalysis of lexical material.' But this is vague. It is particularly vague when we consider that the divide between "lexical

material” and “functional material” has yet to be successfully delineated. A favourite case of grammaticalization theorists is the development of future auxiliaries from verbs meaning ‘to go’; but is this a change of “lexical” material to functional material, or functional to functional? ‘Go’ is, in many theories including DM, often analysed as a *v* head rather than a Root. Furthermore, as I discussed in Chapter Three in the context of M-words becoming Sub-words, a necessary precondition for structural changes is semantic change; but semantic change need not necessarily produce structural change. One of the difficulties one encounters while researching in this domain is the temptation to conflate the semantic and structural changes; since there is no obvious change in terminal type, it is harder to say when the secondary structural changes occur. This needs to be investigated further, with structural and semantic change viewed as completely separate objects requiring distinct explanations.

The other type of linguistic change that bears reconsideration in the terms of this dissertation is analogy, as defined in (9.12).³¹³ Analogy is somewhat notorious among traditional historical linguists, in that it is extremely difficult to define, and is often invoked as a sort of last-ditch explanation when other, more rigorous explanations fail.

(9.12) *Analogy*: a type of linguistic change whereby the phonological (or morphological) structure of morphologically, syntactically, and/or semantically related forms becomes more similar without recourse to regular sound change

The definition in (9.12) is clearly quite vague, and indeed, the kinds of changes that have been classified together as analogy are a diverse group. Morphological re-cuttings (cf. Chapter Six) are generally considered a type of analogy, as are e.g. folk etymology and hypercorrection. These are classified as “unsystematic” analogy by Hock (1991), as opposed to the “systematic” varieties listed in (9.13).³¹⁴ Despite the effort to identify different sub-types of analogy, individual cases often blur together.

(9.13a) *Levelling*: the complete or partial elimination of morphophonemic/allomorphic alternations between forms built to the same Root. [E.g. the generalisation of [z] as the Root-final consonant in the English verb *choose*, which in Old English varied between [s], [z], and [r].]

(9.13b) *Proportional Analogy*: a pattern of morphological relationship between two given forms is generalised to other environments to which it is not native. [E.g. American English *dove* as the past tense of *dive*, on the model of *drive* :: *drove*, replacing the older *dived*.]

Analogy is, by definition, an unpredictable type of change, as opposed to sound change, and traditional historical linguists did not attempt to predict when it would occur, although they did attempt to identify general tendencies of what would happen if it did. The most notable attempts to formalise analogy come in the form of Kuryłowicz (1947) and Mańczak (1958, 1978). Kuryłowicz formulated six “laws” of analogy, most of them concerning morphology, on the basis of his years of research in the area, while Mańczak concentrated more on phonotactics and based his tendencies (not laws) on a statistical investigation of standard historical grammars of various European languages.

It is no coincidence that Kiparsky (2011) discusses both grammaticalization and analogy together as opposing forces of sorts: these two concepts have a great deal in common. Both are essentially descriptive terms for recurring diachronic phenomena; both often involve morphology; both are notoriously difficult to define precisely and have been used differently by many different linguists. Put more concisely, neither is an explanation, but both are often

³¹³ This definition is adapted from Hock (1991:167).

³¹⁴ Definitions adapted from Hock (1991); examples are also from Hock.

treated as such, in that one encounters analyses that conclude, ‘*X* happened by/because of analogy/grammaticalization’ and stop there.³¹⁵ The primary difference between examples classified as “analogy” and examples classified as “grammaticalization” is that the former are typically surface-oriented, while the latter are structure-oriented.

I have not dealt extensively with analogy in this dissertation, since my primary focus has been on structural concerns and analogy (to the extent that it is a meaningful concept) is essentially a surface-oriented phenomenon. Nevertheless, I *have* already ventured a certain distance into the traditional realm of analogy in this dissertation, with the inclusion of morphological re-cutting and the discussion of complex pronominals in Chapter Seven. In any event, I believe that analogy would benefit from an examination from the same ideological perspective and with the same formal precision as this dissertation has used in tackling a corner of “grammaticalization”. The crucial approach of such an examination would be to look at analogy from the learner’s perspective, with the goal of working out what types of input data are likely to lead learners to produce and persist in innovations of the type traditionally lumped together as “analogy”, and what the nature of the resulting innovations is likely to be. One possible starting point to such a research project would be a reformulation of the laws and tendencies of Kuryłowicz and Mańczak from a generative perspective like that used in this dissertation. In addition to enriching our understanding of language change, such an enterprise could in turn enhance our comprehension of grammar in general.

9.3 Final Remarks

My object in writing this dissertation was to provide a detailed account of the structural aspects of a particular set of morphosyntactic changes, specifically those concerning innovations in the linguistic type of a particular morphosyntactic object. I have argued that a syntactic, piece-based approach to morphology provides a formalism that allows a precise, detailed account of morphosyntactic changes without the addition of an additional diachronic-specific component; this accords well with the learner-centric, non-teleological model of linguistic change that has been advocated by scholars as diverse as the Neogrammarians, Antoine Meillet, and Anthony Kroch.

A substantial amount of work remains to be done; in this final chapter, I have made some suggestions of future avenues of inquiry that I find most promising. Many of these involve theories or research programmes that are not intrinsically linguistic, including cognitive dissonance and game theory, as well as a thorough understanding of acquisition and of historical change. It is my belief that the most intractable problems in historical linguistics can be solved only if we take into account features of general cognition.

The available evidence suggests that linguistic change occurs because of three basic facts about humans: we make mistakes; we don’t like to change our minds once we’ve made a decision, particularly if doing so implies that we are wrong about something; and we don’t pay as much attention to each other as we think we do. We do, however, have some redeeming qualities, and some of these are involved in linguistic change as well, albeit to a lesser extent. Sometimes innovations, such as *-doodle* in *goldendoodle* and *outro* (cf. 6.2),³¹⁶ are purely the result of human creativity. In other words, we are fallible, we are stubborn, and we are egocentric, but sometimes we are also wonderfully creative.

³¹⁵ However, while grammaticalization is taken quite seriously by those who consider it to have explanatory power, analogy is often invoked with a sense of embarrassment.

³¹⁶ And, of course, “bear-shark.”

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