# ABSTRACT

Title of Dissertation: LEXICAL CATEGORIES, (RE)CATEGORIZATION, AND LOCALITY IN MORPHOSYNTAX
Maša Bešlin
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Dissertation Directed by: Professor Maria Polinsky
Department of Linguistics

This dissertation is about the nature of syntactic primitives and principles, their status in the grammar, and their interaction with extra-linguistic cognition.

The dissertation has two parts unified by the common goal of streamlining the syntax by asking whether some of its proposed constructs are dispensable, whether the motivation for their existence can be found syntax-externally, and whether they must be assumed to be part of the initial state of the learner. While I discuss a range of phenomena in a number of languages, core empirical evidence throughout comes from adjectival derivation in Bosnian/Croatian/ Serbian (BCS).

In the first part of the dissertation, I consider the status of lexical categories (LCs) in grammar. I argue that LCs *noun*, *verb*, and *adjective* are purely formal, abstract categories which have a distributional role in the syntaxes of individual languages, but which do not have a one-to-one mapping to any interpretive property. I argue against proposals that attribute universal syntactic or semantic properties to the specific LCs. In addition to discussing relevant data from a variety of languages, I provide two detailed case studies on mixed categories: passive and active participles. I show that all participles in the languages under discussion are in fact deverbal adjectives, in every syntactic position they appear in and regardless of their interpretation. While participles may denote (predicates of) properties or eventuali-

ties, I argue that these different interpretations are not cross-linguistically associated with more or less verbal or adjectival structure. This reinforces the conclusion that a direct one-to-one correspondence between an item's LC and its interpretation does not exist. If correct, this proposal has significant consequences for our understanding of Universal Grammar. If there are no universal syntactic or semantic properties we can attribute to the LCs, then it becomes superfluous to assume that the individual LCs are part of the initial state of the learner. I propose that the cross-linguistic tendencies we observe around LCs may stem from the way non-linguistic knowledge is organized in the mind/brain.

In the second part of the dissertation, I turn my attention to the formal principles that operate on grammatical primitives, asking specifically what kinds of locality constraints are employed by the grammar. While locality has been extensively studied in generative linguistics, the current offering of locality theories is arbitrary, redundant, baroque, and/or empirically inadequate. There are in essence three competing locality theories currently in circulation within the field: Featural Relativized Minimality (FRM), Phase theory as currently understood in the syntax literature (where it is a successor of Subjacency), and Phase theory as understood in the context of Distributed Morphology (DM). Despite recent attempts to devise a single, unified Phase theory which is responsible for both syntax-internal locality and interface locality, I argue on both conceptual and empirical grounds that the unification is unfeasible. In a detailed empirical study of deadjectival derivation in BCS, I show that adjectivization imposes a DM-locality boundary (for allomorphy and morphological tone assignment), but not a 'big syntax'-locality boundary (for punctuated movement paths). Nonetheless, I show that the original inventory of locality principles can be reduced if we assume that (i) syntax-internal locality is regulated by FRM, and (ii) interface locality is regulated by Transfer, a modified version of Phase theory which has no syntax-internal effects. I reinterpret the evidence supporting Phase theory through the lens of FRM and demonstrate that the division of labor in (i)-(ii) not only achieves the right empirical cut, but also offers insight into why the grammar may require two distinct locality principles.

# LEXICAL CATEGORIES, (RE)CATEGORIZATION, AND LOCALITY IN MORPHOSYNTAX

by

Maša Bešlin

Dissertation submitted to the Faculty of the Graduate School of the
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Advisory Committee:

Professor Maria Polinsky, Co-advisor, Chair
Professor David Embick, Co-advisor
Professor Norbert Hornstein
Associate Professor Alexander Williams
Professor Michael Israel, Dean's Representative

"Con el tiempo, estos Mapas Desmesurados no satisficieron y los Colegios de Cartógrafos levantaron un Mapa del Imperio, que tenía el Tamaño del Imperio y coincidía puntualmente con él. Menos Adictas al Estudio de la Cartografía, las Generaciones Siguientes entendieron que ese dilatado Mapa era Inútil y no sin Impiedad lo entregaron a las Inclemencias del Sol y los Inviernos."

–Jorge Luis Borges, *Del rigor en la ciencia*

# Foreword

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Puerto Rico, February 12, 2025

# Table of Contents

# List of Abbreviations

| | |
|---|---|
| 1, 2, 3 | = 1st, 2nd, 3rd person |
| A | = set A (ergative) agreement |
| ABS | = absolutive |
| ADJ | = adjective |
| ACC | = accusative |
| ACD | = Antecedent Contained Deletion |
| ACT | = active |
| AOR | = aorist |
| B | set B (absolutive) agreement |
| BCS | = Bosnian/Croatian/Serbian |
| BVA | = bound variable anaphor |
| C | = complementizer |
| CAUS | = causative |
| CMPR | = comparative |
| COP | = copula |
| DAT | = dative |
| DECL | = declarative |
| DEF | = definite |
| DUR | = durative |
| ERG | = ergative |
| EVID | = evidential |
| EXIST | = existential particle |
| EXT | = extraction |
| F | = feminine |
| FUT | = future |
| GEN | = genitive |
| HAB | = habitual |
| HCR | = hypocoristic |
| IMP | = imperative |
| IMPF | = imperfective |
| IND | = indicative |
| INDF | = indefinite |
| INF | = infinitive |
| INS | = instrumental |
| INTERR | = interrogative |
| LV | = linking vowel |
| M | = masculine |

NEG = negation
NEUT = neuter
NF = non-finite
NOM = nominative
NPST = non-past tense
NS = nonsubject voice
OBLV = oblique voice
PART = participle
PASS = passive
PF = perfective
PL = plural
PoS = Poverty of Stimulus
PREF = prefix
PRES = present tense
PRON = pronoun
PST = past tense
Q = question particle
QR = Quantifier Raising
RECIP = reciprocal
REFL = reflexive
RES = resultative
RN = relational noun
S = subject
SG = singular
SI = secondary imperfective
SMT = Strong Minimalist Thesis
SP = specific
SUBJ = subjunctive
SUP = superlative
SV = subject voice
UG = Universal Grammar
V = verb
VDT = derived transitive verb

# Chapter 1: Setting the stage

This dissertation is about the nature of syntactic primitives and principles, their role in the grammar, and their interaction with extra-linguistic cognition. In particular, the first part of the dissertation considers the status of lexical categories (LCs)–nouns, verbs, and adjectives–in the grammar. I examine the mapping of LCs to meaning, and ask whether LCs have any universal syntactic properties, with the goal of understanding what knowledge about them must be innate. The second part of the dissertation looks at various locality phenomena in the syntax and morphophonology, and argues that Phase theory (Chomsky 2000), which attempts to account for (some) syntactic locality effects, should be dispensed with. (Featural Relativized) Minimality is argued to be the only necessary syntax-internal locality mechanism. As will become clear immediately below, the inquiry in this dissertation is undertaken with a minimalist lens; I take seriously the idea that the rich descriptions of linguistic phenomena arrived at during the Government & Binding era of Generative Grammar need rethinking if progress is to be made in providing genuine explanations for linguistic phenomena in the sense of Chomsky 1993.

## 1.1 Motivation

I follow the generative tradition in taking linguistic theory to be a theory of an individual speaker's knowledge of their language. Part of this knowledge is hypothesized to be innate, formalized as Universal Grammar (UG). The UG hypothesis is based on the observation that children come to have linguistic knowledge that is far too sophisticated to be learned from their linguistic experience, unless this experience is shaped and guided by substantial prior biases and constraints (the poverty of the stimulus (PoS)

argument, Chomsky 1980; see the seminal work of Wexler & Culicover 1980, and Lasnik & Lidz 2016, Lidz 2018 for recent discussion). The idea that there are innate, organism-internal factors constraining language acquisition was highly controversial when it was originally proposed in Chomsky's 1959 critique of Skinner's *Verbal Behavior*; while the hypothesis remains controversial in some circles, it has since been adopted into the mainstream of linguistics and cognitive science. Despite criticism, there has been robust empirical support for the PoS from studies looking at children's input and concluding that aspects of their grammatical knowledge are not straightforwardly supported in experience (see e.g., Legate & Yang 2002, Lidz, Gleitman & Gleitman 2003, Lidz, Waxman & Freedman 2003, Leddon 2006, Becker 2006, 2017, Viau & Lidz 2011, Lidz 2018, i.a.).

Still, the existence of UG is much less debated than its content (see Laka 2009 for discussion). UG is said to consist of two types of universals: substantive and formal (Katz & Postal 1964, Chomsky 1965). The former are "the kinds of units that can be attributed to a natural language" and the latter "the manner of their arrangement and interconnection" (Chomsky 1962:537). Beyond this, little is agreed upon. In this dissertation, I present a research program which aims to streamline linguistic theory by paring down the inventory of grammatical primitives and principles that operate on them, without sacrificing empirical coverage. While the work is done by studying the nature of adult grammars, its focus on cross-linguistic variation and universality has consequences for our understanding of the contents of UG.

The nature of (minimal) linguistic units has been a neglected topic in generative linguistics (Baker 2003), with most researchers tacitly or explicitly adopting views espoused in the earliest known works on language. Words are said to belong to distinct lexical categories (LCs), an observation that goes back at least to the the middle of the first millennium BCE, which saw the publication of Pāṇini's grammar of Sanskrit. Briefly, all languages are typically understood to have at least verbs and nouns, while the universality of adjectives is less well-established. Other LCs have also been proposed, including participles and gerunds.[1] While category membership is determined based on syntactic distribution,

[1]These are usually taken to be LCs only in lexicalist approaches to word formation, where words are built in a pre-

LCs are usually furthermore imbued with a specific meaning component, for example temporality for verbs (Uriagereka 1999, Ramchand 2008, Panagiotidis 2015, echoing work in the functionalist framework). It has been explicitly claimed that UG contains specific LCs like noun and verb (Demirdache & Matthewson 1995), as well as information about both their syntactic (Baker 2003) and semantic (Panagiotidis 2015) properties .

In the first part of the dissertation, I will take a close look at LCs, focusing on their role in the syntax and their mapping to meaning. A survey of the literature on LCs, facts about LCs in familiar and less familiar languages, and my investigation of mixed categories (specifically, active and passive participles) will lead me to the conclusion that LCs have no inherent syntactic or semantic properties, beyond their contrastive role in the syntax (contra Baker 2003, Panagiotidis 2015). Hence, there is no reason to suppose that any knowledge about specific LCs should be a part of UG, despite LCs being a part of the adult grammar in the languages I consider. Instead, categorization can be achieved through combining whatever knowledge enables the child to individuate morphemes from the speech signal and a bias to categorize those morphemes based on their distribution. The fact that all of the world's languages seem to have at least a noun-verb distinction is linked to the argument-predicate format of (linguistic and non-linguistic) thought, but this relationship is argued to be indirect.

In the second part of the dissertation, I turn my attention to formal universals, particularly in the domain of locality. Generative linguistics has focused significant attention on discovering the organizing principles of grammar, with the locality of grammatical operations being one of the most active research areas. Two kinds of locality principles have been argued to operate in the grammar: Minimality ("attract closest") and Phases (cyclic spellout). A close look at adjectives in Bosnian/Croatian/Serbian (BCS) shows that there is an apparent clash between two conceptions of "Phase" which have been argued to regulate the cyclic nature of derivations in syntax and in morphology. Adjectivization in BCS imposes a boundary for locality processes discussed in the Distributed Morphology (DM) literature

syntactic lexicon. On syntactic approaches to word formation, participles and gerund are still a distinct category, but they are usually understood to be derived by adding functional structure to verbs, for example. I discuss some of the issues relevant to participles in chapters 3 and 4.

(specifically, allomorphy and morphological tone assignment), but not a locality boundary in the traditional Chomskyan sense (specifically, for punctuated movement paths). This clash is in tension with the received wisdom from the DM literature, which understands morphological phases to be phases à la Chomsky (see Marantz 2001, 2007). The incompatibility between the two kinds of phases threatens to complicate the theory if two distinct (but similar) locality constraints must be said to operate in the syntax. To resolve the clash, I offer a reinterpretation of all syntax-internal locality effects in terms of Minimality, arguing that Minimality is the only locality principle operative in the syntax (and a good candidate for a formal universal, as suggested for the A-over-A condition in Chomsky 1971).

There are several reasons to undertake the task of streamlining linguistic theory by reducing the number of its primitives and principles; they include general theoretical parsimony, learning considerations, and the issue of connecting the concepts of theoretical linguistics with questions about language evolution and its biological (neural) instantiation. The first reason, often invoked in philosophical and scientific inquiry, comes down to the principle of parsimony (Occam's razor), which instructs us, *ceteris paribus*, to prefer explanations that have the smallest possible number of moving parts. The second reason concerns learnability: The more complex the adult grammar needs to be, the more difficult the learning task becomes for the child, unless the child is pre-equipped with exactly the right (domain-specific) tools to facilitate the learning. In the absence of robust positive evidence for a particular grammatical primitive or principle, the child is faced with a PoS problem, and we must postulate a richer UG. For example, we will see that postulating participles as a distinct category in the grammar would face the child with a PoS problem in acquiring this category distinction; there simply is not enough (relevant) positive evidence in the input (or indeed in the sets of possible and impossible sentences in the languages under discussion) to motivate the learner to posit a new category.

The learning problem (and its consequences for UG) is in direct tension with another reason we may prefer a minimal language-specific cognitive machinery, namely the evolvability of the faculty of language (FL). Simply put, the more complex FL needs to be, the more distinct we are from our primate ancestors (who, by hypothesis, did not have language), and the more difficult it becomes to

explain how language could have evolved.[2] One radical approach to the evolvability problem is the Strong Minimalist Thesis (SMT) which takes the initial state of FL to be comprised of a single operation, Merge (Chomsky 2001 2004, *et seq.*; see Hornstein 2024 for recent discussion). Merge is hypothesized to interact with interface conditions at the conceptual interface (for thought) and the sensory-motor interface (for externalization), as well as with general properties of cognition such as computational efficiency. While this dissertation does not take the SMT as a viable alternative in accounting for all the complexities of human language syntax, it takes from it the spirit that the rich descriptions of linguistic phenomena arrived at during the Government & Binding era of Generative Grammar need rethinking if progress is to be made in studying (the human capacity for) language from a biological perspective. Put another way, the simpler the answer we give to the question what constitutes knowledge of language, the simpler it should be to answer the question how it is learned and put to use, what can and cannot be learned from input, how language is instantiated in the brain, and how it evolved. As mentioned, I will not undertake a serious critique of the SMT here, but it is enough to look, for example, at Svenonius's (2016) "Significant mid-level results of generative linguistics" to realize that the SMT likely does not provide the full picture of what FL is. Qualms about what linguistic theory should and should not account for aside, even the most fervent supporters of SMT would agree that the two issues explored in this dissertation–namely the nature of grammatical primitives and the principle of locality/cyclic derivation—are (1) fundamental to language, and (2) do not fall out in any way from the SMT.[3]

Finally, working with fewer tools forces us to make generalizations that we may not have been

[2]While it is reasonable to hypothesize that the evolution of a simpler FL would be easier to explain (this, indeed, is the driving force behind the Minimalist Program, Chomsky 1993, *et seq.*), it has been pointed out that simplicity at one level of analysis (in the sense of Marr 1982) does not necessarily translate into simplicity at another level (see Johnson 2017, Perfors 2017, Martins & Boeckx 2019). Martins & Boeckx make a stronger point, claiming that: "the formal simplicity of an operation deemed crucial to language *cannot* be conflated with simplicity at the biological level.[emphasis mine]" In fact, the idea that simpler phenotypes result from simpler genotypes is extremely wide-spread in the evolutionary theory literature; see Rubin 2016 for discussion. As the paper discusses, there are certain issues with this assumption, but that does not take away from its heuristic utility. As should be obvious, the potential issues arise not only for FL, but for physical and cognitive traits and their genetic underpinnings more generally.

[3]Hornstein (2024:211-2) explicitly notes that his version of the SMT, the Extended Merge Hypothesis (EMH), has "remained largely mute concerning the locality properties also characteristic of natural language grammars [...] the EMH says nothing as to why island conditions exist, or why unbounded dependencies of the A' variety are governed by them. Nothing. Really. Nada, zip, gar nicht, bubkis, zero! As with minimality effects, why island effects exist is outside the purview of the EMH."

able to make otherwise and enables us to ask new questions. In the first part of the dissertation, I show that *participle* is not a grammatical primitive and that both eventive and stative participles in the languages under discussion are deverbal adjectives. This leads me to two conclusions: (i) the LC *adjective* is not reliably associated with stative meanings, and (ii) verbal structure does not necessarily contribute an event(ality) meaning component. I then probe the category-meaning correspondence further, concluding that LCs are fundamentally distributional categories which do not map to any interpretive component in a 1-1 way. Given this disassociation between syntactic category and interpretation, questions arise about the general perceptual versus linguistic-specific underpinnings of the grammatical primitives. I conclude that the connection between LCs and the more general mechanisms for the organization of thought are only indirect. In the second part of the dissertation, the goal of paring down the inventory of grammatical principles brings into focus the fact that (at least) three different theories coexist to explain the cyclic nature of syntactic derivations: Phase theory à la Chomsky, Phase theory in the DM literature, and (Featural Relativized) Minimality. This portion of the dissertation draws attention to the fact that these competing theories (1) make contradictory predictions (in the case of Chomskyan and DM Phase theory), and (2) have overlapping restrictions, thus being somewhat redundant (Chomskyan Phase theory and Minimality). My investigation leads me to conclude that Minimality is the only locality constraint in the syntax, and that a different operation, Transfer, regulates spellout to the interfaces without having any syntax-internal effects. This makes Transfer a good candidate for an interface condition (a legibility condition that can be understood as arising from interface considerations, in the sense of Chomsky 1992) and raises more general questions as to how such conditions should be understood and evaluated.[4]

[4] Treating Transfer as an interface condition does not necessarily mean that UG should have nothing to say about it, as is evident from the following quote from Chomsky 2008: "If SMT held fully, which no one expects, UG would be restricted to properties imposed by interface conditions." What this view of Transfer does do is locate the motivation for its existence outside FL itself.

## 1.2 A note on emergentist approaches

As is obvious from the previous section, I assume that humans are born with certain biases about what their language input will look like, biases that go beyond what a non-linguistic primate of the same intelligence level would have. This is the *innatist* approach to language acquisition. There are, however, some psychologists and cognitive scientists who deny that such proprietary innate knowledge exists; language acquisition is instead thought to be guided solely by the input and domain-general cognitive capabilities. As far as I can tell, those espousing this *emergentist* approach to language acquisition (e.g., MacWhinney 1998, Tomasello 2000) rarely engage with the kinds of facts cited above regarding children's sophisticated knowledge of abstract structure, used to motivate the PoS argument. Also, most of the modern learning literature (e.g., research focusing on Bayesian learning, see Tenenbaum & Griffiths 2001 and the research line that follows them) does not try to suggest that this kind of learning can explain language acquisition *in toto*. Bayes' Theorem entails that the probability of a grammatical generalization (G) being correct, given some body of evidence about sentences (E), is proportional to the probability that the evidence would look like E if G were correct. However, the theorem says nothing about how the child would come to treat a(n unsegmented) speech stream as evidence for any hypothesis. As Lasnik & Lidz 2016 put it: "The input itself does not contain information about the kinds of representations that should be used in building a generative grammar of the language". Furthermore, the Bayesian learner's job is to assess the likelihood of a given hypothesis, but the theorem says nothing about where the candidate hypotheses come from. Hence, both the representations and the hypotheses formulated over them in the course of language acquisition remain unaccounted for in a statistical learning approach and must be attributed to the innate characteristics of the learner. However, there is still significant controversy regarding the role of language-specific versus domain-general (innate) abilities needed to explain language acquisition; see, for example, Pullum & Scholz 2002, Yang 2004, Hoff & Shatz 2007, Spelke 2022 for discussion. I leave this issue aside since this dissertation is primarily concerned with the nature of linguistic knowledge, an ontologically prior question. Simply

put, one cannot study linguistic cognition (or its relationship to other cognitive domains) without first understanding what the object of study—the human capacity for language—actually is. Where I discuss LCs in Chapters 2-4, I will conclude that LCs (e.g., *verb*) that are referenced by the adult grammar are not themselves part of UG since they do not have any stable cross-linguistic properties, but certain inductive biases are certainly still needed to guide the category-learning process. In Chapter 7, I suggest that something like the principle of Minimality helps the language acquirer form correct generalizations about the input. Until it can be shown convincingly that these same biases guide learning in other cognitive domains, the null hypothesis remains that they are proprietary to language.

## 1.3 A note on lexicalist versus non-lexicalist approaches to syntax

Since much of this dissertation is concerned with mixed categories and word-building, it will be useful to discuss at the outset different approaches to word-formation. These approaches fall into two large categories, which can be referred to as *lexicalist* and *non-lexicalist*. The non-lexicalist (syntactic) approach to word-formation has been gaining momentum in the field in the past 30 years or so; I adopt it in this dissertation as it seems to be both empirically and conceptually superior to the alternative.

Lexicalism can be characterized broadly as an approach which takes the minimal units of syntax to be words. Early generative linguistics was a lexicalist enterprise in as much as very little attention was paid to what actually constitutes the lexicon. Words were thought to be the atomic units in the syntax (its terminals), and generativists were concerned with phrasal syntax. An exception to this has always been inflectional morphology (e.g., English past Tense *-ed*), which was understood to be assembled syntactically since Chomsky 1957.

Many lexicalists deny this position, arguing that *played* in *John played the flute* is inserted into the syntax as a pre-assembled, atomic unit; see e.g., Lieber 1980 where this view is originally proposed. We may call this position *strong lexicalism*. Strong lexicalism can be understood as an approach to word-formation which assumes that there exists a generative lexicon in addition to a generative syntax (see e.g.,

Lieber 1980, Kiparsky 1982, *et seq.*, Levin & Rappaport 1986, Di Sciullo & Williams 1987, Reinhart & Siloni 2005, Horvath & Siloni 2008, 2016). The lexicon is constrained by a set of proprietary rules which manipulate sub-word units (roots, stems, affixes), including both inflectional and derivational morphology (Lieber 1980). The category of words is determined in the lexicon. Once built-up in the lexicon, words are inserted into the syntactic computation, which is blind to their internal structure (words are *atomic*). Furthermore, words are taken to be the domain of listedness. While larger syntactic structures are not assumed to be stored in any way, new words must be added to the lexicon; this includes information about their syntactic (categorial, featural), semantic, and phonological properties.

Researchers who adopt strong lexicalism often cite Chomsky's 1970 *Remarks on Nominalization* as the beginning-point for such a view. This is quite an interesting take on *Remarks*, which painstakingly demonstrates that the various similarities in the structure, meaning, and productive nature of clauses (1a) and gerundive nominals (1b) warrant the gerunds a transformational analysis. In other words, gerunds, which are clearly nominal at surface structure, are argued to be derived from the corresponding clauses in the syntax. The lexicalist position is applied to so-called derived nominals (1c), and the arguments given suggest Chomsky 1970 does not consider the lexicon to be a generative component of the grammar.

(1) a. John refused the offer.

b. [John's refusing the offer] surprised us.

c. [John's refusal of the offer] surprised us.

Suspicions about the strict division of labor between syntax and the lexicon quickly arose among researchers who worked on languages with rich agglutinative morphology, which have famously large "words". For example, Baker 1985 shows that changes in affix order in polysynthetic languages have consequences for interpretation. In (2a), the reciprocal relationship is applied to the external argument of *beat*, while in (2b) it applies to the causer of the beating event. This would be naturally explained in terms of syntactic scope (given the morpheme's distance from the root), but it is a complete accident on

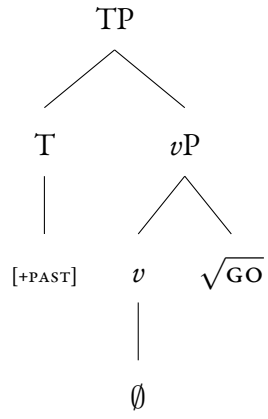
lexicalist approaches to word formation. More generally, it seemed quite unsatisfactory to claim that speakers of polysynthetic languages simply store giant words in their lexicon, with their syntax then being very impoverished.

(2) a. Maqa-naku-ya-chi-n. (Quechua, Baker 1985:374)
beat-RECIP-DUR-CAUS-3S
'He$_j$ is causing them$_i$ to beat each other$_i$.'

b. Maqa-chi-naku-rka-n.
beat-CAUS-RECIP-PL-3S
'They$_i$ let someone$_j$ beat each other$_i$.'

Soon the suspicions around lexicalism accumulated and developed into a full-blown research program. The most prominent single strand of non-lexicalism has been Distributed Morphology (DM), which I will broadly adopt in this dissertation (see the foundational work in Halle & Marantz 1993, Marantz 1997, 2001, 2007, Harley & Noyer 1999, 2000). DM argues that there is no single language module that deals with what has traditionally been called "morphology" (hence the word *distributed* in the name). Morphemes are combined in the syntax, hence "words" are not the terminals of syntax.[5] In fact, "words" are argued to have no privileged status in the grammar. Instead, the syntactic derivation consists of acategorial roots and functional heads, which include what has traditionally been called derivational (categorizing) and inflectional morphemes (3).

(3)




[5] Additionally, operations including *lowering*, *local dislocation*, *impoverishment*, and others have been proposed to operate on morphemes or their exponents post-syntactically; see e.g., Noyer 1997, Embick & Noyer 2001. Such operations will not be of concern in this dissertation, so I do not discuss them any further.

Furthermore, the framework is *realizational*. Morphemes are assumed to be abstract entities; they carry formal features, but only acquire form and meaning at the respective interfaces. Illustrating how this works at PF for $\sqrt{\text{GO}}$ in (4), DM assumes that different allomorphs of a given morpheme stand in competition, which is regulated by the *Elsewhere Principle* (a.k.a. the *Subset Condition*). Essentially, any given morpheme is connected at the PF-interface with a list of exponents. The list may contain one or more members. If a morpheme has more than one potential exponent, the list is consulted starting with the more specific (context-restricted) exponents. In the case of $\sqrt{\text{GO}}$ in (4), the more specific exponent is the one whose insertion is limited to the context [+PAST]. This context is present in (3), so the more specific exponent wins out, and the root is spelled out as *went*. Had the [+PAST] feature not been present in the root's morphosyntactic context, the elsewhere allomorph *go* would have won out. The 'morphosyntactic context' is discussed and defined more precisely in chapters 5 and 6.

(4) $\sqrt{\text{GO}} \longleftrightarrow$ *went* / [+PAST]
$\longleftrightarrow$ *go* / elsewhere

I will conclude this section by offering a couple of further remarks on the conceptual and empirical superiority of the non-lexicalist approach. The clearest conceptual argument against lexicalism has to do with the duplication of operations already available in the syntax. *Strong lexicalism* assumes that there are two ways to build words. One type are roots that are stored in the lexicon along with their category (e.g., nouns like *cat*, verbs like *kill*, and adjectives like *yellow*). The second way is through morphological processes like affixation, adding both derivational and inflectional morphology (e.g., *cert-ifi-es*. Weak lexicalism must arguably then allow three distinct ways of assembling words, since inflectional morphology is thought to be within the purview of syntax. If these world-building processes cannot be shown to be different in kind, then the hypothesis that they are all part of the same language module (as in DM) is clearly the preferable alternative. Looking at Kiparsky 1982, for example, it is quite striking that the morphological processes assumed in this work (e.g., selection, endocentric projection, feeding by syntactic rules) have clear equivalents in the syntax. A parsimonious theory would then be

one where the relevant morphological and syntactic processes are subsumed under one. As there have been no convincing proposals to fully reduce syntactic processes to the morphology, the syntactic approach to word-formation seems like the best way to proceed in eliminating this apparent redundancy from the grammar.[6]

As for empirical arguments, many have accumulated over the last decades in favor of a syntactic conception of word-building, including the ones already mentioned from Chomsky 1970 and Baker 1985. Let me elaborate on how the distinction between gerunds and derived nominals discussed in Chomsky 1970 has, in fact, been taken as an argument for full decomposition. There are, in fact, three different types of event-denoting nominals in English, as exemplified in (5).

(5) a. [John's refusing the offer (suddenly)] surprised us.

b. [John's refusing of the offer (suddenly)] surprised us.

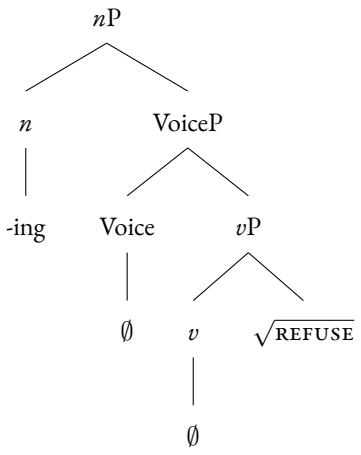
c. [John's refusal of the offer (*suddenly)] surprised us.

The morphological, syntactic, and semantic properties of these nominals have been argued to follow straighforwardly, if one assumes the corresponding structures in (6). Only the gerund in (5a) has full verbal structure in (6a), including VoiceP, which has independently been argued to introduce a verb's external argument and license accusative case on its complement. As shown in Kratzer 1996, the genitive phrase (*X's*) is necessarily understood as the external argument of the corresponding verb only in the case of structures likes (6a), while the other two structures allow a broader possessive reading. Furthermore, only the gerund in (5a)/(6a) licenses an accusative complement, again due to the presence of Voice. As expected, the gerund can be modified by *v*P adverbs like *suddenly* (on the distribution of adverbs with event nominals, see specifically Fu, Roeper & Borer 2001). Both (5b) and (5c) lack Voice, hence cannot have accusative complements. The complement in this case is licensed by *n*, which is why it has the characteristic *of*-form (cf. *a student of physics*). The structural difference between (5b) and (5c), shown in (6b) and (6c), accounts for why only (5b) is able to license *v*P adverbs. The structural

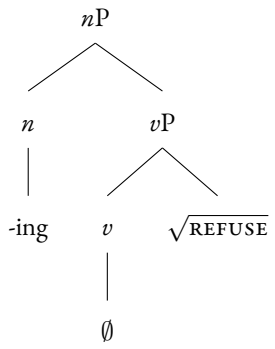
[6] I am not quite sure what such a proposal would even entail. The morphology would then arguably have to care about things like structural hierarchy and c-command, so it would simply be syntax under a different name.

differences have also been argued to explain (i) the differences in the allomorphy of the nominalizer (*-ing* vs. *-al*, see Embick 2010, i.a.; chapters 5 and 6 of this dissertation) , and (ii) the interpretive differences between the different kinds of nominals (Alexiadou 2001, i.a.). [7]

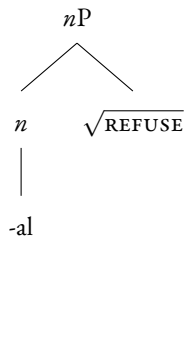
(6) a.




b.




c.




Moreover, the fact that fully-decompositional approaches do not give any significance to the notion "word" has some pleasant empirical consequences. Lexicalism assumes the lexicon (i.e., "words") to be the domains of listed meanings, including both (trivially) root-words like *cat* and derived words like *terrific*. The combination of those words in the syntax should have a compositional meaning–a meaning that is fully predictable from the meaning of its parts given completely general rules of interpretation, which do not refer to specific items. Of course, idioms like *kick the bucket* have long been recognized as problematic for this view, but have mostly been shrugged off as exceptional, "edge cases" which should not guide theory-building. Nonetheless, as shown in Jackendoff 1997 and Marantz 1997, idiomatic readings of large syntactic structures are anything but exceptional, as the examples in (7) show.

(7) a. take a leap (Marantz 1997:207)

b. take a leak

[7] On lexicalist approaches that take seriously the existence of a generative Lexicon, the correlations between the syntactic, semantic, and morphological properties of these nominals are established in the Lexicon. As already mentioned, the biggest issue for these approaches is redundancy. However, there are also many works which do not seem to assume a generative Lexicon, and which assume that the nominals in (6) are atomic. For these works, the established correlations are a complete accident (see Krauska 2024 for recent discussion).

c. take a piss

d. take a break

e. take five, cover, issue, heart, over, up, down ...

There is clearly no sharp divide between word and phrasal special meanings. However, special meanings do have a delimited context, and the delimiters seem to be syntactic (see Marantz 1984, 1997, 2013; Wood 2023 and the references there). Non-lexicalist approaches to word-formation are at an advantage here because they do not give "words" a special status in the grammar.

Finally, I will discuss particular aspects of the lexicalist/non-lexicalist debate as they become relevant in the dissertation. Chapter 3 includes a comparison of the two approaches in the domain of participles specifically, arguing that the syntactic approach to participles has a clear advantage in accounting for their syntactic, semantic, and morphophonological properties. Chapter 5 discusses morpheme-level 'chunking' effects at the form interface, arguing that these fall out naturally on a non-lexicalist approach to word formation, but are unexplained on an approach that takes words to be atomic in the syntax.

## 1.4 Overview of the dissertation

Throughout much of the dissertation, I examine the LC adjective and the (morpho)syntactic process of adjectivization, including the repercussions of this process at the interfaces for meaning and form. As already noted, Chapters 2-4 examine the nature of LCs as grammatical primitives from a minimalist perspective, while Chapters 5-7 do so for the grammatical principle of locality.

In the first half of the dissertation, I examine the status of LCs in linguistic theory. In chapter 2, I argue that LCs serve a purely contrastive, distributional role in the grammar. I bring in evidence from different languages to argue that (what we call) nouns, verbs, and adjectives cannot be shown to have any universal syntactic or semantic properties. I then discuss in detail how my proposal differs from the received wisdom in both functionalist and generative approaches to LCs. I show that functionalist approaches to LCs are generally not tenable for building scientific theories of language. I then give a brief

history of the role of LCs in the generative tradition, arguing against specific analyses which attempt to attribute universal syntactic or semantic properties to the LCs noun, verb, and adjective. I conclude by discussing the consequences that my proposal, if correct, has for the contents of UG. If there are no universal syntactic or semantic properties we can attribute to the LCs, assuming the individual LCs noun, verb, and adjective to be part of the initial state of the learner becomes superfluous. I propose that the cross-linguistic tendencies we observe around LCs may stem from the way non-linguistic knowledge is organized in the mind/brain.

I then present two detailed case studies of mixed categories: Chapter 3 focuses on passive participles and Chapter 4 on active participles in a number of related and unrelated languages. I examine the syntactic, morphological, and interpretive properties of passive and active participles and argue first that *participle* is not an independent category in the grammar. Instead, participles in the languages under discussion are a derived category, deverbal adjectives. I show that this applies to all participles, in all the syntactic positions they appear in and regardless of their interpretation. Participles may denote (predicates of) properties or eventualities, and I argue that the different interpretations are not (cross-linguistically) associated with more or less verbal or adjectival structure. Taken together, these points strengthen the conclusion that there is no 1-1 mapping between an item's LC and its interpretation.

In the second half of the dissertation, I turn my attention to the locality principle(s) that have been argued to be operative in language. In Chapter 5, I first give an overview of how the observed locality effects in syntax and morpho(phono)logy have been modeled via Minimality and Phase theory (Subjacency). I show that Minimality and Phase theory can both successfully account for basic locality effects in the syntax; assuming both therefore results in a significant amount of redundancy, an issue that is resolved in chapter 7. Phase theory has additionally been argued to drive a number of observed morphological locality effects. It may therefore be tempting to attempt to unify syntactic and morphological locality effects under Phase theory, a popular move in the recent DM literature. I conclude this chapter by providing a number of conceptual arguments against said unification.

Then, focusing on (de)adjectival derivation in BCS, Chapter 6 shows that assuming Phase theory

to govern both syntactic and morphological locality leads to incorrect empirical predictions. Specifically, I show that adjectives in BCS do not force (indeed, do not allow for) successive-cyclic movement of the phrasal material in their domain, despite allowing subextraction. On the other hand, BCS adjectives delimit a locality boundary for morphological processes (namely, allomorphy and morphological tone assignment). There is a clash: BCS adjectives constitute a phase in morphological terms but not in syntactic terms.

In Chapter 7, I propose to resolve this clash by reinterpreting the effects of syntax-internal opacity in terms of (Featural Relativized) Minimality rather than phasehood. I argue that Minimality is the only locality principle that operates in the syntax. Minimality is a principle which regulates probe-goal relations, and which cannot by its nature have any effects at the interfaces. I argue that spellout is an independent mechanism regulated by Transfer, a modified version of the PIC that has no effects on the syntactic computation. I discuss the plausible (functional) motivation for having both Minimality and Transfer as part of the grammar. I conclude the chapter by considering what a strong piece of evidence for syntactic phasehood would look like.

# Chapter 2: On the role of lexical categories in linguistic theory

## 2.1 Introduction

Baker opens his influential 2003 monograph on lexical categories (LCs) with the following statement: "It is ironic that the first thing one learns can be the last thing one understands". The remark is true of LCs in two different ways. For the student who comes into their first linguistics class, LC terms may be one of the few things they know about grammar. For the theoretician, the discovery of the familiar LCs goes back at least to the middle of the first millennium BCE, which saw the publication of Pāṇini's grammar of Sanskrit. Yet, when the student leaves the classroom, equipped with many sophisticated linguistics concepts, their understanding of the LCs is likely not much more advanced than in the very first class. This is quite a direct consequence of the state of our theories of LCs.

While linguists have undeniably made many discoveries regarding LCs in the last 2000 years, in particular thanks to studying the systems of many different languages, progress in uncovering universals that hold across languages has been remarkably slow compared to other areas of inquiry. An analysis of LCs that forms the basis of most modern proposals was introduced by the Ancient Greek grammarian Dionysius Thrax, whose Téchnē grammatikḗ (cca. 100 BCE) divided words into eight LCs. The division included already familiar categories like *ónoma* (nouns), which inflect for case and signify concrete or abstract entities, and *rhḗma* (verbs), which inflect for tense and person, and signify activities or processes, but also others like *metokhḗ* (participles), which show mixed behavior. Perhaps the biggest innovation of Thrax's definitions is that they combine morphosyntactic and semantic criteria, a signif-

icant departure from Plato's conception of the LCs, which was purely meaning-based.[1] This innovation makes necessary the introduction of categories such as participles, which pattern with verbs in their meaning, and with nouns (or adjectives) in their morphosyntax. The first question that any theory of LCs must answer, then, is the following: What is a theory of LCs supposed to do? Should it account for its members' syntax, semantics, or both?

As will be evident throughout this chapter, the idea that LCs should be defined using a stable and perhaps universal set of semantic and/or morphosyntactic criteria has persisted into modern linguistics. I will argue that this view cannot be maintained. Emphasizing syntactic and morphological evidence, I will argue that LCs are abstract formal categories which partially determine an item's syntactic properties and therefore its surface distribution, but which do not uniquely map to any semantic property. Moreover, while the abstract notions of *nounhood*, *verbhood*, etc. determine the syntactic behavior of these elements within a single language, there is no evidence that elements we call *nouns* or *verbs* have any stable cross-linguistic syntactic properties.

I will therefore argue that the reason for the field's slow progress in uncovering LC universals may be that very little, if anything, about LCs is attributable to UG. There are certainly some general correlations between syntactic and conceptual (or maybe even ontological) categories and such correlations may be used in acquisition (so-called syntactic or semantic bootstrapping). However, such correlations are far from perfect, as I will demonstrate throughout the chapter, and this undermines the idea that they are encoded by the language system itself. Instead, the tendencies we observe more likely have roots in the way non-lingistic knowledge is organized in the mind/brain.

The rest of the chapter is organized as follows: In section 2.2, I briefly outline the main functionalist positions regarding LCs, and discuss why functionalist accounts of LCs are untenable for building scientific theories of linguistic knowledge. I then turn to a critical examination of the role of LCs in the generative tradition (section 2.3). Sections 2.3.1 and 2.3.2 give an overview of what I take to be the

[1] In his *Sophista* (261E–262A), Plato writes: "The one that specifies *doing* we could call *rhḗma*... And the human-voice signal that is applied to *those who are doing* these things [we may call] *ónoma*." (see Woodard 2023:120).

dominant generative views about the universal syntactic and semantic properties of LCs, respectively. For the sake of concreteness, each section also critiques a particular worked out proposal (of which there are few), in order to substantiate my claims. I argue that while LCs can be unambiguously distingushed in the syntaxes of individual languages, we have not (yet) discovered any universal syntactic property that positively distinguishes all members of one LC from all members of the other LCs. I also argue that we can be quite confident the individual LCs do not map to any meaning component in a 1-1 way, even within a single language. As we will see, the proposals I discuss focus on nouns and verbs, with adjectives almost completely disregarded. This is a very common state of affairs in the literature on LCs, given that the universality of the category adjective is itself controversial. Since the focus in chapters 3 and 4 is on (deverbal) adjectives, section 2.3.3 briefly discusses what has thus far been said about their distinctive properties. In section 2.3.4, I synthesize the main takeaways from this chapter and discuss recent work that proposes to attribute the noun-verb distinction found in all human languages to language-independent cognitive factors.

Before moving on, let me note the following: Morphological evidence for LC distinctions is taken very seriously in this dissertation, certainly too seriously for some tastes. The reason is quite simple: At least in languages with rich morphological systems–which are in focus in this dissertation–morphological evidence for LC membership closely correlates with what is more commonly thought of as syntactic (i.e., distributional) evidence, and can therefore be quite a useful cue for uncovering an item's syntactic properties. The same cannot be said for semantic properties, as items that distribute in similar ways may have wildly different meanings and vice versa.

## 2.2 Functionalist approaches to LCs

Most functionalist approaches adopt a notional characterization of LCs. One common claim is that there is a link between lexical categoryhood and "time stability", the idea being that verbs (prototypically) name actions, nouns name things, while adjectives represent some mid-way point between the

two (see e.g., Givón 1984, Hopper & Thompson 1984). Croft (1991, 2001) lays out a theory in which nouns (prototypically) denote reference to an object, adjectives denote modification by a property, and verbs denote a predication of an action. Similar ideas are adopted elsewhere in the functionalist literature, including in Lakoff 1987, Hengeveld 1992, Bhat 1994, Shopen & Schachter 2007. The LCs' syntactic properties are taken to follow from their meaning/function, and morphological properties are said to track it as well (e.g., the noun *dog* is more prototypical, hence morphologically less complex than the noun *fairness*.) In (8), I provide a table representing the prototypical member of each LC, from Croft 1991.

(8)

| | **Noun** | **Adjective** | **Verb** |
|---|---|---|---|
| *semantic class* | object | property | action |
| *valency* | 0 | 0 | [3]1 |
| *stativity* | state | state | process |
| *gradability* | non-gradable | gradable | non-gradable |
| *pragmatic function* | reference | modification | predication |

A somewhat different understanding of LCs is offered in Langacker 1987a,1987b: While LCs are still based on prototypes and have notional definitions, their characteristic properties are based on different conceptualizations of things in the world, rather than on their ontological properties. In other words, it is not that nouns necessarily denote objects/things, but rather that the conceptualization invoked by nouns involves an object. Similarly, it is not that verbs denote actions/processes, but rather that the conceptualization invoked by verbs involves an action. This view has much in common with the more recent (generative) proposal offered in Panagiotidis 2015; see section 2.3.2.1 for a critique.

As is obvious from the preceding prose, functionalist accounts of LCs are prototype-based, with Hopper & Thompson (1984:707) firmly claiming that "the concept of prototypicality (the centrality vs. peripherality of instances which are assigned to the same category) plays an important role in the

study of grammar".[2] The fact that the definitions of LCs are supposed to describe only prototypical members of a category has two related consequences. First, it means that functionalist approaches are not as vulnerable to the criticisms I will level at generative theories of LCs in section 2.3, since members of a LC can presumably deviate from the prototype in unrestricted ways. However, this same property means that functionalist theories are not predictive, in terms of the semantic, morphological, or syntactic properties of any given member of a category. Moreover, syntactic computation does not seem to work with LCs based on prototypes. Suppose a verb is far from prototypical (e.g., a stative verb). Is the VP it projects then exactly as far from prototypical? There are, to my knowledge, no compelling syntactic arguments that this is the case.[3]

Furthermore, while there may in fact be language-external functional grounding for the existence of multiple LCs in language, we will see throughout this chapter and chapters 3 and 4 that the distinctions that plausibly motivate the existence of different categories are not co-extensive with the LCs referenced by the grammar (see also the discussion of these matters in Newmeyer 1998: ch. 4). This strongly suggests that those distinctions are not encoded by the language system itself. As we will see in section 2.3.2, this is the downfall of generative approaches that attempt to provide a straightforward 1-1 mapping between the LCs and some meaning component. The issue is in a sense even more acute than for the functionalist, given the assumptions of generative linguistics about the nature of linguistic knowledge.

[2]The idea of *fuzzy boundaries* between categories is also often invoked in functionalist approaches, though it does not seem to be universally accepted as valid (e.g., Langacker 1991). While the fuzzy-boundary view is prevalent among functionalists, there is also work within generative semantics espousing such an approach, see Ross 1973a, 1973b, 1975.

[3]Ross (1973a) attempts to make the case that less prototypical nouns (e.g., expletive *it*) participate in fewer syntactic processes characteristic of nouns. While this may be quantitatively true, there are several issues. First, the level of prototypicality of any given noun is established either subjectively, or based on the number of "nouny" constructions it is found in, the latter criterion being circular. Moreover, the incompatibility of certain nouns with particular constructions may be for extra-syntactic reasons. For example, operations like left-dislocation have a particular information-structural effect (topicalization in English), which is not compatible with non-referential nouns.

## 2.3 Generative approaches to LCs

Generative linguistics takes an algebraic approach to language, in that derivations consist of discrete, symbolic entities that can be manipulated in different ways. Linguistic knowledge is taken to include these symbols and the rules for their combination. The symbols very prominently include the LCs, and rules reference them or the phrases they project. Such an algrebraic theory does not leave room for the idea that one verb is "more verby" than another, or that some process may apply only to the best or worst verbs (or, even worse, the ones that are neither). Hence, if there is such a thing as a verb, noun, or adjective, then there must at the very least exist a grammar-internal principle or rule that singles out (all the members of) each of these LCs to the exclusion of the rest.

Moreover, Generativism takes some aspects of linguistic knowledge to be innate and domain-specific, a part of UG (the "language organ", Anderson & Lightfoot 2002). The LCs are a prime example of hypothesized *substantive universals* in the sense of Katz & Postal 1964. This is made explicit in the following quote from Demirdache & Matthewson (1995:69): "Distinctions between the lexical categories N, V and A are a universal property of language. [...] The evidence for these distinctions is subtle, implying that it reflects fundamental property of UG." Nonetheless, LCs have not received nearly as much attention in generative circles as they have among functionalists; the prevalent view that LCs are innate is much more often assumed than argued for. If it is true that the specific LCs are universal, then the syntactic principle or rule that singles out each LC must also be universal across languages. Alternatively (or in addition), the LCs must map straightforwardly to some interpretive property, both within and across languages. As I show throughout this chapter, the available evidence argues against both of these positions.

### 2.3.1 "Syntactic universals"

A common view throughout the history of generative grammar was the one espoused in Chomsky 1970: The LCs are complex objects made up of a set of two binary features, namely [+/-N] and

[+/-V] (see Stowell 1981 who cites Chomsky 1974 for a consolidation of these ideas). The hypothesized values of the features for each LC are given in (9).

(9)

| | |
|---|---|
| **Noun** = [+N, -V] | **Adjective** = [+N, +V] |
| **Verb** = [-N, +V] | **Preposition** = [-N, -V] |

In the 1970s and 1980s, many accounts of different phenomena referenced the idea of natural classes among the LCs. For example, Stowell 1981 cites the fact that, in English, only verbs and prepositions ([-N] LCs) assign accusative case, while nouns and adjectives ([+N] LCs) have *of*-complements. In fact, however, decomposing the LCs into features as in (9) and assuming that syntactic rules reference the features rather than the LCs leads to the wrong predictions. For example, adjectives and verbs are both [+V], but no rule of English seems to reference them as a natural class, to the exclusion of other LCs. Also, while it is true that only verbs and preposisitions assign case directly in English, this is not universally true; both nouns and adjectives can assign case in BCS, for example. All decompositional accounts suffer from similar drawbacks, including the proposals in Jackendoff 1977, Bresnan 1982, Déchaine 1993, and Hale & Keyser 1993. I will therefore not discuss them in detail; see Baker 2003 for more arguments that the feature decomposition of LCs is arbitrary and unlikely to be employed by the grammar. The categories (i.e., categorial features on non-lexicalist approaches) are atomic (Stuurman 1985).

#### 2.3.1.1 Baker 2003: Verbs have specifiers, nouns have referential indices

It is perhaps not too much of a stretch to claim that a substantive proposal about the universal syntactic properties of LCs did not exist until Baker 2003, the [+/-V]/[+/-N] notation being essentially a promissory note. While Baker does ponder the seemingly universal semantic properties of the LCs, he ultimately decides that their core distinguishing properties are syntactic. In a nutshell, verbs are argued to be the only LC that projects a specifier, nouns bear a referential index in the syntax, and adjectives are

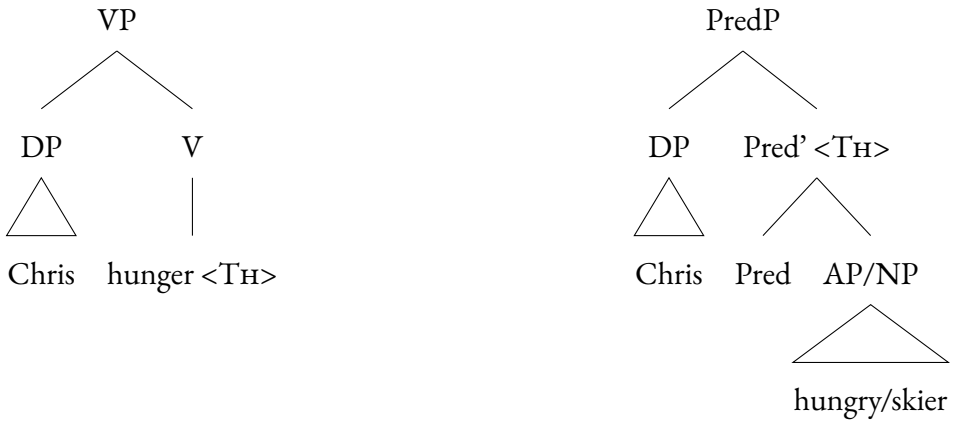
characterized by having neither of these properties. I critically discuss the proposal for verbs and nouns in this section, turning to adjectives in 2.3.3.[4]

As just mentioned, Baker argues that the distinctive property of verbs is that all verbs license a specifier. Moreover, verbs are the *only* LC that licenses a specifier and therefore introduces its own argument; nouns and adjectives require "mediated" predication. I illustrate the hypothesized syntactic difference for the sentences in (10) in (11); see also Williams 1983, contra Bowers 1993 where the PredP analysis is extended to *all* LCs.

(10) a. Chris hungers. (Baker 2003:31)

b. Chris is hungry/a skier.

(11)



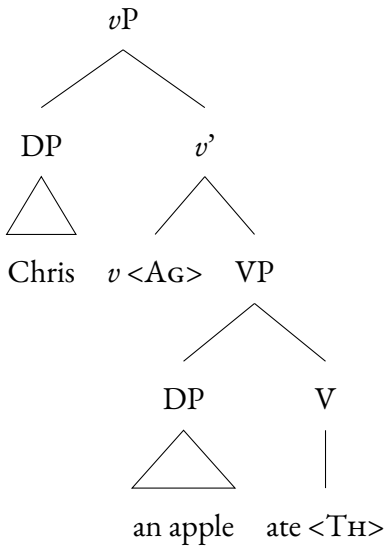

Note that (11) contains what we usually think of as an internal argument (of an unaccusative verb) in the spec, VP position; in fact, for Baker, both internal and external arguments of verbs are introduced in specifiers, as he makes clear in the statement in (12). External arguments of verbs are introduced in spec, *v*P which selects VP, as in (13). For transitive and unergative verbs, Baker's analysis then differs from Bowers' only in the label of the projecton introducing the external argument: *v*/Pred. Baker claims that V–the head that introduces the internal argument–is still the *lexical head* of such

[4]Baker does not not adopt full decomposition, so on his approach the LCs are fully atomic, rather than a combination of acategorial roots and categorizing heads. No substantive changes arise for his theory if one adopts the notion of acategorial roots, so I will ignore the distinction in the discussion that follows. When Baker says "nouns bear a referential index", one can equally read this as "*n* has a referential index".

predicates. The external-argument introducing head is treated as a functional head (see Baker 2003:65-7), as is usual in the literature that argues external arguments should be severed from the verb (Schein 1993, 2017, Kratzer 1996, 2000, Marantz 1997, Pylkkänen 2002, Merchant 2008, Harley 2009, 2013, Williams 2015, i.a.)

(12) a. Agent and theme roles can only be assigned to specifier positions. (Baker 2003:26)

(13)




The arguments for Baker's position are of two kinds. First, he needs to show that the specifier of VP is *always* filled. Then, he needs to show that the spec, NP and spec, AP positions are *never* filled. Both are difficult tasks, and while the proposal is interesting, the final result is unconvincing. Starting with the first task, there are essentially two arguments Baker gives: the presence of *it*-expletives in VP small clauses, and the diverging behavior of unaccusative verbs and non-verbal predicates in what are traditionally taken as unaccusativity diagnostics.

Verbs like *seem* have no DP arguments and according to Baker do not assign a θ-role. The evidence that these verbs have an obligatory specifier comes from the appearance of expletive *it* in sentences like (14). Claiming that the expletive in (14) is there purely because of a lexical requirement of the verb is quite controversial, however. It is well known that expletive *it* does not appear in non-finite embedded contexts like (15). Moreover, there is a whole host of literature arguing that the subject of the sentence,

*they*, does not make an intermediate stop in the embedded clause headed by *seem* on its way to check the EPP feature of finite matrix T (see Castillo, Drury & Grohmann 1999, Boeckx 2005, Grohmann, Drury & Castillo 2000, Hornstein 2001, Bošković 2002, Epstein, Pires & Seely 2005). Most importantly, while I do not claim to fully understand the reason why the expletive is needed in (14), the same requirement is observed in the exact same context with the (raising) adjective *likely* (16). Therefore, the presence of *it* in (14) cannot be taken as evidence that all verbs obligatorily project specifiers, unless the same conclusion is reached for adjectives given (16).

(14) I made *(it) seem that I was happy. (Baker 2003:27)

(15) They are likely (*it) to (*it) seem to want to fight.

(16) I made *(it) likely that I would win.

As for the argument from unaccusativity diagnostics, Baker spends a substantial amount of time defending the idea that the sole argument of unaccusative verbs and the sole argument of predicative nouns/adjectives are found in distinct positions. The idea is that this can be explained by assuming only the verb can license its own argument. The tool that shows this diverging behavior are traditional unaccusativity diagnostics, including Italian *ne*-cliticization, Mohawk incorportation, Hebrew dative possessors, and Japanese floating quantifier distribution, among others. Illustrating with *ne*-cliticization, a genitive case-marked clitic *ne* can be extracted from inside objects of transitive verbs (17a), and the sole argument of unaccusatives (17b), but not from subjects of transitive (17c) or unergative verbs (17d), or from the sole arguments of non-verbal predicates (17e-f). Other unaccusativity diagnostics show the same pattern.

(17) a. Giovanni ne inviterà molti. (Burzio 1986:22-3)
Giovanni of.them will.invite many
'Giovanni will invite many of them.

b. Ne arriveranno molti.
of.them will.arrive many
'Many of them will arrive.'

c. *Ne esamineranno il caso molti.
of.them will.examine the case many
'Many of them will examine the case.'

d. *Ne telefoneranno molti.
of.them will.telephone many
'Many of them will call.'

e. *Ne sono buoni pochi (dei suoi articoli). (Cinque 1990:7)
of.them are good few (of his articles)
'Few of them (his articles) are good.'

f. ?*Ne sono professori molti. (Baker 2003:64)
of.them are professors many
'Many of them (e.g. people who wear glasses) are professors.'

Baker's account for the contrasting behavior of unaccusative verbs and non-verbal predicates invokes the idea that unaccusative verbs license arguments directly (in spec, VP), while nominal and adjectival predicates can only do so in spec, PredP.[5] This is all well and good, but surprisingly little space is dedicated to unergative verbs, especially given Baker's caim that V is the LC (and not *v*P). Since the argument of unergative verbs patterns with the external argument of transitives and the sole argument non-verbal predicates, this suggests (even on Baker's understanding of the facts) that the unergative argument is introduced in spec, *v*P and not by the LC V. It is unclear, then, what the evidence is that unergative verbs also always have a filled spec, VP position, a necessity if this property distinguishes verbs from other LCs.[6]

Since Baker argues that V is the only LC with a specifier, a corollary of (12), which I repeat here in (18), is that simple nouns and adjectives should never be able to assign agent or theme θ-roles. Of course, both nouns and adjectives can have internal (theme) arguments, as in (19).

(18) Agent and theme roles can only be assigned to specifier positions. (Baker 2003:26)

[5]The explanation for the exact facts (here, *ne*-cliticization) is of course more complex, but I do not discuss it as it is not relevant for the questions I raise regarding the universal syntactic properties of LCs.

[6]Baker does point to the possibility that all unergatives are in fact hidden transitives, à la Hale & Keyser 1993. I do not address this proposal in detail, since it seems to me to be unfalsifiable. I simply note that the specifier/complement distinction is crucial on Hale & Keyser's account, in order to explain why external arguments never incorporate. This distinction dissipates on Baker's approach, where all arguments are in specifier positions.

(19) a. student of physics / worthy of her attention

b. student fizik-e / vredan njen-e pažnj-e (BCS)
student physics-GEN worthy her-GEN attention-GEN
'student of physics / worthy of her attention'

It is not clear to me what Baker makes out of examples such as these, where nouns and adjectives seem to have internal arguments. For one, the noun/adjective assigns genitive case to the argument in BCS, and the argument of the adjective is in fact obligatory. Furthermore, it has been observed that languages exhibit argument/adjunct asymmetries in nominal subextraction. For example, BCS allows nominal adjuncts to subextract (20a-b), while the subextraction of nominal complements is disallowed (20c). The examples below are based on those presented by Bošković (2005), who notices that the exact opposite is true for English. The difference between English and BCS is accounted for by assuming the two languages differ in the size of their extended nominal projection (NP in BCS, DP in English), which then has a number of pleasant empirical consequences. Crucially, the NP/DP distinction can only account for the contrast in (20) and its reverse behavior in English if the *wh*-phrase in (20c) is the argument of the noun.

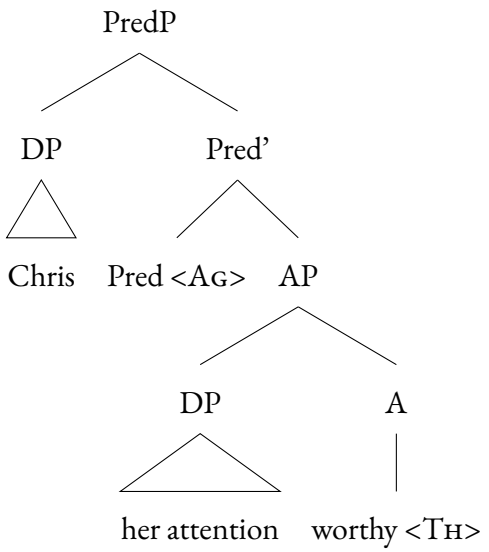
(20) a. Koje$_1$ je video [$t_1$ kuće]?
which is seen houses
'Which houses did he see?'

b. Iz kog grada$_1$ je sreo [devojke $t_1$]?
from which city is met girls
'From which city were the girls that he met?'

c. *Koga si voleo [prijatelje $t_1$]?
who are loved friends
'Who did you like friends of?'

When nouns and adjectives appear in predicative position, Baker assumes that one (presumably external) argument is introduced by Pred. But if this is so, then the most natural position for the other (internal) argument in his system is the specifier of A, as in (21).

(21)




Baker does not give any specific evidence against the existence of structures like (21). Instead, he focuses on justifying the existence of the category Pred, arguing that this category introduces the external argument of nouns and adjectives. The evidence consists of giving distributional arguments for the existence of Pred and cross-linguistic support for the existence of so-called predicative particles. As already noted in Bowers 1993, Pred may be responsible for the fact that predicative adjectives and nouns can be coordinated (22a). On the other hand, coordinating a predicative adjective and a verb is impossible, arguably because the two phrases belong to distinct syntactic categories (22b).

(22) a. I consider John [$_{\text{PredP}}$ crazy] and [$_{\text{PredP}}$ a fool].

b. *Eating poisoned food made Chris [$_{\text{PredP}}$ sick] and [$_{v\text{P}}$ die].

There are several issues with this diagnostic, however. First, there has been recent work arguing that the so-called Law of the Coordination of Likes is a myth. There seems to be no syntactic requirement that two coordinated elements be of the same category, cf. (23); the restriction is instead semantic (for recent detailed discussion, see Matushansky 2019, Patejuk & Przepiórkowski 2023 and the references there).

(23) The surgeon operated [$_{\text{AdvP}}$ slowly] and [$_{\text{PP}}$ with great care]. (Matushansky 2019:67)

Furthermore, assuming that (predicative) nouns and adjectives are always dominated by PredP makes it very difficult to account for the following fact: In small clause contexts, a selecting element is clearly able to distinguish between nominal and adjectival predication (Stowell 1981, 1983), as in (24). This is unexpected if all non-verbal predicates require PredP, since categorial selection is commonly thought to be restricted to the sisterhood relation.

(24) a. She proved the theory false / *a failure.

b. John wants Bill dead / *a doctor.

Baker also spends a substantial amount of time showcasing what are purportedly overt realizations of Pred, as illustrated with Edo in (25). As seen in (25) a so-called predicative particle necessarily appears only with non-verbal predicates. Without going into too much detail here, Matushansky 2019 quite convincingly shows that these predicative particles do not have the distribution we would expect from Pred. In the Edo case, for example, the particle disappears in small clause contexts, including resultatives (26a) and depictives (26b). More generally across languages, the appearance of these particles can depend on finiteness, tense, and predicate category, among other things (see Matushansky 2019 for details). None of this is expected if these particles are simply instantiations of Pred, understood as a category that always accompanies predicative nouns and adjectives.

(25) a. Èmèrí mòsé. (Edo, Baker 2003:40)
Mary be.beautiful.V
'Mary is beautiful.'

b. Èmèrí *(yé) mòsèmòsè.
Mary PRED beautiful.ADJ
'Mary is beautiful.'

c. Úyì *(rè) ókhaẹ̀mwẹ̀n.
Uyi PRED chief.N
'Uyi is a chief.'

(26) a. Òzó gìá ìrhùnmwùn khéréé. (Edo, Matushansky 2019:82)
Ozo cut grass small.ADJ
'Ozo cut the grass short.'

b. À bié Èmèrí mòsèè.
IMPRS give.birth.PST Mary beautiful.ADJ
'Mary was born beautiful.'

Of course, many languages do not have overt predicators or copulas of any kind, see (27) from K'iche'. Adjectives and nouns in K'iche' seemingly establish predication on their own and in the same way verbs do. In (27a-b), "pastness" may be indicated with the distal particle *kan(oq)* or an adverbial, but these devices are optional (see Bešlin 2023b for details). Note also the 1SG agreement marking in (27b), which seems to be established the same way it is with an intransitive verbal predicate (27c).[7]

(27) a. Tel ri ja. (K'iche', my fieldnotes)
open the house
'The house was/is/will be open.'

b. (Ri in) **in** ajchak.
the I **BISG** worker
'I was/am/will be a worker.'

c. (Ri in) k-**in**-b'in-ik.
the I NPST-**BISG**-walk-SS
'I am walking.'

Overall, the morphosyntactic evidence for the existence of Pred in languages like K'iche' (and many, many others, including English) is non-existent.[8] Hence, if Pred exists, it must in these languages be phonologically null. Further, formal semantics routinely takes NPs and APs to be predicates of individuals ($\langle e,t \rangle$). PredPs are also presumably predicates of individuals; this means that Pred must furthermore be semantically vacuous. A functional head that is vacuous at both interfaces is at least highly suspicious and the evidence that it is universally present with nominal and adjectival predicates has thus far fallen short.

Moving on to nouns, Baker argues that nouns are different from other LCs in that they bear a referential index–a syntactic element that allows them to serve as antecedents for binding. Note that, while the referential index typically corresponds to reference in the semantic domain, Baker is careful

[7] There is no agreement marker in (27a) because 3SG absolutive (B-)marking in K'iche' is null for all predicates.
[8] See Baker 2003 for reasons why the English copula *be* cannot be taken to instantiate Pred.

not to equate the two. This is because of examples like (28). As noted by Chomsky (1981:324), these NPs behave like any other NP for the purposes of the syntax, as illustrated by the binding example in (29). Hence, these NPs bear a referential index, but they do not correspond to "things" in the real world on any mind-independent understanding of "thing".

(28) a. the average man

b. the flaw in the argument

c. no letters

(29) The average man$_i$ hates himself$_i$.

To drive the point home, Baker shows the following contrast: Even though a genitive noun phrase and an adjective can both occur prenominally to denote the agent of an English derived nominal (30a-b), only the noun can bind a reflexive anaphor (30c-d), as observed originally in Kayne 1984:139.

(30) a. Albania's resistance

b. the Albanian resistance

c. Albania's destruction of itself

d. *the Albanian destruction of itself

Note that English uses two distinct constructions for strictly possessive meanings (the genitive nominal) and the very broad, intersective attributive meaning (the adjective), which can in some cases be interpreted as agentive, as in (30b). It is only the possessive phrase that can bind a reflexive anaphor. So, is it the possessive versus attributive meaning that is relevant for establishing the binding relation, or is it the syntactic category of the binder? To investigate this question, it is useful to look at a language like BCS, which can use possessive adjectives for both of these functions (31). In (31), the possessive adjective is ambiguous between a possessive and an attributive reading; one or the other reading can be brought out depending on the head noun (see Panagiotidis 2015:40 for parallel examples from Russian).

For arguments that the possessive phrase behaves fully like an adjective (and not a noun) for the purposes of the syntax, see Corver 1992, Zlatić 1997, Trenkić 2004, Bošković 2005, 2008, 2009.

(31) a. vuč-j-i rep
wolf-POSS.ADJ-M.SG tail.M.SG
'a/the wolf's tail'

b. vuč-j-a glad
wolf-POSS.ADJ-F.SG hunger.F.SG
'wolfish hunger'

What seems to have flown under the radar in the work on LC is that it is not, in fact, true that adjectives can never serve as binding antecedents. However, only possessive adjectives can do so. Proper nouns can form only possessive adjectives, and not attributive ones. An example of such a construction is given in (32), where the possessive adjective *Markovo* 'Marko's' binds the reflexive anaphor *sebe* 'self' (see also Zlatić 1997, Despić 2011 for similar examples). The binding possibilities of possessive adjectives in BCS are explored in much more detail in Despić 2011, 2013, who shows that possessive adjectives in BCS c-command out of NP, unlike genitive possessors in English. Hence, in (33), co-reference between the possessive adjective *Kusturicin* 'Kusturica's' and the pronoun is impossible, regardless of whether the the full pronoun or the clitic form is used (Despić 2011:49); cf. the acceptable English translation.[9]

(32) Mark-**ov**-$o_i$ opisivanje (samog) $sebe_i$
Marko-POSS.ADJ-NEUT.SG description.NEUT.SG own self
'Marko's description of himself'

(33) *Kusturic-**in**$_i$ film ($ga_i$) je ($njega_i$) zaista razočarao.
Kusturica's-POSS.ADJ.M.SG movie CL.ACC is him really disappointed
'Kusturica's$_i$ (latest) movie really disappointed him$_i$.'

Furthermore, country names in BCS can often form two distinct prenominal adjectives, one being the equivalent of the possessive genitive in English (34a), and one equivalent to the English attributive adjective (34b). Again, only the possessive adjective can bind a reflexive anaphor, despite both prenominal modifiers being adjectival.

[9]Despić attributes the difference to the idea that nominal phrases in BCS are NPs, while they are DPs in English.

(34) a. Srbij-in-o$_i$ prekoravanje svojih$_i$ komšija
Serbia-POSS.ADJ-NEUT.SG reprehension.NEUT.SG self's neighbors
'Serbia's reprehension of its neighbors'

b. *Srp-sk-o$_i$ prekoravanje svojih$_i$ komšija
Serbia-ADJ-NEUT-SG reprehension.NEUT.SG self's neighbors
'Serbian reprehension of its neighbors'

The observation that prenominal modifiers can only serve as binding antecedents if they are nouns is false. In languages like BCS, where prenominal possessors are adjectival, these adjectives can participate in binding to the same extent as the genitive-marked nouns in English. Bearing a referential index does not seem to be a universal distinguishing property of nouns.

One could object that the BCS possessive adjectives involve denominal adjectivization, so that it is still the nominal part of the expression that is responsible for enabling binding. I am not certain how one would argue against such a claim, so let me note the following. Even in languages where only nominal phrases seemingly bear a referential index, it is dubious that lexical N itself is responsible for this, rather than some higher functional structure.[10] For example, Pereltsvaig (2006:463) shows that precisely nominal phrases with an impoverished structure (*small nominals*) cannot serve as binding antecedents. This is illustrated in (35a) for Russian with the internal argument of a verb with the cumulative prefix *na-*, which Pereltsvaig argues lacks the D layer. Compare (35a) to (35b), where the binding antecedent is a full DP.

(35) a. *Bond na-priglašal krasotok$_i$ na dni roždenija drug druga$_i$.
Bond CUM-invited babes on days birth each other
*intended:* 'Bond invited (many) babes to each other's birthdays.'

b. Bond priglasil krasotok$_i$ na dni roždenija drug druga$_i$.
Bond invited babes on days birth each other
'Bond invited some/the babes to each other's birthdays.'

If nouns bear a referential index, then there should be no issue with binding *by* incorporated nouns, which have an impoverished structure (bare-N in many languages). I have not been able to find

[10] Expletive *it* cannot serve as a binding antecedent, despite being treated in generative theories of syntax as a nominal element.

any such examples in the literature. Baker 1995, for example, discusses some disjoint reference effects that arise if an incorporated noun is c-commanded by a potential binder (36), but there are no examples of biding by an incorporated noun.

(36) (Owira'a) yetshanis t-a-ke-wir-a-hkw-e'. (Mohawk, Baker 1995:14)
(baby) fears DUP-OPT-1sS-baby-∅-pick.up-PUNC
'She (the baby) is afraid that I will pick up the baby.' (disjoint)

There is also the issue of predicate nominals (37), which do not seem to be referential. More pertinently to Baker's proposal that all noun bear a referential index, see Moro 1995 for a discussion of some difficulties with binding inside predicate nominals, which are analyzed as having an impoverished structure.

(37) Marija je učiteljica.
Mary is teacher
'Mary is a teacher.'

More generally, there are many proposals in the formal syntactic and semantic literature that treat DPs but not NPs as referential, at least in some languages (e.g., Longobardi 1994, Chierchia 1998 and the references there). The existing evidence seems to point to the idea that referentiality is encoded by higher functional structure in the extended projection of the noun, at least in the languages that have such structure. If this is true, then bearing a referential index cannot be the distinguishing property of the LC noun itself.

While this may seem somewhat surprising, Baker 2003 remains the only serious attempt to define the universal syntactic properties of the LCs in the generative tradition; for a recent discussion of the issues, see Baker & Croft 2017.[11] I will therefore move on to a discussion of the purported semantic universals in LCs. Note that, while we have not found the categories *verb* or *noun* to have any universal syntactic properties, it is clear that the adult grammar of specific languages does treat verbs and nouns

[11] A possible exception to this is Pesetsky & Torrego 2004. However, Pesetsky & Torrego's system is build to account for a generalization which I do not believe holds cross-linguistically (as I already discussed in the main text), namely that nouns and adjectives cannot have DP complements.

as distinct from each other, and from adjectives, at least in the languages that have them (e.g., for operations like selection, movement, agreement, etc.). Therefore, the discussion here is not meant to deny the existence of the categories *verb* and *noun* in individual languages, but simply to point out that the attempt to find a distinct, universal syntactic property for each LC has thus far been unsuccessful.

### 2.3.2 "Semantic universals"

Before I critique a recent generative proposal for the view that LCs are 1-1 LF interpretable, I want to discuss briefly what formal (generative) theories of meaning have had to say about the interpretive properties of the LCs. Do formal semantic theories make use of distinctions that parallel the grouping of lexical items into LCs? In propositional logic, so-called *propositional variables* are used to represent entire propositions; no distinction is made based on the different kinds of predicates inside those propositions. For example, in an expression ¬P, P can stand for *John is a teacher*, *John is tall*, *John smokes*, *John loves Mary*, etc. Predicate logic similarly does not distinguish between the unary predicates TEACHER(j), TALL(j), and SMOKES(j).

Of course, most researchers interested in meaning do not represent predicates solely with the tools of first-order logic. Formal semanticists most often use a typed lambda calculus; still, the standard assumption is that nouns, adjectives, and intransitive verbs are all expressions of the same kind. In the lambda calculus, the meaning of the above predicates can be written down as $\lambda x$.teacher/tall/smokes($x$). Intransitive predicates are all of type ⟨e,t⟩, denoting functions from individuals to truth values (see Heim & Kratzer 1998, Coppock & Champollion 2024).

One may think that event semantics constitutes an exception to the observation that the notion of LCs (of a predicate) seems to play no role in formal semantics, because event semantics introduces eventualities (events and states) into the denotation of certain expressions, originally verbs. In event semantics, the event is explicitly represented. For example, for Davidson (1967), the sentence in (38a) has the meaning in (38b).

(38) a. John smokes slowly.

b. $\exists e$.smoke(j, $e$) $\wedge$ slow($e$)

However, as noted by Vendler (1967) and discussed at more length by Parsons (1990), the bolded subjects in (39a-b) refer to events despite their nominal syntax, with "nominal gerunds contribut[ing] the very same predicates to logical form as the verbs on which they are based" (Parsons 1990:17). While it can be argued that there is verbal structure inside the gerund that is contributing the eventive meaning, the same cannot be said for the pronoun in (39b).

(39) a. **The singing of the Marseillaise** took a long time.

b. **It** happened slowly.

Finally, Parsons argues that the same underlying state is found in the representation of verbs like *stand* (40a) and adjectives like *clever* in (40b), with the simplified denotations in (41).

(40) a. The painting stood in the corner.

b. Joan was clever in math class.

(41) a. $\exists s$. stand(p, $s$) $\wedge$ in the corner($s$)

b. $\exists s$. clever(j, $s$) $\wedge$ in math class($s$)

The bottom line is that research in formal semantics has thus far not uncovered any meaning component that directly tracks the LCs noun, verb, or adjective. The meaning components of a given expression–even the relatively general meaning categories like eventivity and stativity–are determined by factors beyond membership in a particular LCs. However, if a bespoke meaning could be found for each LC, this would arguably make the syntax-semantics interface simpler. This is the ambition of the recent proposal in Panagiotidis 2015, which I discuss next. As we will see, the proposal faces a number of challenges. This, I argue, is because the LCs do not map straightforwardly to any interpretive property.

#### 2.3.2.1 Panagiotidis 2015: LCs impose a perspective on their complements

The proposal in Panagiotidis 2015 is couched in the DM framework: lexical items are decomposed into acategorials roots and functional heads, including the categorizers *v*, *n*, and *a*.[12] The purported distinguishing properties of the individual LCs are attributed to the categorizers, rather than to the atomic units *noun*, *verb*, and *adjective*, as was the case in Baker 2003. This choice does not seem to me to have any obvious consequences for the proposal in question. As already noted by Baker, in proposals that adopt "full decomposition", the question of whether the LCs have universal syntactic or semantic properties is simply shifted from the atomic unit *noun* to the categorizing head *n*.

Panagiotidis 2015 rejects the ancient idea that LCs line up with distinctions in fundamental ontology, like the distinction between objects and events. As I have pointed out before, linguistic categories cannot be equated with conceptual categories (for example, nouns do not just denote things). Still, for Panagiotidis, categorial features are (1-1) interpretable at the meaning interface, also referred to as L(ogical) F(orm). I will now present what this work takes to be the LF content of *v* and *n*, critiquing the specific proposals as I go along. After this, I offer some empirical arguments which call into to question the proposed characterization of both categorizers. Note that the views espoused in Panagiotidis 2015 have many similarities with Langacker's (1987a, 1987b) approach to LCs presented in section 2.2, and many of the criticisms that follow also naturally extend to that proposal (see also Anderson 1997, Uriagereka 1999 for important predecessors).

Panagiotidis (2015:22) argues the categorizes *v* and *n* "introduce fundamental interpretive perspectives in which the categorizers' complements are to be interpreted" at the meaning interface. The interpretations of the formal features of *n* and *v* are given in (42), from Panagiotidis 2015:84. While the term *fundamental interpretive perspective* is not defined, the main difference between this approach

[12]DM traditionally treats the categorizers as functional heads, grouping them with heads like Tense or Number, rather than with roots. Panagiotidis 2015 claims that categorizers are the only *lexical* head, in that they "serv[e] as the elements that enable roots to structurally combine with bundles [...] of F[aculty of]L[anguage]N[arrow]-intrinsic features, selected from a pool made available by UG." As is well known, the lexical-functional distinction has long been a vexed issue in linguistics, and the line has become even more blurred with the advent of DM. I do not take a stance here as to whether the categorizers are lexical or functional, because it is not clear to me what criteria are supposed to distinguish between the two.

and traditional work in formal semantics is that the meanings of the resulting predicates are not defined truth-conditionally. As will become obvious when we look at some examples, a perspective cannot be a predicate that must be true for the speaker to speak truthfully.[13]

(42) a. An [N] feature imposes a sortal perspective on the categorizer's complement at LF.

b. A [V] feature imposes an extending-into-time perspective on the categorizer's complement at LF.

The characterization of [N] as involving sortality is understood to incorporate the criteria of *application* and *identity*, building on Prasada 2008 and Acquaviva 2014. The criterion of application "means that the representation is understood to apply to things of a certain kind, but not others. Thus, the sortal DOG allows us to think about dogs, but not tables, trees, wood or any other kind of thing" (Panagiotidis 2015:85, citing Prasada 2008:6). I could not access the original unpublished manuscript cited here, but I should note that the prose is somewhat confused given the definitions of the categorial features in (42) and the overall proposal. The first sentence talks about the predicate applying or not–which is a property of any predicate. It does not explicitly mention "thinking about" the things to which the predicate applies, so the appearance of "think about" in the second sentence, which is presented as following from the first, is out of the blue. Moreover, if we say that (the noun) *dog* allows one to think about dogs, but not tables or wood (or theories or weddings), does it make any less sense to say that (the verb) *dog* allows one to think about dogging, but not tabling or theorizing?

As for the criterion of identity, it is presented as follows: "If we take a kind (e.g., the kind *person*), it has instances (i.e., persons) which are particulars and which do not themselves have instances. In this way, being a person is different from being tall: only the property *person* identifies a type of entity." (Panagiotidis 2015:85). Panagiotidis exemplifies the distinction by looking at the the noun *sleep* and the verb *sleep*, which arguably encode the same concept, but impose different interpretive perspectives on it, as he expresses in the following paragraph:

[13]Note that Panagiotidis takes perspectives to be imposed not on things in the world, but rather on concepts themselves. What would perhaps make more sense is that we take various perspectives on things in the world, and possibly that these perspectives are themselves concepts. Changing this assumption would arguably not make a substantial difference for the rest of the proposal.

> "The noun *sleep* forces the viewing of this concept to be a sortal one, which is to say that the perspective over the concept of sleep that the nominal category imposes is of sleep as some type of virtual object or substance. We can therefore *lose our sleep* (like we lose our keys or lose blood), *get more sleep* (like we can get more air, food or water); we can also talk about *morning sleep* being different or sweeter than *early evening sleep*, and so on. On the other hand, the verb *sleep* forces us to view the concept of sleep as a subevent, as extending into time, which readily offers itself to temporal modification and so on. Similarly, the perspective over the concept of sleep that the verbal category imposes is of sleep being expressible as a time interval, as something potentially having duration (long, short etc.)." (Panagiotidis 2015:88)

One immediately puzzling aspect of the proposal is that it assumes a tight connection between the object/event distinction and the sortal/nonsortal distinction, when the two are in fact completely orthogonal. It is routine in the semantics and philosophy literatures to talk about event sortals.

Moreover, it is not clear to me that any of the listed properties are doing the intended work of identifying what is proprietary to the categorizers *v* and *n*. For example, if sortality tells us how to count things of the same kind, and mass nouns like *sleep* in the above quote are sortals because we can say *more/less sleep*, then certainly the verbal predicate *run* in *Mary ran once* must also count as a sortal.[14]

Furthermore, such counting is impossible with predicative nouns. For example, it is impossible to utter (43) to convey the perfectly reasonable thought that the water content of the bookshelf is smaller than the water content of myself. If categorizers are 1-1 LF interpretable, then a property that is not shared by all members of a LC but is shared by at least some members of a different LC can presumably not be due to the categorizer. This is particularly pertinent because Panagiotidis criticizes the approach in Baker 2003 for focusing on nouns in argument positions, and concludes that nouns are probably primarily a predicative category. Yet, the examples in the quote above (which constitutes the gist of his proposal) again focus on argument nouns.

(43) *This bookshelf is less water than I am.

Furthermore, the fact that only the noun *sleep* and not its verbal counterpart is possible as the complement of verbs like *get* or *lose* is already encoded in the c(ategory)-selecting properties of the verb

[14]Note, though, that treating mass nouns as sortals is already at odds with the traditional view in the philosophy literature.

in question, which require a nominal complement. Although there have been attempts to reduce category selection to semantic selection, such attempts have faced insurmountable problems which are by now well-known in the literature.

It is also not clear to me what it means to that say only verbal *sleep* can be expressed as having duration, since one can equally say *They had a long sleep* as *They slept for a long time*. More generally, this characterization of verbs is dubious, as many nominal and adjectival predicates can be said to "extend into time" to the same extent as their verbal counterparts. For example, it is unclear to me that the bolded nominal phrase (gerund) in (44a) and verbal small clause in (44b) differ in any way along the time dimension.

(44) a. **John's kissing her for a long time** surprised me.

b. I saw **John kiss her for a long time**.

The same criticism can be applied to be pair of examples in (45), with the added bonus that the nominal predicate cannot be said to be derived from the verbal predicate in any way. The predicate is a noun in (45a) and a verb in (45b), yet they are both understood as individual-level predicates which translate to English *know*. The nominal predicate "extends into time" to the same extent as its verbal counterpart. It also denotes an eventuality, rather than a(n abstract) object.

(45) a. **R-eta'm** ri K'iche' ri w-ati't. (K'iche', my fieldnotes)
A3SG-know DET K'iche' DET A1SG-grandma
'My grandma knows K'iche'.'

b. **K-∅-u-ch'ob'o** ri K'iche' ri w-ati't.
NPST-B3SG-A3SG-know DET K'iche' DET A1SG-grandma
'My grandma knows K'iche'.'

How do we know the category of the predicates in (45)? For (45b), this is quite straighforward, as only verbs in K'iche' inflect for tense, and only transitive verbs are simultaneously marked for A (ergative) and B (absolutive). For (45a), I argue in Bešlin 2023b that the nominal character of the predicate can be established by observing examples like (46), with the derived transitive verb *eta'maj* 'learn'. De-

rived transitive verbs are transitive verbs "derived from other parts of speech such as intransitive verbs, nouns, positionals, adjectives" (Sis Iboy & López Ixcoy 2004, my translation). Under the reasonable assumption that the transitive verb *eta'maj* 'learn' in (46) is derived from the predicate *eta'm* 'know' in (45a), *eta'm* cannot be considered a verb. The reason is that the suffix *-aj* only attaches to intransitive verbs, and intransitive verbs always carry B-marking, never A-marking, unlike *eta'm* in (45a). One type of predicate that consistently carries A-marking and has a complement (but does not inflect for tense) are so-called relational nouns; in Bešlin 2023b I argue on these grounds that *eta'm* is a relational noun. The reader is also referred to Ilkhanipour 2024 who argues extensively against the proposal in Panagiotidis 2015 based on the notional of nominal temporality.

(46) Ri ak'al **k-∅-r-eta'm-aj** k-∅-b'in r-uk' jun b'ineb'al.
the boy NPST-B3SG-A3SG-know-VTD NPST-B3SG-walk A3SG-RN one walker
'The boy is learning to walk with a walker.'

The same remarks can be applied to adjectives when compared to verbs. As seen in (47), English has synonymous verbal and adjectival predicates like *envy* and *envious* or *like* and *fond of*, and I know of no successful attempts to argue that these predicates differ along the time dimension.

(47) a. The event made John envy/envious of Mary.

b. The event made John like/fond of Mary.

A similar point can be made by looking at which predicates are encoded by which LCs within a language and across languages. For example, the K'iche' predicates equivalent to the English *happy* and *sad* are expressed by verbs and they have no non-verbal equivalents. Note also that both of these can be understood as stage-level or individual-level predicates, just like in English. There are about a dozen simple adjectives in K'iche', see (49) for some examples. Therefore, the state of affairs in (48) does not surprise any Mayanist. Two questions arise then. First, are we supposed to conclude that the perspective of the verbal predicates in (48) is one of *extending into time*, in a way that is different from the adjectival predicates in (49)? And second, are we to think that K'iche' speakers impose an *extending*

*into time* perspective on the concept HAPPY or SAD in a way that is different than speakers of English? I think that very few linguists would want to commit to such conclusions. The same point can be raised, of course, in regard to languages in which all concepts packaged as adjectives in a language like English are expressed as (stative) verbs, as has been claimed for Korean (e.g., Kim 2002, Choi 2019).

(48) a. K-∅-ki'kot ri w-ati't. (K'iche', my fieldnotes)
NPST-B3SG-happy.V the A1SG-grandmother
'My grandmother is happy.'

b. K-in-b'ison-ik.
NPST-B1SG-sad.V-SS
'I am sad.'

(49) a. Yawab' jun ak'al pa ri nu-tinamit. (K'iche', my fieldnotes)
sick one child PREP the A1SG-village
'A child is sick in my village.'

b. Tz'il ri mexa.
dirty the table
'The table is dirty.'

With this, I will close the critique of the proposal in Panagiotidis 2015. Let me also note that chapters 3 and 4 are essentially an extended argument against the idea that only verbs can encode the *extending-into-time* perspective, since I show that eventive participles in a number of languages are adjectives for the purposes of the syntax and morphology, but their interpretation does not differ from that of the verbal forms employed for the same function in other languages.

The bottom line is as follows: We may not want or need the LCs to be 1-1 LF intepretable. If formal semantics is on the right track, we do not need to say anything about the meaning of LCs to arrive at the semantic interpretation of a sentence. We may think of the contrasts between verbs and nouns as similar to the contrast between distinctly case-marked DPs, in at least two ways. First, the difference in category/case has an impact on the distribution of the phrase in question. Second, the intuition is that a nominative-marked DP and an accusative-marked DP have a different meaning, but this does not compel us to go searching for the meaning of nominative (especially independently from

the meaning of the DP). I believe that the same reasoning extends to LCs, and I am in full agreement with Newmeyer (1998) who writes that "the possibility of providing a semantic definition of the categories 'noun', 'verb', and 'adjective' seems remote.". Note that my view here is fully compatible with the Autonomy of Syntax thesis (Chomsky 1957) and the Abstract Morpheme hypothesis (Marantz 2013; Embick to appear for recent discussion), but in stark contrast with many generative and non-generative approaches which maintain varying degrees (and directions) of syntax-meaning isomorphism.

### 2.3.3 A note on adjectives

As I noted at the beginning of this chapter, most accounts of LCs focus on nouns and verbs; neither Baker 2003 nor Panagiotidis 2015 considers adjectives in much detail. For Baker, adjectives are characterized by the *absence* of features relevant for nouns (referential index) and verbs (projection of a specifier). Yet, grammars of individual languages are clearly able to use categorial information (including information about adjectivehood), and it is not clear what referring to an absence of a feature would mean. Furthermore, as noted in Panagiotidis 2015, if adjectives were the typologically unmarked, "elsewhere" category (Baker 2003:230), then we may expect them to (i) be universal, and (ii) be morphologically simplex. In fact, the universality of the category adjective has been repeatedly questioned across theoretical frameworks (e.g., Dixon 1977, Hopper & Thompson 1984, Schachter 1985, Bhat 1994, Hale & Keyser 2002, Kim 2002, Shopen & Schachter 2007, Choi 2019, *pace* Dixon 2004). As for the morphological complexity of adjectives, looking across the Indo-European languages (which robustly have the category), it seems that adjectives can be derived from the other two categories–nouns and verbs–to the same extent that those categories can be derived from each other and from adjectives. Deverbal adjectivization is discussed at length in chapters 3 and 4, and deadjectival nominalization is discussed in chapter 6.

As expected given their controversial status, there exists no worked-out formal proposal for a distinctive (universal) syntax of adjectives. As for their semantics, it has been proposed that adjec-

tives, at least in the languages that have them, introduce a *stative* meaning component (e.g., Parsons 1990, Meltzer-Asscher 2011b, Gehrke 2015). Authors seem to differ in whether they take this to be the distinguishing property of all adjectives, however. For example, Parsons (1990) merely notes that "there is a tendency of Modern English to express state meanings by adjectives instead of by verbs." (Parsons 1990:90). On the other hand, Meltzer-Asscher (2011b) and Gehrke (2015) argue that adjectives/adjectivization necessarily introduces a stative meaning. However, as I have noted in the previous section, there are significant overlaps in the meaning components of stative predicates, regardless of their LCs. Furthermore, chapters 3 and 4 argue against the idea that adjectives are necessarily stative, based on the fact that eventive participles in a number of languages pattern morphosyntactically with (deverbal) adjectives just like stative participles.

### 2.3.4 Conclusion

We have seen that the evidence for universal syntactic properties of the different LCs does not stand up to scrutiny. Furthermore, there is no 1-1 mapping between an item's LC and its meaning. In other words, the way one arrives at the meaning of a lexical item is not trivially based on the categorial features that determine its distribution. Or, as Jackendoff 1992 put it succinctly: "the mapping between conceptual categories and syntactic categories is many-to-many" (Jackendoff 1992:23).

If my conclusions about the syntax and semantics of LCs are both correct, then there is no sense in talking about the cross-linguistic properties of LCs; instead, LC represent language-particular generalizations which cannot be carried over from one language to another one. As I have mentioned, this conclusion is fully in line with the Autonomy of Syntax thesis (Chomsky 1957) and the Abstract Morpheme hypothesis (Marantz 2013; Embick to appear for recent discussion), but in stark contrast with many generative and non-generative approaches which maintain varying degrees (and directions) of syntax-meaning isomorphism. I should note, however, that a number of researchers have reached this conclusion from different angles, both in the functionalist and generative literature, though it is

certainly the minority view (see Croft 2001, Stabler & Keenan 2003, Haspelmath 2007, *et seq.*).

Based on the current state of our knowledge, it seems that positing specific innate categori(zer)s like *v*, *n*, and *a* may not be necessary or helpful, particularly because it is unclear what information about them would be innate.[15] Of course, if the LC distinctions are not themselves universal or innate, then we must find an alternative answer for why we robustly see (at least) a noun-verb distinction in all human languages. Clues to a possible answer lie in the way non-linguistic knowledge is organized in the mind/brain. Research by Elizabeth Spelke, Susan Carey, and colleagues has found that babies in the pre-linguistic stage (at 9-12 months) distinguish canonical countable objects from properties, and show different reasoning patterns appropriate to these different conceptual categories (e.g., Xu, Carey & Welch 1999; see Spelke 2022 for an overview). Spelke 2022 calls object-perception one of the areas of *core knowledge*, knowledge which is observed so early in that it is extremely likely to be innate. Babies could therefore come pre-equipped with the expectation that countable objects and properties may be represented differently in language.

We may further ask where this early conceptual knowledge comes from. The distinction may have its basis in the way that knowledge of individuals is organized in the brain–specifically, the hippocampus–which plays a pivotal role in the encoding of propositional knowledge into long-term memory. Lau (2023) argues that hippocampal memory has its own characteristic data structure, specialized for indexing properties of individuals and encoded in argument-predicate format. This kind of data structure likely developed for the purpose of navigation (in mental maps) and has also been argued to be present in vision (in the binding of features to corresponding objects and the encoding of structured relations) and probabilistic reasoning (see Lau 2023, Quilty-Dunn, Porot & Mandelbaum 2023, Yu & Lau 2023 for references and discussion). The argument-predicate format for storing and recalling information also motivates a distinction between two kinds of *linguistic* entities: those that stand for individuals or singular terms versus those that stand for types or general terms (see Strawson 1959, 2004 for useful

[15] It is also widely recognized that simply positing innate categories does not eliminate the presumed learning problem for the child, it simply creates a different one, the so-called *linking problem*. The linking problem for LCs can be formulated as follows: Assuming innate LCs, how does the child link them up to the distributional patterns they observe in the input?

discussion). Children then may be born expecting that these concepts will be encoded in categorially different ways. This view can possibly explain the (true) generalization that all physical objects are encoded in language as one LC (which we call *noun*), without making the too strong and obviously false prediction that all nouns only denote objects.

# Chapter 3: A case study on passive participles

## 3.1 Introduction

Participles have long puzzled linguists because they exhibit behaviors characteristic of both verbs and adjectives. For example, the participle in (50a) is arguably generated in the same position as a verb in a typical active sentence, whereas its counterpart in (50b) can appear as a prenominal modifier—a canonical adjectival position.

(50) a. The window was opened (by the teacher).

b. the (carefully) opened window

In this chapter, I examine *eventive passive participles*–those that denote an event, as in (50a)–and *resultative passive participles*–those that denote a state resulting from a prior event, as in (50b); see Kratzer 2000, Embick 2004, i.a.[1] I argue that the classification of passive participles into verbal (50a) and adjectival (50b) should be rejected. Instead, I attempt to show that all passive participles in a number of languages (Bosnian/Croatian/Serbian (BCS), Greek, English, and German) should be uniformly analyzed as adjectives that embed varying amounts of verbal structure. I furthermore argue that there is a difference between BCS and Greek, on the one hand, and English and German, on the other, which allows resultative participles to appear with agentive *by*-phrases in BCS-like languages, but not in English-like languages, as seen in the translation of (51).[2] This difference stems from the fact that

[1] In order to streamline the discussion, I will set aside what Embick 2004 terms *(purely) stative participles*, namely elements that have the form of a participle, but denote a simple state–a state without any event implications. These elements are uncontroversially adjectival and it is not even clear that they should contain any verbal structure (see the discussion in Embick 2004).

[2] The BCS examples will never feature proper names in the *by*-phrases. This is because proper names in *by*-phrases are

resultative participles in BCS-like languages encode perfective viewpoint aspect on the verb stem. Since resultative participles in these languages include Asp, the ability to include lower portions of the verbal structure (including VoiceP, the projection that introduces the external argument) follows.

(51) Vaza je ostala **po**-lomljena od strane nestašnih patuljaka.
vase COP.3SG remained **PF**-broken by side mischievous dwarfs
*lit.* 'The vase remained broken by the mischievous dwarfs.'

Before we move on to the chapter's main proposals regarding the categorial status of passive participles (sections 3.3 and 3.4), and their internal structure (section 3.5), I will briefly sketch out the approach to passive participles in work that assumes the existence of a generative lexicon, and show how it fares in the context of more recent theories of word-formation, such as the Distributed Morphology (DM) framework. Because I will conclude that lexicalist approaches are unable to account for the properties of the different types of passive participles in a principled way, the upcoming section also elaborates on some of the basic tenets of DM, which I adopt in the chapter.

## 3.2 Theoretical background

Since at least Wasow 1977, a categorial distinction has been assumed in the generative literature between participles that are verbs (50a) and those that are adjectives (50b). Levin & Rappaport 1986 have claimed that the distributional pattern observed in (50) goes hand in hand with a subtle difference in meaning: Whereas verbal participles have an event reading, adjectival participles are associated with a state reading. Furthermore, works that assume a generative lexicon have proposed either (i) that verbal participles are derived in the syntax, and adjectival participles in the lexicon (Wasow 1977, Horvath & Siloni 2008, Meltzer-Asscher 2011a), or (ii) that both types of participles are derived in the lexicon (Bresnan 1982, Levin & Rappaport 1986).

independently dispreferred for all passives (eventive and resultative) in BCS. Futhermore, some speakers generally dislike agentive *od strane*-phrases '*by*-phrases' in passives. This is unlikely to be because passives for these speakers lack agents completely, since agent-oriented adverbs like *namerno* 'deliberately' remain available. For this reason, these speakers' judgments were excluded in determining whether both eventive and resultative participles allow agentive *od strane*-phrases '*by*-phrases'.

There are several issues with the lexicalist treatment of passive participles. First, since adjectival participles are, by hypothesis, formed in the lexicon, it is predicted that they should be treated by the syntax as ordinary adjectives. However, (Kratzer 2000) and (Embick 2004) show that adjectival passives can be phrasal in nature and exhibit patterns that are impossible with ordinary adjectives, such as modification by manner adverbials, cf. (52a-b). This suggests that the syntax does not treat adjectival participles as simplex adjectives. In section 3.4 I furthermore show that all passive participles in BCS have the external syntax and morphology of adjectives, which makes accounting for the differences between them in a principled way virtually impossible in a lexicalist framework.

(52) a. a hastily blackened wall

b. *a hastily black wall

More broadly, under the view that there exists a separate generative lexicon in addition to a generative syntax (Chomsky 1970), we should in principle not expect the composition of 'words' to resemble the composition of larger syntactic units in any systematic way. Whether such a lexicon exists is, of course, an empirical question. However, research in the past few decades has provided extensive argumentation, both empirical and conceptual, that postulating a generative lexicon is at best superfluous (see, e.g., Baker 1985, Baker 1988, Lieber 1992, Marantz 1997, Alexiadou 2001, Bruening 2018, i.a.). This new line of thinking has had a profound impact on the empirical domain that is of interest here, with most works on passive participles in the past two decades rejecting the lexicalist view (e.g., Anagnostopoulou 2003, Embick 2004, Alexiadou & Anagnostopoulou 2008, Sleeman 2011, McIntyre 2013; *pace* Meltzer-Asscher 2011a). One major argument for doing so is that word-formation rules claimed to account for the existence of adjectival passives amount to a duplication of operations already available in the syntax. This is because, whatever the right formulation of the relevant lexical rules, the majority of them must arguably also be available for verbal passives, only in the syntax (see Levin & Rappaport 1986:624).

I therefore reject the lexicalist position, and pursue an approach to word-formation broadly in

line with the DM framework. In particular, I adopt the view that syntax is the only generative component in language (Halle & Marantz 1993, 1994). There is no generative lexicon; rather, morphological structure is (derived from) syntactic structure. Hence, if any subclasses of passive participles do turn out to exist, they must be shown to follow from structural (or featural) differences, rather than differences in the identity of the grammatical component in which they are derived. I will also adopt the idea that acategorial roots are the minimal open-class units of (morpho)syntactic computation (Marantz 1997). In order to be realized, roots must be categorized by merging with (at least one) functional head. Guided by this assumption, I will argue that passive participles do not start out with a predetermined categorial feature such as 'verb' or 'adjective', but that they instead become categorized in the course of the derivation. Crucially, this derivational view of categorization allows us to argue (in the presence of suitable evidence) that a categorial head may be added to an already categorized element.

The remainder of this chapter is organized as follows. Section 3.3 inspects the purported distinction between verbal and adjectival participles in detail. More specifically, it examines the main diagnostics that have been proposed to distinguish between the two types of participles, and shows that they do not diagnose category differences, not even in English. Section 3.4 argues, based on a combination of morphological and distributional facts, that a categorial distinction between verbal and adjectival passives cannot be maintained for BCS, despite BCS having both eventive and resultative passives. Instead, all BCS passive participles are shown to be adjectives (similar proposals for English can be found in Freidin 1975, Emonds 2006, Lundquist 2013, and for Arabic in Fassi Fehri 2013), which embed varying amounts of verbal structure. I argue that an analysis of all passive participles as being uniformly deverbal adjectives is viable for a number of other languages where they have previously been analyzed in terms of a distinction between verbs and adjectives. In section 3.5, I discuss the effect of perfective and imperfective marking on passive participles in BCS, and the role of aspect more generally. I then tackle the issue of why resultative participles may combine with agentive *by*-phrases in BCS and Greek, but not in English and German. I propose that the two types of languages use different strategies to derive resultative participles. For BCS-like languages, this is done with the perfective aspect, which attaches above

the external argument, and explicitly implicates the completion of the underlying event. In English-like languages, resultative participles are derived using a dedicated stativizing morpheme which selects *v*P–a verbal projection that excludes the external argument. In light of this proposal, I address the claim that adjectival passive participles in English can include external arguments (McIntyre 2013, Bruening 2014, Alexiadou, Gehrke & Schäfer 2014). While this is in principle true, I show, contra Bruening 2014, that English resultative participles cannot include true agentive *by*-phrases. Section 3.6 summarizes the main points and discusses some questions that remain for future research.

## 3.3 Existing diagnostics do not test for category differences

I now turn to a discussion of the diagnostics that have been argued to distinguish between so-called verbal and adjectival participles in English. I will continue to use the terms 'verbal' and 'adjectival' participle in this section of the chapter, in order to make clear what I am arguing against. I will switch to the terms 'eventive' and 'resultative' participle in section 3.4, where I discuss BCS participles which, as I will show, are unambiguously adjectival.

There are a number of ways in which the distribution of English adjectival and verbal participles has been argued to differ: The former are said to appear as prenominal modifiers and as complements of verbs such as *seem* and *remain*, and to allow *un-* prefixation (Wasow 1977, Levin & Rappaport 1986, Embick 2004). In order to test the claim that the enumerated differences stem from a category contrast, proponents of this view often rely on the assumption that only verbal passive participles can be modified by agentive *by*-phrases, presumably because English adjectival passives lack implicit initiators (Levin & Rappaport 1986, Baker, Johnson & Roberts 1989, Grimshaw 1990, Embick 2004, Emonds 2006, Sleeman 2011; see Kratzer 2000 for German).[3] It has also been argued that only verbal participles can be derived from ditransitive verbs and followed by subcategorized material, and that only they allow post-modification by adverbs. In the remainder of this section, I discuss the proposed diagnostics, and

[3] This is not an uncontroversial assumption; I address it in more detail in section 3.5.2.

conclude that none of them truly test for a category contrast between verbs and adjectives.

A clarification is in order before we proceed. The literature on passives recognizes two broad classes of participial *by*-phrases: event-related (agentive) and state-related *by*-phrases (Rapp 1996, 1997, Gehrke 2011, 2013, 2015, McIntyre 2013, Alexiadou, Anagnostopoulou & Schäfer 2015). Event-related *by*-phrases are arguably associated with the participles' underlying verbal structure, while state-related ones are associated with the adjectival layer. Since only the former type is supposed to be unavailable in English adjectival passivess, only they will be used in the discussion of examples that have been argued to be verbal.

### 3.3.1 Prenominal modifiers

A number of authors have proposed that participles in the prenominal modifier position must be adjectives, and that their verbal counterparts are illicit in this context. As shown in (53a-b), attributive participles do not allow modification by agentive *by*-phrases, which have been argued to combine only with verbal participles in English. However, this ban is not limited to *by*-phrases; no material is allowed to intervene between the participle and the noun (53c). What seems to be at issue here is an independently identifiable restriction that holds across a number of languages, requiring that a prenominal modifying expression be head-final (the Head-Final Filter, Williams 1982). This accounts for the unacceptability of (53b-c), but crucially also (53d), which is unambiguously adjectival. Note that it is possible for both the participle and the *by*-phrase to appear to the right of the noun, as in *a cake baked by the students*, but these modifiers have been argued to project a full-fledged CP (see Sleeman 2011). I will therefore not address them here.

(53) a. a baked cake

b. *a baked by the students cake

c. *a baked yesterday/in the kitchen cake

d. *the fond of Sam boy

A contributing factor here is that even among those languages that exhibit the Head-Final Filter, English is special in that it also disallows PPs to appear to the left of a prenominal modifier. Such placement of PPs is possible, for example, in BCS (54a), Dutch (54b), and German (54c), and in these languages the passive participle and the *by*-phrase happily co-occur.

(54) a. od strane naše učiteljice otvoreno pismo
by side our teacher opened letter
b. de door Jan geopende brief (Sleeman 2011:624)
the by John opened letter
c. der vom Kellner eingeschenkte Wein (Rapp 2000:396-7)
the by-the waiter poured wine

The ban on agentive *by*-phrases in this environment in English is then likely due to its rigid word-order rules. This diagnostic can therefore not be used to make clams about the category status of the participle.

### 3.3.2 Complements of *seem*

Another widely used diagnostic is the participles' (in)ability to head the complement of verbs like *seem* and *remain* (55), which can take adjectival, but not verbal complements. Again, the ungrammaticality of (55) with an agentive *by*-phrase has been used as evidence that such participles are verbs.

(55) The suitcases seemed / remained packed (*by Tiyana's friends).

However, as I will show, the claim that eventive participles are illicit in this environment because of their categorial status is inadequate. A more promising account of this data, I argue, combines the fact that the participle in (55) does indeed have an eventive component with the fact that verbs like *seem* have a specific requirements that their bare complements be stative (see also Matushansky 2002). Consider (56a), which demonstrates that *seem* can take nominal complements. Despite this, there is no eventive noun that could take the place of *a fool/bastard* in (56a). In fact, Matushansky (2002) concludes that only so-called *degree nouns* can be the bare complements of verbs like *seem*, cf. (56a) and (56b). There

is no categorial contrast between the nouns *fool* and *wizard*, but rather a difference in meaning, which determines their (in)compatibility with *seem*.

(56) a. He seemed a fool/bastard his whole life.

b. *He seemed a wizard/Arthur's friend.

Additionally, the contrast in (57a-c) is meant to show that *destruction* can appear as the complement of *remain* when it is resultative, but not when it is an eventive, argument-taking nominalization. The issue in (57b) is clearly the eventive interpretation of the noun, not its categorial status.

(57) a. There remained much destruction throughout the city.

b. *There remained much destruction of the city by those left behind.

If we extend the same kind of reasoning to (55), the argument for invoking a categorial contrast within the class of passive participles disappears. It rather seems that, within the class of participles formed from change of state verbs, which can be interpreted as either eventive or stative, the agentive *by*-phrase precludes a stative reading. This then clashes with the requirements of the participles' selecting heads, explaining the badness of (55). Note that this stativity requirement is in addition to, not instead of, the requirement that these verbs take *a*P complements. Therefore, **The children remained love their parents* is bad because *remain* cannot take a verbal complement.

### 3.3.3 Negative *un-*

It has also been argued that only adjectival participles freely combine with the negative prefix *un-* (58a). The reasoning behind this claim is that negative *un-* generally attaches to adjectives (58b), whereas verbal forms may only compose with *un-* if its meaning is reversative (58c).

(58) a. The road seemed unmarked and dangerous.

b. The child seemed unhappy.

c. The truck was unloaded by the workers.

Now, whether we should treat the prefixes in (58a-b) and (58c) as two distinct (but homonymous) morphemes, or as one morpheme that can receive two distinct interpretations depending on the environment it appears in, is an open question (see Horn 1989). However, given (i) the observation that the agentive *by*-phrase forces an eventive interpretation of the English participle and (ii) the fact that one *un-* form is shared by the two meanings, of which only the reversative is itself eventive, it is not surprising that (58c) should get the reversative reading.[4] It is also not surprising that the prefix *un-* in (58a) should get a negative interpretation given our conclusion that *seem* requires stative complements, and would therefore not be compatible with the reversative interpretation of *un-*. None of this, I believe, bears directly on the categorial issue. Note, however, that it is not the case that negative *un-* can *only* attach to participles in typical stative contexts and unaccompanied by agentive *by*-phrases, as seen in (59). I set aside this issue for now, and return to in section 3.5.2.[5]

(59) a. Word-final stops are often unreleased by speakers of US English.

b. The testimony was unchallenged by the appellant.

### 3.3.4 Word order with modifiers

One diagnostic that specifically claims to single out 'verbal' passive participles is post-modification by adverbs. As shown in (60), eventive passive participles allow post-modification by adverbs (60a), like finite verbs (60b), and unlike resultative passive participles (60c). According to Meltzer-Asscher 2010,

[4]Notice that (58c) becomes ambiguous once the *by*-phrase is removed. Once the *by*-phrase is removed, we get a classical case of structural ambiguity—viz. [un [load ed]] (negative stative) vs. [[un load] ed] (reversative eventive).

[5] Interestingly, while combining passive participles with negative *un-* and an agentive *by-* phrase is not always possible in a main clause (ia), it is fine in a reduced relative clause (ib).

(i) a. *The bills were unpaid by our parents.

b. The bills unpaid by our parents will remain for us to pay.

This contrast also obtains in BCS (where participles that denote states resulting from prior events can otherwise be modified by agentive *by*-phrases, as I discuss in section 3.5.1), and the reasons for it are poorly understood. One possibility is that the contrast is due to some as-of-yet unidentified semantic differences between finite main clauses and reduced relative clauses. Another possibility is that the contrast in (ia-b) is due to the structural differences between the two. In other words, it could be that *paid by our parents* is a chunk of verbal structure that, under the particular syntactic circumstances that occur in reduced relative clauses, can be stativized in its entirety, and therefore is a candidate for negative *un*-affixation.

this contrast shows that the eventive participle is a verb. Meltzer-Asscher does not give an analysis of these facts, but merely points to the pattern in (60), a legitimate move.

(60) a. The silver was polished carefully. (*eventive participle*)

b. He polishes the silver carefully. (*finite verb*)

c. *The silver seemed polished carefully. (*resultative participle*)

However, there is an explanation for the contrast in (60) which does not appeal to a categorial contrast between eventive and resultative participles. Namely, it could be that the verb/participle is moving over the adverb, and it can do so in (60a-b), but not in (60c). To see this, consider (61), where the adverb *heavily* intervenes between the verb *relied* and the PP *on me*.

(61) He relied heavily on me.

There are several pieces of evidence that *rely* and the *on*-phrase must combine first, despite being separated by the adverb in the surface sentence. Note first that *rely* selects an *on*-phrase as its complement in English, even though there is nothing about the meaning of the expression that necessitates this (cf. Spanish *depender de*). This is then a straightforward case of l-selection, which is commonly taken to be maximally local (only available under sisterhood). Moreover, the adverb *heavily* is modifying the event of relying on me, which suggests that *rely on me* needs to compose first, before *heavily* enters the structure. A related point about the meaning which conveys the same point is as follows: The merger of *depend* and *on* results in a special (root-specific) rule of interpretation, that is, a special meaning in the sense of Marantz 1997, 2013. While the locality conditions on such special meanings are not fully agreed upon, the following is widely accepted: Adjuncts that do not condition the meaning rule cannot be inside the structure that triggers a root-specific rule. Hence, *rely* and *on me* must form a constituent to the exclusion of the adverb.

Furthermore, the fact that the complement of *at* is pronominal makes this PP a bad candidate for extraposition. Therefore, the only way to derive the word-order in (61) is to assume that the verb

moves above the adverb, for example to Voice, the projection that introduces the external argument (e.g., Harley 1995, Kratzer 1996, Marantz 1997). Independently of these facts, I will argue in section 3.5 that the English resultative passive participle (unlike the eventive participle) lacks the Voice layer. This will mean that the verbal material cannot move leftward/upward, and will also immediately give us an explanation for the contrast in (60).

### 3.3.5 Subcategorization

Wasow (1977) suggests that some passive participles must be verbs because they are followed by subcategorized material that is selected (62a). He argues that this is impossible with pure adjectives (62b). However, this observation is empirically unjustified, given that some adjectives have selectional requirements. For example, *reliant* in (62c) selects for a PP headed by *(up)on*.

(62) a. John is considered a fool.

b. *John is obvious a fool. Wasow (1977:341)

c. John is reliant (up)on his parents.

Relatedly, Wasow argues that adjectival participles cannot be derived from double object verbs (63a). Nonetheless, this generalization also seems to be empirically incorrect, as witnessed by (63b). Searching the Web, one can easily find examples of adjectival participles derived from ditransitive verbs such as *grant*, *allow*, *deny*, and others. At least a partial explanation for the badness of (63a) can be found in Matushansky 2002. Namely, the verb *seem* requires its bare internal argument to refer to a state that is perceptually available to the experiencer. Whereas the state that results from having been granted the ability to recognize things for what they are may have visible consequences on my behavior, for example, the state resulting from having been given first prize every time there is a contest will not be perceptible on John.[6] It should also be noted that, while a lexicalist approach to adjectival passives will struggle to account for data like (63b), a syntactic account can easily do so by invoking the presence

[6]It could also be that (63a) is perceived as bad by some speakers because it garden paths into *given* as a P, not a participle.

of the verbal layer that introduces the oblique argument.

(63) a. *John seems given first prize every time we have a contest.

b. [...] I seemed granted the ability to recognize things for what they truly were.

(D. Crouse, *Copy Cats*, p. 140)

We can therefore draw two conclusions about this diagnostic: (i) It is not true that only verbs can select for the category of their arguments, and (ii) While in English only verbs can have DP complements, these can appear with so-called adjectival participles as well.

### 3.3.6 Degree modification

The final diagnostic I discuss pertains to the behavior of degree modifiers such as *very (much)*. Wasow notes that whereas verbs and adjectives cannot be modified by the same type of degree modifier (64a-b), participles seem to allow either (64c). He then assumes that the two different modifiers in (64c) are possible because the string in (64c) can arise from two different derivations, with two different participles; one participle is a verb and the other an adjective. Although this analysis is in principle possible, my purpose here is to show that this kind of data can equally well be accounted for under a syntactic approach to word-formation. Namely, even if the participle is a deverbal adjective in both cases, the two possibilities could be accounted for by appealing to different heights of attachment of the modifiers. As illustrated in the schematic representation in (65), *very* attaches to the adjectival layer, and *very much* attaches to the verbal layer embedded below.[7]

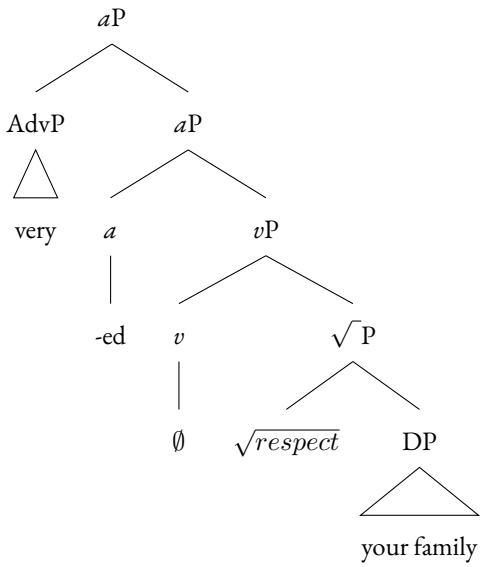
(64) a. John very *(much) respects your family.

b. John is very (*much) fond of your family.

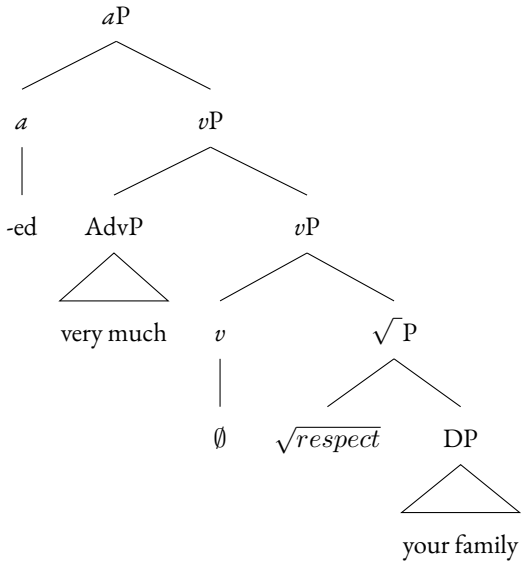
c. Your family is very (much) respected.

[7]Note that the approach I take does not suppose that all participles will need to admit both modifiers. After all, *very*, which modifies simple adjectives, is simply incompatible with non-gradable adjectives (e.g., **very parliamentary elections*). Therefore, if some participles resist modification by *very* (e.g., *?The glass was very broken*, **The man was very arrested*), this tells us nothing about their categorial status.

(65) a.




b.




What is important to highlight before we move on to a discussion of BCS passive participles is that (i) none of the diagnostics found in the literature seem to successfully identify contexts that host passive participles and can independently be shown to host verbs, but not adjectives, and (ii) we were able to give alternative explanations for why agentive *by*-phrases and specific modifiers are unacceptable in certain contexts. This state of affairs is compatible with the claim that all passive participles are, in fact, deverbal adjectives. Although positive evidence for this claim is difficult to come by in English, let us look at one argument to this effect from the closely related German. In German, like in English,

agentive *by*-phrases are disallowed in stative contexts (66a).[8] As with English, the reason for the badness of (66a) is taken to be that this participle is an adjective (see Alexiadou, Gehrke & Schäfer 2014 for more details of one such analysis). Now, compare (66a) and (54c), repeated here as (66b). Once the participle is in the adnominal position, the agentive *by*-phrase can reappear. Importantly, however, Rapp 2000 reports that the participle in (66b) obligatorily has an eventive interpretation. If we want to maintain that prenominal modifiers in German are adjectival phrases, the conclusion must be that agentive *by*-phrases in such a language are licensed only in eventive contexts, even when the outermost structural layer of the participle is clearly adjectival.[9]

(66) a. *Der Wein ist vom Kellner eingeschenkt. (Rapp 2000:396-7)
the wine is by-the waiter poured
b. der vom Kellner eingeschenkte Wein
the by-the waiter poured wine

Finally, I would like to draw attention to a more general issue with using the (un)availability of agentive *by*-phrases to make claims about category differences. Whereas it is undoubtedly true that the insertion of the *by*-phrase can, under the right circumstances, give participles a more eventive flavor, it is helpful to keep in mind that *by*-phrases are also possible with eventive nominalizations, which clearly have the distribution of nouns. This suggests that the *by*-phrase is not sensitive to the categorial status of the element it modifies (i.e., its external syntax), so long as that element contains enough verbal structure. This observation weakens the cogency of the diagnostics used to make claims about the category distinction between adjectival and verbal participles, given that many of them rely on the assumption that only verbal elements may appear with agentive *by*-phrases.

[8] German participles in the complement position of the verb *sein* 'be', as in (66a), are obligatorily stative. Eventive participles in the predicative position are introduced by the verb *werden* 'become'.

[9] An alternative analysis is given in Sleeman 2011, where it is argued that prenominal participial modifiers are reduced relative clauses. For Sleeman, then, concord between the modifier (participle or adjective) and the noun is seen as "an attributive property rather than a purely adjectival property" (Sleeman 2011:fn. 8). This could account for the relevant data in a language like Dutch or German, where only attributive adjectives show concord. However, it cannot account for why all adjectives and participles (but not other categories) in a language like BCS also show number/gender agreement with the noun in the predicative position.

## 3.4 Category membership: Evidence from BCS

Let us now introduce data from BCS, whose rich morphology can inform our analysis of passive participles more generally. Passive participles in BCS have a distribution largely similar to their English counterparts, modulo the fact that BCS participles are additionally influenced by grammatical aspect. I discuss the influence of aspect in more detail in section 3.5.1, once we have established the categorial status of the participle. As in the English (50), the participle in (67a) forms part of a passive predicate, whereas in (67b) it is used attributively.

(67) a. Prozor je po-lomljen od strane huligana.
window COP.3SG PF-broken by side hooligans
'The window was broken by the hooligans.'

b. po-lomljen prozor
PF-broken window
'a broken window'

The BCS perfective participle in (67a) is ambiguous between a bounded eventive reading and a resultative reading.[10] I discuss the reasons behind this ambiguity in more detail in section 3.5.1. As I focus on the categorial status of the participle in the remainder of this section, I will use disambiguating contexts that only admit one interpretation (eventive or resultative). Each claim I make will be tested against both types of participles. When talking about eventive participles, I will use imperfective-marked participles with a *by*-phrase modifier (68a), which ensures an (unbounded) eventive interpretation of the passive construction in BCS. When talking about resultative participles I will use perfective-marked participles that appear as complements of *činiti se* 'seem' (68b) or a similar verb, and are modified by an event-related adverbial.[11]

(68) a. Opomene su prošle nedelje pisane od strane vlade.
warnings COP.3PL last week written(IMPF) by side government
'Warnings were being written by the government last week.'

[10] I use the term *bounded* to indicate that an event has a (linguistically expressed) temporal boundary, see Declerck 1989.
[11] The adverbial is there to prevent the purely stative reading of the participle; see Embick 2004.

b. Te pesme su mi se činile skoro na-pisane.
those poems COP.3PL me SE seemed recently PF-written
'Those poems seemed recently written to me.'

In what follows, I discuss evidence from adjectival morphology, including adjectival affixation and $\varphi$-marking (3.4.1), concord (3.4.2), the distribution of the so-called definite form (3.4.3), and comparison (3.4.4).

### 3.4.1 Adjectival morphology

Going back to the question of categorial status, the most obvious reason to claim that BCS passive participles are adjectives is that both resultative (69a) and eventive (69b) participles are derived by means of adjectival morphology; cf. (69c), a pure adjective. The adjectival suffix *-n* is separated with a hyphen.

(69) a. Taj telefon mi se činio nedavno kuplje-n.
that telephone me SE seemed recently buy(PF)-ADJ.M.SG
'That telephone seemed to me recently bought.'

b. Njegov novac je uzima-n od strane države.
his money COP.3SG take(IMPF)-ADJ.M.SG by side state
'His money was being taken by the state'

c. Kraj romana je tuža-n.
end novel COP.3SG sad-ADJ.M.SG
'The end of the novel is sad.'

I should note that, with a limited number of verbs, the passive participle has a form distinct from the one given in (67–69). In addition to the dominant suffix *-n*, the passive participle may also be formed using the suffix *-t* (70).[12] BCS does also have the (less frequent) adjectival suffix *-it* (e.g., *ponos-it* 'proud'; cf. *ponos-an* 'proud'). There is no obvious semantic difference between this suffix and the more common adjectival suffix *-n*. It could therefore be the case that the adjectival suffix *-it* (or a version of it) is involved in deriving (70). Since there are no distributional or semantic differences between the

[12]Both suffixes (*-n* and *-t*) are inherited from Proto-Indo-European (**-no-* and **-to-*, respectively), not only in Slavic, but also in English (cf. *giv**en*** and *brough**t***). Their distribution in the different Indo-European languages has, of course, diverged from the original picture. None of this is unexpected when it comes to derivational morphology, which often exhibits these types of idiosyncrasies cross-linguistically.

participles derived with *-n* and *-t* in BCS, I assume that they belong to the same category, and abstract away from these differences in the remainder of the chapter.

(70) Pehar je da-t mojoj majci.
cup COP.3SG give(PF)-ADJ.M.SG my mother
'The cup was given to my mother.'

Now, going back to the suffix *-n*, most traditional BCS grammars state that the adjectival suffix in question is actually *-an* (cf. (69c)), though many do place the vowel in parentheses: *-(a)n*. I believe there is good evidence that this vowel is epenthetic (used to break up an illicit coda cluster, e.g., [ʒn] in (69c)), and that the adjectival suffix is, in fact, *-n*. Namely, the epenthetic vowel is only present when the adjective is indefinite (non-specific) masculine, as in (69c), and disappears when the adjective is feminine (71a), neuter (71b), or masculine definite (71c). Unlike the indefinite masculine form, (71a-c) have an additional final agreement vowel, which has the effect of producing a word that conforms to the phonotactic rules of the language. Since *-n* is then no longer part of an illicit coda, the epenthetic vowel does not appear (cf. **tužana priča* 'a sad story').

(71) a. tuž-n-a priča 'a sad story'

b. tuž-n-o dete 'a sad child'

c. tuž-n-i kraj 'the sad ending'

I therefore take the adjectival suffix, which appears with both simple adjectives and participles, to be *-n*. Determining the status of the vowel in the adjectival suffix is relevant because the vowel that separates the root and the adjectival suffix on the past participle in, for example, (69a-b) behaves differently—it is preserved in all contexts. This suggests that the nature of the vowel in past participles is different from the nature of the vowel in underived adjectives. I discuss the role of this so-called thematic vowel on participles in section 3.5.1.

### 3.4.2 Concord

The agreement vowels in (71a-c) also appear on passive participles. This is true both for resultative (72a) and eventive passives (72b). Matching in gender and case features is indeed characteristic of adjectives; purely verbal forms in BCS agree with their subjects only in person and number (72c).

(72) a. To čanč-e je izgledalo nespretno o-pra-n-**o**.
that bowl-NOM.NEUT.SG COP.3SG looked clumsily PF-wash-ADJ-**NEUT.SG**
'That bowl looked clumsily washed.'

b. Ove čarap-e su štrika-n-**e** od strane moje bake.
these sock-NOM.F.PL COP.3PL knit(IMPF)-ADJ-**F.PL** by side my grandma
'These socks were knitted by my grandma.'

c. Moji drugari i ja gradi-**mo** splav.
my friends.NOM.M.PL and I build(IMPF)-**1PL** raft
'My friends and I are building a raft.'

The agreement pattern exhibited by BCS passive participles is by no means unique; in fact, it is pervasive among Indo-European languages that have agreeing adjectives (Emonds 2006 for French and German, Schoorlemmer 1995 for Russian). A particularly interesting observation is made by Emonds: In German, where attributive (but not predicative) adjectives share the $\varphi$-features of their head nouns, both resultative and eventive participles show concord only in the attributive context (cf. (66b), which shows $\varphi$-feature concord on an eventive participle). This suggests that German eventive participles also have an adjectival layer, contrary to what has been claimed in the literature.

### 3.4.3 The definite form

In BCS, both eventive and resultative participles show the same restriction as adjectives: their definite forms, which are generally allowed in attributive position, are disallowed in predicative position (73b-b). The reasons for this restriction need not concern us here (but see, for example, Aljović 2000). What is important is that this diagnostic again aligns all passive participles with adjectives (73c), and not with verbs, as BCS verbs (finite and non-finite) do not have a definiteness contrast.

(73) a. Gelender je farba-n /*farba-n-i od strane dečice.
railing COP.3SG paint-ADJ(INDF) / paint-ADJ-DEF by side kids
'The railing was being painted by the kids.'

b. Peškir se činio nedavno opra-n /*opra-n-i.
towel SE seemed recently wash.V-ADJ(INDF) wash.V-ADJ-DEF
'The towel seemed recently washed.'

c. Ovaj dečak je tuža-n /*tuž-n-i.
this boy COP.3SG sad-ADJ(INDF) / sad-ADJ-DEF
'This boy is sad.'

### 3.4.4 Comparison

Furthermore, both eventive and resultative participles can undergo comparison, and form the superlative with the prefix *naj-*, as in (74a-b). This makes both of them like adjectives (74c), but unlike finite verbs, which may only express superlativity with the adverb *najviše* 'the most' (74d). We observe a somewhat similar effect in English, reflected in the position of the modifier 'the most' in the translations (74a-c) versus (74d).

(74) a. Ova aplikacij-a je naj-korišćen-ij-a od strane moje ćerke.
this app-F.SG COP.3SG SUP-use.IMPF-CMPR-F.SG by side my daughter
'This app is (the) most used by my daughter.'

b. Njene oči su mi se činile naj-na-šminkan-ij-e (od svih).
her eye.F.PL COP.3SG me SE seemed SUP-PF-made_up-CMPR.F.PL of all
'Her eyes seemed (the) most made-up (of all).'

c. Ova devojčica je naj-opasn-ij-a.
this girl.F.SG COP.3SG SUP-happy-CMPR-F.SG
'This girl is the most dangerous.'

d. Moja ćerka najviše korist-i / *naj-korist-i ovu aplikaciju.
my daughter the_most use-3SG SUP-use-3SG this app
'My daughter uses this app (the) most.'

In this section, the focus has been on highlighting the adjectival properties of passive participles. However, despite having the external morphology and syntax of adjectives, passive participles undeniably have at least some underlying verbal structure, which I examine next. In section 3.5.1, I consider

the internal structure and interpetation of BCS eventive and resultative participles. Whereas both types of participles contain a verbalizing morpheme, I show that the presence of the perfective aspectual layer is required to derive the resultative (and the bounded eventive) passive participle. I argue that this is because the perfective denotes a relation between an event and its completion. Section 3.5.2 examines the eventive/resultative contrast in English. I argue that the resultative in English is derived by means of a stativizer which selects for a *v*P complement. Despite recent claims that English adjectival passive participles may appear with agentive *by*-phrases, I show that none of these are resultative participles.

## 3.5 Inside the passive participle in BCS and beyond

### 3.5.1 Eventivity and resultativity in BCS, and the role of Aspect

Let us now focus on the verbal properties of BCS participles. Recall the claim in the discussion of (69) that the vowel found between the stem and the adjectival suffix with passive participles behaves differently than the epenthetic vowel found with pure adjectives. Namely, its presence is not dependent on the phonological properties of the participle. In order to determine the role of this vowel, let us look at some verbs and their corresponding passive participles (75). As usual, the dashes indicate suggested morpheme boundaries.

(75) a. gled-a-ti 'watch' gled-a-n 'watched'

b. šut-nu-ti 'kick' šut-nu-t 'kicked'

c. vol-e-ti 'love' volj-e-n 'loved'

d. uč-i-ti 'teach' uč-e-n 'taught'

e. pas-∅-ti 'graze' pas-e-n 'grazed'

The infinitival forms of the verbs in (75) consist of a root, a theme vowel, and the infinitival suffix. Theme vowels in Indo-European languages have traditionally been used to divide verbs into classes. Although the theme vowel may vary across the paradigm of a single verb (present tense forms

being notoriously irregular), we can predict the theme vowel of the participial form based on a verb's membership in one of the five conjugation classes in (75). What we observe in (75) is that, when going from the infinitive to the participle, the theme vowel remains the same for (75a) and (75b), while it changes systematically to *-e* for the classes in (75c-e).[13] This kind of systematicity is crucial considering the fact that verbal theme vowels are not found with other (root-derived) categories. Based on the fact that Slavic theme vowels attach to clearly non-verbal forms to produce verbs (e.g., *crven* 'red'/*crven-i-ti* 'make red', *lit.* 'red-V-INF') and the observation that these vowels may signal argument structure changes in verbs (e.g., *crven-i-ti* 'make red' vs. *crven-e-ti* 'become red'), they have been argued to be exponents of the verbalizing head, *v* (Svenonius 2004, Caha & Ziková 2016, Biskup 2019).[14] I take the presence of the theme vowel to indicate that all passive participles in BCS contain *v*, which is associated with verbalization and eventivity (Harley 1995, Anagnostopoulou 2003, Embick 2004, A&A 2008, i.a.).[15]

In addition to obligatorily containing *v*, all BCS passive participles encode grammatical aspect in the same way as verbs (76a-b). The majority of BCS verbs (and Slavic verbs, more generally) are interpreted as imperfective in their base form, and perfectivity is most commonly encoded with the addition of prefixes. The exact contribution of grammatical aspect in Slavic verbs is a controversial issue (see e.g., Brecht 1984, Smith 1991, Klein 1995, Schoorlemmer 1995, Verkuyl 1999, Babko-Malaya 1999,

[13] Alternatively, (75c-e) are derived by adding the independently attested adjectival suffix *-en* to the verbal stem (see Simonović & Arsenijević 2020), and the verbal theme vowel is deleted because it is followed by a vowel-initial morpheme (Jakobson 1948).

[14] Ora Matushansky (pers. comm.) suggests that treating Slavic theme markers as verbalizers across the board may not be a good idea for various reasons, most generally because they do not all pattern alike. For instance, the theme *-nu-* in (75b) makes a semantic contribution (perfectivity), unlike the other theme vowels. Even so, the specific thematic vowels we find with verbs seem to be involved only when we have other evidence for verbal structure (e.g., in eventive nominalizations). It is not crucial for my purposes whether the theme vowels are exponents of *v*, or whether they are inserted as a result of some morphological well-formedness rule that applies to *v*, as in Oltra-Massuet 1999. Although I will continue to represent the theme vowel in *v* for convenience, it is sufficient for us to assume that verbal thematic vowels diagnose the presence of *v* in the structure.

[15] There is a whole host of pairs consisting of a pure adjective and a passive participle which differ only in the vowel that intervenes between the root and the adjectival suffix. Some examples include *siromaš**a**n* 'poor'–*siromaš**e**n* 'made poor', *umor**a**n* 'tired'–*umor**e**n* 'made tired', *zadovolj**a**n* 'content'–*zadovolj**e**n* 'made content', etc. As already discussed, the so-called theme vowel on the participle is always present, whereas the epenthetic vowel of the simple adjective disappears when the right conditions are met. The presence/absence of eventivity in the above examples corresponds to the presence/absence of the verbal theme vowel, additionally suggesting that the theme vowel diagnoses the presence of *v*.

Babko-Malaya 2003, Bertinetto 2001, Borik 2002, Filip 2003, Filip 2005, Ramchand 2004, Romanova 2004, 2007, Svenonius 2004, Arsenijević 2006, Łazorczyk 2010, Tatevosov 2008, 2011, 2014, 2015, de Swart 2012). I will adopt one prominent view on which the perfective form, but not the imperfective form, marks the situation as temporally bounded.[16] In (76), this distinction is illustrated for participles using the material in parentheses. In (76a) with the imperfective participle, the speaker makes no commitment as to whether the painting process was (or is) completed. On the other hand, (76b) with the perfective participle asserts that the process is completed, making the material in parentheses deviant.

(76) a. Kupol-a je slik-a-n-a od strane talentovanih umetnika
dome-NOM.F.SG COP.3SG paint(IMPF)-V-ADJ-NOM.F.SG by side talented artists
(ali ni-je završena).
but not-COP.3SG finished
'The dome was being painted by (the) talented artists (but it wasn't completed).'

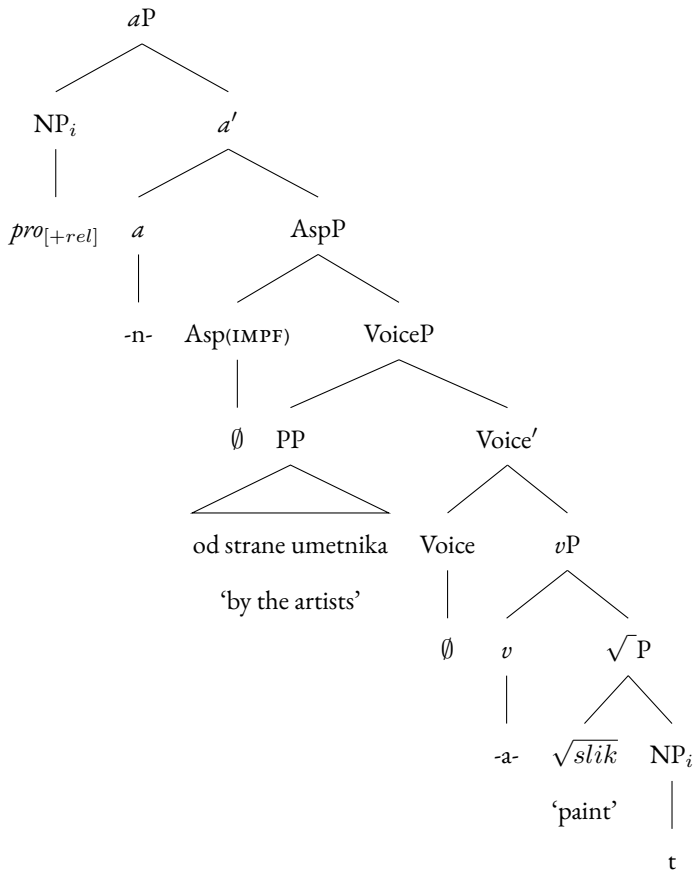
b. Kupol-a je o-slik-a-n-a od strane talentovanih umetika
dome-NOM.F.SG COP.3SG PF-paint-V-ADJ-NOM.F.SG by side talented artists
(# ali ni-je završena).
but not-COP.3SG finished
'The dome is/was painted by (the) talented artists (#but it wasn't completed).'

In (77), I synthesize what has been said about the individual pieces of morphology that make up the eventive participle in (76a). The acategorial root $\sqrt{slik}$ 'paint' is verbalized, and VoiceP attaches above the verbalizing head, introducing the external argument (Kratzer 1996). The assumption that the *by-* phrase is introduced in VoiceP is not uncontroversial (see e.g., Baker, Johnson & Roberts 1989); I will address and justify it once we have considered the differences between BCS and English resultative participles in more detail. I furthermore assume that dedicated aspectual projections host aspectual features, and that they are to be found above the projection that introduces the agent (Schoorlemmer 1995, Svenonius 2004, Ramchand 2004, Tatevosov 2008, Pazelskaya & Tatevosov 2008, Łazorczyk

[16] This need not mean that the situation has reached its natural end, only that it is delimited in time (see Borik 2002 for an analysis that distinguishes between telicity and perfectivity in Russian). That said, BCS transitive perfective verbs *are* always telic. The only exception to my knowledge is the perfective verb *po-tražiti*, *lit.* 'PF.-search'. This is relevant because resultative participles can be derived only from telic VPs, in BCS, but also in English and in other languages (e.g., **The house seems painted for a long time*). Unlike BCS, Russian has a productive delimitative prefix *po-*, which marks the verb it attaches to as perfective but not telic. Verb stems that contain the delimitative prefix *po-* cannot be used to derive resultative participles.

2010, i.a.).[17] Finally, the structure is adjectivized.[18] I return to the status of the internal argument of the root immediately below.

(77)




Semantically, the resulting *a*P should be a predicate that is true or false of the Theme argument. To achieve this, we may treat the adjectivized structure in the same way relative clauses are treated by (Heim & Kratzer 1998), among others. As illustrated in (77), a silent NP (*pro*$_{[+rel]}$) merges as the complement of the root, and then moves to the specifier of the adjectivizing head, where the movement is interpreted as $\lambda$-abstraction over the variable that interprets its trace. This yields the denotation in (78), where *a* stands for the artists. The reader is free to understand the denotation of the imperfective in

[17] We may question the utility of the aspectual projection in (77), since no overt material is associated with it. Eventive participles *can* nonetheless contain overt aspectual affixes, for example, the secondary imperfective *o-slika-va-na* 'painted.SI', derived from the perfective participle *o-slikana* 'PF-painted' with the addition of the imperfective morpheme *-va-*. One question I leave open is whether the interpretation of the base-imperfective in (77) is derived by assigning imperfective semantics to the empty Asp head, or whether this form is underspecified for aspectual features, with the imperfective being a default rule of interpretation.

[18] I remain agnostic as to whether there may be an additional projection on top of *a*P that hosts $\varphi$-features (gender, number, and case) obtained through concord with the noun.

(78) as a placeholder for whatever the precise representation of incompleteness may turn out to be.

(78) $\lambda y \lambda e$. T iff $painting(e)$ & $Theme(e, y)$ & $Agent(e, a)$ & $Incomplete(e)$

Note that the LF in (78) is applicable to both predicative and attributive participles. When the participle is used predicatively, a higher functional head (Pred) introduces the clausal subject, as I discuss below. When the participle is NP-adjoined, the resulting phrase is interpreted as the intersection of the set denoted by the noun and the set denoted by the participle.

Note also that (78) makes no reference to states. This distinguishes it from analyses explicitly given in Meltzer-Asscher 2011b and Gehrke 2015, but also implicitly assumed in much other work on adjectival passives, where adjectivization creates a predicate that is always true of states. Abandoning this view is necessary since BCS eventive passive participles unambiguously belong to the category of adjectives. If the goal is to have a generalizable semantics for lexical categorizers cross-linguistically, then the adjectival head should not itself encode stativity. The stative component of resultative participles (and root-derived adjectives) needs to come from a different source.

Before moving forward, I should clarify on the use of aspectual affixes as exponents of Asp. Slavic aspectual affixes have been argued to belong to at least two subclasses–lexical and superlexical (see e.g., Svenonius 2004)–both of which are available with passive participles. Most authors assume that at least lexical affixes are base-generated inside the *v*P, but the received wisdom is either (i) that they move to the position indicated in (77) because, as operators over an event variable, they must take scope over this variable (e.g., Svenonius 2004), or (ii) that they stay in their original position, but force a particular value on the aspectual head (e.g., Ramchand 2004). Some authors even argue that all "aspectual" affixes are, in fact, resultative affixes found within *v*P (Arsenijević 2006, Tatevosov 2011, 2015). Regardless of this, there are diagnostics that are sensitive particularly to the perfective/imperfective distinction (Borik 2002). One such diagnostic is the ability to appear as a complement of a phasal verb (*start*, *continue*, *end*, etc.). As expected, only imperfective participles can appear in this position; this is true for both telic (79a) and atelic predicates (79b). For the sake of simplicity, I represent all aspectual affixes in AspP.

What matters for our purposes here is the position and value of the aspectual projection, rather than the exact position of the affixes.

(79) a. Kupola je počela da bude **(*o)**-slikana prošle nedelje.
dome COP.3G started DA be **PF**-painted past week
'The dome started being painted last week.'

b. Ovaj model je počeo da bude **(*po)**-tražen prošle godine.
this model COP.3G started DA be **PF**-sought last year
'This model started being sought after last year.'

Let us now focus on the perfective participle in (76b). This participle is ambiguous between the resultative and the bounded eventive reading, and the modifiers in (80) help disambiguate between the two. Both constructions in (80) involve the same form of the copula, and the same form of the participle. However, the resultative in (80a) denotes a present state resulting from a prior event, whereas the eventive in (80b) denotes a past completed event. Additionally, only the resultative reading is possible when the participle is the complement of *činiti se* 'seem' (81)—since *činiti se* is marked for present tense, this sentence cannot be uttered if the dome is no longer in a painted state which resulted from an event of painting by the relevant artists.

(80) a. Kupola je sada zauvek o-slikana od strane (ovih) talentovanih umetnika.
dome COP.3SG now forever PF-painted by side these talented artists
*lit.* 'The dome is now forever painted by (these) talented artists.'

b. Kupola je juče o-slikana od strane (ovih) talentovanih umetnika.
dome COP.3SG yesterday PF-painted by side these talented artists
'The dome was painted by (these) talented artists yesterday.'

(81) Kupola mi se čini o-slikana od strane (ovih) umetnika.
dome me SE seems PF-painted by side these artists
*lit.* 'The dome seems to me painted by (these) artists.'

I will argue the perfective participles in (80) are identical, and that a higher functional head (Pred) is responsible for their distinct interpretations. Let us set aside this difference for now, and focus on the resultative participle. We have seen that both BCS and English eventive participles allow agentive *by*-phrases, suggesting they project an external argument. As illustrated by (51), (76b), (80a) and (81),

BCS resultative participles are also compatible with agentive *by*-phrases. In addition to appearing with complements of verbs like *seem*, which require stative complements, agentive *by*-phrases in BCS may occur in other typical stative contexts, for example when the perfective participle is a superlative (82); also (74a). The interpretation of these participles is stative, and the *by*-phrase names the agent of the event that brought about the state. As seen in the translations, this is impossible in English.

(82) Jovana je od strane policije naj-obavešten-ij-a od svih komšinica.
Jovana COP.3SG by side police SUP-informed(PF)-COMP-F.SG of all neighbors
'Of all the female neighbors, Jovana is the most informed by the police.'

This cross-linguistic variation has already been discussed in Anagnostopoulou 2003 and Alexiadou, Gehrke & Schäfer 2014 for Greek, which also allows agentive *by*-phrases with resultative participles (see also Alexiadou, Anagnostopoulou & Schäfer 2015). Comparing Greek with German (which patterns with English), Alexiadou, Gehrke & Schäfer claim that the presence of an aspectual projection in the syntax of Greek participles is responsible for the availability of *by*-phrases with resultatives. They follow Gehrke (2011, 2013, 2015) in claiming that the observed cross-linguistic variation stems from the fact that verb stems (*v*Ps) that notionally name events are semantically predicates, not of events, but of event kinds. Event kinds are abstract. They do not have locations, times, or participants, unlike the concrete events that realize them. A predicate centered on such a verb stem cannot include thematic or spatiotemporal modifiers, unless it also includes verbal functional structure to introduce a relation of realization between an event and the kind that it names. Resultative participles in English and German, the argument goes, are not directly embedded under such functional structure, but must first be adjectivized. By stipulation, this intervening adjectival projection existentially binds the event argument of the predicate, and prevents the event kind associated with the verb from being instantiated. NPs naming participants in the event, such as those in *by*-phrases, cannot be used to name actual event participants since there is no actual event to begin with. In Greek (and BCS, by analogy) the additional aspectual structure below the adjectivizing layer is presumably sufficient to instantiate the event kind, and this why naming the agent of the event is permissible even with resultative participles.

My main objection to this analysis concerns the role of grammatical aspect in the formation of Greek (and BCS) resultative participles. An important point that this approach misses is that it is not sufficient for a verb to encode aspectual information for it to be compatible with agentive *by*-phrases in stative contexts, or even with stative contexts as such; rather, it must specifically encode perfect(ive) aspect. Whereas Greek verb stems *do* encode aspectual distinctions, the relevant participle is always derived from the perfect stem.[19] Greek has a synthetic eventive passive; it does not use participles for this purpose. What sets BCS apart is that the perfective participle patterns with Greek (81), whereas the imperfective is generally incompatible with stative contexts, with or without the *by*-phrase (83). We can therefore trace the cross-linguistic variation in the availability of *by*-phrases with resultatives to the presence/absence of a *particular kind* of grammatical aspectual information with a reasonable degree of certainty.

(83) *Ova vaza se čini lomljena (od strane nestašnih patuljaka).
this vase SE seems broken(IMPF) by side mischievous dwarfs
*lit.* 'This vase seems being broken by the mischievous dwarfs.'

In fact, even participles derived from BCS secondary imperfective verbs are incompatible with stative contexts (84). The badness of (84) suggests that aspectual interpretation, rather than the amount of aspectual structure, determines the participles' compatibility with verbs like *seem*.[20]

(84) *Ova kupola se čini o-slik-a-va-n-a (od strane talentovanih umetnika).
this dome SE seems (PF)-paint-V-SI-ADJ-F.SG by side talented artists
*lit.* 'This dome seems being painted (by the talented artists).'

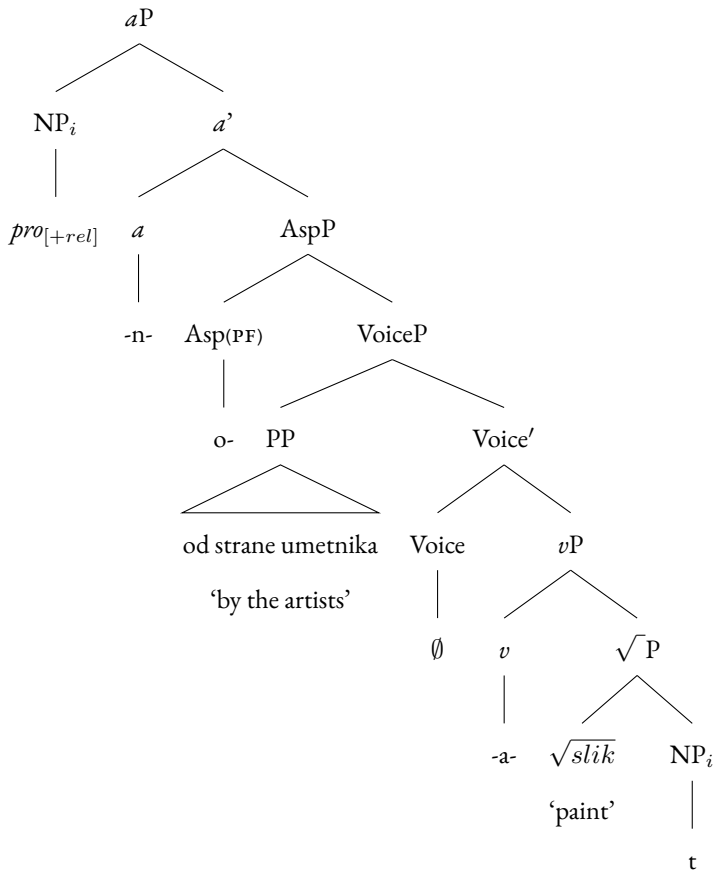
Explaining why agentive *by*-phrases are available with resultative participles in BCS-like languages is a two step-process. First, recall that grammatical aspect enters the derivation *after* all of the verb's arguments have been introduced. Using the participle *oslikan* 'painted' in (80), I present the structure

[19] A salient property of the Greek *perfect* (and of the BCS perfective) is the notion of completion; see Moser 2003.

[20] Secondary imperfectives in BCS may have either a durative or a repetitive reading. (84) is interesting because it is unacceptable if interpreted as progressive, and it improves if interpreted as habitual. Intuitively, this makes sense: the durative interpretation is the same as with base imperfectives, whereas with the repetitive reading the event is completed several times, and thus more like the perfective. I will have to leave the question of how, if at all, these differences are encoded in the syntax for future research.

for perfective participles in (85). Let us further assume that the perfective aspect denotes a two-place relation between an event and the state of its completion (86a). Combining this with what we have said for eventive participles and the adjectivization operation, the denotation for the perfective participle is given in (86b).

(85)




(86) a. $\lambda P \lambda e \lambda s$. T iff $[\![VoiceP]\!](e)$ & $Compl(e, s)$

b. $\lambda y \lambda e \lambda s$. T iff $painting(e)$ & $Agent(e, a)$ & $Theme(e, y)$ & $Compl(e, s)$

Note that both the event and the state variable in (86b) are available for further modification. This is desirable because, as we have seen, the perfective participle may have both bounded eventive and resultative interpretations. In both cases, we want the participle to contain an eventive and a stative component; however, their compatibility with different types of adverbs in (80) suggests that the eventive component is more "salient" in the bounded eventive passive, whereas the stative component

is more "salient" with the resultative passive.[21]

How may we formally implement this intuition? I would like to suggest that the two participles are selected by distinct Pred(icative) heads (see e.g., Adger & Ramchand 2003, Roy 2005). Both Preds introduce an argument that saturates the variable *y* of the predicate in (86b) (*kupola* 'dome' in (80)) . Additionally, $\text{Pred}_1$, call it *State Promotion*, existentially binds the event argument of the participial predicate, and returns a predicate of states, yielding the resultative interpretation (87a). Applied to the resultative participle in (80a), we get the LF in (87b), where *a* stands for 'artist' and *d* stands for 'dome'. Conversely, $\text{Pred}_2$, call it *State Closure*, existentially binds the state variable introduced by the perfective, and yields a predicate of completed events (88a). The LF in (88b) gives the interpretation for the bounded eventive construction in (80b).[22]

(87) a. StatePromotion(Q) = $\lambda s \exists e.$ T iff $Q(e)(s)$

b. StatePromotion($[\![a\text{P}]\!]$) = $\lambda s \exists e.$ T iff $painting(e) \& Theme(e,d) \& Agent(e,a) \& Compl(e,s)$

(88) a. StateClosure(Q) = $\lambda e \exists s.$ T iff $Q(e)(s)$

b. StateClosure($[\![a\text{P}]\!]$) = $\lambda e \exists s.$ T iff $breaking(e) \& Theme(e,d) \& Agent(e,a) \& Compl(e,s)$

Since the proposed Pred heads are homonyms in (80), one may be skeptical that they are distinct elements. However, there is some independent evidence from their interaction with Tense that the two Preds are, in fact, different.[23] When *je* 'PRED' appears with resultative participles (80a), it refers to

[21] We have already seen that the BCS resultative participle also allows event-modification, both by manner adverbs and agentive *by*-phrases (i). This is expected on my analysis since event-modifiers and agentive phrases enter the syntactic derivation before the perfective layer introduces the result state. The participle in (i) still refers to a state, but one that came about through a violent event carried out by the hooligans.

(i) Prozor je ostao nasilno iz-lomljen od strane huligana.
window COP.3SG remained violently PF-broken by side hooligans
*lit.* 'The window remained violently broken by the hooligans.'

[22] It is worth pointing out here that the appeal to distinct Pred heads is not sufficient to salvage analyses that assume a categorial contrasts between the different participles. Such a move would still fall short of explaining the adjectival morphosyntax of all passive participles.

[23] See Biskup 2019 for evidence that supports the existence of two verbs *be* in Czech; see also Salzmann & Schaden 2019,

present states, same as with root-derived adjectives, which can only have a stative reading (89a). On the other hand, with bounded eventive participles (80b), *je* 'PRED' refers to past events, just like with active participles, which can only have an eventive reading (89b). In order to get a present tense interpretation of the copula with the eventive participle, the imperfective form of the copula–*biva(-ti)*–must be used (89c). With *biva(-ti)*, it is impossible for the participle to refer to a state. Furthermore, if we use the past form of the copula, the sentence with a resultative participle (and a simple adjective) refers to a past state (89d-e), while the same form with the eventive passive participle (and the active participle) can only have the pluperfect reading (89f-g). Some (particularly younger) speakers actually reject (89f-g) and use adverbs like *već* 'already' to signal the pluperfect interpretation. What is crucial to note is that there seem to be two copulas here, one that combines with eventive elements, and one that combines with stative ones. I summarize this in the table in (90).

(89) a. Kuća **je** veoma velika.
house COP.3SG very large
'The house is very large.'

b. Kuća **je** (na)-pravi-la senku.
house COP.3SG PF-make-ACT.PART shadow
'The house was making/made a shadow.'

c. Kuća **biva** (po-)rušena.
house COP.IMPF PF-demolished
'The house is being demolished.'

d. Kupola **je** **bila** za sva vremena o-slikana (od strane najvećih majstora).
dome COP.3SG was for all times PF-painted by side
*lit.* 'The dome was forever painted (by the greatest masters).'

e. Kuća **je** **bila** veoma velika.
house COP.3SG was very large
'The house was very large.'

f. %Kuća **je** **bila** po-rušena (od strane njihovog tima).
house COP.3SG was PF-demolihed by side their team
'The house had been demolished (by their team).'

which accounts for the difference in the interpretation of eventive and stative participial constructions in Alemannic in terms of the different semantics of the verbs that introduce them.

g. %Kuća **je** **bila** (na)-pravi-la senku.
house COP.3SG was PF-done-ACT.PART shadow
'The house had made a shadow.'

(90)

| | *eventive participle* | *resultative participle* |
|---|---|---|
| **present** | biva | je |
| **past** | je | je bio |
| **pluperfect** | %je bio | je bio |

We have seen that eventive and resultative passive participles in BCS have distinct aspectual properties. I have also shown that the presence of the perfective is crucial in the derivation of resultatives in BCS-like languages. I now examine the eventive/resultative dichotomy in English more closely.

### 3.5.2 Passive participles in English-like languages

While explaining the BCS and Greek facts, the analysis in section 3.5.1 cannot account for how resultative participles are derived in a language like English. Since English does not mark grammatical aspect on participles, there has to be another way for it to derive the meaning in (91a), namely that the documents remained in the resulting state of having been carefully alphabetized. How does this participle differ from its eventive counterpart in (91b)?

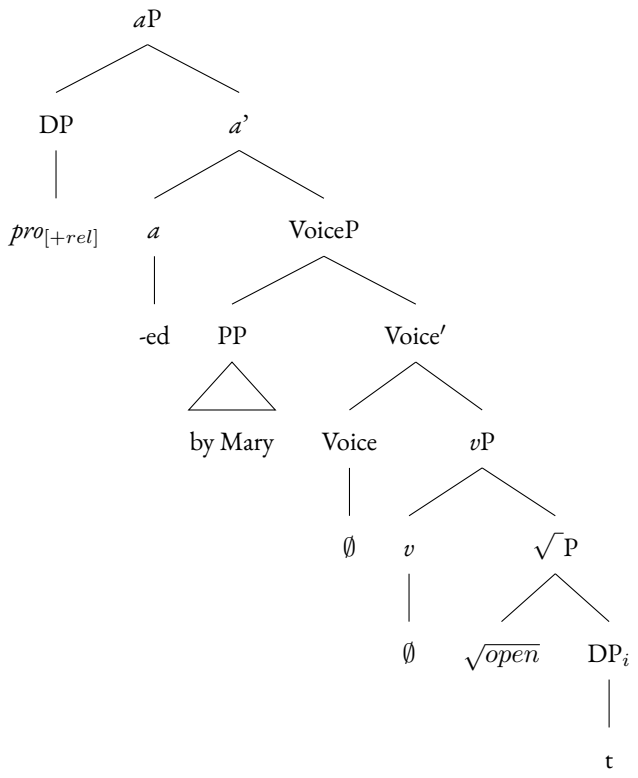
(91) a. The documents remained carefully alphabetized.

b. The documents were carefully alphabetized by Mary.

Considering first the English eventive participle, its structure in (91b) is the same as its BCS counterpart in all crucial aspects; it contains verbal structure (*v* and Voice), and the adjectivizing layer (92a). It differs in not having aspectual projections. The meaning of the English eventive participle is almost identical to its BCS counterpart: it is a predicate of events, and it is true or false of the Theme argument. Note that English eventive passive participles can correspond to both BCS imperfective (unbounded eventive) participles (e.g., *Sculptures were made on the beach for two hours*) and BCS perfective

(bounded eventive) participles (*A sculpture was made on the beach in two hours*). In terms of its semantic import (namely, boundedness), the telicity of the VP in English plays roughly the role that grammatical aspect plays in BCS. Since this chapter is about BCS primarily, I will not engage in a detailed analysis of telicity here.

(92) a.




b. $\lambda y \lambda e$. T iff $opening(e)$ & $Theme(e, y)$ & $Agent(e, m)$

Turning to the English resultative participle, let us first look at its interaction with agentive *by*-phrases. Even though early accounts have claimed that "adjectival" (resultative and purely stative) participles in English-like languages lack implicit initiators altogether (e.g., Kratzer 2000, Anagnostopoulou 2003, Embick 2004), a number of more recent works have argued that "adjectival" participles in these languages do in fact allow external argument *by*-phrases (McIntyre 2013, Bruening 2014, Alexiadou, Gehrke & Schäfer 2014). Importantly, these authors claim that "adjectival" participles (can) therefore include VoiceP. However, we already know that *by*-phrases used to name causes of states are generally allowed in resultative contexts.[24] Further, given our conclusion (based on BCS data) that adjectiviza-

[24] I use *cause* here in the sense of Lewis 1974, who proposes a counterfactual analysis of causation. In the words of Hume,

tion does not equal stativization, I will argue that a dedicated stativizer needs to be assumed for English resultative participles. Therefore, the presence of state-related *by*-phrases can easily be associated with this projection, and not with VoiceP, which is related to the event. The goal, then, will be to determine whether English resultative participles ever allow event-related *by*-phrases. I take up this task in the rest of this section, and show that the answer is negative. Indeed, even authors who claim that "adjectival" participles allow overt external arguments acknowledge the unacceptability of sentences like (93a). McIntyre (2013:31) suggests that there is inter-speaker variation in the acceptability of (93b), but the *by*-phrases are always construed as state-related. In other words, the underlying agent must be identifiable from the resulting state for the *by*-phrase to be licensed. As discussed in 3.3, this kind of *by*-phrase is generally acceptable with resultative participles, and it does not necessarily require the presence of Voice.

(93) a. *The door seemed broken/opened/painted by Mary.

b. %The text seems written by a genius/foreigner/ghostwriter.

Moreover, works that argue for the presence of Voice with English "adjectival" passives still often use verbs that are ambiguous between an eventive and stative interpretation even in their active form, as in (94), taken from McIntyre (2013:31). Particularly telling in this respect is (94b), which is acceptable when the *by*-phrase names an inanimate cause, but not when it names an animate agent. As seen in (95), the animate *flatter* is compatible with the progressive aspect, and therefore eventive, while the inanimate *flatter* is stative.[25] Importantly for our purposes, these stative participles will naturally have state-related external arguments. The LF for *blocked by police* (94a) is given in (96), where *p* stands for *the police* and *y* will evaluate to the road.

(94) a. The road remained blocked by police/supported by pylons.

b. Edeltraud seemed flattered by the report/??the journalist.

"if the first object had not been, the second never had existed." This does not presuppose any kind of "action" on the part of the "first object."

[25]For further stativity diagnostics, see Dowty 1979:55–56.

(95) a. *The report was flattering Edeltraud all day.

b. The journalist was flattering Edeltraud all day.

(96) $\lambda y \exists s$. T iff $block(s)$ & $Theme(s, y)$ & $Cause(s, p)$

The only other type of example that is commonly used to make the claim that "adjectival" participles contain a VoiceP is illustrated in (97), from Bruening (2014:379-80). What the participles in these sentences have in common is that they are prefixed by negative *un-*. Note that (97a-b) seem to be derived from stative (psychological) predicates, so the acceptability of *by*-phrases there may receive the same explanation as for the sentences in (94) above. Nonetheless, in (97c-d), the participles that serve as input to *un*-prefixation are true eventive verbs. I'd like to suggest that the only difference between these participles and their eventive counterparts (cf. (92)) is that the prefix *un-* adds a negative component. Specifically, it negates the existence of the event. I illustrate this in (98) for the participle in (97c); *c* stands for *TX congressman* and *y* will evaluate to millions. Here, as in the previous case, there is no state resulting from an event; rather, negation is applied directly to the event[26]. This means that the participles in (97c-d) are not resultative in the relevant sense. They are rather derived in the same way as regular eventive participles, and thus allow agentive *by*-phrases to the same degree.

(97) a. Biden's optimism undisturbed by Iraqi bombs (headline)

b. Toddler unfazed by lion encounter (headline)

c. Millions undisclosed by TX congressman (headline)

d. Steve Jobs' birthday doesn't go unnoticed by spammers.

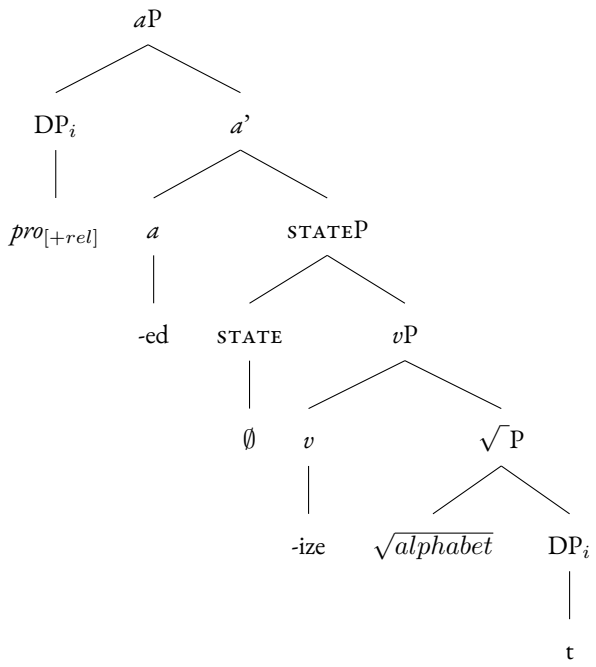
(98) $\lambda y \neg \exists e$. T iff $disclosing(e)$ & $Theme(e, y)$ & $Agent(e, c)$

What we have shown so far is that, besides with uncontroversially eventive participles discussed earlier, event-related *by*-phrases in English may appear only with participles prefixed by negative *un-*. These participles do not denote a state resulting from a prior event, but rather the absence of the

[26] More precisely, negation is applied to the claim that there is a V event; the *un-* here signals negative quantification over the event predicate.

event denoted by the predicate. Coupled with the unacceptability of (93a), I take this to suggest that resultative participles in English are truly incompatible with event-related *by*-phrases. Nonetheless, we still need to be able to derive participles such as the one in (91a), where the Theme is in a state resulting from an event. We will need to assume a phonologically null stativizer, STATE, which crucially selects for *v*P (rather than VoiceP, as was the case for the perfective in BCS-like languages). This is illustrated in (99a), which is in line with Kratzer 2000, Embick 2004, McIntyre 2013 i.a., contra Alexiadou, Gehrke & Schäfer 2014, Bruening 2014. Like the perfective, STATE introduces a state component, and a causal relation between the event and the state, as shown in (99b). It existentially binds the event variable, making it unavailable for further modification. Applying the adjectivizer, we get the LF in (99c).

(99) a.




b. $\lambda P \lambda \exists e$. T iff $[\![vP]\!](e)$ & $Cause(s, e)$

c. $\lambda y \lambda s \exists e$. T iff $alphabetizing(e)$ & $alphabetized(s)$ & $Theme(e, y)$ & $Cause(s, e)$

Before concluding this section, let me address the claim that agentive *by*-phrases are introduced in VoiceP, originally made in section 3.4. If we look closely, the empirical domain examined in this chapter offers evidence in support of this view. The reasoning goes as follows: We were unable to detect any (relevant) semantic differences between resultative participles in German and English on the one

hand, and Greek and BCS on the other. In fact, we know that sentences like *The package remained carefully opened* say something about an underlying causative event of opening, and this event has an opener. Yet, agentive *by*-phrases are available with BCS (and Greek) resultative participles, but not with English (or German) ones. If the *by*-phrases were *v*P modifiers (e.g., Baker, Johnson & Roberts 1989), it would be possible to exclude them in English resultatives only if there was a semantic difference between them and the BCS ones, causing some kind of semantic anomaly in the English case. Since the semantic difference does not exist (or has not yet been detected), such an explanation is not available. If, however, the agentive *by*-phrase is introduced in VoiceP, then we can exclude it in English by appealing to the height of attachment of the *a*P (to *v*P, not to VoiceP).[27]

Summing up, this section has shown that eventive and resultative passive participles must differ in terms of the verbal structure they embed below the adjectival layer. The stative component of resultative participles is derived differently in the two different classes of languages. In languages like BCS, which encode grammatical aspect on the verb stem, perfective viewpoint aspect is a prerequisite for the derivation of passive participles that involve completed events, namely the bounded eventive and the resultative. I argued that this is because the perfective denotes a relation between an event and its completion. Since resultative participles in BCS-like languages are perfective, they must already include more verbal structure than, for example, English resultatives, which include *v*P (not VoiceP and not AspP). Furthermore, since BCS resultatives include Asp, the presence of lower portions of the verbal structure (including VoiceP) follows. In fact, something additional would need to be said to prevent VoiceP (and hence agentive *by*-phrases) in their structure.

[27] I remain agnostic as to whether VoiceP is projected in short passives. In fact, Williams 2015 makes a good case that the diagnostics generally used to argue that the agent argument is syntactically present in short passives are not valid. If VoiceP is missing in short passives, the only thing that would need to be said is that the *v* in passives is different than the one in anti-causatives, say *v*[cause] vs. *v*[become] (see Folli & Harley 2005, Harley 2008). Indeed, the theme vowel of certain BCS verbs tracks this distinction (cf. *crven-**i**-ti* 'make red', *crven-**e**-ti* 'become red').

## 3.6 Conclusion

This chapter has challenged the long-held assumption that passive participles fall into two subclasses —adjectival and verbal—which belong to distinct lexical categories. I have shown that the diagnostics which have been proposed to zero in on the differences between adjectival and verbal participles in English do not in fact test for category differences. I provided evidence that all BCS passive participles unambiguously belong to the category of adjectives, but that stative and eventive participles must nonetheless be structurally different, only in terms of the verbal structure they embed under the adjectival layer. I have shown that this analysis of passive participles may be viable for a number of languages, in particular English, German, and Greek.

I then argued that the result state component of resultative participles is introduced differently in the two types of languages we considered, namely languages that morphologically encode aspect on verb stems and those that do not. While the result state component in the former languages is introduced by Asp (which attaches above VoiceP), the latter languages have a dedicated stativizing morpheme which, in English and German, selects for *v*P complements. This accounts for the fact that only the former languages allow agentive modifiers with resultative participles. The conclusions reached here also predict that there may be languages that do *not* encode aspect on participles, but still allow resultative participles to combine with agentive *by*-phrases. This would be the result of the stativizing morpheme attaching above VoiceP. One such language seems to be K'iche' (Mayan), as I show in (100); see Duncan 2016 for arguments that the *Vnäq* form is adjectival.[28] The adjectival participle in (100) does not encode aspect, yet the agentive *by-* phrase headed by the relational noun *-umal* is licit.[29]

(100) ...Ixk'at tij-taj-inäq chi le aj rumal. (K'iche', my fieldnotes)
Ixk'at eat-PASS-ADJ.RES already the elote RN
*lit.* 'The elote is already eaten by Ixk'at.'

[28] Semantically, Duncan characterizes the *-Vnäq* element as a perfect, though he acknowledges that its meaning when combined with the passive affix seems to be closer to a resultative.

[29] A general consensus in the Mayan literature is that K'iche' verbs encode Aspect on the verb stem, though these "aspectual" markers are absent on the participle in (100). However, see Bešlin 2023b for arguments that the relevant markers encode Tense, and not Aspect.

The prevalent assumption that eventive and resultative participles differ in category is, first, insufficient to explain the differences between these two types of participles on its own, and, second, incompatible with the morphosyntactic findings from BCS. Most crucially, it is unnecessary if we adopt the analysis developed here. Even if the reader is not convinced by my analysis or finds the postulation of phonologically null stativizers in English-like languages dubious, I think it is important to note that the mere presence/absence of the adjectival layer on the analysis that assumes "adjectival" and "verbal" participles cannot account for the stative/eventive distinction. Data from BCS clearly shows that "being an adjective" (i.e., having the adjectival layer as the topmost structural layer) does not guarantee a stative interpretation, and eventive nominalizations in English (and more broadly) make a similar point. The arguments in this capter therefore support the general conclusion reached in chapter 2, namely that there is no 1-1 mapping between an item's category and its meaning.

The central claims of the chapter, namely that there are no verbal participles and that the passive participles in the examined languages are deverbal adjectives, raise interesting questions for languages that have been claimed to lack adjectives (e.g., Dixon 1977, though see Dixon 2004), or whose adjective inventories are in the single digits. Do these languages have participles? For the latter type of languages, it is worth noting that a small inventory of root-derived adjectives should in principle not be correlated with the degree to which a language can derive adjectives from other lexical categories. For example, the Mayan language K'iche' has few root-derived adjectives, yet its productive perfect participle has been argued to be a deverbal adjective (Duncan 2016). As for the former type of languages, granting that they exist, it is possible that they use other types of non-verbal predication (e.g., deverbal nominals) or relative clauses instead of participial modifiers, though these are perhaps less likely to be called participles. If it turns out that what have been termed "participles" are cross-linguistically simply adjectives (or nouns) which embed varying amounts of verbal structure, this has the desirable consequence of curbing the proliferation of categories (e.g., PartP in various analyses), both in the linguist's arsenal and in the speaker's mental grammar.

# Chapter 4: A case study on active participles

This chapter examines so-called active participles in three languages with different morphological systems (Bosnian/Croatian/Serbian, English, and Hebrew). Based on a wide range of morphological, syntactic, and interpretational diagnostics, I argue that these elements are uniformly deverbal adjectives. This is in contrast to a substantial body of work claiming that active participles show an adjectival/verbal ambiguity, but in line with the previous chapter, which analyzes passive participles as deverbal adjectives. Importantly, deverbal adjectives may denote properties or eventualities (events or states), depending on the characteristics of the verbal structure they embed. If these results generalize to other languages, then there is no need to assume that (verbal) participles constitute a separate grammatical category, which is a desirable theoretical outcome. The results presented in this chapter argue for an architecture of the grammar in which there is no 1-1 mapping between an item's syntactic category and its meaning.

## 4.1 Introduction

In the generative tradition, active participles (101a) have received less attention than passive participles (101b). Nonetheless, research on the two types of participles has taken a similar trajectory. Specifically, there has been a general consensus regarding the active participles' categorial status, with most authors claiming that at least some of them show an adjectival/verbal ambiguity (Chomsky 1957, Fabb 1984, Brekke 1988, Milsark 1988, Bennis & Wehrmann 1990, Parsons 1990, Meltzer-Asscher 2010, 2011b, Biskup 2016, 2019). The disagreement thus far has been restricted to the question of whether all prenominal active participles, like (102), are unambiguously adjectival (Borer 1990, Par-

sons 1990), or if they can be verbal as well (e.g., Brekke 1988, Milsark 1988, Meltzer-Asscher 2010, 2011b). Note at the outset that active participles are distinct from gerunds (103); gerunds have the clausal distribution of nouns, despite looking identical to active participles in English.

(101) a. The police are **arresting** John.

b. John was **arrested** (by the police).

(102) The **smiling** boy entered the room.

(103) **Arresting** John / the evening was not fun for them.

In this chapter, I investigate the interpretation, morphology, and distribution of active participles in English, Bosnian/Croatian/Serbian (BCS), and Hebrew, with the goal of arguing that they are deverbal adjectives. I examine English participles because they have received the most attention in the literature. Being a morphologically poor language, however, English does not always provide us with the strongest positive data about categorization; therefore, I also look at two morphologically-rich languages, one with concatenative morphology (BCS), the other with a non-concatenative/templatic morphological system (Hebrew). Substantial evidence converges on the conclusion that all active participles in these languages have the external syntax (i.e., clausal distribution) and morphology of adjectives, while they are internally verbal.[1] This will be shown to be the case even for active participles that have an eventive interpretation.

The findings in this chapter strengthen the two main conclusions reached in the previous chapter, namely that syntactic category membership is not always straighforwardly reflected in interpretation and that "participle" is unnecessary as an independent category in the grammar. The latter conclusion sets the present analysis apart from both lexicalist and non-lexicalist approaches that argue for the existence of "verbal" participles. A rarely spelled out consequence of adopting "verbal" participles is that they must be a distinct category in the grammar. This is because "verbal" (eventive) participles do not

[1] See Emonds 1991 for an early analysis along these lines; see also Salzmann & Schaden 2019 on the adjectival nature of the (eventive active) double compound perfect in Bernese German.

have the same distribution as other (finite or non-finite) verbs in the languages under consideration. Then, maintaining that the participles are "verbal" forces us to posit a new category in order to account for their syntactic properties.[2] For fully lexicalist analyses, this would have to mean that Part(iciple) is a separate grammatical category, while for non-lexicalist analyses of verbal participles an extra verbal functional projection must be assumed (for example PartP in Doron & Reintges 2005, Migdalski 2006, Meltzer-Asscher 2010, 2011b, Biskup 2019).

The chapter is organized as follows. In section 4.2, I establish the basic interpretive characteristics of active participles and discuss some issues that arise when one attempts to make a strong link between the distributional properties of a linguistic item and its interpretation, for participles and more broadly. In section 4.3, I provide a basic overview of the relevant data in English and sketch out the two competing analyses. In section 4.4, I show that active participles contain some verbal morphology close to the root, but their morphological features are otherwise distinctly adjectival. I show that Hebrew active participles appear in a verbal template, and that BCS active participles are marked with verbal theme vowels. I analyze the "participial" suffix in the concatenative languages as an exponent of the adjectivizing morpheme, and I show that the same suffix appears on root-derived adjectives. In Hebrew, the prefix that appears on the active participle is analyzed as an adjectivizing morpheme. Active participles are shown to inherit their formal features (e.g., gender, number, case and/or definiteness) from nouns to the same extent as adjectives in the languages under consideration. Section 4.5 focuses on the active participle's distributional properties, showing that they mirror the distribution of adjectives, and not verbs. Evidence comes from copula selection, depictive constructions, reduced temporal clauses, attributive modification, *it*-cleft/pseudocleft constructions, and selectional restrictions in BCS deadjectival nominals.

Finally, section 4.6 shows that the diagnostics that have been used to argue for the verbal status

[2]In section 4.6, I will argue that certain distributional differences between two types of participles are due to their meaning rather than their grammatical category. Technically, certain unacceptable sentences will be grammatically well-formed; the deviance will be argued to follow from a semantic clash. Therefore, one could attempt to account for the distributional differences between (finite and non-finite) verbs and verbal participles based on some principled meaning difference (without assuming a category contrast), but no such attempt has been made to my knowledge.

of certain active participles either (i) rest on problematic assumptions or wrong empirical generalizations, or (ii) are sensitive to the semantic properties of the elements they examine. As we will see, some of the diagnostics in that section in fact provide positive evidence that active participles pattern with adjectives, and not with verbs (word order restrictions on modification, *-ly* and *non*-affixation, phasal verb complements, and coordination). In section 4.7, I conclude by discussing the importance of the findings presented in this chapter for our general understanding of grammatical categories.

## 4.2 Interpretation

Many researchers have noted that at least some active participles can have two distinct interpretations; the participle in (104a) denotes an event while the participle in (104b) denotes a state.[3] This has led to the conception that the participle in (104a) is a verb–because verbs canonically denote events–and that the participle in (104b) is an adjective–because adjectives canonically denote states (see e.g., Meltzer-Asscher 2010, 2011b and the references therein).

(104) a. The child is annoying the teacher.

b. the annoying child

Positing a system of transparent mappings from syntactic category to meaning components such as eventivity or stativity is theoretically appealing. In a world where adjectives always denoted states, verbs always denoted events, and nouns always denoted entities, the syntax-semantics interface would arguably be quite straightforward, at least in this particular domain. The view that adjectives and adjectival participles invariably denote states is explicitly adopted in Parsons 1990, Meltzer-Asscher 2010, 2011b, and Gehrke 2015, and tacitly assumed in most generative work on participles.

However, note first that the eventive/stative ambiguity with active participles is clearest in cases like *annoying* in (104) whose verbal counterparts can have both an eventive an a stative reading (105); see Dowty 1979, Pesetsky 1995, i.a. The distinction is much less clear with participles derived from

[3] Throughout, I write the shorthand *denotes X* to mean *denotes a predicate of Xs* or *is of type* $\langle X,t \rangle$.

verbs which do not show such ambiguity. Consider (106), which is derived from an unambiguously eventive verb.

(105) a. Mary (intentionally) annoyed John. *(eventive)*

b. The state of the world annoyed John. *(stative)*

(106) a. The dancing child came into the room.

b. The child was dancing as she came into the room.

Though it is true that *dancing* in (106a) can be understood as either *currently dancing* or *habitually/generally dancing*, it is unclear how this ambiguity is different from the famous stage-/individual-level ambiguities of certain prenominal adjectives; see (107a) with the two interpretations in (107b) and (107c). Crucially, Cinque (2010) argues that the ambiguity is due to the attributive versus predicative origin of the adjective in (107a), and has nothing to do with its category (see Chomsky 1957 and Bolinger 1967 for important predecessors to this idea). The same analysis could arguably be applied to (106a), without positing that the two interpretations arise because of a category difference. I will have more to say about the ambiguity of *annoying*-type participles in sections 4.3 and 4.6.7; for now, it is sufficient to flag that we should be extremely careful about using any diagnostics that invoke meaning contrasts to determine syntactic category.

(107) a. The visible stars include Aldebaran and Sirius.

b. 'The stars that are generally visible include Aldebaran and Sirius' *(individual-level)*

c. 'The stars that are visible now include Aldebaran and Sirius' *(stage-level)*

More generally, it is well-known that verbs can denote permanent properties (e.g., *God exists*) and stage-level adjectives denote transitory eventualities (e.g., *John is hungry*), suggesting that interpretation is not a reliable diagnostic for category membership. Furthermore, as I showed in chapter 3 for passive participles in BCS, Greek, English, and German, having the external syntax and morphology of an adjective is in no way causally related to having a stative interpretation or denoting a property. Instead,

both stative and eventive participles in these languages are adjectives which embed varying amounts of verbal structure. This understanding of the facts is complementary to a prominent line of analysis of deverbal nominals within the Distributed Morphology (DM) framework. Namely, it is well-established that deverbal nouns can embed more or less verbal structure, both across and within languages (e.g., Alexiadou 2001). Differences in interpretation come about due to the presence/absence of the various layers of (non-categorizing) functional structure, and in spite of the presence of the categorizers *v* and *n* (see Wood 2023 for a recent implementation). Yet, the ultimately nominal character of deverbal nouns has not been frequently challenged, which is in stark opposition to participles, where an adjectival/verbal ambiguity is routinely assumed. Hence, in the domain of participles, research has persistently (and erroneously) equated stative interpretations with adjectivehood, and eventive interpretations with verbhood, as we will see throughout the chapter. Here, I will claim that all active participles are adjectives that embed varying amounts of verbal structure. Their external syntax is adjectival, while their internal syntactic properties depend on the properties of the verbal functional structure they contain. While interpretation seems to be correlated to certain syntactic factors (e.g., having a direct object forces an eventuality interpretation of a participle–or noun–regardless of its external syntax), we will see that a specific interpretation is not a direct result of categorization.

## 4.3 Data and competing analyses at a glance

In this section, I will first provide a brief summary of the relevant data in English; sections 4.4 and 4.5 detail the morphological and distributional properties of participles in BCS and Hebrew, as well as English. Then, I will lay out the two competing analyses of participles, making clear at the outset the differences between them.

Active participles can appear in a number of different positions, the most prominent ones shown in (108)-(109). The verb corresponding to the participle in (108), call it PART-ST, is a stative verb; the verb corresponding to the participle in (109), call it PART-EV, is eventive. This is important because

PART-ST and PART-EV behave differently, as noted in Meltzer-Asscher 2010. Namely, while both types of participles can appear as matrix predicates (108a)-(109a), attributive modifiers (108b)-(109b), and in reduced relative clauses (108c)-(109c), only PART-ST can appear as complements of verbs like *seem*, cf. (108d)-(109d). Independently of participles, the verb *seem* can take adjectival complements, but not (bare) verbal complements (110). The complement of *seem* position has therefore been taken as one of the foremost diagnostics for the adjectival status of participles.[4] Note that PART-ST can only appear in the complement position of *seem* if it is not followed by a direct object. This is despite the fact that the verb *love* is transitive, and that the participle in (108a) and (108c) does have a direct object.

(108) a. John is loving this.

b. a treat-loving child

c. Anyone loving this woman is bound to be unhappy.

d. The children seem loving (*their parents).

(109) a. John is washing the car.

b. a self-washing filter

c. The children washing the car will be rewarded.

d. *The children seem washing the car.

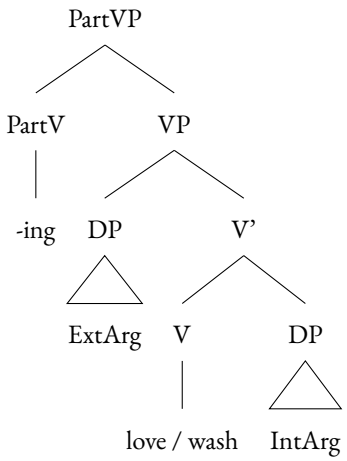
(110) The street seemed calm / *glimmer.

Based on the contrast in (108d)-(109d) and a number of other diagnostics I discuss in section 4.6, the consensus in the literature has been that at least some active participles show an adjectival/verbal ambiguity. As I mentioned in section 4.1, there is some disagreement about the verbal versus adjectival status of PART-EV in attributive position (109b). Beyond this, PART-EV have been argued to be verbal,

[4]There are two verbs *seem* in English, one perceptual and one epistemic, see Matushansky 2002. The restriction on the complement only applies to perceptual *seem*, which takes small clause complements, and not to epistemic *seem*, which takes TP/CP complements. Therefore, it is impossible to say **Mary seems jumping*, but *Mary seems to be jumping* is completely fine. The literature on active participles is concerned only with perceptual *seem*, because it differentiates between PART-ST and PART-EV.

while PART-ST are thought to be ambiguous between verbs and adjectives. For Meltzer-Asscher 2010, for example, the PART-ST in (108b) and (108d) are adjectives, while those in (108a) and (108c) are verbs. Let us spell out in more detail what this distinction entails. Meltzer-Asscher adopts a semi-lexicalist approach, in which adjectival participles are formed in the lexicon and verbal participles in the syntax. For verbal participles, she assumes the structure in (111). Note that, on this view, any morphological and distributional similarities between "verbal" (i.e., eventuality-denoting) participles and adjectives would have to be treated as purely accidental.

(111)




For adjectival participles, she assumes a lexical rule which applies only to stative verbs, (i) changing their category to adjective, and (ii) marking the internal argument for existential closure. An illustration of the lexical rule in action is given in (112a); an example sentence and its denotation are given in (112b), as they appear in Meltzer-Asscher 2010:2231.[5] Syntactically, the participle in (112b) is treated as a simple adjective. There are a number of issues with this lexicalist treatment, but I do not address them here since a non-lexicalist alternative could be made compatible with my account, and my main goal here is to argue against the existence of "verbal" participles.

(112) a. **reveal**-V ($\theta_{cause}$, $\theta_{theme}$, [s]) $\rightarrow$ **revealing**-ADJ ($\theta_{cause}$, $\theta_{theme}$, [s])

b. The shirt is revealing.

$\exists$s[REVEAL(s) & Cause (s, the shirt) & $\exists$x[Theme(s, x)]]

[5]The picture is slightly different for object-experiencer verbs; Meltzer-Asscher takes the existential closure in those cases to introduce a special variable, $x_{arb}$, which ranges over groups of humans. This distinction need not concern us here.

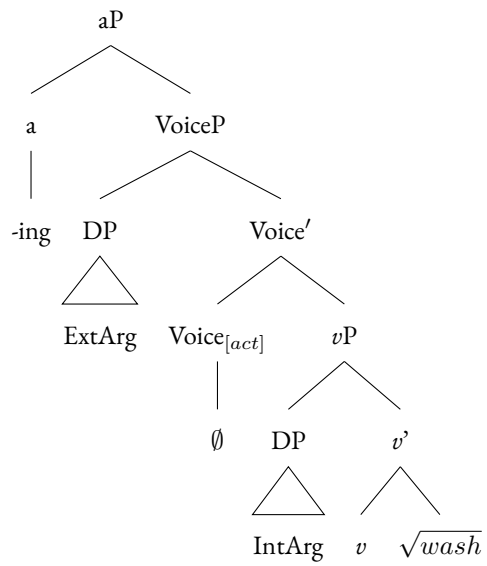
'There is a state of revealing of which the Cause is the shirt, and there is some x which is the Theme of this state.'

The alternative I will argue for in this chapter is broadly in line with the DM framework, in which word-building is a syntactic process and morphological structure is (derived from) syntactic structure. I will argue that all active participles in the languages under discussion are deverbal adjectives. This will mean that they are internally verbal–embedding more or less verbal structure–while their topmost structural layer is adjectival. Active participles of eventive verbs like *wash* in (109) are adjectives that embed a full active VoiceP (113). In (113), *v* is the categorizer that selects the acategorial root and introduces the internal argument. Voice introduces the external argument and licenses accusative case on the internal argument (Kratzer 1996). Finally, the structure is adjectivized. Participles derived from stative verbs are also able to appear in this kind of structure, as in (108a), (108b), and (108c).[6] As we will see in the following sections, the topmost adjectival layer will be responsible for the fact that these participles have the morphological properties of adjectives and appear in the same positions as adjectives, when no further semantic restrictions are imposed.[7] Semantically, these (deverbal) adjectives should (i) be of type $\langle e,\langle v,t\rangle\rangle$, that is, denote a relation between an individual (the Agent/Holder argument) and an eventuality, and (ii) be non-scalar, which will be important in accounting for why they are unable to appear in certain positions available to some other adjectives. The structure in (113) is the structure I assume for Hebrew and BCS eventuality-denoting participles as well.

[6] Active participles are also available from unaccusative verbs like *arrive* and object-experiencer verbs like *frighten*, which have been argued to be unaccusative as well (e.g., Belletti & Rizzi 1988, Pesetsky 1995). These participles have the same structure as (113) except that VoiceP is not projected. More generally, different verbs may have different syntactic structures–the important thing for us is that, however their thematic domain is syntactically represented, the participle has an adjectival projection on top.

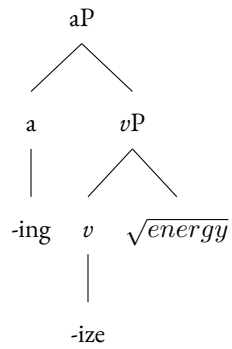
[7] Implementing this is straightforward for operations that are generally thought to involve c-selection, like the ability to combine with a particular affix (putting aside additional semantic restrictions). C-selection is robustly determined under sisterhood, so we expect the selecting element to only be able to see the adjectival layer of the participle, and not its internal structure. With other configurations in which the participle behaves like an adjunct (e.g., depictives, noun modifiers) it is more difficult to appeal to selection, since selection is usually thought to regulate the distribution of obligatory elements. This is a more general issue, however–whatever mechanism regulates the fact that adjectives can modify nouns (*a sad child*), but (infinitive) verbs cannot (**a know child*), will also explain the fact that participles can modify nouns (*a knowing child*).

(113)




As noted by Meltzer-Asscher 2010, stative verbs also give rise to participles like (108d), which can appear in the complement position of verbs like *seem*. I will argue that stative verbs additionally give rise to participles with an impoverished verbal structure, as in (116).[8]

(116)




These participles denote properties, and evidence that they are not simple adjectives comes from their morphology. As we will see, these participles always appear in a verbal template in Hebrew, include a verbal theme vowel in BCS, and some of them have overt verbalizers in English (e.g., *The performance seemed electr**ify**ing / energ**iz**ing)*, see Harley 2009. This is not to say that some of the relevant participles in English are not root-derived adjectives; for example, the equivalent of the Eglish *boring people* in

[8]The absence of the DP internal argument in (116) is presumably due to the absence of Voice, which licenses accusative case. At least some speakers allow these small participles to appear with PP complements instead, as in (i), lending support to this claim.

(114) %As far as we're concerned, the performance seemed [energizing to the crowd].

(115) %She seemed [adoring of her husband].

BCS is *dosad-n-i ljudi* 'boredom-A-PL.M people', where *dosadni* is a root-derived adjective whose morphology clearly differs from deverbal adjectives (participles), as we will see immediately in the following section. Since English does not give us any clues as to the derivation of *boring*, we cannot determine with certainty whether it is a simple adjective or a deverbal adjective that embeds minimal verbal structure.[9]

Why is an adjectivized *v* not interpreted as a predicate of eventualities? There are several options. It could be that the structure [ *v* [ root ]] never denotes a predicate of eventualities, and that it is only some higher functional structure that supplies this meaning; see Anagnostopoulou & Samioti 2013 for such a proposal. Alternatively, in a structure like [ *a* [ *v* [ root ]]], DM architecture allows the presence of *a* to influence the meaning of *v*; in this case the alloseme of *v* in the context of *a* would be null (see Wood 2023 for a development of this idea in the domain of nominalizations).

## 4.4 Morphological generalizations

In this section, I discuss the morphological generalizations that pertain to active participles in the languages under discussion, namely verbal morphology, "participial" marking, and $\phi$-marking. I conclude that there is no morphological evidence that participles are verbs (though there is evidence that they contain verbal structure). In fact, the evidence clearly suggests that participles are treated by the grammar as deverbal adjectives.

### 4.4.1 Verbal morphology

We cannot rely on the morphology of English active participles to tell us much about their category.[10] As already mentioned, some active participles have overt verbalizers, suggesting that they contain at least *v*P (see Harley 2009, i.a.). In BCS, both finite verbs and infinitives, and well as participles

[9]Manner adverbs are often used as a test for the presence of *v*P structure. However, since these structurally small participles are only derived from stative verbs, and stative verbs and adjectives generally have the same modification possibilities, we cannot tell whether it is the verbal or the adjectival projection that is hosting the modifier.

[10]But see Malak 1993 for evidence that the active participle in Old English, a language with richer morphology, bore all the hallmarks of adjectivehood.

(but not members of other categories) contain a so-called verbal theme vowel, which immediately follows the root. This suffix has been argued to be the exponent of *v* (Svenonius 2004, Caha & Ziková 2016, Biskup 2019, Bešlin 2023c). The suffix is different for different classes of verbal stems; I illustrate the three main classes in (117). The verbal theme vowel may vary across the paradigm of an element that contains verbal structure (present tense being notoriously irregular), but the theme of the active participle is always identical to that of the infinitive.

(117)

| | | |
|---|---|---|
| a. spav-**a**-m<br>sleep-V-ISG<br>'I (am) sleep(ing)' | d. vol-**i**-m<br>sleep-V-ISG<br>'I (am) lov(ing)' | g. uč-**i**-m<br>teach-V-ISG<br>'I (am) teach(ing)' |
| b. spav-**a**-t-i<br>sleep-V-INF<br>'(to) sleep' | e. vol-**e**-t-i<br>love-V-INF<br>'(to) love' | h. uč-**i**-t-i<br>teach-V-INF<br>'(to) teach' |
| c. spav-**a**-l-a<br>sleep-V-A-F.SG<br>'(has) slept' | f. vol-**e**-l-a<br>love-V-A-F.SG<br>'(has) loved' | i. uč-**i**-l-a<br>teach-V-A-F.SG<br>'(has) taught' |

In Hebrew, the participle consist of a root in a verbal template, and a prefix (which I analyze as an adjectivizer in the following section). Setting aside the prefix (and $\phi$-marking) for now, note that the verbal template found with participles (118a)–namely *XaXeX*–is also used to form the future (118b), as well as the infinitive (118c), and sometimes the imperative (118d). This template is not used in the derivation of simple nouns or adjectives.

(118)

a. me-**xanex**-et
PREF-educate.V-F.SG
'she is educating'

b. te-**xanex**
FUT.2SG.M-educate.V
'you will educate'

c. le-**xanex**
INF-educateV
'(to) educate'

d. **xanex**-∅
educate.V-IMP-2SG.M
'educate!'

What we have seen is that participles in the three languages show morphological evidence of containing verbal structure. Verbal morphology is found immediately adjacent to the root, suggesting that it attaches low in the structure. In Hebrew in particular, templates are determined based on the first categorizing morpheme (here, *v*), while further derivation is done with affixation, as we will see immediately in the following section.

### 4.4.2 "Participial" marking

The status of the English participial suffix *-ing* is controversial, with most recent literature treating it as a verbal aspectual suffix. The picture is muddled even further by the fact that the suffix also appears in gerunds (e.g., *John's marrying Jane surpised me.*). This has led some researchers to argue that *-ing* is special, and that the result of its affixation may be of any category (see e.g., Milsark 1988). However, the distribuional facts in section 4.5 will lead us to the conclusion that *-ing* affixation never produces verbs. In addition to attaching to certain roots (e.g., *cunning*, *grueling*, *fleeting*), *-ing* may attach to verbal stems (e.g., *electifying*, *energizing*, *jumping*), producing adjectives in both cases. I will therefore conclude that *-ing* is an exponent of the adjectivizing morpheme (*a*).[11]

[11] In the case of English gerunds, there are several possibilities. (1) It may be that the *-ing* suffix is the adjectivizing *-ing*, and the nominalizing suffix is null. (2) English may have an additional nominalizing suffix *-ing*. The latter view does receive some support from the existence of entity-denoting nouns like *building*, *lining*, *painting*, and others. Historically, the two suffixes were distinct: nominal *-ing* comes from Old English *-ung/-ing*, and participial *-ing* from the Old English *-ende* (e.g., Hogg & Fulk 2011), but it is possible that they have merged into one suffix in the synchronic grammar of English speakers. (3) *-ing* may be a root, following recent work that argues some or all derivational affixes to be roots rather than (functional) categorial heads (Lowenstamm 2014, Creemers, Don & Fenger 2018). I will put aside proposal (3) in the remainder of the chapter, for a number of reasons. First, I believe the concerns that prompted Lowenstamm's proposal are successfully dealt with in Embick 2014 without recourse to the assumption that derivational affixes are roots. Second, it is not clear that this model explains the purported categorial flexibility of certain affixes any better than (1) or (2). Instead of assuming (1) or (2) above, this view must stipulate that certain roots can combine only with certain categorial heads, despite their apparent flexibility. For example, *-ian* can combine with adjectives (*reptilian*) and nouns (*librarian*), but not verbs. Furthermore, it is not just *-ian* but the combination of the roots *librar(y)* and *-ian* that determines the availability of the (purportedly null) nominalizer in this case, since *librarian* cannot be an adjective. All these stipulations must be stated somewhere in the grammar. Finally, even if one were to subscribe to the view that some affixes are roots, *-ing* would not be considered a root in the system developed in Creemers, Don & Fenger 2018, given that it is not a stress-shifting affix.

For Hebrew, Meltzer-Asscher 2010, 2011b argues that active participles "appear in a morphological form identical to that of verbs in the present tense, in any one of the five non-passive verbal templates of the language (*XoXeX*, *niXXaX*, *meXaXeX*, *maXXiX*, and *mitXaXeX*)" (Meltzer-Asscher 2010:2212).[12] However, she also claims that, unlike the present tense form (119a), the active participle in (119b) is actually uninflected for Tense (and it instead receives temporal interpretation from the main verb), as in English.

(119) a. dina **kotev-et** mixtav
Dina write-F.SG letters
'Dina is writing letters.'

b. dan ra'a et dina **kotev-et** mixtav
Dan saw ACC Dina write-F.SG letters
'Dan saw Dina writing letters.'

A likely explanation for the identity of the active participle and the "present tense" form in Hebrew is that the "present tense" form in this language is also a participle–a deverbal adjective on my account.[13] As we have seen, the verbal template that is found on participles is accompanied by a prefix; I analyze this prefix as an adjectivizer *a*. On Meltzer-Asscher's account, present tense verbs, "verbal" participles, and adjectival participles all belong to distinct categories, yet they all share identical morphology. This identity has to be treated as a complete accident, despite the fact that Hebrew generally has very few instances of zero-derivation. On my account, all three forms are deverbal adjectives, which is why they share the same morphology.

Note that, in order to obtain present interpretations, uncontroversially non-verbal predicates

[12]Meltzer-Asscher refers to the Hebrew participles and their English *-ing* counterparts as *present* participles. While this is the traditional term, I use the term *active* participle instead because the eventualities denoted by these participles can be interpreted as prior to, simultaneous with, or following the utterance time, as I make clear in the main text.

[13]This has the consequence that Hebrew does not have verbs in the present Tense. While intuitively uncomfortable, this conclusion can be made less unusual by assuming that Hebrew has a (not typologically uncommon) two-way distinction between past and non-past Tense (Comrie 1985). Indeed, Hebrew has a synthetic past Tense form, and a synthetic form that has been called the future, but that I assume is non-past. The same two-way distinction is observed for the copula *h.y.y.* 'be'. Additionally, where most languages that make a past/non-past Tense distinction would use modifiers to disambiguate present from future (at least in matrix clauses), Hebrew uses participles to achieve the same communicative goal (i.e., to indicate present time reference). Since I independently argue that the category–meaning mapping is quite indirect, this seems like a reasonable payoff in that treating participles as (deverbal) adjectives explains their distributional and morphological properties.

must also appear in the form SUBJECT+PREDICATE with no intervening copula (120). This well-known fact shows that Hebrew matrix clauses need not contain a(n overt) verb, so its absence in (119a) cannot be taken as evidence for the verbal status of *kotevet*. Furthermore, both the participial and the nominal predicate require the same copula for past interpretations (121). If *kotevet* in (119a) were a true present tense form, (121a) would involve the addition of a past marker to an overtly marked present tense, a typologically unattested pattern.

(120) dina mor-a
Dina teacher-F
'Dina is a teacher.'

(121) a. dina **hai-ta** me-xanex-et heitev
Dina COP.PAST-F.3SG PART-teach-F.SG well
'Dina used to educate well.'

b. dina **hai-ta** mor-a
Dina COP.PAST-F.3SG teacher-F
'Dina was a teacher.'

Overall, the morphological facts from Hebrew do not support the conclusion that the active participle in this language is verbal. On the contrary, the data suggests that it patterns with non-verbal predicates. The distributional facts in section 4.5 will allow us to pinpoint the category of this non-verbal element as an adjective.

In BCS, I focus on the active participle known in the Slavic literature as the *l*-participle. Note that participles derived from unergative/transitive verbs exhibit the allomorph *ć-* in attributive position; I will avoid these examples to maintain uniformity across examples. Historically, the *l*-participle was used to express so-called retrospective or resultative meanings (see Migdalski 2006: chapter 1). In contemporary BCS, it is very difficult to delineate the meaning of the *l*-participle itself, since it can be used in a number of different configurations, giving rise to meanings as different as (active) simple past, present and past perfect, resultative, and future II (used in embedded and conditional contexts), see Migdalski 2006. In fact, the distinct meanings can largely be attributed to the (independent) aspectual makeup of the copula (*biti* 'be') and the participle; see Migdalski 2006 and Todorović 2016 for detailed

analyses. I detail the distribution of the *l*-participle in section 4.5; for now, note that it is used attributively (122a) and as a complement of the copula in predicative position (123a). In addition to the verbal theme vowel, the *l*-participle contains a suffix that appears on some simple adjectives: *-o* for masculine singular (122b), and *-l* for all other gender/number combinations (123b). Some other examples of such simple adjectives include *kiseo* 'sour', *vreo* 'hot', and *okrugao* 'circular'.

(122) a. o-nemoć-a-o čovek
PF-weakness-V-ADJ
'a man who (has) became weak'

b. zre-o čovek
mature-ADJ man
'a mature man'

(123) a. Danica je is-prič-a-l-a priču.
Danica COP.3SG PF-tell-V-ADJ-F.SG
'Danica (has) told a story.'

b. Šljiva je zre-l-a.
plum COP.3SG mature-ADJ-F.SG
'The plum is ripe.'

This marker seems to be an exponent of the adjectivizing morpheme; note that this is not a $\phi$-feature marker–I will turn to $\phi$-marking next. Before I do that, let me note that the adjectival suffix *-l* is no longer productive in BCS, except with verbal bases. Furthermore, some simple adjectives derived with the suffix *-l* likely date back to Proto-Slavic, for example **kỳslъ* 'sour' from **kys-* + **-lъ*, **-lъ* being the participial/adjectival suffix equivalent to the BCS *-l* (Derksen 2008). However, decomposition into a root and an adjectivizing suffix is still supported in such cases in modern BCS. We can contrast adjectives like *bel-(a)* 'white-F.SG', which were monomorphemic already in Proto-Slavic, with adjectives like *kise-l-a* 'sour-ADJ-F.SG'. While derivation involving *bel-* in BCS always includes the "adjectival suffix" (now part of the root) as in *beliti* 'make white', the root of decomposable adjectives in *-l* can appear independently (e.g., *kisiti* 'to taste sour', *vreti* 'to boil', *zreti* 'to mature' (not, e.g., **zreliti*)).

### 4.4.3 Φ-marking

As expected, the morphology of English is not particularly telling when it comes to $\phi$-marking. However, I will mention that in closely related German agreement (or concord) properties of active participles mirror those of simple adjectives; namely, the participle inherits the $\phi$-features of the noun in the attributive position, and it is uninflected in the predicative position, as seen in (124)-(125), adapted from Haiden 2001:195.

(124) a. ein sing-end-**es** Kind
a sing-PART-NEUT.SG child
'a singing child'

b. ein traurig-**es** Kind
a sad-NEUT.SG child
'a sad child'

(125) a. Sie stieg sing-end(*-e) in den Zug.
she stepped sing-PART-F.SG into the train
'She boarded the train singing.'

b. Sie stieg traurig(*-e) in den Zug.
she stepped sad-F.SG into the train
'She boarded the train sad.'

As we have seen, BCS *l*-participles can also appear in attributive or in predicative position (126). The *l*-participle inflects for case, number and gender the exact same way an adjective does in both of these positions, cf. (127). In both cases, the $\phi$-marking of the adjective/participle is entirely dependent on the formal features of the noun it is associated with: the head noun of the NP modified by the participle in (126a)-(127a), and the subject noun in (126b)-(127b). Note further that a restriction exists on so-called "long forms", in that they are only available in the attributive position; the "short form" is available in both positions, and the pattern is exactly the same for participles and adjectives. In BCS, this morphological distinction is correlated with a meaning contrast between specific and non-specific NPs (Aljović 2002).[14]

[14]In the literature on Slavic participles, one can identify a generalization that the long form is related to stativity and the

(126) a. onemoć-a-**o** / onemoć-a-l-**i** vojnik-∅
weak-V-ADJ.**M.SG** weak-V-ADJ-**M.SG.SP** soldier-M.SG
'a/the soldier who (has) became weak'

b. Ovaj vojnik–∅ je onemoć-a-**o** / *onemoć-a-l-**i**.
this soldier-M.SG COP.3SG weak-V-ADJ-**M.SG** weak-V-ADJ-**M.SG.SP**
'This soldier (has) became weak.'

(127) a. kise-**o** / kise-l-**i** osmeh-∅
sour-ADJ.**M.SG** sour-ADJ-**M.SG.SP** smile-M.SG
'a/the sour smile'

b. Osmeh-∅ im je kise-**o** / *kise-l-**i**.
smile-M.SG they.DAT COP.3PL sour-ADJ.**M.SG** sour-ADJ-**M.SG.SP**
'Their smile is sour.'

In Hebrew, simple attributive adjectives inflect for gender, number, and definiteness, while predicative adjectives inflect only for gender and number (128). The pattern is exactly the same for active participles (129). In this way, Hebrew participles differ from verbs, which index person, in addition to gender and number (130). Analyzing participles as adjectives immediately explains their pattern of agreement.

(128) a. ha-sir-ot **ha**-xum-**ot** (Glinert 2004:104)
DEF-boat-F.PL **DEF**-brown-**F.PL**
'the brown boats'

b. ha-sir-ot xum-**ot**
DEF-boat-F.PL brown-**F.PL**
'The boats are brown.'

(129) a. ha-par-ot **ha**-kofc-**ot**
DEF-cow-F.PL **DEF**-jumping-**F.PL**
'the jumping cows'

short form to eventivity. As I say in the main text, this is not true of BCS (for active or passive participles); the contrast is instead in specificity. While there is of course much to say about the properties of the long and short forms in different Slavic languages (e.g., their clausal distribution, functions, and their aspectual and argument structural restrictions), a full discussion of the facts goes beyond the scope of this dissertation. It is possible and in fact likely that not all Slavic languages retained the adjectival character of the *l*-participle. In Polish, for example, the clitic of the copula *być* 'be' has been reanalyzed as a suffix on the *l*-participle, *pojecha-ł-em* 'go-LPART.M.SG-ISG' 'I went'. Note that the (bare) *l*- used to signal masculine on the participle, but it no longer does so in Polish. Thus, the Polish *l*-participle has been reanalyzed as a finite verb (and has diverged from frozen *l*-adjectives). In a sense, then, the term "*l*-participle" for these forms in Polish is nothing more than a statement about their diachronic origin.

b. ha-par-ot kofc-**ot**
DEF-cow-F.PL jumping-**F.PL**
'The cows are jumping.'

(130) etmol ha-para kafc-**a**
yesterday DEF-cow jump.PAST-**3SG.F**
'Yesterday, the cow jumped.'

We have seen that, while adjectives have language-specific morphological patterns, active participles follow these patterns perfectly. In what follows, I will show that BCS, Hebrew and English active participles have the syntactic distribution of adjectives.

## 4.5 Distributional evidence

In this section, I consider two types of syntactic evidence for the claim that active participles are (deverbal) adjectives. In sections 4.5.1-4.5.3, I show that active participles pattern with adjectives and not with verbs. While these diagnostics do not single out adjectives (to the exclusion of all other categories), the only two options that have been entertained for the languages under consideration are that their participles are verbs or adjectives. Therefore, if we can give evidence that active participles do not pattern with verbs, this provides indirect support for the claim that they are adjectives. Then, in 4.5.4-4.5.6, I offer an additional set of diagnostics that provide positive distributional evidence for the claim that active participles pattern exactly like adjectives, and not like verbs.

### 4.5.1 Copula selection

Without going into too much detail for reasons of space, I simply note that the both adjectives and active participles appear with a copula in predicative position. In this way, they pattern with nouns and PPs, but differently than other finite and non-finite verbs in the languages under consideration. While 'be' and its equivalents in other languages have often been called auxiliaries when they appear with participles, and copulas when they appear with adjectives, Becker 2000 shows that they are mor-

phosyntactically identical in English; the same is true in BCS. As we will see in section 4.6.7.3, certain participles cannot appear with the future copula in Hebrew; comparing the future copula construction and the synthetic future tense, I will argue that the future copula imposes a semantic restriction on its complement. The inability of certain participles to appear as complements of the future copula will be shown to be a consequence of this semantic restriction.

### 4.5.2 Depictives

Let us now look at depictive constructions: constructions that predicate a property of a DP (external or internal argument) that holds throughout the event denoted by the matrix predicate. English depictives can be encoded as root adjectives, participles (active or passive), PPs or DPs (131a), but they can crucially not be verbal elements, be it infinitives or tensed forms (131b). We observe the same pattern in BCS (132). Hebrew does not have depictives; see Schultze-Berndt & Himmelmann 2004 for a cross-linguistic perspective.[15]

(131) a. She found him naked / dancing / annoyed / in a state / a poor man.

b. *She found him (to) dance(d) in the yard.

(132) a. Našla ga je umor-n-og / po-crven-e-l-og /
found him COP.3SG tire-ADJ-ACC.M.SG PF-red-V.ACT-ADJ-ACC.M.SG
očar-a-n-og / u užasnom stanju.
charm-V.PASS-ADJ-ACC.M.SG in terrible state
'She found him tired/reddened/enchanted/in a terrible state.'

b. *Našla ga je po-crven-e-ti / crven-i.
found him COP.3SG PFV-red-V-INF red-V.PRES.3SG
'She found him (to) get red.'

Finite verbs include Tense, so they are categorially different from participles even on a verbal analysis of participles. Influential analyses of depictives treat them as small clauses (e.g., Rothstein 1983) and small clauses never include Tense. What is more difficult is explaining why participles are acceptable in

[15] The BCS pattern is identical in all relevant respects. BCS depictives of the type in (132) cannot be nominal, but that is irrelevant for the point made here.

depictive constructions, but (bare) infinitives are not. Related to this point, it is not clear on a verbal analysis of participles why participles do not combine with modals, but infinitives do. While it is not impossible to describe these contrasts in technical terms (e.g., by stipulating that the participle contains some additional feature), this seems to be unnecessary. Taking into account all the facts presented in this chapter, the simplest account is one where the outward-most layer of participles is adjectival, allowing them to appear in positions available to adjectives, but unavailable to verbs.

### 4.5.3 Reduced temporal clauses

English adjectives and participles (both active and passive) may occur in what I will call a reduced temporal clause, as illustrated in (133a-c). Crucially, the infinitive cannot appear in this construction (133d). At least for English, we can use this test to further show that the distribution of participles mirrors that of adjectives, and not verbs.

(133) a. When wet, the floor is very slippery.

b. When opening the door, make sure to do it quietly.

c. When opened, the door stays that way the whole night.

d. *When (to) open the door, make sure to do it quietly.

Before moving on, I should mention that this test is inconclusive when applied to BCS and Hebrew because none of the equivalents of sentences in (133) are possible, for reasons that are poorly understood.

### 4.5.4 Attributive position

Participles also appear in positions that are otherwise occupied *only* by adjectives. As I showed in (122a) and (126a), BCS active participles can act as prenominal modifiers. In addition to passive participles (which are adjectives, see chapter 3) and simple adjectives, active participles are the only element that can appear in this position in BCS. Meltzer-Asscher acknowledges that even the active participles

that fail her other diagnostics for "adjectivehood" appear in the attributive position in both English and Hebrew (134).

(134) a. a jumping / crying /growing boy

b. yeled kofec / boxe / oxel
boy jumping / crying / eating
'a jumping / crying / eating boy'

She also incorrectly claims that participles derived from eventive transitive verbs cannot appear in the prenominal position. In fact, it is simply the case that participles derived from transitive verbs need to overtly express their internal argument (see Borer 1990). The internal argument cannot follow the participle in attributive position because English obeys the Head-Final Filter, a generalization that attributive adjectives need to appear adjacent to the noun they modify (Williams 1982). This is true both of complements of simple adjectives and of participles (135). English is able to work around the Head-Final Filter by incorporating the object into the participle (136a-b), while languages which have more flexible word order can work around it by expressing fully case-marked internal arguments to the left of the attributive participle, see Dutch in (137).

(135) a. *a fond of Mary boy

b. *a making bricks machine

(136) a. a self-destroying person

b. a brick-making machine

(137) een mij veel overlast bezorgende machine
a me.DAT much trouble.ACC causing machine
'a machine that is causing me a lot of trouble' (Bennis 2004:100)

The claim that the prenominal position in languages like English is occupied only by adjectives has been challenged, but the arguments do not stand up to scrutiny. Sleeman 2011 argues that participial modifiers contain verbal structure, but gives no evidence that they are not (ultimately) adjectives. As

already noted, the fact that prenominal participles contain verbal structure is problematic for the adjectival hypothesis only if one has lexicalist assumptions. Both Sleeman (2011) and Laskova (2007) also assume that being eventive equals being a verb, and conclude from the possibility of eventive interpretations in cases like (134a) that the prenominal position can be occupied by verbs. However, we saw at the beginning of this chapter that it is untenable to equate eventivity with verbhood and stativity with adjectivehood. Moreover, authors who accept this position are on the hook to explain why infinitives do not similarly appear in the prenominal position. Once we accept that interpretation cannot determine category, the hypothesis that the prenominal position in languages like English is occupied only by adjectives is conceptually sound again.

In fact, despite what is often claimed, the prenominal position in English can be occupied by some PPs in addition to adjectives (138). In this position, there is an interesting distibutional contrast between adjectives and participles on the one hand, and PPs on the other. As already mentioned, both prenominal participles (138a) and simple adjectives (138b) in English have to obey the Head-Final Filter. On the other hand, PPs are not subject to the same restriction (138c). This is another instance where participles show the same distribution as simple adjectives, suggesting that syntax does not discriminate between the two based on their category.

(138) a. a smiling (*from ear to ear) boy

b. a happy (*about everything) student

c. an in-your-face management style

We have seen that attributive participles pattern with adjectives on two counts: (i) adjectives, but not verbs, can be attributive modifiers, and (ii) adjectives and participles, but not PPs, obey the Head-Final Filter in English. I therefore conclude that all active participles in these languages, including those derived from eventive verbs, are adjectives.

### 4.5.5 *It*-clefts and pseudoclefts

English adjectives and *-ing* participles are both incompatible with the cleft focus position (139a-b), see Emonds 1991:97. This is in contrast to infinitives, which can appear in this position at least for some speakers (139c).[16] The data in (139) provides clear evidence that the distribution of participles mirrors that of adjectives, and not of verbs.

(139) a. *It was guilty about the exams that the students felt.

b. *It was talking about the exams that the students kept.

c. %It was take the dog to the vet that she didn't do.

Moreover, Emonds observes that in dialects of English in which adjectives may appear in the focus position of a cleft, active participle phrases may also appear there. In some varieties of Irish English, sentences like (140a) are grammatical. In these dialects, (140b) is also grammatical.

(140) a. % It's cold and wet we are.

b. % It is trying to milk the poor you are.

While there is dialectal variation in the *it*-cleft case, let me first note that none of the variation provides evidence for a verbal treatment of participles. In other words, there are no speakers for whom the present participle patterns with infinitives in *it*-clefts, to the exclusion of adjectives. Moreover, presudoclefting provides further evidence that active participles and adjectives share distributional properties. The contrast in (141) is widely accepted. Namely, while infinitival VPs easily undergo pseudoclefting, the same is not true for simple adjectives or the active participle.

(141) a. What she didn't do was take the dog to the vet.

b. *What she didn't keep was warm.

c. *What she didn't keep was taking the dog to the vet.

[16] I do not address the question why the VP *it*-cleft seems to require *do*-support. *Do*-support does not improve (139a-b).

The data we have just seen shows that the distribution of active participles follows that of simple adjectives; where there are dialectal differences in distributional possibilities, the participle still patterns with the uncontroversial adjective. Since distribution is largely determined by the category of an item, I conclude from this that the external syntax of these two elements is identical, namely they are both adjectives.

### 4.5.6 C-selection below the word level

Finally, I discuss the selectional restrictions of the BCS nominal suffix *-ic-*, broadly 'one who is $X_{ADJ}$' (Babić 2002:565). Even though the present discussion is concerned with elements below the 'word' level, I include it in the section on distribution because it pertains to a prime example of c-selection. Namely, the BCS suffix *-ic-* can select for adjectival input, including participles, but it cannot select for verbs. We can observe examples where *-ic-* attaches to simple adjectives (142) and active participles (143) in *-l*, and to simple adjectives (144) and passive participles (145) in *-n*. In (146), I provide a couple of examples to illustrate a general pattern, namely that infinitives cannot serve as input to *-ic-* affixation.

(142) a. debe-l-ic-a fat-ADJ-N-NOM.F.SG

b. z-l-ic-a evil-ADJ-N-NOM.F.SG

(143) a. lut-a-l-ic-a wonder-V-ADJ-N-NOM.F.SG

b. sij-a-l-ic-a light-V-ADJ-N-NOM.F.SG

(144) a. rav-n-ic-a flat-ADJ-N-NOM.F.SG

b. perja-n-ic-a feather-ADJ-N-NOM.F.SG

(145) a. kov-a-n-ic-a mint-V-ADJ-N-NOM.F.SG

b. izabr-a-n-ic-a choose-V-ADJ-N-NOM.F.SG

(146) a. *kov-a-t(i)-ic-a mint-V-INF-N-NOM.F.SG

b. *izabr-a-t(i)-ic-a choose-V-INF-N-NOM.F.SG

All things equal, if active participles are (deverbal) adjectives, we expect them to be able to serve as input to affixation anywhere that a simple BCS *l*-adjective can. While this issue requires a more detailed investigation, (142)-(146) shows that participles behave the same way as adjectives (and not as verbs) in this domain, thus supporting the hypothesis that they are adjectival.

## 4.6 Existing tests do not diagnose a category contrast

In this section I discuss the diagnostics that have been claimed to distinguish between verbal and adjectival participles. Closer examination reveals that some of these diagnostics rely on problematic assumptions or incorrect empirical generalizations. Other diagnostics are instead sensitive to well-established semantic differences which are not dependent on syntactic category.

### 4.6.1 DP-complements

Bennis & Wehrmann (1990) argue that English active participles are verbs because they can have accusative-marked DP complements (147a), while prototypical adjectives cannot (147b). Meltzer-Asscher (2010) shows that the same contrast obtains in Hebrew (148). A similar pattern obtains in BCS. Namely, participles can have accusative-marked complements to the same degree that verbs can; simple adjectives can have genitive- but not accusative-marked nominal complements (149).

(147) a. John is watching her.

b. John is fond *(of) her.

(148) a. hem šam'u ota xosef-et et sodoteha
they heard her reveal.(PART-F.SG) ACC secrets.her
'They heard her reveal her secrets.'

b. ha-viduy šela haya xosfani (*et sodoteha)
the-confession hers was revealing.ADJ ACC secrets.her

(149) a. Jovana je poljub-i-l-a Mark-a.
Jovana COP.3SG kiss-V-ADJ-FEM.SG Marko-ACC
'Jovana (has) kissed Marko.'

b. Jovana je vred-n-a pažnj-e / *pažnj-u.
Jovana COP.3SG worthy attention-GEN ACC
'Jovana is worthy of attention.'

The conclusion that this makes the participles verbs is warranted only on a lexicalist approach, where "being an adjective" entails having no verbal syntactic structure. On a syntactic approach to word formation, the participle will have an accusative-marked complement if it contains the portion of verbal structure that is responsible for licensing it (VoiceP). This does not preclude the claim that the participle is externally adjectival, recall (113). In a similar vein, I have show in chapter 3 that English passive participles derived from ditransitive verbs can have DP complements in unambiguously adjectival positions (150). This should be impossible on a lexicalist account where the adjectival participle is essentially a simple adjective for the purposes of the syntax. On a syntactic account, the pattern can be easily accommodated: the participle in (150) is an adjective which embeds the portion of the verbal structure that licenses the oblique argument.

(150) [...] I seemed granted the ability to recognize things for what they truly were.

(D. Crouse, Copy Cats, p. 140)

More generally, the argument that DP complements diagnose verbhood does not stand up to scrutiny given the well-known case of gerunds (151a) The (ultimately) nominal status of gerunds has seldom been questioned, and yet they appear with accusative-marked DP complements, though simple nouns cannot (151b). On syntactic approaches to word formation, this is accounted by positing a full-fledged VoiceP below the nominal structure.

(151) a. John's marrying her surprised everyone.

b. John sat in the corner *(of) the room.

Taken together, these facts show that having an accusative-marked DP complement–while sug-

gestive of the presence of VoiceP–does not entail that the element in question will have the clausal distribution of a verb.

### 4.6.2 Word order with modifiers

Laskova (2007)notes that English eventive passive participles pattern with verbs in that they allow postmodification by adverbs (152a-b). She contrasts this with the behavior of resultative participles, namely participles that denote a state resulting from an event (Nedjalkov & Jaxontov 1988, Embick 2004). Unlike eventive passive participles and verbs, resultative participles do not allow postomodification by adverbs (152c). Based on this, Laskova concludes that English eventive passive participles are verbs. Building on this work, Meltzer-Asscher (2010) argues that English *-ing* participles must necessarily be verbs, because they are readily postmodified by adverbs (153).

(152) a. The silver was (carefully) polished (carefully).

b. John (carefully) polished the silver (carefully).

c. The silver seems (carefully) polished (*carefully).

(153) a. John was jumping enthusiastically.

b. I saw John jumping enthusiastically.

In chapter 3, I argued that (152c) is ungrammatical because the English resultative participle lacks VoiceP, so there is not enough verbal structure for the verbal stem to move past the adverb to Voice. I showed that the movement generally happens by pointing to examples like (154), where the necessarily local relation between *rely* and *on* is disrupted on the surface because the verb has moved (see chapter 3 for more details).

(154) He relied heavily on me.

Therefore, all that needs to be said for (153) is that the verbal structure of active participles is not impoverished in a relevant way when compared to the finite verb or the eventive passive participle. In

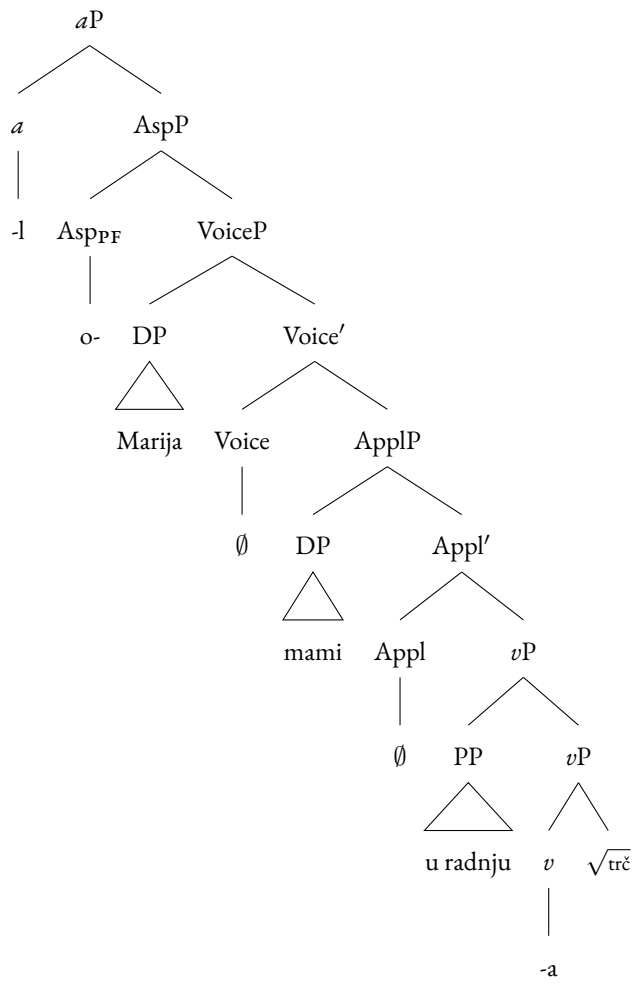
other words, active participles in (153) do not lack VoiceP, which allows the verbal stem to move past the adverb. This seems correct given that VoiceP hosts thematic agents, and *John* in (153) is the thematic agent of the event of jumping, denoted by the *-ing* participle. Therefore, the argument for the category contrast between resultative participles and other participles dissipates. Since the argument presented here provides an analysis of the contrasts, rather than a mere observation, the conclusion reached here carries more weight than the one given in Laskova 2007 and Meltzer-Asscher 2010. My analysis of this data also makes no claims about the categorial status of the relevant elements (i.e., their external syntax) and is compatible with the idea that all participles have the external syntax of adjectives.

If active participles project both *v* and Voice, we also predict that they should license verbal projections which are located above *v* but below Voice. This includes high applicatives, if they are otherwise available in the language in question (see e.g., Harley 2013, 2017). Accordingly, high applicatives are possible with BCS active participles (155); the structure is illustrated in (156).[17] While a closer investigation of this prediction is necessary, I am not aware of any counterexamples to the prediction.

(155) Marija je o-trč-a-l-a **mam-i** u radnju.
Mary COP.3SG PFV-run-V-ADJ-NOM.F.SG mother-DAT in store
'Mary ran to the store for her mother.'

[17] I represent the prefix *o-* in a high aspectual projection here for simplicity; alternatively, the prefix is merged low, and the (perfective) aspectual projection has null exponence (see e.g., Ramchand 2004, Svenonius 2004, Arsenijević 2006, Tatevosov 2011, 2015). Nothing in my proposal hinges on this choice.

(156)




What happens with modifiers in the other languages? BCS is a heavy scrambling language; both pre- and post-modification is possible regardless of the category of the modified element. For Hebrew, I am unaware of previous attempts to use the placement of modifiers to diagnose a category contrast between verbs and adjectives. However, my Hebrew consultants report judgments that support my analysis, and argue against an analysis that some Hebrew active participles are verbs. In particular, PP modifiers with active participles show the same placement possibilities as adjectives, and not as finite or non-finite verbs. PP modifiers that are available with simple adjectives in Hebrew are always postmodificational (157). For finite and non-finite verbs, both pre- and postmodification are possible (158).[18]

[18]Postverbal modification is strongly preferred if the adverb is in focus, for example in (158b) in response to "How is it important to fold the laundry?" The preverbal order is possible if the whole sentence is new information, in response to a question like "Which chores are important?" Information-structural manipulation does not affect the judgments with adjectives and participles.

Crucially, the order PP-verb is available, while the order PP-adjective is not. For active participles, only the postmodification option is available, like with simple adjectives (159).

(157) yeled (*be-mikre) sameax (be-mikre)
child in-occurrence) happy in-occurence
'an accidentally happy boy'

(158) a. dani (?be-zehirut) kipel (be-zehirut) et ha-kvisa (be-zehirut)
Dani in-care fold.PAST.3SG.M in-care ACC the-laundry in-care
'Dani folded the laundry carefully.'

b. xašuv (?be-zehirut) le-kapel (be-zehirut) et ha-kvisa (be-zehirut)
important in-care INF-fold in-care ACC the-laundry in-care
'It is important to fold laundry carefully.'

(159) a. hine yeled (*be-hitlahavut) mekapec (be-hitlahavut)
lo boy in-enthusiasm hop.PART.M.SG in-enthusiasm
'lo, the boy hopping enthusiastically'

b. dani (*be-zehirut) mekapel (be-zehirut) et ha-kvisa (be-zehirut)
Dani in-care fold.PART.M.SG in-care ACC the-laundry in-care
'Dani is folding the laundry carefully.'

We have shown that the English modification pattern is independent of the question of category, and compatible with the claim that all participles are adjectival. In Hebrew, participles are modified in the same positions as adjectives and not verbs, as expected on an adjectival analysis.

### 4.6.3 Phasal verbs

In Meltzer-Asscher 2010, Emonds 1991 is cited for the claim that phasal verbs (*keep*, *resume*, *cease*) take only verbal, but not adjectival complements. In fact, this is not what is claimed in the original paper; the claim is that these verbs select elements with a [+V] feature, regardless of their external syntactic structure (see Emonds 1991:99-100). Nevertheless, let us evaluate Meltzer-Asscher's claim at face value. The idea is that (160) demonstrates that these verbs only take verbal, but not adjectival complements and thus that participles (of intransitive verbs) in (161) must also be verbs.

(160) a. John kept / resumed / ceased watching / annoying me.

b. *John kept / resumed / ceased intelligent / mad at Sam.

(161) John kept / resumed / ceased walking / jumping.

A general point about this diagnostic is that it is not precise to say that the complement of these verbs "must be a verb phrase"; in fact, these verbs specifically require *-ing* complements and no other verb form can take their place (cf. **keep runs/ran/(to) run*).[19] Since the category of the participle is what is at issue, we cannot use this as a diagnostic for categorial status. Furthermore, it is not quite true that these verbs never combine with adjectives; for example *keep* can have adjectival complements, as in *keep calm*, *keep busy*, *keep close*, etc.

However, it is true that *resume* and *cease* cannot take any (root-derived) adjectives as complements. If participles are (deverbal) adjectives, we still have to explain why *-ing* adjectives are allowed as complements of these verbs in (160a)-(161), whereas simple adjectives are not (160b). I'd like to suggest that the *-ing* forms with *cease* and *resume* are, in fact, not participles at all, but rather nominal phrases (gerunds). The first reason to believe this is that these verbs do actually take simple nominals as their complements, as in (162a). Furthermore, (161) can be expanded to include the nominal possessor *his* with no change in meaning (162b), suggesting that the *-ing* form in (161) may be nominal as well. An additional argument for the nominal status of the *-ing* form in the complement of *cease*/*resume* comes from the fact that it can be coordinated with uncontroversial DPs, as seen in (163).[20]

(162) a. They ceased / resumed the peace talks.

b. John ceased / resumed **his** walking / jumping.

(163) He has not resumed running or actual football-related activities.

[19] *Cease* can have infinitival complements, as in *Our region ceased to attract investment*, but the other two verbs in question cannot.

[20] This example is from The Washington Post, available here. Many such examples can be found on the internet; they are judged by native speakers as acceptable and completely unremarkable.

Of course, complements of *cease/resume* can be modified by adverbs, as in (164a), which may be taken as evidence for their verbal (or adjectival) status. However, I take the bracketed constituent in (164a) to have essentially the same structure as (164b), which is a nominalized VoiceP (see Kratzer 1996) that can appear in unambiguously nominal positions, as in (164c).

(164) a. They ceased/resumed [bombing the capital thoroughly].

b. They ceased/resumed [their bombing the capital thoroughly].

c. We were surprised by [their bombing the capital thoroughly].

Even more compellingly, we can provide positive evidence that the *-ing* complements of *cease* and *resume* are not adjectival. It has long been noted that *very* modifies adjectives (though not all adjectives, see below) and not elements belonging to other categories (e.g. Brekke 1988, Emonds 1991, Meltzer-Asscher 2010). Observe moreover that some active participles can be modified by *very*, showing that they are uncontroversially adjectival (165). Now compare (165) to (166), where the *a*P *very flourish-ing* is the complement of the phasal verb–the result is ungrammatical. The ungrammaticality of (166) strongly indicates that the *-ing* complement of *cease* and *resume* is not adjectival, thus explaining why simple adjectives cannot appear in this position.

(165) a very flourishing town

(166) *The town ceased / resumed very flourishing.

Summing up, the complement of *keep/cease/resume* diagnostic cannot be used to determine verb-hood (in English) because (i) no verb form other than the *-ing* form, whose category is in question, can appear in this position, (ii) some adjectives can appear as complements of *keep*, and (iii) *cease* and *resume* take gerundive, not participial, *-ing* complements. We have shown that participles are as impossible in the complement position of *cease* as adjectives are. This is a distributional pattern that sets both apart from infinitives, thus lending further support to the idea that participles are adjectives.

In Hebrew, phasal verbs always take infinitival complements, making the test non-applicable in this language. In BCS, however, we again see participles patterning with adjectives and not with verbs.

Namely, BCS phasal verbs can never take participial or adjectival complements (167a). Instead, they can take finite and infinitival verbal complements (167b), in addition to PPs (167c), and bare nominal complements (167d).

(167) a. *Marija je počela grad-i-l-a kuću / mir-n-a.
Mary COP .3SG started build-V-ADJ-F.SG house calm-ADJ-F.SG
*intended:* 'Mary started building a house/ feeling calm.'

b. Marija je počela grad-i-ti / da grad-i-∅ kuću.
Mary COP.3SG started build-V-INF DA build-V-3SG house
'Mary started building a house.'

c. Marija je počela sa gradnj-om kuće.
Mary COP.3SG started with building.N-INS house
'Mary started building a house.'

d. Marija je počela gradnj-u kuće.
Mary COP.3SG started building.N-ACC house
'Mary started building a house.'

We have seen that complements of phasal verbs do not provide a suitable diagnostic for verbhood in English. On the other hand, phasal verbs in BCS can take both finite or infinitival verbal complements, but not participial or adjectival complements. Once again, participles can be shown to have the distribution of adjectives and not verbs.

### 4.6.4 Adverbial affixation

In English, the suffix *-ly* attaches to adjectives to produce adverbs (168a). A number of authors have observed that only some active participles serve as input to *-ly* suffixation and took this to indicate that only certain active participles can be adjectival in addition to being verbal (Fabb 1984, Brekke 1988, Meltzer-Asscher 2010, i.a.). Meltzer-Asscher 2010:2215 gives the lists in (168b-c) to illustrate the contrast. However, we should first of all recognize that not all simple adjectives serve as input to *-ly* suffixation (168d); therefore, an element that fails to combine with *-ly* may still be adjectival.

(168) a. careful-ly, slow-ly, similar-ly, absolute-ly, annual-ly, particular-ly, sad-ly, curious-ly, mature-ly, furious-ly, usual-ly, sudden-ly...

b. interestingly, surprisingly, excitingly, pleasingly, fittingly, lastingly, compromisingly, forgivingly, shiningly, glimmeringly, inspiringly...

c. *sittingly, *cryingly, *jumpingly, *walkingly, *writingly, *chewingly, *drawingly, *findingly, *foldingly...

d. *parlamentarily, *awarely, *unknownly, *pedestrianly, *bluely, *deadly, *leftly...

Perhaps even more damaging for the view that the different behavior of the participles in (168b-c) stems from a category contrast is the fact that the participles that allow *-ly* suffixation are not necessarily the same participles that appear in other "adjectival" contexts. For example, *glowingly*, *cryingly*, and *jumpingly* are well-formed adverbs according to the Merriam-Webster dictionary (contra Meltzer-Asscher 2010), but the underlying participles cannot appear, for example, as complements of *seem* (e.g., **The girl seemed jumping / crying / glowing*). If the category of the participle is supposed to account for both of these facts, we find ourselves in a paradox. On the account developed here, all participles are (deverbal) adjectives. The reason that some participles cannot appear as bare complements of *seem* has to do with their meaning, not their category, as I discuss in 4.6.7.2. While I am not able to provide a definitive explanation for the contrast between *cryingly* and **walkingly*, some of these contrasts may also be explained by appealing to meaning. The paraphrase *in a walking manner* sounds very odd, while *in a crying manner* is acceptable, possibly because one does not quite know what doing something "in a walking manner" would mean. On the other hand, participles describing *ways* of walking are quite productive in this construction (e.g., *in a limping/stumbling/strutting manner*), and the difference between *walking* and *limping* is unlikely to be one of category. Regardless, the contrast between *cryingly* and *limpingly* on the one hand, and **walkingly* on the other, shows that the relevant restriction on *-ly*-affixation is distinct from the restriction on the complement of *seem*. The restriction on *-ly* affixation is better understood at least in part as a restriction based on our understanding of which par-

ticiples/adjectives can describe ways of doing things in a "in an X manner" construction. Furthermore, if we concede that *-ly* attaches only to adjectives, as is standard in the literature, this means that *crying* and *limping* are (eventive) adjectives, contra Fabb 1984, Brekke 1988, Meltzer-Asscher 2010. The existence of adjectives which denote events goes against the idea that adjectival participles must be stative, and that they are formed only from stative verbs (contra Meltzer-Asscher 2010).

According to Meltzer-Asscher, we observe a pattern similar to English *-ly* affixation in Hebrew, where "adverbs can be formed periphrastically using *be-ofen Adj* ('in a Adj manner')" (Meltzer-Asscher 2010:2215). Again, some participles can serve as input to *be-ofen*, while others cannot (169a-b), leading Meltzer-Asscher to conclude that only the participles in (169a) are adjectival. The first thing to notice is that, again, not all adjectives can appear with *be-ofen* (169c), so the badness of (169b) is not convincing evidence for their non-adjectival status.

(169) a. be-ofen me'anyen / mafti'a / merageš / matmid
in-manner interesting surprising exciting lasting
'in an interesting / surprising / exciting / lasting manner'

b. *be-ofen boxe / kofec / holex / kotev
in-manner crying jumping walking writing
*intended:* 'in a crying / jumping / walking / writing manner'

c. *be-ofen kachol / xasar-xaim
in-manner blue missing-life
*intended:* 'in a blue / dead manner'

Additionally, *be-ofen xasar-xaim* 'in-manner missing-life' in (169c) is fine if *xasar-xaim* is interpreted metaphorically to mean 'lifeless', but not if it is interpreted literally as 'dead'. This further supports the idea that a problem may arise not because of the category of the item, but because of its lexical meaning. Simply put, one does not know what 'in a dead manner' is supposed to mean. I therefore take that the expression of manner in Hebrew is constrained by lexical meaning, in addition to the restriction on syntactic category; it is the lexical meaning that drives the contrast in (169), not category membership.

### 4.6.5 Negative *un-*

Negative *un-* attaches to adjectives, but not to verbs. It also attaches to some *-ing* participles, but not all, as seen in (170) from Meltzer-Asscher 2010:2216. From this contrast, Meltzer-Asscher concludes that only the participles in (170a) are adjectives. Meltzer-Asscher does acknowledge that *un-* cannot attach to all adjectives; for example, adjectives like *unsmart* and *ungood* are ill-formed, and we do not have a good understanding of why that is.[21] This means that we cannot conclude from the fact that an element fails this diagnostic that it is not an adjective.

(170) a. uninteresting, unsettling, unsurprising, unexciting, unpleasing, unfitting, uncompromising, unforgiving, unsuspecting, unassuming, unreasoning, unsparing, unrevealing

b. *uncrying, *ungrowing, *unjumping, *unwalking, *unwriting, *unchewing, *undrawing, *unstanding, *unfinding

We can use the prefix *non-*, which also attaches to adjectives (and nouns), but not verbs, to show that the contrast between (170a) and (170b) is not one between adjectives and verbs. For example, *non-suspecting* is possible alongside *unsuspecting*, and *non-jumping* (e.g., exercises) and *non-chewing* (e.g., diet) are also good, despite these participles' incompatibility with *un-*. This provides positive evidence that (at least some of) the purportedly verbal participles in (170b) are also adjectives. Moreover, not all adjectives in (170b) are bad; for example, *an uncrying baby* or *the nucleus of ungrowing cells* are attested and acceptable. The reason this is relevant is because Meltzer-Asscher's account depends on the idea that all of her diagnostics show a split between the same two groups of participles, which we see is clearly not the case (cf. *ungrowing* and *non-growing*, but **growingly* and **very growing*).

[21] Zimmer 1964 notes that *un-* does not attach to adjectival stems that have a negative value on an evaluative scale, but this does not explain the unavailability of *unsmart* and *ungood*.

### 4.6.6 Coordination

Meltzer-Asscher (2010) argues that it is not possible to coordinate some active participles with simple adjectives; the judgments in (171) are reported as they appear in Meltzer-Asscher 2010:2217. From the purported unacceptability of these coordinated phrases, combined with the view that identity of category is a sufficient (though not necessary) condition for coordination, she concludes that the *-ing* participles in (171) cannot be adjectives.

(171) a. ??a crying and beautiful girl

b. ??a rude and jumping boy

The first thing to note is that, while the above examples may be somewhat odd, they are not unacceptable, especially when compared to, for example, **a rude and jump(s) boy*, which is judged as emphatically bad. Note that we would have no explanation for this contrast in acceptability on the view that both *jumping* and *jump(s)* are verbs. In fact, if *jumping* were a verb, it is not clear how one would account for the contrast between *a jumping boy* and **a jump(s) boy*, even in cases that do not involve coordination.

Furthermore, we can identify several factors that conspire to make (171) sound odd, none of which have to do with category. First off, the two attributes in (171a) stand in opposition, so using *and* is strange, the same way that (172a) is strange compared to (172b), despite both examples coordinating simple adjectives; (172c) sounds much better compared to the original example in (171a).

(172) a. ??a beautiful and miserable girl

b. a beautiful but miserable girl

c. a crying but beautiful girl

Another factor that may contribute to the degraded character of (171) is that coordination of adjectives that do not belong to the same lexical-semantic class often sounds odd in English, as in (173).

This relates to the fact that there are distinct lexical-semantic classes of adjectives which appear in a hierarchy that determines their order in a complex structure (Dixon 1977). In English, the default strategy for attributive adjectives from different classes is to order them according to class, without an overt coordinator.[22] Examples like (171)/(173) are perfectly acceptable without an overt coordinator (174). The same pattern carries over to the coordination of an *-ing* participle and a simple adjective (175).

(173) a. ??a hungry and Serbian girl

b. ??a pink and plastic chair

(174) a. a hungry Serbian girl

b. a pink plastic chair

(175) a. ??a jumping and blue robot

b. a jumping blue robot

This is not to say two adjectives from different classes can never be coordinated in English, see (176). The same is true for active participles and simple adjectives (177), indicating that the generalization in Meltzer-Asscher 2010 is incorrect.[23]

(176) a. a quick and clever response

b. a big and valuable gem

c. a fast and modern car

(177) a. a perspiring and smelly teenager

b. all the raucous and head-banging fans

[22] In Spanish, the only strategy for multiple attributive adjectives is to conjoin them with an overt coordinator. Therefore, *Estamos buscando a un chico Guatemalteco y hambriento*, *lit.* 'We are looking for a hungry and Guatemalan guy' is a perfectly fine sentence.

[23] The fact that the *-ing* participle and the simple adjective can appear in variable order–*a jumping blue robot* and *a blue jumping robot*–suggests that the *-ing* element is not a nominal (gerund) in this construction. While nouns can sometimes appear as prenominal modifiers, they must be the modifier closest to the head noun, as the following example from Kennedy 2013:331 illustrates: *a majestic towering* ***home run*** ball vs. **a majestic* ***home run*** *towering ball*.

c. a ruthless and insulting lowlife

Hence, not only does coordination not provide evidence for the different categorial status of adjectives and active participles, it in fact shows us that they pattern exactly alike (and unlike verbs). Coordination data should therefore be taken to provide positive evidence the claim that active participles are adjectival.

### 4.6.7 Eventualities vs. (scalar) properties

Next, we turn to tests that have been used to argue that only certain participles are adjectives (while others are verbs), but which are better suited for singling out those participles that denote (scalar) properties, rather than a difference in category. I discuss modification by *very*, complements of *seem*, and the compatibility with the future copula in Hebrew. Eventuality-denoting active participles will be shown to be banned from these positions not because they are verbal, but because the position in question requires that the element occupying it be a predicate of (scalar) properties. In other words, some constructions involving active participles will be argued to be syntactically well-formed, but unacceptable due to a a semantic clash. Since this semantic difference is relevant on any account, I will argue that the categorial distinction can be dispensed with completely.

#### 4.6.7.1 Modification by *very*

There is a common observation that *very* generally modifies adjectives; in fact, Brekke 1988:169 takes modification by *very* as "the conventional test for true adjective status" (see also Chomsky 1957). From here, it has been argued that participles which are not modifiable by *very* are not adjectives, cf. (178)-(179) from Meltzer-Asscher 2010:2216. Of course, we must first acknowledge the simple fact that not all simple adjectives are modifiable by *very* (180), so the fact that some participles are incompatible with *very*-modification cannot by itself be taken as evidence against their adjectival status.

(178) a. The movie is very interesting / amusing / boring.

b. Florence is very flourishing.

c. Your brother was very understanding.

(179) *Max is very jumping / growing / crying.

(180) a. *very parliamentary elections

b. *very former presidents

c. *very atomic physics

Additionally, Borer (1990) shows that the compatibility of a participle with *very* and other degree modifiers depends on semantic factors, those that determine whether the verb related to the participle is compatible with the degree modifier *very much*. In (181)-(182), from Borer 1990:97-8, we see that *very* is compatible with a participle only if the verb it is derived from is compatible with (the degree reading of) *very much*.[24]

(181) a. This story amazed/ interested/bothered me very much.

b. a very amazing/interesting/bothering story

(182) a. *This car jumped very much.

b. *a very jumping car.

Even more strikingly, Borer observes that Hebrew *me'od* 'very' can modify both verbs and adjectives, and yet only those verbs that can be modified by *me'od* give rise to participles that allow *me'od* modification (183)-(184). Despite *me'od*'s ability to modify both verbs and adjectives, the split is the same as in English, suggesting that it is not the category of the modified element that is the problem. From here, Borer concludes that the contrasts we observe have nothing to do with the participles' categorial status, but rather with a meaning component that distinguishes the two types of verbs, and, by extension, the participles they give rise to.

[24]In (182), *very much* can only have the irrelevant quantity (amount) reading, and not the intensifier (degree) reading. While *very much*, like many other intensifiers, is generally ambiguous between a quantity and an intensifier reading, *very* is an intensifier only. Only those verbs that allow *very much* as an intensifier have participles that can be modified by *very*; see Bolinger 1972 for detailed discussion.

(183) a. ha-sipur (me'od) 'inyen / shi'amem / hifti'a 'oti (me'od)
the-story (very) interested bored surprised me (very)

b. ha-sipur haya (me'od) me'anyen/ mesha'amem/ mafti'a (me'od)
the-story was (very) interesting boring surprising (very)

(184) a. *ha-para (me'od) kafca (me'od)
the-cow (very) jumped (very)

b. *para (me'od) kofecet (me'od)
cow (very) jumping (very)

What is this meaning component? Brekke (1988) states that the relevant component cannot be gradability because even some gradable verbs such as *grow* give rise to active participles incompatible with *very*, cf. **a very growing child*. However, it is unclear what criteria Brekke uses to determine that *grow* (or *growing*) is gradable. In assuming that the participle *growing* ought to be gradable, Brekke seems be thinking that, when a thing is growing, it may change along some dimension that comes in degrees. For example, a growing child may grow in weight or height. But from this it does not follow that the growing, an event of change, also comes in degrees.

According to Bolinger (1972), degree (or scalar) verbs are those verbs that allow modifiers like *very much* to have an intensifier (degree) reading rather than a quantity (amount) reading. Only verbs like (181a) are considered to be degree verbs and only they give rise to participles modifiable by *very*. Since the relevant difference in the ability to be modified for degree already exists in the two classes of verbs, the simplest explanation for the contrast in (178)-(**??**) and (183b)-(184b) is that the contrast in scalarity is inherited by the participles they give rise to. The simplest explanation, then, does not motivate a category difference between the two types of participles any more than it motivates a category difference between eventive and stative verbs.[25]

[25] *Very* and *me'od* are degree modifiers only; they do not have the quantity reading like *very much* does, which is why (182b) and (184) are plainly unacceptable.

#### 4.6.7.2 Complements of *seem*

As argued at length in Matushansky 2002, (perceptual) *seem* must combine with a complement that denotes a scalar predicate of type ⟨d, ⟨e,t⟩⟩ (a function that maps degrees to functions from individuals to truth values). On the other hand, participles that embed full-fledged VoicePs are of type ⟨e, ⟨v,t⟩⟩ (a function that maps individuals to functions from eventualities to truth-values). Simply put, the denotations of these participles involve eventualities, and the verb *seem* requires a complement with a different denotation, causing a semantic clash.

The fact that *seem* requires a scalar predicate as its complement would also explain why the same participles that are compatible with *very* are able to appear as bare complements of *seem*. This is illustrated in (185); cf. (186) where the participial phrase necessarily denotes an eventuality.

(185) a. John's health seemed (very) worrying.

b. Wearing a tie seemed (very) fitting.

c. She seemed (very) loving.

(186) a. *The boy seemed (very) jumping / growing / crying / eating.

b. *John's health seemed (very) worrying me.

c. *Wearing a tie seemed (very) fitting him.

d. *She seemed (very) loving him.

This correlation between degree modification and the ability to appear in the complement of *seem* position is not restricted to participles either. As Matushansky notes, only those nouns that can be modified by degree adjectives like *complete* or *utter* can be complements of *seem*, cf. (187a-b).

(187) a. He seemed a (complete/utter) fool.

b. *He seemed a (complete/utter) postman.

Since no one is tempted to posit a syntactic category difference between the nouns *fool* and *postman*, the pattern in (185)-(186) should not tempt us to do so for participles either. Certain positions require that the elements occuping them be scalar, and the lexical meaning of some, but not all, nouns and adjectives allows them to be understood as (predicates of) scalar properties.

#### 4.6.7.3 The future copula in Hebrew

The (in)compatibility of some Hebrew participles is another pattern that may be better explained by appealing to the distinction between property- and eventuality-denoting predicates rather than to a category difference. Meltzer-Asscher shows that present participles behave non-uniformly in this context: some are able to follow the future copula (188a-b) and others not (188c). Additionally, Hebrew adjectives but not verbs can follow the future copula (189); see Doron 2003.

(188) a. ha-yeled yihiye me'anyen / mafti'a / meša'aše'a / margiz
the-boy will.be interesting surprising amusing annoying
'The boy will be interesting / surprising / amusing / annoying.'

b. ha-ir tihiye mesagseget
the-town will.be flourishing
'The town will be flourishing.'

c. *ha-yeled yihiye kofec / holex / gadel / boxe
the-boy will.be jumping walking growing crying
*intended:* 'The boy will be jumping / walking / growing / crying.'

(189) a. ha-yeled yihiye yafe / xaxam / xacuf
the-boy will.be beautiful smart rude
'The boy will be beautiful / smart / rude.'

b. *ha-yeled yihiye lo'es mastik / mekapel niyarot
the-boy will.be chewing gum folding papers
*intended:* The boy will be chewing gum / folding papers.'

From this, Meltzer-Asscher concludes that the participles in (188a-b) are adjectives, and that those in (188c) are verbs. This conclusion is premature. The same participles that allow *me'od* modification are also able to appear with the future copula; see Meltzer-Asscher 2010:2215 for details. We

may therefore suspect that, like *me'od* modification, compatibility with the future copula depends on the participle's meaning (namely, whether it denotes a property or an eventuality). In fact, it seems that the future copula in Hebrew cannot combine with eventuality-denoting predicates.

To see this, consider the following. Hebrew has a template for the future tense, and the roots in (188a-b) can also appear in that template, see e.g., (190). Is there a difference in the interpretation of (188b) and (190)?

(190) ha-ir tesagseg
the-town flourish.FUT
'The town will flourish.'

The answer seems to be 'yes'. Consider the ability of the predicate to combine with *be-atsmo* 'by itself' in the two constructions. An element's ability to combine with *be-atsmo* 'by itself' has been argued to diagnose the syntactic presence of a Cause argument (Levin & Rappaport Hovav 1995, Alexiadou & Anagnostopoulou 2004, Koontz-Garboden 2009, Alexiadou & Doron 2012, Kastner 2017, i.a.). Since the Cause argument is associated with the presence of a causing subevent, we expect it to be unavailable with predicates that do not denote eventualities. As expected on my hypothesis, *be-atsmo* 'by itself' is available with the Hebrew equivalent of 'flourish' in the future template, but not when the participle combines with the future copula (191a-b). This suggests that only the participle in (191b) denotes an eventuality. The unacceptability in (191a) arises despite the fact that the participle *mesagseget* can otherwise combine with *be-atsmo* (192a). It is specifically the presence of the future copula that precludes *be-atsmo*. Recall that participles like *mesagseget* are ambiguous between eventuality-denoting and property-denoting predicates; the future copula can only combine with the property-denoting participle.

(191) a. *ha-ir tihiye mesagseget be-atsm-a
the-town.F will.be flourishing from-itself-F.SG
'The town will be flourishing by .'

b. ha-ir tesagseg be-atsm-a
the-town.F flourish.FUT from-itself-F.SG
'The town will flourish by itself.'

(192) a. ha-ir mesagseget be-atsm-a
the-town.F flourishing from-itself-F.SG
'The town is flourishing by itself.'

The evidence clearly suggests that the predicate following the future copula cannot denote an eventuality. Eventive verbs always give rise eventuality-denoting active participles, and they are incompatible with the future copula, as seen in (188c). Since this restriction seems to hold in addition to any c-selectional restrictions of the future copula, the (in)ability to combine with the copula does not tell us anything about the participles' category.

## 4.7 Conclusion

In this chapter, I have challenged the assumption that active participles fall into two subclasses–adjectival and verbal–which belong to separate lexical categories. I argued that interpretation is not a reliable cue for determining category membership. I also showed that both morphological and distributional facts point to the conclusion that active participles are externally adjectival. The adjectival/verbal distinction one finds in the literature is the result of applying diagnostics which (i) rely on problematic assumptions or wrong empirical generalizations, or (ii) are sensitive to the participles' semantic properties. Based on this and a number of positive diagnostics, I argued that all participles in the languages under discussion are (deverbal) adjectives, that there are no "verbal participles", and that "participle" is not a distinct grammatical category. Adopting this conclusion, we are left with a simpler grammar which provides us with better empirical coverage, both desirable results. Since participles are argued not to be an independent category in the adult grammar, we can be relatively confident that they also do not form part of the initial state of the learner, or the inventory of *substantive universals* in the sense of Chomsky 1965.

# Chapter 5: On theoretical approaches to locality in syntax and morphology

## 5.1 Introduction

This chapter focuses on the cyclic nature of syntactic derivations, in particular the notions of successive-cyclicity and the cyclic (piecemeal) spellout of syntactic structure to the interfaces.[1] Note straight away that this is not intended to be a comprehensive overview of the vast literature on these two topics. The purpose of this chapter is to introduce the kinds of locality effects that need to be explained and the theoretical machinery that has thus far been used to capture them. The ultimate aim is to understand what a unified theory of successive-cyclicity and cyclic spellout would look like, and to assess whether this goal seems attainable.

Locality of operations has played an important role in generative theories of syntax, morphology, and phonology since their inception, the general idea being that certain rules or processes have access only to a limited portion of the derivation/representation. The original proposal, formulated in the domain of phonology was that rules operate cyclically, "beginning with the smallest constituent and proceeding to larger and larger constituents" (Chomsky, Halle & Lukoff (CHL) 1956:75) and that "the same rules are reapplied to each constituent in a repeating cycle until the highest constituent is reached" (Halle & Chomsky 1960:275).

Even at these early stages of Generative Grammar, it was understood that one must "take into consideration the hierarchical organization of the utterance" (CHL 1965:65) in order to provide an

[1] This chapter focuses on the spellout of structure to the form interface. There have so far been few attempts to examine the consequences of spellout on semantic interpretation (see Marantz 2013, Wood 2023). The null hypothesis is that cyclic spellout sends chunks of syntactic structure to the interface with form and meaning simultaneously, though there have been suggestions that this is not the case (Marušič 2005 *et seq*.).

explanation of phonological processes such as English phrasal stress. This understanding assumes a 'syntax-first' architecture of the grammar, in which the output of the syntactic derivation feeds the input to the phonology. Additionally, it is nowadays common to assume that syntax delineates (non-trivial) chunks of the structure that it sends to the interfaces at one time, which then at the PF interface serve as domains for the application of (morpho)phonological processes. This idea often goes by the name 'cyclic spellout'.

The cyclic nature of syntactic derivations has itself received its fair share of attention in the literature.[2] Due to empirical considerations, the original notion of *the (Strict) Cycle* was strengthened in syntactic theory with the introduction of the Subjacency condition (section 5.2.2), which essentially prohibits unbounded movement. One of the central questions researchers have asked in the domain of locality has been whether the same mechanism that prohibits unbounded movement is also responsible for the cyclic spellout of syntactic structure, which in turn imposes locality boundaries on the application of morphophonological processes.

The chapter is organized as follows: Sections 5.2 and 5.3 introduce two types of locality effects found in natural language: successive-cyclicity and 'chunking', respectively. I then flesh out the accounts that have been proposed to explain the observed locality effects. Specifically, sections 5.2.1-5.2.3 show that two distinct kinds of proposals have been made to deal with successive-cyclicity effects, namely Minimality and Phase theory (Subjacency). As we will see, these two proposals often produce overlapping restrictions, which raises questions about whether both are needed to regulate the observed locality effects. In section 5.3, I look at 'chunking' effects in the morphophonology, the observation that some morphemes can affect each other's form, while others cannot. Showing that this is not a sim-

[2] I do not go into the details of how *the Cycle* itself has been understood in the history of Generative Grammar and how it is derived. The current understanding seems to be that most, if not all, syntactic properties we attribute to cyclicity come out of the definition of the structure-building operation—binary Merge (i).

(i) a. Merge (X, Y) yields X, Y (Chomsky 2008:138)
b. Merge of X and Y leaves the two syntactic objects unchanged

Specifically, if Merge is understood as in (i), then we can derive the fact that movement is only to c-commanding positions (Extension Condition, Chomsky 1995) and that the output of the syntactic derivation does not contain anything beyond its input (Inclusiveness Condition, Chomsky 1995); see Hornstein 2024 for discussion and extended applications of (i).

ple linear effect, I then present a proposal in the Distributed Morphology (DM) framework, which takes the idea that Phase theory regulates cyclic spellout and attempts to use it to account for morphophonological locality effects. In this way, DM attempts to unify syntactic and morphophonological locality. Section 5.4 provides a conceptual critique of the proposed unification. An extended empirical argument against the unification is offered in chapter 6.

## 5.2 Successive-cyclicity effects

Successive-cyclicity is a property of displacement. As is well known, human languages allow the displacement of material across potentially unbounded distances (193). In (193), the *wh*-phrase *which book* is pronounced many clauses away from its thematic position in the complement position of the most embedded verb *buy*.

(193) Which book did Mary tell Ian [Sally believed
[John remembered
[Leslie thought
. . .
[Kelly said
[Perry's sister would buy <which book>?]]]]]

Now consider (194). In (194a), the complement of the embedded verb, *which book*, moves to the periphery of the embedded clause and no further. In (194b), *which book* moves all the way to the periphery of the matrix clause. A question that arises, then, is whether the derivation of (194b) also involves a step where *which book* moves to the edge of the embedded clause, as in (195).[3]

(194) a. John remembered [which book Perry's sister would buy <which book>].

b. Which book did John remember [(that) Perry's sister would buy <which book>]?

[3]I do not discuss so-called feature percolation theories of successive-cyclicity, employed mostly in the LFG and HPSG frameworks; for discussion and some empirical reasons to prefer (feature-driven) movement approaches, see van Urk 2020b.

(195) Which book did John remember [<which book> Perry's sister would buy <which book>]?

It has become uncontroversial in syntactic theorizing to assume that the movement in (193)/(194b) is not actually unbounded, and that the moving element instead stops (at least) before crossing each clausal boundary on the path of movement (196). Hence, long-distance movement is thought to proceed successive-cyclically through intermediate landing sites.[4]

(196) Which book did Mary tell Ian [ <which book> Sally believed
[<which book> John remembered
[<which book> Leslie thought
. . .
[<which book> Kelly said
[<which book> Perry's sister would buy <which book>?]]]]]

Assuming (196) to be the correct representation has some pleasant empirical consequences. For example, it provides an account for the three-way ambiguity of (197), where the anaphor *himself* can be interpreted as co-referential with any one of the R-expressions. Compare this to (198), where the anaphor can only be bound by the closest R-expression *Bill*.

(197) Which rumors about himself$_{i/j/k}$ did John$_i$ think [Perry$_j$ believed [Bill$_k$ would hate?]]

(198) John$_i$ thought [Perry$_j$ believed [Bill$_k$ would hate those rumors about himself$_{*i/*j/k}$]]

The reasoning is as follows: Anaphors have to be bound within their binding domain, which is (with some caveats) the minimal clause that contains them. This is illustrated in (199), where only *Mary*, but not *Bill* is a potential binder for the anaphor. A clausal boundary intervenes between *Bill* and the anaphor, making the co-reference between the two in (199) illicit.

(199) John$_j$ remembered [Mary$_k$ hated those rumors about herself$_k$/*himself$_j$].

[4]The facts discussed here are not unique to question formation; they have also been shown to hold of relativization, focus structures, and others.

Moreover, embedded questions like (200) show that moving a *wh*-phrase containing an anaphor the embedded clausal periphery clearly counts as local enough and allows the anaphor to be bound by the matrix subject.

(200) John$_i$ remembered [which rumors about himself$_i$ Mary hated <which rumors about himself$_i$>]

Then, if we assume that the DP *which rumors about himself* in (197) moves successive-cyclically, stopping in each clausal periphery on its way to the matrix clause, the ambiguity in binding follows, as illustrated in (201) by showing the binding relations in each position with the relevant indices.

(201) Which rumors about himself did John$_i$ think [<which rumors about himself$_i$> [Perry$_j$ believed [<which rumors about himself$_j$> [Bill$_k$ would hate <which rumors about himself$_k$>?]]

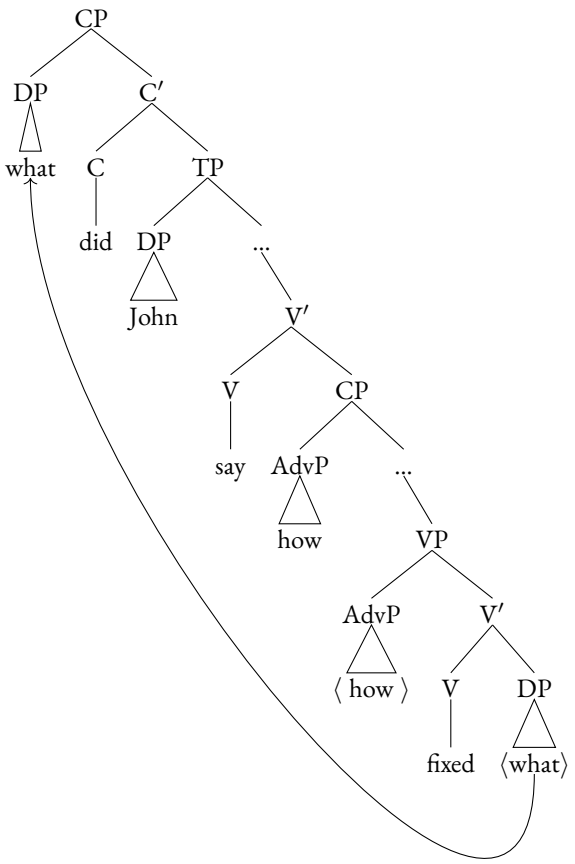
The idea that *wh*-movement is bounded in some way receives further support from contrasts like (202)/(203). Note that (203) is ungrammatical despite the fact that the complement of *say* can generally be an embedded question (204). Moreover, the intended interpretation of (203) is perfectly reasonable, namely 'What was X such that John said how Mary fixed X?'. The issue in (203)–which has been dubbed a *Wh*-Island–is then clearly in combining the extraction of *what* with the embedded question. Now, if *what* were able to move to the matrix clause in one fell swoop, as in (205), the unacceptability of (203) would be a mystery.

(202) What did John say [that Mary fixed <what> with a wrench]?

(203) *What did John say [how Mary fixed <what> <how>]?

(204) John said [how Mary fixed her car <how>].

(205)




A solution to this conundrum is possible if we make the following assumptions: (i) spec, CP of the embedded clause in (203)/(205) is occupied by the phrase *how*, (ii) English CP has only one specifier, (iii) the phrase *what* needs to make an intermediate stop in embedded spec, CP in order to move to the matrix clause. Thus, the locality condition that accounts for the contrast in (202)/(203) has to explain (iii) by prohibiting movement that is too long (in a sense that is made precise below). As we will see, successive-cyclicity effects have received accounts both within Minimality-based approaches (section 5.2.1) and Subjacency theory (section 5.2.2), with its more modern successor Phase theory (section 5.2.3). I explain how the different locality constraints account for the successive-cyclic property of displacement in the relevant sections.

Note that the goal of this introductory chapter is to illustrate the basic patterns and present the theories that have been put forth to capture them and understand their predictions. Many more phe-

nomena have been used to demonstrate the successive-cyclic property of displacement in natural language; they are discussed (with a critical lens) in chapter 7. As we will see, while evidence for the existence of successive-cyclicity is strong, Phase theory, which aims to be a unified theory of locality, fails to capture the observed effects with surprising regularity. I will instead propose that Minimality, which has been argued to explain some overlapping and some distinct effects to Phase theory, is the only mechanism that regulates syntax-internal locality.

### 5.2.1 Minimality

The simplest way to understand the idea behind Minimality is the following: In establishing a syntactic probe-goal relation, a closer goal is preferred over a further away potential goal. I give the formal definition of the Minimal Link Condition (MLC) from Chomsky 1995 in (206), though note that this general idea has a long history in Generative Grammar, including the A-over-A constraint (Chomsky 1964), Superiority condition (Chomsky 1973), Relativized Minimality (Rizzi 1990), and Shortest Move (Chomsky 1993); see Ke 2019 and Branan & Erlewine to appear for recent discussion and formalization of the MLC.

(206) Minimal Link Condition (MLC):

If in a structure $\Sigma$ = H . . . [. . . $\alpha$ . . . [. . . $\beta$ . . . ] . . . ] (where H c-commands $\alpha$ and $\beta$, and $\alpha$ asymmetrically c-commands $\beta$) both $\alpha$ and $\beta$ are of the right type to establish a relation with H, then H can establish a relation only with $\alpha$ (but not with $\beta$).

Of course, it is crucial to determine what is meant by "both $\alpha$ and $\beta$ are of the right type to establish a relation with H". I adopt the idea that, in the configuration in (206), H can establish a relationship with $\alpha$ but not $\beta$ if both $\alpha$ and $\beta$ have the feature H is probing for. This is known as Featural Relativized Minimality (FRM, Rizzi 2004, 2013, i.a.; see also Starke 2001). This is the version of Minimality I adopt in the rest of the dissertation; I will refer to it interchangeably as FRM or simply as Minimality.

So, how does Minimality account for the data we saw in the previous section, which illustrated

the successive-cyclic property of displacement? Recall that we could account for the three-way ambiguity of (201)/(207) by assuming that the *wh*-phrase moves successive-cyclically, stopping in each clausal periphery along its way. Note, however, that the ambiguity we observe shows only that intermediate movements are possible, not that they are obligatory. Hence, while a certain movement may be necessary to derive a desired interpretation due to Binding Theory requirements, it is not at all obvious from this example that the movement is independently necessary in (208), which lacks the relevant binding-sensitive elements.

(207) Which rumors about himself did $\text{John}_i$ think [<which rumors about $\text{himself}_i$> [$\text{Perry}_j$ believed [<which rumors about $\text{himself}_j$> [$\text{Bill}_k$ would hate <which rumors about $\text{himself}_k$>?]]

(208) Which rumors did John think [$\text{Perry}_j$ believed [$\text{Bill}_k$ would hate <which rumors>?]]

Assuming that matrix C in (208) is specified for a [+wh] feature, it will find no interveners on its way to the most embedded clause, and the *wh*-phrase *which rumors* will be able to move to this position without making any intermediate stops. We know from embedded questions, however, that English embedded C can host *wh*-phrases. Hence, the intermediate movement in (207) could still happen, but it would be driven by binding requirements rather than Minimality.[5,6]

The *Wh*-Island case is more interesting for Minimality because it shows what can be thought of as a prime example of intervention. Recall the contrast between (202) and (203), repeated here as (209)/(210).

[5]When I say *driven by binding requirements*, I do not mean that there are binding-related features that drive this movement. Simply, movement to an intermediate spec, CP can happen or not, and if the *wh*-phrase does not move to this position, the relevant interpretation will not be possible. I am agnostic as to what drives this movement. Either movement can be free, possibly in addition to being feature-driven (see Lasnik & Saito 1994; Bešlin 2022 for arguments that at least some movement must be free) or EPP features (one or indefinitely many, depending on the head in question) can be added to heads, which is an assumption often made in the Phase theory literature. See also Bošković 2007 for the view that intermediate movement does not involve feature-checking.

[6]Which features drive movement is an important question in a Minimality-based locality theory. As we will see in chapter 7, these features may vary from probe to probe and from language to language, such that what counts as an intervener may also vary accordingly. For example, we will see that in Dinka (among other languages) non-*wh* DPs count as interveners for *wh*-DPs they c-command, which can be accommodated if C in these languages is simply looking for a [D] feature (Keine & Zeijlstra 2023). While this variability may make a Minimality-based theory less predictive than Phase theory at the outset (since each probe's feature requirements must be individually analyzed), we will see that a less rigid understanding of locality is necessary for empirical reasons. Our theory is still predictive: once we (like the child) figure out the featural specification of a probe, we expect it to be stable across different constructions in the language.

(209) What did John say [that Mary fixed <what> with a wrench]?

(210) *What did John say [how Mary fixed <what> <how>]?

In (209), the [wh]-probe on matrix C finds the only *wh*-phrase in the derivation and moves it to its specifier; there is no intervention and the sentence is grammatical.[7] In (210), the embedded clause is a question, which means that embedded C also has a [wh]-probe. Since the adverb *how* c-commands the internal argument *what*, and both have a [wh]-feature, Minimality ensures that the adverb is attracted to spec, CP of the embedded clause. Matrix C is also endowed with a [wh]-probe. However, for the sentence in (210) to be derived, *what* would need to move to matrix CP across the intervening *wh*-phrase *how*. This constitutes a Minimality violation and (210) is ruled out. What is possible in such a scenario is that the matrix C probes and attracts the closest wh-phrase *how*, in which case we derive (211).

(211) How did John say [<how> Mary fixed what <how>]?

Note that the Minimality-based account of the ungrammaticality of (210) depends on the idea that English C has only one available specifier. If this were not the case, then *what* could conceivably move to an outer specifier of the embedded CP, and then be attracted by matrix C, as in (212), deriving the ungrammatical (210).

(212) *What did John say [CP <what> [CP how Mary fixed <what>]?

The number of specifiers a phrase can have seems to vary cross-linguistically. Thus, Rudin (1988) notes that *Wh*-Islands do not exist in Bulgarian (213), and attributes this to the idea that Bulgarian CP can have indefinitely many specifiers, in contrast to English CP, which can only have one (see also

[7] I will keep referring to [wh]-probes and [wh]-features, but note that the [wh] specification cannot be literally true. This is because fronted VPs, for example, are also sensitive to *Wh*-Islands, as illustrated by the minimal pair in (i). Thanks to Jeff Lidz for bringing my attention to these examples.

(i) a. I thought that John might eat a pizza and eat a pizza I heard he did.
b. *I thought that John would eat a pizza and eat a pizza I heard when he did.

Therefore, fronted VPs and *wh*-phrases must share a more abstract feature which makes them available to the same probe and leads to intervention in (ib). This is supported by the fact that VP-fronting and *wh*-movement cannot co-occur in a simple clause in English (cf. **I asked if John would eat a pizza and eat a pizza when he would?*).

Richards 1997). The *wh*-phrase external argument of both embedded clauses intervenes between C and the most embedded *wh*-phrase, thereby blocking its extraction (214). In order to be extractable, the most embedded *wh*-phrase must first move around each of the external arguments—so-called leapfrogging (Chomsky 1992, Bobaljik 1995, McGinnis 1998, 2001, Keine & Zeijlstra 2023, Halpert & Zeijlstra 2024), see (215). At this point, the *wh*-phrase *koja od tezi knigi* is the closest *wh*-phrase to the matrix C and can satisfy C's [wh]-probe. This leapfrogging movement is available in Bulgarian, whose CP has indefinitely many specifiers, but not in English, whose CP only has one. In this way, the Minimality-based account can accommodate the existence of *Wh*-Islands in some languages but not others.

(213) ?Koja ot tezi knigi se čudiš koj znae koj prodava? (Rudin 1988:457)
which of these books SE wonder-2SG who knows who sells
*lit.* 'Which of these books do you wonder who knows who sells?'

(214) $C_{[wh]}$ se čudiš [CP koj znae [CP koj prodava [WHP koja ot tezi knigi]]]

(215) $C_{[wh]}$ se čudiš [CP <koja ot tezi knigi> [CP koj znae [CP <koja ot tezi knigi> [CP koj prodava <koja ot tezi knigi>]]]]

If the number of specifiers any one projection has can vary from language to language, we may worry about how the child is able to figure this out from the input. At least in the domain of CP, the answer to this question seems pretty straightforward: The number of specifiers can be deduced from looking at the position of *wh*-phrases in matrix environments. In Bulgarian, all *wh*-phrases in matrix clauses move to clause-initial position, giving the child evidence that there can be indefinitely many spec, CPs in this language (216). In English, on the other hand, only one (structurally highest) *wh*-phrase moves to spec, CP in multiple *wh*-questions, as seen in the translation of (216). The child can reason from this that there is only one specifier position in English CP, and that leapfrogging is therefore not possible over a filled spec, CP in English.

(216) Koj kakvo na kogo e dal? (Rudin 1988:461)
who what to whom has given
'Who has given what to whom?'

Before closing this section, let me note that Minimality seems necessary in accounting for many locality phenomena for which a rigid theory like Subjacency/Phase theory (which I turn to next) is inadequate. Minimality also has conceptual advantages as a locality theory over Subjacency/Phase theory in that interference-based effects for the retrieval of information are also observed in tasks that implicate other domains of cognition, including visual, motor, and arithmetic tasks. It is therefore possible that Minimality is a conventionalized functionally-grounded mechanism that arises as a response to memory interference. For a discussion, see chapter 7.

### 5.2.2 Subjacency

In the 1960s, a number of discoveries were made about the locality of movement, most famously in Ross 1967. The so-called *Wh*-Island case we saw in (203) was one famous case, see also (217a); others include the Left Branch Island (217b), which (descriptively speaking) prohibits movement of nominal specifiers/adjuncts; the Adjunct Island (217c), which prohibits movement out of adjoined modifiers; and the Sentential Subject Island (217d), which prohibits movement from clausal subjects.

(217) a. *What [TP did John say how [TP Mary fixed <what> <how >]]? (Wh- Island)

b. *Whose [TP did she fix [DP <whose> car]]? (Left Branch Island)

c. *What [TP did Mary laugh [CP after [TP John fixed <what> ]] (Adjunct Island)

d. *What [TP did [CP that [TP Mary fixed <what>]] please John]? (SS Island)

Subjacency was a theory that came out of an effort to reduce what seemed like a laundry list of unrelated constraints to a single, more general locality principle that could explain all or most of the observed island effects and successive-cyclicity effects more generally. The Subjacency condition, first formulated in Chomsky 1973 and revised in Chomsky 1977, is given in (218). The cyclic nodes (at least in English) are TP and DP. No movement can cross more than one cyclic node at a time.[8]

[8]Despite the early ambitions of Subjacency theory to be a unified theory of islands, it is in fact likely that different islands serve as barriers to movement for different reasons. Some of the island constraints later received explanations that are independent of specific theories of successive-cyclicity. For example, Subject and Adjunct Islands may arise as a consequence

(218) Subjacency condition (Chomsky 1977):

A cyclic rule cannot move a phrase from position Y to position X (or conversely) in ... X ... [$\alpha$... [$\beta$... Y ... ] ... ] ... X ..., where $\alpha$ and $\beta$ are cyclic nodes.

Let us return to our original evidence for successive-cyclicity and see how Subjacency accounts for it. In our binding example, repeated here as (219), moving the *wh*-phrase in one fell swoop is impossible, since the movement path would involve crossing three TPs (bolded). In fact, skipping even a single clausal periphery on the way would involve crossing at least two TPs and would therefore result in a violation of Subjacency. In order to obey Subjacency, then, the *wh*-phrase moves through each clausal periphery (spec, CP) on its way to its landing site, thus accounting for the observed binding patterns in long-distance movement.

(219) Which rumors about himself did [**TP** $\text{John}_i$ think [CP <which rumors about $\text{himself}_i$> [**TP** $\text{Perry}_j$ believed [CP <which rumors about $\text{himself}_j$> [**TP** $\text{Bill}_k$ would hate <which rumors about $\text{himself}_k$>?]]]]]

In the *Wh*-Island case, repeated here as (220), the embedded clause is a question containing a [wh]-probing C, and the initial movement of *how* to spec, CP of the embedded clause is permitted. Then, the movement of *what* to spec, CP of the matrix clause cannot happen in one step because this would involve crossing two TPs (bolded). Usually, the strategy would be to make an intermediate stop in spec, CP of the embedded clause (as in (219)), but this position is occupied by *how*, and English CP has only one available specifier.

(220) *[CP What [**TP** did John say [CP how [**TP** Mary fixed <what> <how>]?

Assuming there are no Minimality-based constraints in the grammar, the derivation of the *Wh*-Island could also proceed by first moving *what* to spec, CP of the embedded clause (and then ultimately

of how complex specifiers and adjuncts are merged to the spine of the tree (Uriagereka 1999, Stepanov 2001). Left-Branch Extraction is in fact not universally banned; it is famously allowed in Slavic languages. See Bošković 2005, 2008, 2009, which ties the (un)availability of Left Branch Extraction to the structural properties of nominal phrases, which may vary cross-linguistically.

onward to spec, CP of the matrix), but at this point there is no way to also move *how* to the periphery of the embedded clause, again because English CP has only one specifier. There is no Subjacency-obeying derivation of (217a), which is why the sentence is deemed unacceptable.

As we can see, Subjacency can deal with the basic cases of successive-cyclicity quite effectively. There were, however, a number of problems with this theory, including the fact that it predicts any A'-movement out of DP to be ungrammatical, since such movement would cross both a DP and TP, (cf. *What did Mary write a book about <what>?*). I now turn to a discussion of the more modern version of Subjacency, namely Phase theory.

### 5.2.3 Phase theory

A shift in thinking about locality comes with the advent of Minimalism (Chomsky 1993), which takes language to be an optimal solution to the constraints imposed by the extra-linguistic cognitive systems for thought and externalization. Though the effects that are intended to be captured do not change dramatically from those that motivated Subjacency, the idea becomes that syntactic locality constraints come about *because* the syntactic derivation has to communicate with language-external systems at (some) specific points in the derivation. This idea takes shape in Chomsky's writings in the early 2000s (Chomsky 2000, 2001, 2008) and is eventually given the name Phase theory. The effects of successive-cyclicity (including some island constraints) are therefore attributed to a model of cyclic spellout, where spellout occurs at multiple points in the syntactic derivation (see Uriagereka 1999 for an important predecessor to this idea). Hence, while Minimality and Subjacency were only supposed to regulate syntax-internal relations, Phase theory aims to be a theory of successive-cyclicity and a theory of cyclic spellout at once.[9]

The condition that regulates cyclic spellout and motivates successive-cyclicity, called the strong Phase Impenetrability Condition (PIC), is formalized in Chomsky 2000, (221), where "the domain

[9]In this chapter, I focus only on the basic evidence taken to motivate Phase theory as a unified locality theory; I consider (and critique) other arguments, both conceptual and empirical, for the existence of phases in chapter 7.

of H" can be read as "the complement of H" and "H and its edge" as "H and any and all specifiers of H". Thus, when phase HP is completed, its complement is sent to the interfaces, and that complement becomes inaccessible for further syntactic computation. Phase heads are usually taken to be (at least) CP and *v*P, though various extensions have been proposed in the literature, the most extreme one being that that every phrase is a phase (Bošković 2002; Boeckx 2003; Müller 2004, 2010, 2011).[10] Coupled with (222), which essentially provides "escape hatches" for moving phrases, Phase theory derives the observed successive-cyclicity effects.

(221) strong Phase Impenetrability Condition, a.k.a. strong PIC (Chomsky 2000:108)
In phase HP with head H, the domain of H is not accessible to operations outside HP, only H and its edge are accessible to such operations.

(222) The head H of phase Ph may be assigned an EPP feature. (Chomsky 2000:109)

Let me spell out how (221)/(222) achieve the desired effects. I will ignore the phasehood of *v*P in this section for simplicity, and only treat C as a phase head. Recall the three-way ambiguity of (197), repeated here as (223). I showed that the ambiguity could be accounted for if it is assumed that the moving phrase stops over at the periphery of each embedded clause, as in (224). (221)/(222) achieve this result easily: each CP is a phase, so its completion triggers spellout of its complement. The complement of the most embedded C in (224) includes the object *wh*-phrase. However, being a phase head, C may (arbitrarily) be assigned an EPP feature, driving movement of the *wh*-phrase to its specifier. The same procedure is repeated for the next higher level of embedding. This is in fact the only way the *wh*-phrase can make it into the matrix spec, CP without getting spelled out prematurely.

[10]Since the hypothesis that every phrase is a phase is not mainstream in current linguistic theorizing, I will not pursue it in detail here. Note that such a conception brings the predictions of Phase theory closer to the predictions of Featural Relativized Minimality (with some important differences, see below). In particular, Phase theory then predicts every locality restriction Minimality predicts. The reasoning is as follows: In the structure [ZP Z ... [$XP_{[+F]}$ X ... [WP W ... [$YP_{[+F]}$ Y]]], if XP asymmetrically c-commands YP, then at minimum, there is some head W that c-commands YP but not XP. Now assume that XP and YP share a feature that is probed for by Z, which c-commands both XP and YP. Minimality predicts that only XP is visible to Z. On the 'every-phrase-is-a-phase' approach, WP is a phase. Since WP intervenes between XP and YP and induces spellout immediately after its completion, the prediction is that YP is inaccessible to operations outside WP (hence, inaccessible to probing by Z). However, the predictions of the 'every-phrase-is-a-phase' approach are in many ways too strict and insufficiently relativized in regard to different probes and goals; this will become abundantly apparent in chapter 7.

(223) Which rumors about himself$_{i/j/k}$ did John$_i$ think [Perry$_j$ believed [Bill$_k$ would hate?]]

(224) Which rumors about himself did John$_i$ think [<which rumors about himself$_i$> [Perry$_j$ believed [<which rumors about himself$_j$> [Bill$_k$ would hate <which rumors about himself$_k$>?]]

In the *Wh*-Island case (225), *how* is first moved to spec, CP of the embedded clause. At this point, *what* is trapped in its base position, unable to move out of the embedded CP because its only available specifier is occupied. Before merging with the predicate of the matrix clause, the CP phase is spelled out with *what in situ*–there is thus no way to derive (225).

(225) *[CP What did John say [CP how Mary fixed <what> <how>]?

Almost immediately after presenting Phase theory in *Minimalist Inquiries*, Chomsky (2001) revises the PIC to the version in (226), which I refer to as the weak PIC. The difference between the strong and weak PIC is in how stringent the locality conditions they impose are. In a nutshell, the difference is the timing of when the complement domain of a phase become inaccessible to further syntactic computation—as soon as the phase is completed (strong PIC) or only when the next phase head is merged (weak PIC); see Citko 2014 for a detailed overview.

(226) weak Phase Impenetrability Condition, a.k.a. weak PIC (Chomsky 2001:14)
Given the structure [ZP Z . . . [HP $\alpha$ [H ' H YP ]]], where H and Z are phase-heads, the domain of H is not accessible to operations at ZP; only H and its edge are accessible to such operations.

The reasons for switching to the weak PIC were entirely empirical and internal to the syntactic system. First note that the PIC is stated over syntactic operations; this includes movement, but also other operations like agreement. According to the strong PIC, and on the standard view that CP and *v*P are phases, *v*P should get spelled out immediately after its completion, and T should never be able to agree with in-situ verbal complements. This prediction is not borne out. In Icelandic 'quirky subject' constructions, (227), the $\phi$-probe on T agrees with the nominative object, and not with the dative subject (that the dative argument is truly the subject is argued for in Andrews 1976, Thráinsson 2014,

Zaenen, Maling & Thráinsson 1985, Sigurðsson 1989, Taraldsen 1995, i.a). Sigurðsson (2000, 2002, 2004) provides compelling evidence that the nominative object in (227) is not moved out of the *v*P, and yet it is available for agreement probing by T.[11]

(227) Henni höfðu leiðst þeir. (Sigurðsson 2000: 87)
her.DAT had.3PL bored.at they.NOM
'She had found them boring.'

This pattern cannot be accounted for with the strong PIC, which predicts *v*P-internal structure to become inaccessible for further syntactic computation immediately upon *v*P's completion. It can, however, be accounted for with the weak PIC, which predicts *v*P-internal structure to be accessible until the next phase-head (C) is merged.

Since its inception, Phase theory has undergone many further revisions. Despite intensive work on the topic in the last 20 years, there does not currently exist a consensus on any of the following questions: Which heads are phase heads? Are they universal or do they vary cross-linguistically? Does the same phrase always act as a phase or not, at least internal to one language? Is strong or weak PIC responsible for spellout? What gets spelled out, phases, phasal complements, or are both options available? Does the PIC make syntactic structure opaque for all syntactic processes, or just movement? For example, is agreement possible across a phasal boundary? Guided by empirical considerations in any given domain of investigation, researchers will choose between the various possibilities. Given this amount of variability, we should be at least highly suspicious of Phase theory as a unified theory of locality, and we may wonder if the theory is simply wrong.

## 5.3 'Chunking' effects

I now turn to a discussion of so-called 'chunking' effects, the observation that some morphemes are visible to each other for the purposes of morphophonological processes, while others are not. In

[11] If Minimality rules, then these data are easily explained if T is a case-discriminating probe; dative DP does not count as an intervener; see chapter 7 for discussion.

this chapter and next, I focus on morphophonological effects, by which I mean effects on form that require reference to specific morphemes and cannot be derived just by appealing to regular phonological processes in the language.[12] An extreme case that I present below and discuss in detail in chapter 6 is contextual, morphologically-conditioned allomorphy (a.k.a. suppletion). This type of allomorphy is found when a morpheme has more than one exponent, and the choice of exponent cannot be predicted based on phonological factors.

An interesting point in the study of morphophonological locality comes with the advent of Distributed Morphology (DM), which explicitly assumes that morphological structure is (derived from) syntactic structure.[13] On this view, syntax is responsible for combining morphemes into hierarchical structures; there is no need for a separate pre-syntactic morphophonological module (the Lexicon). As would be natural in such a framework, the chunks that serve as the domains for the application of morphophonological processes are thought to arise from the cyclic spellout of syntactic structure. We then expect to find cyclic domains within words, and those cyclic domains should be derived from the narrow syntactic structure.

Consider the morphological properties of English nouns. There are a number of different root-attached *n*(ominalizer)s in English, as exemplified in (228). These allomorphs are not interchangeable; the choice of *n* is based on the identity of the root it combines with. Gerunds, on the other hand, show uniform nominal morphology across roots (229). This pattern can be accounted for if we assume that

[12] Though see Newell & Scheer 2007, Newell 2008, Scheer 2012a, and the work that follows them for an application of the approach I present below to 'chunking' effects in phonology proper; see also Scheer 2012b for a historical overview of chunking effect treatment on the syntax-phonology interface.

[13] An alternative derivational view of chunking is pursued in Lexical morphology and phonology (Kiparsky 1982) and its successor Stratal Optimality Theory (Kiparsky 1998 i.a.), which take as a given the existence of a pre-syntactic lexicon. LMP assumes that each level of word-formation process in the lexicon is followed by phonological processes associated with that level. Phonological rules are then divided into lexical, those that apply after one or more level(s) of morphological computation, and postlexical, those that apply after the units produced by the lexicon have undergone combination in the syntax. Lexical phonology is thought to be cyclic, while postlexical phonology is thought to be "intrinsically non-cyclic" (Kiparsky 1982:4). This largely ignores the insights from early Generative phonology, namely that the application of certain phonological rules seems to be influenced by phrasal syntax. Core architectural assumptions (Lexicalism) prevent LMP from deriving the locality of morphophonological rules from syntactic structure, though most morphological processes assumed in Kiparsky 1982 (e.g., selection, endocentric projection, feeding by syntactic rules) have clear syntactic equivalents. As I reviewed in chapter 1, evidence has been mounting over the past three decades in favor of the hypothesis that morphemes are combined by the syntax to the same extent as larger phrases. I therefore pursue the DM conception of 'chunking', leaving aside lexicalist alternatives.

the *n* is able to 'see' and interact with the root in the nominals in (228), but not in the gerunds in (229).

(228) marri-age, grow-th, remov-al, free-dom, divers-ity, strateg-y, ...

(229) marry-ing, grow-ing, remov-ing, free-ing, divers-ify-ing, strateg-iz-ing,...

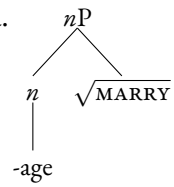
Why would this be? The elements in (228)/(229) are all nominal; they appear in clausal positions that are occupied only by nominals, and can be accompanied by nominal possessors (230)/(231). However, gerunds exhibit syntactico-semantic (and sometimes morphological) properties that suggest they contain some verbal structure. This includes the following properties: (i) gerunds have accusative objects (231), despite the fact that simple nouns in English require *of*-complements (e.g., *a student of physics*); (ii) gerunds are modified by adverbs, not adjectives; (iii) gerunds have an eventive interpretation, which is associated with the presence of verbal structure; (iv) some gerunds include overt *v*(erbalizer)s, for example *-ify* in *diversifying*.

(230) We were surprised by [their thorough diversity of opinions].

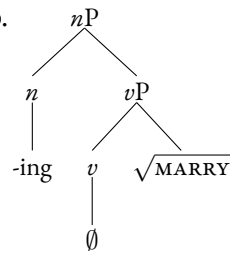
(231) We were surprised by [their diversifying their investments thoroughly].

Considering our discussion of DM architecture from chapter 1, coupled with (i)-(iv), a minimal schematic representation of *marriage* and *marrying* is given in (232). Based on the pattern of allomorphy we observe, it seems that the nominalizer is in the same locality domain to the root in simple nouns (232a), but in some way in a separate locality domain than the root in gerunds (232b).

(232) a.



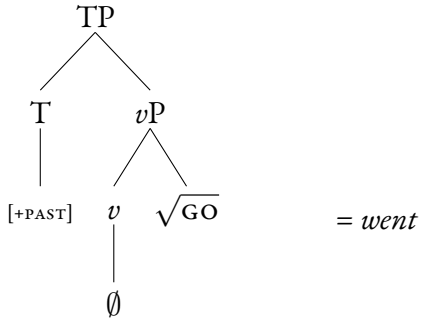

b.




How do we then define the locality domain for morphophonological interactions? It is important to note that the uniform suffix *-ing* in gerunds persists regardless of whether the intervening verbal

structure is overt (as in *diversifying*) or not (as in *marrying*). This suggests that we are dealing with a structural intervention effect, rather than a simple linear one. Furthermore, not every syntactic node counts as an intervener in the sense we are interested in; for example, the English root $\sqrt{\text{GO}}$ is conditioned by T[+PAST] across *v* to produce the allomorph *went* (233). Hence, something more needs to be said about which syntactic nodes close-off the locality domain for morphophonology.

(233)




= *went*

Marantz (2001, 2007) capitalizes on the fact that Phase theory formalizes syntactic locality with an eye towards the interfaces–phases are thought to be chunks of structure which at some point in the derivation become inaccessible to syntactic operations because they have been spelled out to the interfaces. With this in mind, Marantz proposes that the principle regulating morphophonological locality is the same one that is relevant for syntax. This idea has become mainstream in research adopting the DM framework (see e.g., Di Scullio 2004, Volpe 2005, Embick & Marantz 2008, Newell 2008, Embick 2010, 2014, 2015, 2021, Lomashvili & Harley 2011, Ingason & Sigurðsson 2015, Moskal 2015, Choi & Harley 2019, i.a.). If the categorizers "close off" the domain for morphophonological processes, then that domain includes both the root and *n* in (232a), but the root and *n* are not in the same domain in (232b); the root-domain there is delineated by *v*. In early DM Phase theory, the idea was that morphophonological locality could be modeled with the strong PIC in (221), repeated here as (234), as demonstrated by the following quote from Marantz (2001:6-7): "Thus the combination of root and little *x* is shipped off to LF and to PF for phonological and semantic interpretation [...] when a head attaches outside of little *x*, it sees the features of *x* locally, not the features, properties, or identity of the root merged with *x*".

(234) (strong) Phase Impenetrability Condition, a.k.a. strong PIC (Chomsky 2000:108)

In phase HP with head H, the domain of H is not accessible to operations outside HP, only H and its edge are accessible to such operations.

However, Embick 2010 brings examples like (233) to bear on the theory of cyclic spellout: root-allomorphy is not infrequently triggered by functional heads above the first categorizer, which should be prohibited under the strong PIC. Embick (2010, 2014) then adapts a version of the weak PIC, repeated in (235), to the empirical desiderata of morphophonology, as in (236), where *x* and *y* are phase-heads and X and Y are not phase-heads.

(235) weak Phase Impenetrability Condition, a.k.a. weak PIC (Chomsky 2001:14)

Given the structure [ZP Z . . . [HP $\alpha$ [H ' H YP ]]], where H and Z are phase-heads, the domain of H is not accessible to operations at ZP; only H and its edge are accessible to such operations.

(236) Schematization of cyclic domains:

a. Step 1: cyclic *y* merged with [X [ Y [ *x* $\sqrt{\text{ROOT}}$... ]]]

b. Step 2: cyclic domain centered on *x* = [ X [ Y [ *x* $\sqrt{\text{ROOT}}$ ... ]]]] sent to interfaces.

In order to fulfill the desiderata of cyclic morphophonology, it seems that we need a version of the PIC that incorporates components of both the strong and the weak PIC. In particular, given (236), the PIC must be formulated such that spellout does not occur until the second phase-head is merged (reminiscent of the weak PIC) but what is spelled out at this point is the entire domain of the phase head triggering spellout, rather than the complement of the lower phase head (reminiscent of the strong PIC). I propose the formulation in (237).

(237) weak Phase Impenetrability Condition *(amended version)*

Given the structure [ZP Z ... [HP H ... [YP ]]], where H and Z are phase-heads, the domain of Z is spelled out; operations outside ZP cannot reference the morphosyntactic features of Z's domain.

This formulation of the PIC specifically mentions morphosyntactic features, which has the effect that (i) syntactic operations like movement and agreement cannot apply in spelled out domains (a desideratum of the original Chomskyan formulation of Phase theory),[14] and (ii) morphemes in the spelled-out domain can no longer be identified as particular morphemes: for example spelled out roots are no longer visible (qua morphemes) for morphophonological processes like morphologically-conditioned allomorphy (a desideratum of DM Phase theory). The domain of Z is not, however, inaccessible to further phonological operations; see Newell 2008, Embick 2014 for discussion.

I believe that the formulation of the PIC in (237) comes closest to a principle that could potentially unify successive-cyclicity effects and the effects of cyclic spellout at the PF interface. However, in the next brief section, I point out some general conceptual difficulties for the unification of the two types of effects under Phase theory; the unification cannot be successful after all. An extended empirical argument against the unification is presented in chapter 6. Chapter 7 discusses a number of cases for which Phase theory does not seem like an adequate theory of syntax-internal locality.

## 5.4 Against the proposed unification

I have noted in the previous section that Phase theory can account for the relevant morphophonological effects only if a version of the weak PIC is adopted, on which spellout only happens after the second phase-head is merged. This is because non-phasal nodes that intervene between the first and second phasal node have to be able to influence the form of the root, and this would be impossible if the phase was sent to spellout immediately upon its completion. This version of locality is also needed in the BCS cases I discuss in chapter 6. As we will see, BCS *a*(djectivizer)s delimit the locality domain for morphophonological processes, but comparatives and adjectival negation count as part of the first phase (and thus have access to the root) across (overt) phasal *a*s.

For Phase theory as a theory of punctuated movement paths, it seems that strong PIC is still re-

[14] As far as I can tell, the formulation of the PIC in (237) can also accommodate the Icelandic-type cases in (227), which originally motivated the switch to the weak PIC.

quired, which states that the completion of a phase immediately triggers the spellout of its complement. The amended version of the weak PIC is not suitable because it does not force successive cyclic movement of the kind that the literature on Phase theory assumes. Consider (238). The looser way the weak PIC in (237) is stated allows an XP to escape the opacity of the first phase *y*P that dominates it not by moving to the spec of *y*P, but potentially by moving directly to spec, *x*P (since *x* cannot trigger spellout before *x*P is completed, otherwise movement out of *x*P would never be possible). The PIC in (237) does not therefore force movement through the first phase-edge. To the extent that evidence for such movement is robust (and I will argue in chapter 7 that it is), a Phase theory that can unify syntactic and morphophonological locality domains cannot capture it.[15]

(238) $[_{x\text{P}}\ x\ [\ \text{ZP Z}\ [\text{WP W}\ [_{y\text{P}}\ y\ [\ \text{XP}\ ]]]]]$

There has been work attempting to unify syntactic and morphological locality domains using a model in which the whole phase (not the phasal complement) gets spelled out, but assuming that head-movement drives phase-extension in the sense of den Dikken 2007 (e.g., Fenger 2020). The problem here is that a non-phasal head Z must be allowed to see the root across the first phase-head, as in (233). To allow this, we would need to say that the root moves to the first categorizer and then both move to the non-phasal Z, which drives phase extension (239).

(239) $[_{y\text{P}}\ y\ [_{\text{ZP}}\ \textsc{root}\text{+}x\text{+Z}\ \ [_{x\text{P}}\ \textsc{root}\text{+}x\ \ [\textsc{root}\ ]]]]$

At this point, ROOT+*x*+Z is spelled out. However, given the final landing site of ROOT+*x*+Z, we then predict that the next phase-head *y* should have access to the root to the same extent it has access to *x*, which is precisely the outcome DM Phase theory aims to preclude, and which I show in chapter 6 to be impossible.

[15] The original formulation of the weak PIC from Chomsky 2001, given in (226), can be similarly criticized: This formulation of the PIC allows an XP to escape the opacity of the first phase *y*P that dominates it not only by moving to the specifier of *y*P, but also by moving to the spec of any non-phasal phrase (WP, ZP). Therefore, unlike the strong PIC, the weak PIC does not force movement to proceed through specifiers of dedicated phase-heads.

Thus, attempts to unify morphological and syntactic locality effects with Phase theory face serious challenges. Namely, there seems to be no way to state Phase theory in a way that is permissive enough to be able to explain morphophonological effects that would result from the piecemeal spellout of syntactic structure, but constrained enough that it triggers the successive-cyclicity effects as understood in the Chomskyan framework.

# Chapter 6: An argument for distinct locality domains in syntax and morphology

## 6.1 Introduction

In Chapter 5, I presented attempts to unify successive-cyclicity effects and 'chunking' effects under a single theory of locality, namely Phase theory. I then identified a number of irreconcilable differences in the way Phase theory needs to be formulated in order to account for both syntactic and morpho(phono)logical locality effects. Specifically, I showed that while a version of the weak PIC is needed to capture morphological locality effects, only the strong PIC can plausibly have the desired effect on the syntax side, in terms of forcing successive-cyclic movement through specifiers of dedicated phasal heads.

In this chapter, I show that assuming syntactic and morphological locality effects to have a common origin (viz. Phase theory) leads to the wrong empirical predictions. Section 6.2 shows that adjectivization in Bosnian/Croatian/Serbian (BCS) does not impose a locality boundary in the Chomskyan sense (for punctuated movement paths). Specifically, adjectives in BCS do not allow intermediate movement through their specifier, despite allowing their complements to subextract. The two case studies used to demonstrate this point are reconstruction for binding and Quantifier Raising (QR) in Antecedent Contained Deletion (ACD). Conversely, section 6.3 shows that adjectivization in BCS imposes a DM-locality boundary (for allomorphy and morphological tone assignment). These conflicting results suggest that we should rethink the way in which we derive the effects of locality constraints, either on the syntax side or on the morphology side. This conclusion is at odds with the current dominant view, but unsurprising when the conceptual issues regarding a unified Phase theory are taken into ac-

count. This chapter provides an empirical argument against attempts to unify the two types of locality effects without offering a resolution. See chapter 7 for a proposal to dispense with Phase theory as a principle that regulates syntax-internal operations.

## 6.2 BCS adjectives are not Chomskyan phase-heads

In this section I employ the reconstruction for binding diagnostic (section 6.2.1) and the QR in ACD diagnostic (section 6.2.2) to show that (the specifier of) *a*P in BCS cannot serve as an intermediate landing site for A'-movement.[1] Since phases are supposed to force movement though their specifiers, this pattern suggests that BCS *a*P is not a Chomskyan phase-head.

Before moving on, it is worth pointing out here that the diagnostics employed in this section can only ever work in one direction, namely to show that a phrase is *not* a phase. If phases exist, they should force movement to proceed through their specifier. However, diagnostics that show movement to some position is possible are phasehood diagnostics only if it can be shown that phase-heads are the *only* heads that allow movement to their specifier. Such an argument is attempted in Abels 2003, but shown to be problematic in Boeckx 2008 and Müller 2011. Hence, it is possible that non-phases allow movement through their specifiers as well. Therefore, the ability to make an intermediate stop in a certain position could be independent of phasehood. In other words, it does not follow from the ability of a phrase XP to host moving phrases in its specifier that XP is a phase. In cases where, for example, a binding violation would occur if said intermediate stop was not made, the seeming obligatoriness of the intermediate move could be because moving is the only way to satisfy the relevant binding conditions and get the derivation to converge, and not due to phasehood itself. However, since phases are supposed to explain the effects of successive-cyclicity, the *in*ability to make an intermediate stop in a certain position can be taken as evidence for non-phasehood. This is how I will employ the phasehood diagnostics in this

[1] In fact, these diagnostics show that no projection in the extended *a*P in BCS can host moving phrases, a stronger claim. I will focus on the more specific claim regarding *a*P because *a*P will be the target of comparison for the issue of morphophonological locality.

section, though they are typically not used in this way in the work that motivates them.[2]

### 6.2.1 Reconstruction for binding

Reconstruction is an operation assumed to return *wh*-phrases to the extraction site for interpretation at LF (Chomsky 1977). Syntactic evidence for this operation comes from binding-sensitive elements. In (240), the binding requirements of the anaphor *himself* cannot be satisfied in its surface position, because the anaphor is not c-commanded by the potential binder *John*. However, *John* c-commands (and can therefore bind) the anaphor in its base position as the complement of the verb *read*. Assuming reconstruction does not only account for the acceptability of (240), it also provides quite a straightforward way to interpret it, namely as in (241).

(240) [Which book about himself$_i$] did John read <which book about himself$_i$>?

(241) which x did John read [x a book about himself]

Hence, reconstruction for binding should be able to detect movement, including the availability of intermediate stops along the movement path. In order to test for intermediate movement stops, it is necessary to construct examples in which some binding violation occurs in both the surface position and the base position of the *wh*-phrase. If the resulting sentence is grammatical, one can then hypothesize about intermediate movement. Consider (242). First note that both sentences in (242) contain a number of binding-sensitive elements: R-expressions (*Ms. Brown*, *every student*), pronouns (*she*), and bound variable anaphors (BVAs, *he*).

(242) a. [Which (of the) paper(s) that he$_j$ gave to Ms. Brown$_k$]$_1$ (Fox 2000:162)
did every student$_j$ hope [$_{CP}$ **t$_1$'** that she$_k$ would read t$_1$?]

b. *[Which (of the) paper(s) that he$_j$ wrote for Ms. Brown$_k$]$_1$
did she$_k$ hope [$_{CP}$ **t$_1$'** that every student$_j$ would revise t$_1$?]

[2] In chapter 7, I also discuss other kinds of phasehood diagnostics, including morphological reflexes of successive cyclicity, which are not found in BCS or English.

Furthermore, both sentences incur binding violations both when the bracketed *wh*-phrase is evaluated in its surface position and in its base position in the complement of the embedded verb *read/revise*. In the surface position, the requirements of the BVA are not met because it is not c-commanded by its potential binder (*every student*). In the base position, the R-expression *Ms. Brown* is bound by *she*, which yields a Condition C violation. Yet, (242a) is grammatical and (242b) is not. Lebeaux (1988) argues that this divergence can be explained by assuming there is an intermediate landing site in spec, CP of the embedded clause (**$t_1$'**). In (242a), being in this position allows the BVA *he* to be bound by *every student*, which now c-commands it, while *Ms. Brown* remains free with no potential c-commanding binders. On the other hand, the intermediate position in (242b) now incurs both binding violations at the same time: *Ms. Brown* is bound by *she*, and the BVA *he* remains unbound. Similar examples are offered in Fox 2000 to argue for the possibility of intermediate A'-movement to spec, *v*P. The exact same reasoning we applied to (242) can be extended to (243).

(243) a. [Which (of the) paper(s) that $he_j$ wrote for Ms. $Brown_k$]$_1$ (Fox 2000:164)
did every $student_j$ [$_{vP}$ **$t_1$'** get $her_k$ to read $t_1$?]

b. *[Which (of the) paper(s) that $he_j$ gave to Ms. $Brown_k$]$_1$
did $she_k$ [$_{vP}$ **$t_1$'** get every $student_j$ to revise $t_1$?]

Starting with Chomsky 2000 and Legate 2003, data of this kind has been used to argue that CP and *v*P are phases. Recall, however, that this reasoning is not entirely sound. In order to avoid the deductive fallacy I identified, I will draw conclusions about phasehood only from the *un*availability of reconstruction/intermediate movement. Specifically, I will show that an intermediate A'-movement stop is *un*available in spec, *a*P in BCS. Since phases are supposed to explain the effects of successive-cyclicity, the unavailability of an intermediate movement position is a solid indicator that a phrase is not a phase.[3]

[3]This reasoning should hold unless reconstruction is prohibited for independent reasons, as in the case of weak islands, see Abels & Bentzen 2009. BCS *a*P does not seem to be a weak island, as evidenced by the possibility of reconstruction within the *a*P in (i). The anaphor in the fronted *wh*- phrase can only be bound in its base position, namely the complement

The reconstruction diagnostic is only applicable if movement out of a particular domain is generally possible. Hence, I first note that (long-distance) A'-movement out of BCS *a*P is allowed, as illustrated with the *wh-* extraction of its complement in (244).

(244) [Čije pažnj-e]$_1$ je rekla da je Marko vredan $\mathbf{t_1}$?
whose attention-GEN COP said that COP Marko worthy
'Whose attention did she say that Marko is worthy of?'

Evidence that this is true movement (and not, for example, base-generation in the clause-initial position) comes from two sources. First, notice the case connectivity effects; the adjective *vredan* 'worthy' assigns genitive case to its complement, and the case-marking persists on the extracted *wh-* phase in (244). Second, if an island configuration is inserted between the presumed base position and the landing site of the *wh-* phrase, this should lead to ungrammaticality only if movement is taking place. Indeed, if a conjunction phrase is placed in the complement of *a*P and one of the conjuncts appears sentence-initially, the result is unacceptable, presumably due to a violation of the Coordinate Structure Constraint. The unacceptability of (245) suggests that the fronted *wh-* phrase is moved.

(245) *[Čije pažnj-e]$_1$ je rekla da je Marko vredan $\mathbf{t_1}$ i Petrov-e ljubavi?
whose attention-GEN COP said that COP Marko worthy & Petar's-GEN love
*lit.* 'Whose attention did she say that Marko is worthy of and Petar's love?'

I demonstrate the same point with a different kind of island, the Complex NP Constraint, in (246), with the acceptable minimal pair in (247). The complement of the adjective *vredan* 'worthy' in (246)/(247) is a nominal phrase headed by the pronoun *to-ga* 'PRON.NEUT-GEN' which takes a clausal complement. The unacceptability of (246) can be attributed to the fact that movement is generally disallowed from clausal complements of nominals. Hence, fronting from the complement domain of the adjective is genuine A'-movement.

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position of the *a*P.

(i) [Koj-ih svoj-ih$_i$ mana]$_1$ je Marija$_i$ svesna $\mathbf{t_1}$?
which-GEN self's-GEN flaws-GEN is Mary aware
'Which of her flaws is Mary aware of?'

(246) *[Čiji dres]$_1$ je rekla da je Marko vredan to-ga da kupi $\mathbf{t_1}$?
whose jersey.ACC COP said that COP Marko worthy PRON.NEUT-GEN that buy
*lit.* 'Whose jersey did she say that Marko was worth it to buy?'

(247) Rekla je da je Marko vredan to-ga da kupi Ronaldov dres.
said COP that COP Marko worthy PRON.NEUT-GEN that buy Ronaldo's jersey
'She said that Marko was worth it to buy Ronaldo's jersey.'

Now, if Phase theory were to explain the effects of successive-cyclicity in natural language, and if BCS *a* were a phase-head, A'-movement from within its domain should proceed through its specifier. However, the impossibility of reconstruction-for-binding in this position suggests that this is not the case. In (248), the PP moving from the domain of the adjective contains two binding-sensitive elements, the bound variable anaphor (BVA) *svojoj* 'self's' and the R-expression *Marija* 'Mary'. (I corroborate the hypothesized constituency of this sentence below (253)/(254)). The only position in which both elements' binding requirements are fulfilled is the spec, *a*P position. This is presented schematically in (249). In the PP's base position, the R-expression is bound by the pronoun *njene* 'her', which constitutes a Condition C violation. This is because in BCS, unlike in English, possessors c-command outside of their respective NPs, as evidenced by (250); see Despić 2013 for a discussion.[4] On the other hand, the BVA cannot get bound in either the final landing site or the potential stopping point in spec, *v*P of the matrix clause.[5]

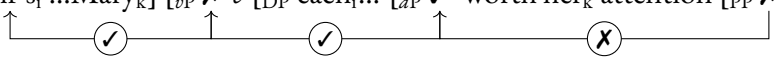
(248) * [Na kojoj svojoj$_i$ žurci na kojoj je bila Marija$_k$]$_1$ je (kasnije) smatran
at which self's party at which COP was Mary COP later considered
svaki od momaka$_i$ vrednim njene$_k$ pažnje $\mathbf{t_1}$?
each of guys worthy her attention
'At which one of his parties that Mary attended was each of the guys later considered worthy of her attention?'

[4]This is not just an effect of linear order; co-referential pronouns can precede R-expressions in BCS as long as they do not c-command them (i).

(i) Kad njegova$_i$ družina svira, Jovan$_i$ igra.
when his group plays Jovan dances
'When his groups plays, Jovan dances.'

[5]As for the internal argument of the passive, BCS allows it to remain in its base position and appear after the verb, as in (248)-(249); in fact, this is the preferred word order under neutral intonation; see Godjevac 2000 for a discussion of unaccusatives versus unergatives. Passives pattern with unaccusatives.

(249) [$_{CP}$[$_{PP}$ at which...self's$_i$ ...Mary$_k$] [$_{vP}$ ✗ *v* [$_{DP}$ each$_i$... [$_{aP}$ ✓ worth her$_k$ attention [$_{PP}$ ✗ ]]]]



(250) * Njegova$_i$ majka voli Jovana$_i$.
his mother loves John
*intended:* 'His$_i$ mother loves John$_i$.'

If a stopping point were available in spec, *a*P in (248)/(249), then the R-expression would escape being bound by the pronoun, and the BVA could get bound by the quantified DP. The fact that (248) is ungrammatical indicates that this stopping point is not available, and thus suggests that BCS *a*P is not a (Chomskyan) phase. If this reasoning is on the right track, then (248) should be grammatical if the BVA is removed, since the binding conditions can then be satisfied in the PP's final landing position. The prediction is borne out, (251).

(251) [Na kojoj žurci na kojoj je bila Marija$_k$]$_1$ je (kasnije) smatran svaki od
at which party at which COP was Mary COP later considered each of
momaka$_i$ vrednim njene$_k$ pažnje **t$_1$**?
guys worthy her attention
'At which party that Mary attended was each of the guys later considered worthy of her attention?'

Let me remark on a couple of things before moving on. First, the acceptability of the English translation of (248) is expected, since the English passive subject obligatory moves to TP. If the subject is in TP and if the *wh-* phrase can stop in spec, passive *v*P (Legate 2003), the binding conditions can be satisfied in this position, without implicating the *a*P. Passive subjects in BCS optionally move out of the *v*P, and (248) with the subject in preverbal position is grammatical (252). For an argument that an intermediate movement stop is available in spec, passive *v*P in BCS, see Bešlin 2023a.

(252) [Na kojoj svojoj$_i$ žurci na kojoj je bila Marija$_k$]$_1$ je svaki od momaka$_i$
at which self's party at which COP was Mary COP each of guys
(kasnije) smatran vrednim njene$_k$ pažnje **t$_1$**?
later considered worthy her attention
'At which one of his parties that Mary attended was each of the guys later considered worthy of her attention?'

Second, note that the discussion here assumes the moved PP to be generated lower than the adjectival complement, in a cascade structure à la Pesetsky 1995. The justification for this comes from examples like (253). The BVA's antecedent *svaki od momaka* 'each of the guys' c-commands the PP-internal BVA regardless of whether the PP attaches above or below the adjective. Therefore, the relative position of the adjectival complement 'X's attention' and the *at*-PP must be determined by looking at the (im)possible placements of the bolded R-expression and pronoun. The only grammatical option is to have the R-expression in the adjectival complement, and the pronoun in the *at*-PP (253). The opposite is impossible (254), presumably because the pronoun would c-command and bind the R-expression in the *at*-PP. This pattern confirms the hypothesized structure in (249).

(253) Kasnije je smatran svaki od momaka$_i$ vrednim **Marijine$_k$** pažnje na svojoj$_i$
later COP considered each of guys worthy Mary's attention at self's
božićnoj žurci na kojoj je **ona$_k$** bila.
Christmas party at which COP she was
'Each of the guys was later considered worthy of Mary's attention at his Christmas party that she attended.'

(254) * Kasnije je smatran svaki od momaka$_i$ vrednim **njene$_k$** pažnje na svojoj$_i$
later COP considered each of guys worthy her attention at self's
božićnoj žurci na kojoj je **Marija$_k$** bila.
Christmas party at which COP Mary was
*lit.* 'Each of the guys was later considered worthy of her attention at his Christmas party that Mary attended.'

In this section I looked at reconstruction for binding and concluded that BCS *a*Ps do not allow intermediate A'-movement to proceed through their specifier, despite allowing subextraction from their complement domain. This suggests that BCS *a*P is not a Chomskyan phase.[6] I now turn to another diagnostic which demonstrates the same point.

[6]BCS participles, argued in chapters 3 and 4 to be deverbal adjectives, do allow intermediate reconstruction of the kind shown to be impossible for adjectives. This is unsurprising given that participles contain verbal structure, and BCS *v*P allows reconstruction to the same extent as its English equivalent in (243), see Bešlin 2023a.

### 6.2.2 Quantifier Raising in Antecedent Contained Deletion

The non-phasehood of BCS *a*Ps can also be demonstrated with Quantifier Raising (QR) in Antecedent Contained Deletion (ACD). ACD is a construction in which the elided constituent is contained within its antecedent, as illustrated in (255). In (255), the antecedent $v\text{P}_1$ contains the elided constituent, $v\text{P}_2$.

(255) Mary will [$_{v\text{P}1}$ read every book Liz will [$_{v\text{P}1}$ e]].

Now, ellipsis is thought to require syntactic and/or semantic identity of the antecedent and the ellipsis site (see Lasnik & Funakoshi 2018 and Merchant 2018 for accessible overviews). As can be seen in (256), requiring identity of the two *v*Ps creates an infinite regress problem, which should arguably lead at the very least to reduced acceptability. Yet, (255) is acceptable and completely unremarkable (Bouton 1970).

(256) Mary will [$_{v\text{P}1}$read every book Liz will [$_{v\text{P}2}$ ~~read every book Liz will [$_{v\text{P}3}$ read every book~~... ]]]

In order to account for how the identity requirements on ellipsis are satisfied contrary to appearance, it has been suggested that the QP in (255)/(256) needs to vacate the *v*P, along the lines of (257); the mechanism proposed to do this is QR (May 1985, Larson & May 1990, Fiengo & May 1994, Kennedy 1997, Harley 2002, *pace* Hornstein 1994). QR is covert (A'-)movement, movement which happens after (a portion of) the derivation has been shipped off to the form interface (PF).[7]

(257) [$_{\text{CP}}$ [$_{\text{QP}}$ every book Liz will [$_{v\text{P}2}$ ~~read~~ ]]$_1$ [ $_{\text{CP}}$ Mary will [$_{v\text{P}1}$ read t$_1$ ]]]

Circling back to phases, Legate 2003 uses ACD to argue for the phasehood of *v*P. The reasoning is as follows: ACD forces QR, and QR must obey cyclicity, just like overt movement. Then, "since the

[7]Note that the QR solution, and in fact any solution that involves movement to achieve parallelism between the antecedent and the ellipsis site, only works if movement leaves a trace and not a copy. The copy theory of movement, proposed in Chomsky 1993, has been preferred on both conceptual and empirical grounds for about two decades now, but on a copy theory the infinite regress problem persists, cf. [$_{\text{CP}}$ [$_{\text{QP}}$ every book Liz will [$_{v\text{P}2}$ read ]]$_1$ [ $_{\text{CP}}$ Mary will [$_{v\text{P}1}$ read <[$_{\text{QP}}$every book Liz will [$_{v\text{P}2}$ read [$_{\text{QP}}$ every book... ]]]>]]. As with other controversial assumptions made in the Phase theory literature, I ignore them for the time-being in order to assess the behavior of *a*Ps on their terms.

phase is the minimal unit sent to LF for interpretation, the phase edge is the only possible target for QR" (Legate 2003:509).[8] Recall that this conclusion does not logically follow unless one can independently show that non-phases behave differently in this respect. Then, in order to prevent QR from targeting a position higher than the TP-subject, Legate uses scope-sensitive elements, as in (258). Since the subject is in spec, TP and the quantificational DP needs to vacate $v$P$_1$, its landing site must be in spec, $v$P$_1$ in order to obtain the most salient reading of (258), where the existential scopes over the universal quantifier; see (259). From the apparent availability of this covert movement, Legate 2003 concludes that $v$P is a phase.

(258) Some woman [$_{v\text{P1}}$ gave John [$_{\text{DP}}$ every message you did [$_{v\text{P2}}$ e]]]. (Legate 2003:509)

(259) Some woman [$_{v\text{P1}}$ [$_{\text{DP}}$ every message you did [$_{v\text{P2}}$ ~~give John~~]]$_1$ [$_{v\text{P1}}$ gave John t$_1$]].

To apply this test to BCS $a$P, the $a$P must be placed in a small clause environment so as to avoid the scenario where QR is to a position licensed by the (matrix) copula. Consider (260), with the schematic representation of the adjectival small clause given in (261). As with the examples shown so far, the fact that the elided $a$P is contained in the antecedent $a$P should lead to infinite regress (261). The ungrammaticality of (260) suggests that the violation cannot be rectified by QRing the offending QP to spec, $a$P, as in (262). Hence, BCS $a$P is unable to host QRing phrases. To the extent that the (un)availability of QR be used as a movement diagnostic, it suggests that BCS $a$P is not a phase, since the hallmark of Chomskyan phases is that they should force successive-cyclic movement to proceed through their specifiers.

(260) * Jovan je smatrao neku ženu vrednom svake žrtve koje si ti
John COP considered some woman worthy every sacrifice which COP you

[8] This would follow only if one were to adopt the strong PIC. If the weak PIC were adopted, then an XP QRing from within a phase could escape interpretation by targeting any non-phasal YP merged before the next phase head. As I showed in chapter 5, the strong PIC constrains the locality of syntactic operations more than is empirically desirable. In this chapter, I furthermore show that it makes the wrong empirical predictions in terms of the timing of spellout, which is instead regulated by the weak PIC. For the time being, I ignore this issue as I continue to give the diagnostics in this section the best chance to work. They indeed seem to show that BCS spec, $a$P cannot be the site of intermediate A'-movement, but also that it does trigger spellout. This clash is resolved in chapter 7 by dispensing with Phase theory as a mechanism that regulates syntax-internal processes.

smatrao svoju mamu.
considered your mom
*lit.* 'J considered some woman worthy of every sacrifice you considered your mom.'

(261) some woman [$_{aP}$ worthy [$_{QP}$ every sacrifice which COP you considered your mom [$_{aP}$ worthy [ $_{QP}$ every sacrifice which COP you considered your mom [$_{aP}$ worthy ...

(262) some woman [$_{aP}$[$_{QP}$ every sacrifice...considered your mom [$_{aP}$~~worthy~~]]$_1$ [$_{aP}$ worthy $t_1$]]

In (263), I give a grammatical minimal pair without attempting ACD of the *a*P; this shows that the issue is indeed with the attempted deletion. Finally, (264) shows that *a*P ellipsis is possible in BCS when it does not involve ACD.

(263) Jovan je smatrao neku ženu vrednom svake žrtve koje si ti
John COP considered some woman worthy every sacrifice which COP you
smatrao svoju mamu vrednom.
considered your mom worthy
'J considered some woman worthy of every sacrifice you considered your mom worthy of.'

(264) Jovana je lepa, a i Marija je (takođe) [$_{aP}$ ~~lepa~~].
Jovana COP pretty and too Mary COP also
'Jovana is pretty and Mary is too.'

This section has applied diagnostics that attempt to identify Chomskyan phases to BCS *a*P.[9] These diagnostics, taken at face value, show that BCS *a*P cannot host copies of intermediate A'-movement, despite allowing elements from its domain to subextract. This should mean that BCS *a*P is not a phase. Since the ambition of Phase theory is to be a theory of successive-cyclicity and a theory of spellout to the interfaces, I examine the predictions for spellout in the next section. I show that that there is a clash: Despite not forcing successive-cyclic movement, BCS *a*P does impose a locality boundary for morphological processes, that is, it triggers spellout. The clash is resolved in chapter 7 by dispensing with Phase theory as a mechanism that regulates syntax-internal processes.

[9]Legate 2003 argues that the parasitic gap construction can also be used as a phasehood diagnostic. The argument is not iron-clad since it is based on a particular semantic analysis which involves a type mismatch unless movement to the phase-edge takes place. Furthermore, even if the analysis is adopted, it can show only that movement to a specific position (a hypothesized phase-edge) is possible, and not that it is obligatory because of phasehood. Hence, the fact that *v*P can license a parasitic gap in English is not evidence for *v*P phasehood. Regardless of all this, I simply note here that BCS does not have the parasitic gap construction, so the (in)ability of a phrase to license it cannot be taken as evidence for either position regarding phasehood.

### 6.2.3 A note on Bošković 2013, 2014

Bošković 2013, 2014 makes a pair of related claims, both of which implicate the phasehood of BCS *a*P (AP). The 2013 paper claims that all lexical categories project phases and the 2014 paper claims that the highest phrase in the extended projection of every lexical category is a phase. In both cases, *phase* is understood in the Chomskyan sense, namely as a phrase that drives successive-cyclic movement as a way to escape spellout. Since a significant portion of Bošković's data comes from BCS, and I have just claimed that BCS *a*P does *not* drive (and, in fact, does not even *allow*) successive cyclic movement through its specifier, I take a moment to dwell on the diagnostics presented in his papers. I conclude that the arguments for the phasehood of *a*P are particularly weak, even granting that the data presented can be used as phasehood diagnostics.

In Bošković 2013, adjectives are discussed only very briefly (p. 103). There are three main ingredients to the argument. (i) Movement is assumed to be regulated by an anti-locality constraint à la Abels 2003, which prohibits an element from moving from the complement position to the specifier position of the same phrase (265). Such movement is deemed to be too short. (ii) Genitive is taken to be the structural case for non-verbal case assigners; for diagnostics see Bošković 2013. (iii) Inherent case assigners are argued to have a richer functional structure than structural case assigners, based on the fact that subextraction from dative-assigning nominals is permitted, while subextraction from genitive-assigning ones is not (266). Hence, the reasoning goes, the nominal in (266a) is an NP and subextraction is barred via anti-locality, while the nominal in (266b) projects at least one functional projection above NP, and subextraction is able to proceed through that functional projection.

(265) * [$_{ZP}$ XP [$_{Z'}$ Z [ [XP ]]]

(266) a. *Ko-ga$_i$ si pronašla [knjigu t$_i$]? (Bošković 2013:85)
who-GEN COP.2SG found book
'Of whom did you find a book?'

b. Ko-me$_i$ je [otpor $t_i$] bio snažan? (Bošković 2013:86)
who-DAT COP.3SG resistance been strong
'To whom has resistance been strong?'

Then, Bošković shows that subextraction is available from BCS adjectival phrases, but only shows the pattern with adjectives that assign inherent case (267); see also the *wh*-movement example in (268). Since subextraction is allowed, we are supposed to conclude that the inherent case-assigner *a*P has functional structure which allows the moving phrase to be extracted without violating anti-locality.

(267) ?Student-ima$_i$ je on [lojalan $t_i$]. (Bošković 2013:103)
students-DAT COP.3SG he loyal
'It's to his students that he is loyal.'

(268) ?Ko-me$_i$ je on [lojalan $t_i$]?
who-DAT COP.3SG he loyal
'Who is he loyal to?'

Of course, the other conclusion one can draw from this data is that *a*P does not force movement to proceed through its specifier, which would equally well capture the data in (267)-(268) given (i)-(iii). Bošković acknowledges as much stating that: "The alternative would be to assume that APs are not phases; there would then be no reason to expect that the above movements should be blocked with APs" (Bošković 2013:103). I believe we can in fact strengthen the conclusion that *a*Ps are not phases by looking at genitive-assigning adjectives. Since genitive is the structural case for non-verbal case assigners, subextraction from genitive-assigning *a*Ps should not be possible due to an anti-locality violation. In fact, as we have already seen in section 6.2.1, subextraction from genitive-assigning *a*Ps is perfectly acceptable. I show representative examples in (269). If anything, if one accepts the various stipulations that come together to create this phasehood diagnostic, the results suggests that *a*Ps should be treated as non-phases.

(269) a. Tvoj-e pažnj-e je Marko [vredan $t_i$].
your-GEN attention-GEN COP.3SG Marko worthy
'It's your attention that Marko is worthy of.'

b. Čij-e pažnj-e je Marko [vredan $t_i$]?
whose-GEN attention-GEN COP.3SG Marko worthy
'Whose attention is Marko worthy of?'

In Bošković 2014, which argues that the highest phrase in the extended projection of every lexical category is a phase, an attempt is made to use ellipsis as a phasehood diagnostic (see also Gengel 2009). The specific claim is that only two types of phrases can undergo ellipsis: phases or phasal complements. The ellipsis diagnostic is applied to adjectives in a single example (Bošković 2014: ex. (103)), see (270). I give the BCS equivalent in (271). Bošković independently shows that copulas do not behave like phase heads. Hence, if only phases or phasal complements could undergo ellipsis, the sentences in (270)-(271) would constitute good evidence for the phasehood of *a*P.[10]

(270) John must be tired, and Peter must be too.

(271) Jovana je umorna, a i Marija je takođe.
Jovana COP.3SG tired and so Mary COP.3SG also
'Jovana is tired and Mary is too.'

What kinds of elements undergo ellipsis is, of course, an empirical question, and a consensus has not been reached in the literature regarding this issue (however, see Aelbrecht 2009 for explicit arguments that phasal spellout and ellipsis licensing are distinct phenomena). While the question merits further research, let me submit one example that seems to suggest ellipsis is not regulated by phasehood, given the assumptions made in Bošković 2014. Consider (272). First, the highest phrase in the extended projection is assumed to be the phase head; in (272) this is QP. Second, only the phase or its complement should be able to undergo ellipsis; in (272), this would be QP or the highest NP contained in it.[11]

(272) [$_{QP}$ pet [$_{NP}$ [$_{AP}$ mojih] [$_{NP}$ [$_{AP}$ žutih] [$_{NP}$ haljina]]]]
five my yellow dresses
'five yellow dresses of mine'

[10]Or at least they would provide evidence that some phrase in the extended projection of *a*P is phasal, if not *a*P itself. Recall that my arguments in sections 6.2.1 and 6.2.2 showed that *no* phrase in the extended projection of *a*P could host A'-moving phrases.

[11]See Bošković 2005, 2008, 2009 for evidence that BCS nominals phrases lack the DP layer and that possessors are morphosyntactically adjectival modifiers.

Yet, (273) shows that this prediction is not borne out: ellipsis can leave the highest projection of (the phasal complement) NP untouched, stranding the highest adjective. Hence, ellipsis seems to be able to target phrases that are neither phases nor phasal complements, suggesting that this construction cannot be used as a phasehood diagnostic.

(273) Marija je kupila pet mojih žutih haljina.
Mary COP.3SG bought five my yellow dresses

Jovana je kupila [$_{QP}$ osam [$_{NP}$ [$_{AP}$ tvojih] [~~$_{NP}$ [$_{AP}$ žutih] [$_{NP}$ haljina]~~]]].
Jovana COP.3SG bought eight your yellow dresses
'Mary bought five yellow dresses of mine. Jovana bought eight of yours.'

The purpose of this section was to address existing claims that BCS *a*P is a (Chomskyan) phase head. I concluded that the diagnostics used to arrive at this conclusion do not stand up to scrutiny. The arguments presented in sections 6.2.1 and 6.2.2 show that BCS *a*P does not trigger successive-cyclic movement through its specifier. I now turn to evidence that BCS *a*P nevertheless serves as a locality boundary for morphophonological processes.

## 6.3 BCS adjectives are DM phase-heads

The broad consensus in the DM literature is that categorizers (*v, n, a*) are phase-heads: They are the syntactic elements that trigger the spellout of syntactic syntactic structure to the interfaces, thereby delimiting locality domains for morpho(phono)logical processes. Recall that the (amended) weak PIC in (237), repeated here as (274), prohibits a phase-head from seeing the complement of a phase-head it c-commands. In terms relevant for us here, a root should be inaccessible to a phase-head *y* if a phase-head *x* intervenes between the two (275). Any non-phase WP merged between the first and second phase head is in the locality domain of the root since root spellout happens only at the merger of the second phase head. As I discussed at length in chapter 5, the weak PIC is required to account for the morphophonological locality effects we observe, the strong PIC being too restrictive for this purpose.

(274) weak Phase Impenetrability Condition *(amended version)*

Given the structure [ZP Z . . . [HP H ... [YP ]]], where H and Z are phase-heads, the domain of Z is spelled out; operations outside ZP cannot reference the morphosyntactic features of Z's domain.

(275) $[_{yP}\ y \ldots [_{WP}\ \text{W} \ldots [_{xP}\ x \ldots [\ \textsc{root}\ ]]]$

In this section, I show that the behavior of BCS *a*Ps qualifies them as DM phases, since they impose a locality boundary for morpho(phono)logical processes. I first examine the effects of adjectivization on root-conditioned allomorphy and then on the placement of morphological tone. The data in this section come from four sources: a Matica srpska Serbian dictionary (Nikolić 2007) and reverse dictionary (Nikolić 2000), a thesaurus (Ćosić 2008), and a detailed description of word-formation processes in Croatian (Babić 2002).

### 6.3.1 *a*P blocks root-conditioned allomorphy

The first piece of evidence that BCS *a* acts as a domain delimiter for the morphology comes from root-conditioned allomorphy patterns. BCS has rich derivational morphology; in this section I focus first on (root-derived and deadjectival) agentive nominals, followed by (root-derived and deadjectival) property-denoting nominals.

The broadly agentive (person-denoting) nominalizing suffixes in BCS are at least *-ar, -aš, -er, -ac, -ač, -ic(a), -ik,* and *-džij(a).* Root-derived nouns may take any of the *n* allomorphs on offer; the choice of nominalizer (*n*) is determined by the root (276). Some further examples featuring each of the suffixes are given in (277).

(276)
a. kormil-*ar* 'helmsman'
b. batin-*aš* 'beater'
c. poz-*er* 'poser'
d. pis-*ac* 'writer'
e. voz-*ač* 'driver'
f. izdaj-*ica* 'traitor'
g. proza-*ik* 'prose writer'
h. bureg-*džija* 'börek maker'

(277) a. put-ar ‘road worker’, bank-ar ‘banker’, zadrug-ar ‘co-op worker’, cveć-ar ‘florist’...

b. kočij-aš ‘carriage driver’, gubit-aš ‘loser’, frul-aš ‘flutist’, mafij-aš ‘mafia boss’ ...

c. friz-er ‘hair-dresser’, luz-er ‘loser’, boks-er ‘boxer’, futbal-er ‘footballer’, dil-er ‘dealer’...

d. bor-ac ‘fighter’, lov-ac ‘hunter’, honorar-ac ‘freelancer’, padobran-ac ‘parachuter’ ...

e. potroš-ač ‘consumer’, trk-ač ‘runner’, or-ač ‘plowman’, glas-ač ‘voter’, igr-ač ‘player’...

f. škrt-ica ‘cheapskate’, ljut-ica ‘angry person’, poglav-ica ‘chief’ lovokrad-ica ‘poacher’...

g. alkohol-ik ‘alcoholic’, empir-ik ‘empiricist’, akadem-ik ‘academic’, fanat-ik ‘fanatic’...

h. kamion-džija ‘truck driver’, sladoled-džija ‘ice-cream man’, šešir-džija ‘hat maker’...

Importantly, a root may influence the choice of nominalizing suffix only if there is no intervening categorizers between the two, in line with DM phase-theoretic predictions.[12] If a categorizer (in this case *a*) intervenes between the root and *n*, the root can no longer determine the form of *n*, see (278)-(280), which have the structure ROOT-*a*-*n*. What happens in the deadjectival nominals in (278) is that the *a*(djectivizer) *-n* intervenes between the root and the *n*(ominalizer) *-ik*, and it is only the *a* that can influence the form of *n*, which is now uniform regardless of the root in question. In (279) and (280), I illustrate the same phenomenon with different *a*s (*-av* and *-ljiv*), both of which force the *n*-allomorph *-ac* to surface. In other words, the *a*djectivizer *-n* conditions the insertion of the *n*-allomorph *-ik*, and the *a*s *-av/-ljiv* condition the insertion of the *n*-allomorph *-ac*; the root is invisible to *n* in this configuration. None of the other *n*-allomorphs in (276) can appear in (278)-(280), suggesting that the choice of *n* is governed by true allomorphic competition.

[12] The evidence in this chapter suggests that the intervening categorizer projects a phrase, namely *a*P. There is an interesting new line of work that argues some complex syntactic structures are built small, as complex heads (see Embick 2023, Paparounas 2023, Wood 2023). The idea is that these small structures do not undergo phasal spellout in the same way as phrases. The data in the above-mentioned work involves deverbal structures, and the evidence comes from phenomena such as argument introduction and the existence of root-specific interpretations across intervening categorizers, among others. Since adjectives do not have interesting argument-taking properties, I do not have the means to test the (purely syntactic) predictions of the complex head analysis. In addition to the morphophonological evidence, which points to a phrasal analysis, I note here that the meanings of the revelant BCS deadjectival nouns are compositionally derived from the meanings of the underlying adjectives.

(278) a. smrt-*n-ik* 'mortal one'
b. put-*n-ik* 'traveler'
c. boles-*n-ik* 'sick one'
d. bestid-*n-ik* 'shameless one'
e. duž-*n-ik* 'debtor'
f. gubit-*n-ik* 'loser'

(279) a. prlj-*av-ac* 'dirty one'
b. mrš-*av-ac* 'skinny one'
c. mut-*av-ac* 'mute one'
d. peg-*av-ac* 'freckled one'
e. prg-*av-ac* 'grumpy one'
f. hvalis-*av-ac* 'boastful one'

(280) a. plaš-*ljiv-ac* 'scared one'
b. smrd-*ljiv-ac* 'stinky one'
c. grab-*ljiv-ac* 'predatory one'
d. povod-*ljiv-ac* 'gullible one'
e. var-*ljiv-ac* 'cheating one'
f. vaš-*ljiv-ac* 'lousy one'

The locality effect is best observed when the same root can produce both a simple nominal and a deadjectival nominal (281). As illustrated in (281), the two words can be synonyms and contain the same root, yet they have distinct nominalizers which are not interchangeable (cf. **gubit-ik*, **gubit-n-aš*), presumably due to the presence of *a* in (281b).

(281) a. gubit-*aš*
lose-N
'loser'
b. gubit-*n-ik*
lose-ADJ-N
'loser'

Put in slightly different terms, even though the root in (281) clearly picks out the nominalizer *-aš*, it can no longer do so if an adjectivizer intervenes between the two.[13] This behavior can be accounted for if both *a* and *n* are phase heads in BCS; in a ROOT-*a-n* configuration, the root is spelled out when *n* is merged, hence the root (*qua* morpheme) can no longer be identified when *n* undergoes VI.

[13] This cannot be due to a simple linear adjacency requirement for allomorph selection (Embick 2010), as I show immediately below. The context for allomorphy in BCS is not generally restricted to adjacent morphemes (see Bešlin 2024). Phases (categorizing heads) play a crucial role in closing off the locality domain for allomorphy.

I have shown that the form of *n* in BCS deadjectival nominals can be influenced by *a*, but not by the root. In case the insertion context is not met for any of the specified allomorphs, *-ar* is inserted; I give a schematic representation of Vocabulary Insertion for $n_{[+\text{human}]}$ in (282). Support for *-ar* as the elsewhere allomorph comes from its appearance in the most diverse set of contexts and its use in nonce-words, for example, *zaves-ar* 'curtain-maker', *lamp-ar* 'lamp seller/fixer', *kifl-ar* 'kifla-maker'.[14]

(282) $n_{[+\text{human}]} \leftrightarrow$ /ac/ // $a_{\text{AV}}$, $a_{\text{LJIV}}$, $\sqrt{}$pis, $\sqrt{}$bor, $\sqrt{}$lov, $\sqrt{}$padobran, $\sqrt{}$festival, ...

$\leftrightarrow$ /ik/ // $a_{\text{N}}$, $\sqrt{}$alkohol, $\sqrt{}$empir, $\sqrt{}$akadem, $\sqrt{}$fanat, ...

...

$\leftrightarrow$ /ar/ // elsewhere

I will demonstrate this seemingly general point, that BCS *a* delimits the context for allomorphy, with another set of examples. This time, I examine property-denoting nouns. I take property-denoting nouns to be nouns that can be used as *X* in the sentence *His/its X is surprising* to mean "Him/it having property X is surprising", as in (283).

(283) Njegova dobr-ota je iznenađujuća.
his good-N COP.3SG surprising
'His goodness is surprising.'

The pattern is the same as with agent-nominals above: The relevant *n* has a number of allomorphs, at least: *-ost*, *-ota*, *oća*, *-ina*, *-je*, *-stvo*, *-ija*, and *-izam*. The allomorph is selected based on the root *n* attaches to (284); the allomorphs in (284) are not interchangeable.[15] Some further examples featuring each of the suffixes are given in (285).

[14] A *kifla* is a type of pastry with many regional varieties, sold in bakeries. In Vojvodina (Serbia), it is made from a savory yeast dough with a salty coating.

[15] Some roots allow more than one allomorph to surface, as in *mil-ota* 'sweetness', *mil-ina* 'sweetness', *mil-ost* 'sweetness', *mil-je* 'sweetness' (but not **mil-oća*, **mil-stvo*, **mil-ija*, **mil-izam*). There is nothing in the system that prevents this; the root is simply found on multiple VI lists for the same syntactic unit, and the forms are in free variation. That is, I take there to be no (grammar-internal) competition in determining which form is chosen in this instance (see Embick & Marantz 2008, Embick 2016, Embick, Benz & Paparounas 2023 for discussion). I leave open the possibility that subtle pragmatic differences determine their context of use.

(284) a. slab-*ost* 'weakness'
b. dobr-*ota* 'goodness'
c. mek-*oća* 'softness'
d. svež-*ina* 'freshness'
e. mil-*je* 'sweetness'
f. rastroj-*stvo* 'distractedness'
g. hohštapler-*ija* 'fraudulentness'
h. asket-*izam* 'asceticism'

(285) a. blag-ost 'mildness', mil-ost 'sweetness', glup-ost 'stupidity', svet-ost 'holiness' ...
b. lep-ota 'beauty', gol-ota 'nudity', topl-ota 'warmth', mil-ota 'sweetness' ...
c. tvrd-oća 'hardness', čist-oća 'cleanliness', fin-oća 'niceness', jasn-oća 'clarity'...
d. deblj-ina 'thickness', ljut-ina 'spiciness', svetl-ina 'brightness', bel-ina 'whiteness'...
e. licemer-je 'hypocrisy', milosrd-je 'clemency', bogoljublje 'god-loving-ness'...
f. juna(k)-štvo 'heroism', moma(k)-štvo 'youth', barbar-stvo 'barbarity' ...[16]
g. infam-ija 'infamy', kleptoman-ija 'kleptomancy', samoiron-ija 'self-irony' ...
h. optim-izam 'optimism', ekstremizam 'extremism', narc-izam 'narcissism' ...

Now, let us again take a look at deadjectival nominals. As expected, an intervening *a* stops the root from being able to influence the form of the *n*. Instead, only the *a* is now visible for the purposes of *n*-allomorph selection. I illustrate this with the adjectivizers *-n* (286), *-av* (287), and *-ljiv* (288), all of which force the nominalizer *-ost* to surface, regardless of the underlying root.

(286) a. optimistič-*n-ost* 'optimism'
b. imuć-*n-ost* 'affluence'
c. bezver-*n-ost* 'faithlessness'
d. blaž-*en-ost* 'blissfulness'
e. blagovrem-*en-ost* 'timeliness'
f. blagoslov-*en-ost* 'blessedness'

(287) a. buđ-*av-ost* 'moldiness'
b. blebet-*av-ost* 'chatiness'
c. bles-*av-ost* 'silliness'
d. prlj-*av-ost* 'dirtiness'
e. blist-*av-ost* 'radiance'
f. čađ-*av-ost* 'sootiness'

[16] [s] is realized as [ʃ] if the root ends in a velar.

(288) a. oset-*ljiv-ost* 'sensitivity'
b. mer-*ljiv-ost* 'measurability'
c. koleb-*ljiv-ost* 'hesitance'
d. uver-*ljiv-ost* 'credibility'
e. izdrž-*ljiv-ost* 'resilience'
f. uoč-*ljiv-ost* 'noticeability'

The pattern is again particularly striking in cases where either the root or an already adjectivized stem can be nominalized to produce a property-denoting noun, see (289). Crucially, once the root is adjectivized, the allomorph found on the root-nominal is no longer available (**bezver-n-je*), suggesting that the root cannot be used as a context for *n*-allomorph selection across *a*.[17] Once again, this pattern can be explained if *a* (and *n*) is taken to be a phase head and the spellout of structure to the interfaces is regulated by the (weak) PIC.

(289) a. bez-ver-*je*
without-faith-N
'faithlessness'
b. bez-ver-*n-ost*
without-faith-ADJ-N
'faithlessness'

Now, I have argued that a categorizer and a root are visible to each other when there are no intervening categorizers because the two are in the same spellout domain in the former case, but in separate spellout domains in the latter. Hence, my claim is that any two elements in the same spellout domain may have an influence on each other's form.[18] A few factors conspire to make it seem like linear adjacency is required for morpho(phono)logical visibility, in addition to containment in the same phase (see Embick 2010). For one, all categorizing affixes in BCS are suffixes, so it seems that a root can condition allomorphy only if a categorizer is linearly adjacent to it. However, there are cases where the form of the nominalizer is conditioned by so-called lexical prefixes in conjunction with the root, or by both roots in root+root compounds; see (290)-(291).[19] The suffixes in (290)-(291) are not interchangeable (e.g., **dav-ac*). The root $\sqrt{\text{DAV}}$ conditions the insertion of the (broadly agentive) nominalizer *-ač* (290a).

[17] It does not matter for my purposes whether the specific *a*s in (286)-(288) force the allomorph *-ost* to surface, or if *-ost* is the elsewhere form. What matters is that this form surfaces regardless of the underlying root, suggesting that the root is invisible when the form of *n* is determined.

[18] Though see Bobaljik 2000 for arguments about the directionality of spellout, thought to be from the root outwards. This may affect which features (syntactic or phonological) of an item can be used to contextually determine the form of other elements in its domain. The data presented here is fully compatible with Bobaljik's proposal.

[19] [o] is the most frequent linking vowel in BCS root+root compounds (Babić 2002).

This nominlizer is also used when the root combines with a number of lexical prefixes (290). However, as seen in (291), the lexical prefix *pro-* triggers the insertion of the allomorph *-(a)c* in the same context, and root+root compounds involving the root $\sqrt{\text{DAV}}$ do so too. The first members of root+root compounds are clearly separate from the root, and the literature on Slavic languages treats lexical prefixes as synchronically distinct from the root as well (Ramchand 2004, Romanova 2004, Svenonius 2004, Arsenijević 2006, Tatevosov 2011, i.a.). If this is correct, then the allomorphy of *n* is triggered by a clearly non-adjacent element.

(290) a. dav-*ač*
give-N
'giver'

b. do-dav-*ač*
PREF-give-N
'passer'

c. iz-dav-*ač*
PREF-give-N
'publisher'

d. pre-dav-*ač*
PREF-give-N
'lecturer'

(291) a. pro-dav-*ac*
PREF-give-N
'seller'

b. stan-o-dav-*ac*
apartment-LV-give-N
'landlord'

c. savet-o-dav-*ac*
advice-LV-give-N
'advisor'

d. dar-o-dav-*ac*
gift-LV-give-N
'gift-giver'

Moreover, neither linear nor structural adjacency is required for contextual (root) allomorphy in BCS more generally (see Bešlin 2024). In the next subsection, I show that root visibility for other morphophonological processes in BCS is not constrained by linear adjacency either. Thus, BCS *a* acts as a locality boundary for root-conditioned allomorphy in a manner predicted by the (amended) weak PIC. Next, I turn to the role of *a* in mediating morphological tone placement, which demonstrates the same general point. The empirical focus of the following section remains the same, detailing the behavior of root-derived and deadjectival nominals in BCS.

### 6.3.2 *a*P mediates the placement of morphological tone

BCS is a so-called pitch-accent language. Prosodic words have a pitch contour, which can be described as falling or rising. An idealized representation of a falling and rising pitch contour is given in (292), borrowed from Inkelas & Zec 1988:228; long vowels are represented as geminates. In their influential proposal, Inkelas & Zec (1988) argue that the pitch contours arise because of the existence of tone in the morphophonological system of BCS. They claim that only H(igh) tones are represented in the BCS lexicon; roots and affixes are idiosyncratically marked or unmarked for H. That is, whether an element bears a H or not does not seem to be predictable based on any of its (morpho)syntactic or (morpho)phonological properties. A falling contour results from word-initial H and a rising contour from a non-word initial H that spreads to the preceding syllable, as in (293). I will adopt this basic analysis throughout, marking the origin position of the H with the accute accent (á). Thus, the examples in (292) will be represented as in (294).

(292) a. *za astava* 'flag' b. *ra azli ka* 'difference'

(293) a. σ σ (σ) (falling) b. σ σ (σ) (rising)
H H

(294) a. zá.sta.va 'flag' b. ra.zlí.ka 'difference'

Before I proceed, I should note that there is a significant amount of variability in the literature in the way terms such as "tone", "accent", and "stress" are used. Before I turn to a discussion of deadjectival nouns, I therefore dedicate the following paragraph to clearing up some of the potential confusion.

For Inkelas & Zec 1988, "tone" in BCS is a phonological object whose phonetic correlate is pitch prominence, while the phonetic correlate of "stress" is loudness and/or duration. The existence and placement of a "tone" is listed on morphemes, while the position of "stress" is calculated via phonological rule (in a way that is detailed below). What Inkelas & Zec call "tone" (and I adopt their naming

convention) is often called "accent" in the Slavic literature (see e.g., Halle 1997).[20] The reader should not assume anything based on my use of the term "tone" here, since "tone" in BCS may very well have the same underlying phonological representation as "stress" in stress languages. The reason for using the term "tone" in Inkelas & Zec 1988 stems largely from a focus on its phonetic correlate (pitch prominence), which is irrelevant here and likely irrelevant for the phonological computation.[21]

Inkelas & Zec show that the location of tone (pitch prominence) fully predicts the location of stress (duration/loudness) in a BCS prosodic word: Stress placement is on the leftmost syllable that bears a H(igh) tone. Tone placement, on the other hand, is listed and not predictable. The asymmetry is apparent in (293), the schematic representation of (294). The stress in both instances is on the first syllable because that is the leftmost H-bearing syllable. The tonal properties of the two are different, with H on the first syllable in (293a) and on the second syllable in (293b). Once again, the tone pattern cannot be predicted based on the stress.

This analysis, which takes tone (pitch prominence) to be primitive and stress (duration/loudness) to be derived, successfully captures the facts of BCS stress placement. For example, stress cannot fall on the final syllable of polysyllabic words (because tone on a non-initial syllable always spreads leftward). Furthermore, monosyllabic words can only have a falling pitch contour (because there are no syllables to the left that H can spread to, in order to create a rising pitch contour). In the rest of this section, I focus on the placement of tone; stress will be irrelevant for my purposes. Vowel length information is also irrelevant and is omitted throughout.

[20] The analysis developed here differs from Halle 1997 in a number of ways, however. For instance, for empirical reasons I will argue that the structurally highest tone (accent) in the relevant domain is realized. On the other hand, Halle extends the idea that seems to be correct for Russian, namely that the leftmost accent in the relevant domain gets realized, to BCS. I do not undertake a detailed critique of Halle's proposal here; interested readers should note, however, that a number of accent patterns from BCS presented in Halle 1997 are not accurate, even for the (standard, Štokavian) dialect he describes. A careful look at any dictionary or grammar that describes this variety of BCS can confirm this.

[21] Thanks to Alex Chabot and Bill Idsardi for a discussion of these issues.

### 6.3.3 Morphological tone in polymorphemic words

BCS imposes a restriction on the realization of underlying Hs in polymorphemic words. In the rest of this section, I will argue for the following conclusion: The placement of morphological tone in BCS polymorphemic words is mediated by spellout in a way that is predicted by the (amended) weak PIC iff categorizers (*v*, *n*, *a*) are the triggers of spellout.

In the examples below, (295) shows a derivation of simple nouns (ROOT-*n*), and (296) of deadjectival nouns (ROOT-*a*-*n*).[22] The underlyingly H-marked nominalizer *-ik* influences the placement of tone if it attaches to a root (295), but not if it attaches to an already adjectivized stem (296). The adjectivizer in (296) blocks the subsequent addition of *-ik* from influencing the position of the H, despite itself not being marked for tone.[23]

(295) a. so.krát → so.kra.t-ík
'Socrates' 'Socratic'

b. pro.zá → pro.za.-ík
'prose' 'prose-writer'

c. sa.tí.r(a) → sa.ti.r-ík
'satire' 'satirist'

d. ál.ko.hol → al.ko.ho.l-ík
'alcohol' 'alcoholic'

e. a.nal.gét.(sko) → a.nal.ge.t-ík
'analgetic(ADJ)' 'analgetic(N)'

f. pa.ra.no.(já) → pa.ra.no.-ík
'paranoia' 'paranoiac'

(296) a. lju.báv → lju.báv-n-ik
'love' 'lover'

b. na.pást → na.pás.-n-ik
'nuisance' 'predator'

c. pro.mét → pro.mét.-n-ik
'traffic' 'patrol officer'

d. sa.vré.me(n) → sa.vré.me.n-ik
'modern' 'contemporary(N)'

e. bez.-í.me(n) → bez.í.me.-n-ik
'no-name(ADJ) 'no-name(N)'

f. bé.stid → be.stid.-n-ik
'brazenness' 'brazen person'

[22] As far as I can tell, syllabification does not play a role in tone placement. I include syllable boundaries in this section for completeness (σ.σ.σ), in case others might benefit from it or notice some pattern I missed.

[23] The role of categorizers in regulating morphophonological processes was first cashed out in this way by Marvin (2002), who argues that lexical stress-assignment in English and Slovenian is mediated by phasal spellout below the word level; see also Embick 2014 for important extensions of this work.

The contrast in the realization of H in (295)-(296) can be explained if the *a* in BCS is a phase-head: *n* is not able to see the root across it and influence the position of H. In other words, tone placement in BCS is determined within the first phase, and further material is unable to modify it (see Newell 2008 for arguments that stress assignment in Turkish and Cupeño is confined to the first phase).

The same effect is observed with other exponents of BCS *a* which also mediate the placement of H, as I detail in sections 6.3.3.1-6.3.3.5. Before I move on, however, I would like to point out that the position of H is not always determined between the first two elements that merge, nor is it dependent on linear adjacency to the root. First, there are contexts where some composition has taken place before the first categorizing affix is added, but the categorizer is still able to determine the position of H, as in the case of derivation based on root+root compounds (297)[24] or non-categorizing affixes+roots (298). Furthermore, non-phasal (non-categorizing) material merged after the first categorizer is still able to influence the position of H, as in the case of adjectival comparative morphology (299) and negation (300). These facts are fully predicted by the (amended) weak PIC.[25]

(297) a. du.b-o.-re.z-ác
deep-LV-cut-N
'woodcarver'

b. pa.d-o.-bra.n-ác
fall-LV-defend-N
'parachuter'

c. le.d-o.-lo.m-ác
ice-LV-break-N
'ice-breaker'

(298) a. i.s+to.va.r-áč
PREF-load-N
'unloader'

b. ra.z-no.s-áč
PREF-carry-N
'delivery guy'

c. i.z-re.z-áč
PREF-cut-N
'cutter'

(299) a. do.sto.jan.stv-e.n-í.j-i (cf. do.sto.jan.stvén)
dignity-ADJ-CMPR-M
'more dignified'

b. dru.štv-e.n-í.j-i (cf. dru.štvén)
social-ADJ-CMPR-M
'more social'

[24] I take the roots to merge first because *dub-o-rez* 'woodcarving', *pad-o-bran* 'parachute', and *brod-o-lom* 'shipwreck' exist as independent words, while **rez-ac*, **bran-ac*, and **lom-ac* do not.

[25] The comparative data in (299) are noted in Zec 1999 and Werle 2009, but locality restrictions are not discussed.

c. le.d-e.n-í.j-i (cf. le.dén)
ice-ADJ-CMPR-M
'icier'

(300) a. né.-ko.na.č-an (cf. ko.ná.čan)
NEG-end-ADJ
'non-final'

b. né.-mo.ra.l-an (cf. mó.ra.lan )
NEG-moral-ADJ
'immoral'

c. né.-pri.me.r-an (cf. pri.mé.ran)
NEG-example-ADJ
'improper'

The following subsections illustrate the fact that BCS *a* generally closes off the domain for morphological tone assignment in deadjectival nouns. I look at nouns in *-(a)c*, *-ica*, *(j)ača*, *-če*, and *-ina*, derived from adjectives in *-ljiv*, *-av*, *-ov*/*-ev*, and/or *-en*.

#### 6.3.3.1 Nouns in *-ac*

The agentive *n*-suffix *-ac* realizes its underlying H when it attaches to a root (301), but not when it attaches to an already adjectivized stem. I demonstrate the role of *a* in closing off the locality domain for tone assignment with the dispositional *a -ljiv* (302), descriptive *a -av* (303), and possesive *a -ov* (304). In deadjectival nouns, the tone surfaces wherever it is found on the adjective (shown on the left side of the arrow). For *-ljiv* adjectives, this in on the adjectivizing suffix, since it itself carries an underlying H; see also (306).

(301) a. pi.tóm → pi.to.m-ác
'tame' 'stipendiary'

b. zla.tí.bor → zla.ti.bo.r-ác
'Zlatibor' ethnonym

c. škŕt → škr.t- ác
'stingy' 'scrooge'

d. dú.bo.rez → du.bo.re.z-ác
'woodcarving' 'woodcarver'

e. pá.do.bran → pa.do.bra.n-ác
'parachute' 'parachuter'

f. al.žír → al.ži.r-ác
'Algeria' 'Algerian(N)'

(302) a. gra.b-ljív → grab.-ljí.v-ac
'predatory' 'predator'

b. kra.d-ljív → krad-ljí.v-ac
'thievish' 'thief'

c. br.b-ljív → br.b-ljí.v-ac
'chatty' 'chatterbox'

d. bo.ja.ž-ljív → bo.ja.ž-ljí.v-ac
'fearful' 'fearful one'

e. do.vi.t-ljív → do.vi.t-lji.v-ac
'resourceful' 'resourceful one'

f. na.me.t-ljív → na.me.tljív-ac
'instrusive' 'intrusive one'

(303) a. bá.l-av → bá.l-a.v-ac
'snotty' 'brat'

b. klém.p-av → klém.p-a.v-ac
'lop-eared' 'lop-eared one'

c. cmíz.dr-av → cmíz.dr-a.v-ac
'weepy' 'cry-baby'

d. lá.j-av → lá.ja.v-ac
'barking' 'loudmouth'

e. lá.sk-av → lá.ska.v-ac
'flattering' 'flatterer'

f. mú.s-av → mú.s-a.v-ac
'messy' 'ragamuffin'

(304) a. baj.ró.n-ov → baj.ró.no.v-ac
'Byron's' 'Byron follower'

b. a.ri.sto.té.l-ov → a.ri.sto.té.lo.v-ac
'Aristotle's' 'Aristotelian'

c. dí.na.m-ov → dí.na.mo.v-ac
'Dinamo's' 'Dinamo supporter'

d. frój.d-ov → frój.do.v-ac
'Froyd's' 'Freudian'

e. no.bé.l-ov → no.bé.lo.v-ac
'Nobel's' 'Nobel winner'

f. ful.brájt.-ov → ful.brájt.ov.-ac
'Fulbright's' 'Fulbright scholar'

Hence, nouns in *-ac* further illustrate the pattern predicted by the (amended) weak PIC: the underlying H on the *n*-suffix *-ac* can surface on root-derived nouns, but not on deadjectival nouns. This is because BCS *a* serves as a point of spellout and tone placement is determined within the first phase.

#### 6.3.3.2 Nouns in *-ica*

Nouns in *-ac* are masculine in BCS. One of the feminine equivalents is the suffix *-ica*.[26] Nouns in *-ica* show the same tone pattern as nouns in *-ac*, both in root-derived contexts (305) and in deadjectival contexts (306)/(307).

(305)
a. šéf → še.f-í.ca
'boss' 'boss(F)'
b. škŕt → škr.t-í.ca
'stingy' 'scrooge'
c. đávo(l) → đa.vo.l-í.ca
'devil' 'devil(F)
d. maj.mún → maj.mu.n-í.ca
'monkey' 'monkey(F)
e. tí.gar → ti.gr-í.ca
'tiger' 'tigress'
f. a.dvo.kát → a.dvo.ka.t-í.ca
'lawyer' 'lawyer(F)

(306)
a. gra.b-ljív → gra.b-ljí.v-i.ca
'predatory' 'predator(F)'
b. kra.d-ljív → kra.d-ljí.v-i.ca
'thievish' 'thief(F)'
c. br.b-ljív → br.b-ljí.v-i.ca
'chatty' 'chatterbox(F)'
d. bo.ja.ž-ljív → bo.ja.ž-ljí.v-i.ca
'fearful' 'fearful one(F)'
e. do.vi.t-ljív → do.vi.t-ljí.v-i.ca
'resourceful' 'resourceful one(F)'
f. na.me.t-ljív → na.me.t-ljí.v-i.ca
'intrusive' 'intrusive one(F)'

[26] The other is *-ka*, but *-ka* does not have an underlying H so it is irrelevant for my purposes here.

(307) a. bá.l-av → bá.l-a.v-i.ca
'snotty' 'brat(F)'

b. kmé.z-av → kmé.za.v-i.ca
'weepy' 'cry-baby(F)'

c. cmíz.dr-av → cmíz.dra.v-ac
'weepy' 'cry-baby(F)'

d. lá.j-a.v → lá.ja.v-i.ca
'barking' 'loudmouth(F)'

e. lá.sk-av → lá.ska.v-i.ca
'flattering' 'flatterer(F)'

f. mú.s-av → mú.s-a.v-i.ca
'messy' 'ragamuffin(F)'

The underlying H of the *n*-suffix *-ica* can surface on root-derived nouns (305), but not on deadjectival nouns (306)-(307). The placement of tone is determined within the first phase delineated by *a*, both when *a* is itself underlyingly H-marked (306) and when it is not (307).

#### 6.3.3.3 Nouns in *-(j)ača*

The same pattern is observed with the *n*suffix -(j)ača.[27] The H tone surfaces on the suffix in root-derived nouns (308) and in the same place it is found on the adjective in deadjectival nouns in *-ev/-ov* (309).

(308) a. glúp → glu.p-á.ča
'stupid' 'dummy(F)'

b. mú.šk(o) → mu.ška.r-á.ča
'male' 'tomboy'

c. krom.pír → krom.pi.r-á.ča
'potato' 'potato pie'

d. kŕ.m(e) → kr.m-á.ča
'pig' 'pig(F)'

e. kré.men → kre.me.n-já.ča
'flint' 'flintlock'

f. blé.s(a) → ble.s-á.ča
'silly one' 'silly one(F)

[27] [j] is inserted if the stem ends in [n], Babić 2002:100.

(309) a. dú.nj-ev → dú.nj-e.v-a.ča
'of quince' 'quince schnaps'

b. tré.šnj-ev → tré.šnj-e.v-a.ča
'of cherry' 'cherry schnaps'

c. ví.šnj-ev → ví.šnj-e.v-a.ča
'of cherry' 'cherry schnaps'

d. o.rá.h-ov → o.rá.ho.v-a.ča
'of walnut' 'walnut schnaps'

e. já.bu.k-ov → já.bu.k-o.v-a.ča
'of apple' 'apple schnaps'

f. šljí.v-ov → šljí.v-o.v-a.ča
'of plum' 'plum schnaps'

An interesting example is *pridev-ača* 'pin needle', whose stem ends in [ev], but where this ending is not the possessive adjectival suffix; cf. (309a)-(309c). This appears to be the case because (a) *pridev* is a non-existing adjective and (b) the root is, in fact DEV, as in the verbs *devati* 'put', *na-devati* 'call (a name)', *pri-devati* 'attach'. The tone in this root-derived nominal is on the *n* (pri.de.v-á.ča) despite its superficial similarity to the deadjectival nouns in (309a)-(309c). This is expected given our hypothesis that tone assignment in BCS is morphological, rather than a regular phonological process, in the sense that it is a process that must reference specific Vocabulary Items. Accordingly, the tone placement process must be able to differentiate between morphemes in a way that is independent of their form.

#### 6.3.3.4 Nouns in *-če*

The exact same pattern is observed with nouns in *-če*, which has a broadly hypocoristic meaning.[28] Namely, H is realized on the suffix *-če* in root-derived nouns (310), but not in deadjectival nouns, where it surfaces in the same position as on the underlying adjective (311)-(313).

[28]I leave it open whether *-če* is the exponent of *n*, some projection, call it HCRP, in the nominal functional spine, or an exponent both of those terminals which have undergone Fusion (Halle & Marantz 1993). These details are irrelevant because the prediction regarding locality is the same in either case: According to the (amended) weak PIC, the underlying H of *-če* should get realized on root-derived nominals (but not on deadjectival nominals), regardless of whether *-če* is the exponent of *n* or one of *n*'s functional projections. This version of the PIC states that non-phasal material in the functional spine of the first phase counts as part of the first phase for morphophonological processes.

(310) a. jé.len → je.len.-čé
'deer' 'deer(HCR)'

b. pŕ.sten → pr.sten.-čé
'ring' 'ring(HCR)'

c. pa.stír → pa.stir.-čé
'shepherd' 'shepherd(HCR)'

d. ví.no.grad → vi.no.gra.-čé
'vineyard' 'vineyard(HCR)'

e. maj.mún → maj.mun.-čé
'monkey' 'monkey(HCR)'

f. ma.rá.m(a) → ma.ram.-čé
'scarf' 'scarf(HCR)'

(311) a. bá.l-av → bá.l-av.-če
'snotty' 'brat(HCR)'

b. šmŕ.k-av → šmŕ.k-av.-če
'snotty' 'brat(HCR)'

c. pŕ.lj-av → pŕ.lj-av.-če
'dirty' 'dirty one(HCR)'

d. bán.g-av → bán.g-av.-če
'klutzy' 'klutz(HCR)'

e. ljí.g-av → ljí.g-av.-če
'sleazy' 'sleaze(HCR)'

f. kmé.z-av → kmé.z-av.-če
'weepy' 'cry-baby(HCR)'

(312) a. grab.-ljív → grab.-ljív.-če
'predatory' 'predator(HCR)'

b. kr.me(lj).-ljív → kr.me.(lj)-ljív.-če
'eye-booger-y' 'eye-booger-y one(HCR)'

c. brb.-ljív → brb-(lj)ív.-če
'chatty' 'chatterbox(HCR)'

(313) a. gó.j-en → gó.j-en-če
'fattened' 'fattened one(HCR)'

b. dó.j-en → dó.j-en.-če
'breastfed' 'breastfed one(HCR)'

c. no.v-o.-ró.d$^{j}$-en → no.v-o.-ró.d$^{j}$-en.-če
'newborn(ADJ)' 'newborn(HCR)'

Again, it is interesting to compare nouns whose roots end in [en] (310a)-(310b) with nouns derived from adjectives with the suffix *-en* (313). As expected, the tone on root-derived nouns is on the suffix, while on deadjectival nouns it is on the adjectival stem, despite the superficial similarity in form.

#### 6.3.3.5 Nouns in *-in(a)*

Finally, I demonstrate the same pattern with nouns in *-ina*: The underlying H surfaces on *-ina* when this suffix attaches to roots (314), but not when it attaches to an already adjectivized stem (315).

(314) a. kí.se.o → ki.se.l-í.na
'sour' 'acid'

b. bŕz → br.z-í.na
'fast' 'speed'

c. bé.-o → be.l-í.na
'white' 'whiteness'

d. čvŕst → čvrs.t-í.na
'hard' 'hardness'

e. ó.ko.l(o) → o.ko.l-í.na
'around' 'surroundings'

f. ko.lí.k(o) → ko.li.č-í.na
'how much' 'quantity'

(315) a. bré.z-ov → bré.z-o.v-i.na
'of birch' 'birch wood'

b. já.vo.r-ov → já.vo.r-o.v-i.na
'of maple' 'maple wood'

c. ku.pú.s-ov → ku.pú.s-o.v-i.na
'of cabbage' 'cabbage patch'

d. dé.spo.t-ov → dé.spo.t-o.v-i.na
'despot's' 'despotate'

e. cár-ev → cár-e.v-i.na
'tzar-s' 'empire'

f. víš.nj-ev → víš.nj-e.v-i.na
'of cherry' 'cherry wood'

This section showed that BCS categorizers–the hypothesized phase-heads of DM–behave as domain delimiters for morphological processes; specifically, adjectivizers, but not non-categorizing morphemes, "close off" the domain of root-allomorphy and morphological tone placement.

### 6.3.4 A note on "postaccenting stems"

Before concluding, let me remark on a group of examples that form a systematic exception to the generalizations made here. These are what Halle 1997 refers to as "postaccenting stems", a class of roots which carry an underlying H, but realize it on the first post-root syllable.[29]

To set the scene, recall first that the adjectival suffix *-ev/-ov* does not have an underlying H, see (304)/(309)/(315); (309) is repeated here as (316) for convenience. In (316), the left-hand side shows root-attached *-ev/-ov* producing a possessive adjective. The right-hand side shows the result of further nominalizing the adjectival structure. In both cases, the H is realized on the root, which indicates that the root-attached suffix *-ev/-ov* is not underlyingly H-marked.

(316) a. dú.nj-ev → dú.nj-e.v-a.ča
'of quince' 'quince schnaps'

b. tré.šnj-ev → tré.šnj-e.v-a.ča
'of cherry' 'cherry schnaps'

c. ví.šnj-ev → ví.šnj-e.v-a.ča
'of cherry' 'cherry schnaps'

d. o.rá.h-ov → o.rá.h-o.v-a.ča
'of walnut' 'walnut schnaps'

e. já.bu.k-ov → já.bu.k-o.v-a.ča
'of apple' 'apple schnaps'

f. šljí.v-ov → šljí.v-o.v-a.ča
'of plum' 'plum schnaps'

However, there is a relatively small group of examples where the H does surface on *-ev/-ov*, illustrated in (317). While this may initially seem problematic, note that this pattern is not restricted to the suffixes *-ov/-ev*; *any* (first) suffix that combines with these roots will carry a H. The H is never on the root, except when the suffix is phonologically null (318g).

(317) a. lo.z-óv → lo.z-ó.v-a.ča
'of grape' 'grape schnaps'

b. glo.g-óv → glo.g-ó.v-a.ča
'of hawthorn' 'hawthorn schnaps'

[29] Halle 1997 refers to stems; "stem" is the traditional term used for the input to morphological processes. Since (i) having a tone/accent is a property of individual exponents in BCS, (ii) all the postaccenting morphemes discussed by Halle and in this section are roots, and (iii) stems play no theoretical role in DM (Embick & Halle 2005), I will continue to refer to these elements as roots.

(318)

| | | | |
|---|---|---|---|
| a. | lo.z-á ‘grape-N.NOM.SG.F’ | g. | glóg-∅ ‘hawthorn-N.NOM/ACC.SG.M’ |
| b. | lo.z-é ‘grape-N.GEN.SG.FEM’ | h. | glo.g-á ‘hawthorn-N.GEN.SG.M’ |
| c. | lo.z-í ‘grape-N.DAT/LOC.SG.F’ | i. | glo.g-ú ‘hawthorn-N.DAT/LOC.SG.M’ |
| d. | lo.z-ú ‘grape-N.ACC.SG.F’ | j. | glo.g-óm ‘hawthorn-N.INST.SG.M’ |
| e. | lo.z-óm ‘grape-N.INST.SG.F’ | k. | glo.g-ínj-a ‘hawthorn-N-NOM.SG.F’ |
| f. | lo.z-í.c-a ‘grape-N.DIM-NOM.SG.F’ | l. | glo.g-íć ‘hawthorn-N.DIM’ |

Now compare this to our regular examples in (316) and their paradigm in (319). Here, the H is always on the root itself and never on the suffix.

(319)

| | | | |
|---|---|---|---|
| a. | dú.nj-a ‘quince-N.NOM.SG.F’ | g. | o.ráh-∅ ‘walnut-N.NOM/ACC.SG.M’ |
| b. | dú.nj-e ‘quince-N.GEN.SG.F’ | h. | o.rá.h-a ‘walnut-N.GEN.SG.M’ |
| c. | dú.nj-i ‘quince-N.DAT/LOC.SG.F’ | i. | o.rá.h-u ‘walnut-N.DAT/LOC.SG.M’ |
| d. | dú.nj-u ‘quince-N.ACC.SG.F’ | j. | o.rá.h-om ‘walnut-N.INST.SG.M’ |
| e. | dú.nj-om ‘quince-N.INST.SG.F’ | k. | o.rá.š-ar ‘walnut-N.NOM.SG.M’ |
| f. | dú.nj-i.c-a ‘qui-N.DIM-NOM.SG.F’ | l. | o.rá.š-čić ‘walnut-N.DIM.NOM.SG.M’ |

Given (i) the finding that the H of the highest (usually rightmost) element in the first phase gets realized, and (ii) the uniformity of the patterns in (318)-(319), it is reasonable to assume that something special needs to be said about the roots in (317). Specifically, it seems that these roots contain an underlying H which wants to dock on the first syllable after the root (hence the term “postaccenting” in Halle 1997).[30] If tone is an autosegment, as I have been assuming following Inkelas & Zec 1988, then the grammar needs to contain a special tone-linking rule that applies to this limited set of roots to produce the desired outcome.[31] For Halle, who adopts the metrical approach to stress and accent developed in

[30] Halle considers a number of stress and accent systems across the Indo-European language family. According to him, these postaccenting stems are an innovation of the East and South branch of the Slavic languages.

[31] Zec 1993 attempts to derive the existence of postaccenting roots by proposing that an underlying H is not linked to a (root-final) syllable on the first cycle if that syllable is heavy (i.e., contains a long vowel). In this case, the H is linked to the

Idsardi 1992, postaccenting roots are simply specified to place a left parenthesis after their last syllable. I do not wish to go into the details of these phonological analyses; the goal here was simply to point to the known existence and specialness of such roots, as they are potential surface counterexamples to the generalizations made here.

It is also worth mentioning that the alternatives are not appealing. We could posit two segmentally identical but tonally distinct suffixes for each example in (318)-(319), e.g., *-a* and *-á*. This leads to a proliferation of suffixes and misses a generalization, namely that the bifurcation remains constant across all suffixes for a particular root. We could also assume that the suffixes in question always have an underlying H. Where the H surfaces on the root, we would then conclude this is because both the root and the suffix carry a H, and leftmost H wins out. This is the essentially Halle's proposal for Russian; it however, clashes with what we found for tone in BCS more generally, namely that the outwardmost H (rightmost in a ROOT-SUFF configuration) in the first phase wins out. We saw this pattern illustrated throughout this section, and it is particularly obvious with those roots that have an underlying H on a non-initial syllable (cf. zla.tí.bor – zla.ti.bo.r-ác).

## 6.4 Conclusion

I have shown that a unified explanation of locality effects in syntax and morphology in terms of Phase theory faces considerable difficulties, both conceptual (chapter 5) and empirical (this chapter). There has also been considerable dissatisfaction in the field regarding Phase theory as a theory of successive-cyclicity (see e.g. Boeckx & Grohmann 2007) and it has been pointed out that Phase theory is redundant with certain other syntactic locality principles that have been proposed over the years, like Minimality (see e.g., Müller 2011).

This chapter has shown an empirical domain where the attempted unification is problematic. Specifically, BCS *a*P behaves like a non-phase in that it does not seem to allow movement to proceed

final syllable on the next higher cycle, which includes the suffix. The empirical generalization seems to be incorrect, however, as the final syllable of postaccenting roots can also be short. In fact, all words presented in this section have syllables with short vowels, yet they show the bifurcating behavior I discussed above.

through its specifier, despite allowing subextraction. The impossibility of (intermediate) movement throught spec, *a*P was demonstrated using the reconstruction-for-binding and QR-in-ACD diagnostics. On the other hand, BCS *a*P was shown to behave as a point of spellout to the interfaces in that it closes off the locality domain for morphophonological processes, namely allomorphy and morphological tone assignment.

In the following chapter, I argue that Minimality is the only syntax-internal locality principle, which regulates probe-goal relations and drives successive-cyclicity effects. On the other hand, a modified version of the weak PIC, namely *Transfer*, regulates the spellout of syntactic structure to the interfaces, but has no effects syntax-internally. Elements that have been transferred to the interfaces can still be accessed for the purpose of syntax (e.g., displacement, agreement), but can no longer be internally manipulated for the purposes of the morphophonology or semantic interpretation.

# Chapter 7: Phases vs. Minimality, and Transfer to the interfaces

## 7.1 Introduction

As I noted in chapter 5, there is a tension between absolute locality constraints (viz. Phases) and relative locality constraints (viz. Minimality) because they produce overlapping restrictions (e.g., in *Wh-* Islands). This redundancy has thus far been disregarded in much of the locality literature, with some notable exceptions. For example, Müller (2004, 2011) suggests that Minimality-based locality should be dispensed with in favor of (a particular implementation of) Phase Theory.[1] In recent work, Keine & Zeijlstra (2023) and Halpert & Zeijlstra (2024) argue that previous phase-based accounts of specific phenomena should be dispensed with in favor of a Minimality alternative. As far as I am aware, there have been no attempts to dispense with Phase theory as a theory of successive-cyclicity and propose a Minimality alternative for syntactic locality effects across the board. This is the task I attempt in this chapter. Before moving on, I would like to refer the reader to Michal Starke's 2001 dissertation, in which he also argues that syntactic locality reduces to Minimality. Starke's concerns, and the implementation of his proposal are substantially different than what I will propose here, however. Moreover, the dissertation does not explicitly engage with arguments for Phase theory/Subjacency, despite offering an

[1] Müller argues for an implementation of Phase theory in which every phrase (i.e., every maximal projection) is a phase. As I mentioned in chapter 5, such a conception of phases brings the predictions of Phase theory closer to the predictions of Featural Relativized Minimality (with some important differences). In particular, Phase theory then predicts every locality restriction Minimality predicts. The reasoning is as follows: In the structure [ZP Z ... [$XP_{[+F]}$ X ... [WP W ... [$YP_{[+F]}$ Y]]], if XP asymmetrically c-commands YP, then at minimum, there is some head W that c-commands YP but not XP. Now assume that XP and YP share a feature that is probed for by Z, which c-commands both XP and YP. Minimality predicts that only XP is visible to Z. On the 'every-phrase-is-a-phase' approach, WP is a phase. Since WP intervenes between XP and YP and induces spellout immediately after its completion, the prediction is that YP is inaccessible to operations outside WP (hence, inaccessible to probing by Z). However, as we will see in a number of the cases below, the predictions of the 'every-phrase-is-a-phase' approach are in many ways too strict (e.g., for agreement) and insufficiently relativized in regard to different probes and goals (and their instantiations in different languages).

account for some of the phenomena that have traditionally been within its purview (e.g., weak islands).

Let me proceed with a refresher on the two kinds of syntactic locality constraints discussed in this chapter, namely Phases and Featural Relativized Minimality (FRM). Phase theory is formalized via the Phase Impenetrability Condition (PIC) in (320). Phase heads are generally taken to be C and *v*. FRM can be formalized via the Minimal Link Condition (MLC) in (321), granted that "$\alpha$ and $\beta$ are of the right type to establish a relation with H" is understood as "$\alpha$ and $\beta$ share a feature H is probing for."

(320) (strong) Phase Impenetrability Condition (Chomsky 2000)

In phase HP with head H, the domain of H is not accessible to operations outside HP, only H and its edge are accessible to such operations

(321) Minimal Link Condition (Chomsky 1995)

If in a structure $\Sigma$ = H . . . [. . . $\alpha$ . . . [. . . $\beta$ . . . ] . . . ] (where H c-commands $\alpha$ and $\beta$, and $\alpha$ asymmetrically c-commands $\beta$) both $\alpha$ and $\beta$ are of the right type to establish a relation with H, then H can establish a relation only with $\alpha$ (but not with $\beta$).

The way I will talk about FRM in this chapter presumes for the most part that probes are simple entities looking for a single goal. This is arguably the case in all the examples that have been prominently discussed in the Phase theory literature, and thus in all the examples I discuss in sections 7.2 and 7.3 below. Successive-cyclic movement/leapfrogging happens because a probe that is only able to interact with one goal can only interact with the closest one. While the probing is presumably executed in the same way in all cases (via Minimal search, see below), the discussion in these sections glosses over some very interesting recent work on *probe interaction* versus *probe satisfaction*, as well as proposals about *insatiable probes*, all proposed in the agreement literature. In particular, Deal (2015, in press) argues that certain probes may have multiple specifications for the features they interact with versus features that satisfy them and halt the probing. For example a probe specified [int: $\varphi$, sat: ADDR] will interact with every phrase that bears $\varphi$-features, but will only halt its search (be satisfied) when it finds a phrase that bears an ADDR (2nd person) feature, or when the search space is exhausted. Insatiable probes are probes

that have no satisfaction condition; they keep searching until the search space is exhausted. Probes may also be specified for a specific (e.g., case) feature, such that DPs that have a different case feature will not count as interveners (on case-discriminating probes, see Bobaljik 2008, Preminger 2014, Deal 2017). We will see some such cases in section 7.4. As I mentioned, however, a version of the MLC, namely Minimal search, will still hold in all such cases, where we may describe Minimal search as in (322); for a formalization of the Minimal search algorithm, see Ke 2019, 2024, Branan & Erlewine to appear.

(322) Minimal search (descriptive)

If in a structure $\Sigma$ = H . . . [. . . $\alpha$ . . . [. . . $\beta$ . . . ] . . . ] (where H c-commands $\alpha$ and $\beta$, and $\alpha$ asymmetrically c-commands $\beta$) both $\alpha$ and $\beta$ are of the right type to establish a relation with H, then H **must first establish a relation with** $\alpha$.

This chapter is organized as follows: In section 7.2, I argue that a substantial proportion of so-called phasehood diagnostics cannot, in fact, be taken as such, because they have the wrong modal force. Specifically, they show that a certain locality boundary *can* be imposed, but not that it *must* be imposed. Since both Phase theory and FRM make predictions about obligatory locality restrictions, these phenomena cannot be taken to provide evidence supporting either. This includes reconstruction diagnostics, QR in ACD, parasitic gap licensing, quantifier float, *wh-* copying, optional inversion, and optional agreement marking in long-distance extraction. Even where it looks like the locality effect is obligatory (e.g., in the reconstruction for binding cases), I will argue that this apparent obligatoriness does not stem from locality considerations.

In section 7.3, I look at the remaining evidence for phases, including data from so-called Merge-over-Move patterns, interface independence, V2 satisfaction by intermediate movement copies, obligatory extraction and inversion patterns, so called lexical choice effects at "the phase edge", and Long Distance Agreement patterns. In many cases, I will be drawing from existing criticisms for a particular phasehood diagnostic. The force of this section is in bringing the data together to show that none of the existing evidence for phases is particularly strong. In some cases, the diagnostic will simply be shown

to be problematic or unsubstantiated, and for some cases we will see that a Minimality alternative can account for the observed data equally well or better. I will also make it a point to show in the latter case that Minimality is operative in the grammar of a given language regardless of whether we adopt phases or not. Then, if an explanation for a particular phenomenon can be handled both in phase terms and in Minimality terms, I will argue that economy considerations favor the latter.

Then in section 7.4 I consider various syntactic phenomena for which a Minimality-based explanation is superior to a phase-based one. This includes EPP satisfaction and Superiority effects, (Long-Distance) Agreement with unaccusative predicates, ergative extraction patterns, intervention effects in binding and A-movement (Scandinavian object shift and Romance subject-raising), and hyper-raising patterns. The goal is to show that Minimality is an indispensable component of human language grammar, whose effects are observed with many different, unrelated phenomena.

Finally, section 7.5 considers what the findings from chapter 6 and this chapter mean for the theory of cyclic spellout. In particular, since I argue that syntax-internal locality is regulated by Minimality, and Minimality as such has no bearing on cyclic spellout, this must mean that cyclic spellout is regulated by a distinct mechanism. I call this mechanism Transfer, and argue that it is defined over syntactic heads, but that it must have no syntax-internal opacity effects. I consider the plausible motivation for having both Minimality and Transfer as part of the Faculty of Language.

Before I move on to discussing the specifics of the evidence for phases, it is worth repeating what I take to be the main take-aways from the previous two chapters. I argued in chapter 5 that there are irreconcilable differences in the way Phase theory needs to be formulated to account for syntax-internal locality effects and interface (morphophonological) locality effects. Chapter 6 offered an extended empirical argument from BCS adjectivization that (spellout-driven) morphophonological locality effects can be found where the evidence clearly shows there can be no punctuated movement. Taken together, these two chapters show that Phase theory is not to be preferred over a Minimality-based account of syntax-internal locality, at least not on the grounds that Phase theory can be a unified theory of successive-cyclicity and spellout.

## 7.2 What is *not* evidence for a phase

Before diving into the critical appraisal of phasehood diagnostics, it is important to consider what good evidence for phase-induced successive-cyclicity would even look like. Recall the (strong) PIC (323); crucially, for a phrase to escape spellout at phase HP, it *must* move to the edge of HP before the completion of HP. There is nothing optional about this movement, since any phrase inside HP will get spelled out and become inaccessible for further syntactic computation.[2]

(323) (strong) Phase Impenetrability Condition (Chomsky 2000)

In phase HP with head H, the domain of H is not accessible to operations outside HP, only H and its edge are accessible to such operations

However, many so-called phasehood diagnostics show that movement to a certain position is *possible*, not that it is *obligatory*. Importantly, it has not been shown that phases are the only type of phrase that can host moving elements. Such an argument is attempted in Abels 2003, but shown to be problematic in Boeckx 2008 and Müller 2011. Hence, it is possible that non-phases allow movement through their specifiers as well. Therefore, the ability to make an intermediate stop in a certain position could be independent of phasehood.[3] Even diagnostics that identify seemingly obligatory (intermediate) movement may not be suitable phasehood diagnostics, if the movement can be shown to be obligatory for reasons that are not due to phasehood. I illustrate both of these cases immediately below.

A prime example of such a problematic phasehood diagnostic is reconstruction. Arguments from reconstruction are frequently used in the literature on Phase theory and its predecessors, including reconstruction for binding and reconstruction for scope (Lebeaux 1988, Rullmann 1995, Fox 1999, Legate 2003, van Urk 2020a, i.a.). Despite its wide-spread use (and my use of this diagnostic in chapter

[2]The criticism that follows would also apply to the weak PIC, though recall that the weak PIC cannot force movement to proceed through specifiers of dedicated phase heads and as such is not suitable to govern the type of successive-cyclic movement assumed in the Phase theory literature in the first place (see section 5.4).

[3]Note also that, without further stipulations, all phase heads should allow the stranding of strandable material at their edge. This is clearly too strong of a prediction, as exemplified by the fact that there is no intermediate P-stranding in English:

(i) *Who$_1$ do you believe [CP [PP to t$_1$] Mary thinks [CP Joan talked t$_1$ ]] ? (Postal 1972:213)

6), evidence of reconstruction cannot, in fact, be taken as evidence for phasehood. Arguments from reconstruction show that extraction out of some domain can proceed with an intermediate stop. However, the claim that a domain is a phase is substantially stronger than this, as it states that movement must proceed through the edge of the phase. As observed by Keine (2016:407): "[T]his is a general property of evidence from reconstruction. The option of reconstructing an element into an intermediate position is uninformative with respect to the question of whether this intermediate position has to be created or merely may be created. Because an argument for phases depends on establishing the former, reconstruction evidence as a matter of principle does not provide evidence for phasehood." Hence, the three-way ambiguity of (324) shows that movement through intermediate clause-peripheral positions is possible, as in (325), but it does not show that it is obligatory. Had the intermediate movement not applied, it would simply be the case that a given interpretation would be unavailable.

(324) Which rumors about himself$_{i/j/k}$ did John$_i$ think [Perry$_j$ believed [Bill$_k$ would hate?]]

(325) Which rumors about himself did John$_i$ think [<which rumors about himself$_i$> [Perry$_j$ believed [<which rumors about himself$_j$> [Bill$_k$ would hate <which rumors about himself$_k$>?]]

In fact, the criticism for the reconstruction diagnostic applies even in cases where positing the intermediate stop is the only way to derive a grammatical sentence, as in the famous cases from Lebeaux 1988 (326). In the schematic representation in (327), we can see that the R-expression is illicitly bound in the base position of the *wh-* phrase (complement of *read*). On the other hand, the bound variable anaphor *he* is not bound in the surface position. The only way to satisfy all relevant binding conditions in one position is to move the *wh-* phrase through the embedded-clause periphery, as in (327). While this shows that an intermediate movement stop is obligatory in order to derive a grammatical sentence (326), it is perfectly possible that this movement has nothing to do with phasehood. It could equally be that the intermediate position is available (i.e., CP has an available specifier), and the *wh-* phrase can move through it or not. The only way to derive the grammatical sentence in (326) is to move through the intermediate position, making the movement seemingly obligatory. To reiterate, the movement is

seemingly obligatory because it allows the *wh-* phrase to avoid a binding violation; this is uninformative in regard to Phase theory.

(326) Which of the paper(s) that $he_j$ gave to Ms. $Brown_k$ did every $student_j$ hope $she_k$ would read?

(327) [Which of the paper(s) that $he_j$ gave to Ms. $Brown_k$] did every $student_j$ hope
[CP <**which of the paper(s) that $he_j$ gave to Ms. $Brown_k$**> that $she_k$ would
[*v*P read <which of the paper(s) that $he_j$ gave to Ms. $Brown_k$>]]?

Other evidence that has been argued to diagnose phasehood, but which suffers from the same problem as reconstruction evidence, is as follows: (i) patterns of Quantifier Raising (Legate 2003, Cecchetto 2004, Wurmbrand 2018; see also Bruening 2001); (ii) parasitic gap licensing (Nissenbaum 2000, Legate 2003, van Urk 2020a); (iii) quantifier float (McCloskey 1979, 2000, 2002, Henry 2012, Davis 2020, Doliana 2021); (iv) optional agreement marking in long-distance movement contexts (see Collins 1994 for Ewe, Bruening 2001 for Passamaquoddy, Torrence 2005, 2012 for Wolof); (v) optional inversion, such as that seen in some varieties of Hiberno-English like Belfast English (Henry 1995); (vi) overt intermediate *wh*-copies, which are always optional (Felser 2004, van Urk 2020a; see den Dikken 2017 for a critique of this diagnostic as evidence for intermediate movement). All of these phenomena show that movement to a particular position is possible, but not that it is obligatory because of locality considerations.

## 7.3 A critical examination of the evidence for phases

In this section, I examine the remaining evidence for phases. I begin with a discussion of the evidence from the Merge-over-Move Principle (7.3.1), then continue with interface independence (7.3.2), V2 satisfaction by intermediate movement copies (7.3.3), obligatory extraction marking patterns (7.3.4), lexical choice effects at the "phase edge" (7.3.5), obligatory inversion (7.3.6), and Long-Distance Agreement (7.3.7).

### 7.3.1 Merge-over-Move

One of the first empirical arguments for Phase theory involved the so-called Merge-over-Move (MoM) Principle (Chomsky 1995). Consider first the following contrast:

(328) a. There seems <there> to be a man in the garden.

b. *There seems a man to be <a man> in the garden.

Chomsky argues that there is a feature-checking requirement on the infinitival T, the question then being why this requirement cannot be satisfied by moving the DP *a man* into this position (328b). Chomsky argues that this shows evidence for MoM: Merging the expletive *there*, which is already in the numeration, is less computationally costly than moving the DP. The only licit result, then, is (328a). An empirical problem with MoM was then noted by Alec Marantz, who pointed out (329). If MoM holds, and (329) clearly contains an expletive, how is it that *a man* is moved to spec, TP of the embedded clause?

(329) There was a rumor [CP that **a man** was <a man> in the room].

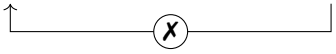
To deal with this issue, Chomsky invokes phases, understood as lexical subarrays, points of cyclic access to the lexicon. The embedded CP in (329) constitutes a separate phase, so the expletive *there* is not present in the derivation at the point *a man* is moved.

There are several issues with using this paradigm as evidence for phases. First, at the time it was proposed, MoM was justified as an economy principle by the assumption that Move was a complex operation involving the steps Copy+Merge. In Chomsky's subsequent writings, Merge and Move are reimagined as two sides of the same operation—E(xternal)-Merge and I(nternal)-Merge, so the conceptual motivation for MoM dissipates. Furthermore, Chomsky 2013 suggests that the grammar shows the opposite preference, namely Move-over-Merge. This clearly constitutes a clash, suggesting that the conceptual motivation for the MoM principle is quite weak.

On the empirical side, the original motivation for MoM relies on the assumption that non-finite T in (328) has an EPP requirement (requires a specifier). There has since been a substantial body of work arguing against the existence of EPP on non-finite T (see Castillo, Drury & Grohmann 1999, Boeckx 2005, Grohmann, Drury & Castillo 2000, Hornstein 2001, Bošković 2002, Epstein, Pires & Seely 2005). Moreover, the original paradigm still requires recourse to Minimality if (330a) is to be ruled out. By hypothesis, (330a) is built up from only one numeration, which contains both the DP *a man* and the expletive *there*. The DP *a man* is merged in the thematic position, as an argument of the verb *be*, and *there* is merged in spec, TP due to MoM (330b). At this point however, there is nothing about phases that can preclude *a man* from raising to the matrix subject position. Instead, we need to appeal to Minimality: since the probe on matrix T is looking for a DP, and there is a closer goal than *a man*, only *there* can be found by the probe and moved, which explains the ungrammaticality of (330b).

(330) a. *A man seems there to be in the garden.

b. *A man seems there to be <a man> in the garden.



Therefore, this (problematic) phasehood diagnostic cannot be made to work unless Minimality is also operative in the grammar. Note finally that the version of Phase theory invoked in this diagnostic is about cyclic access to the lexicon (so-called 'lexical subarrays'), rather than about syntax-internal opacity or successive-cyclicity. Recall that I will retain a cyclic spellout principle, namely Transfer, which regulates the transfer of syntactic structure to the interfaces (thus creating chunking effects at the interface), but has no effects syntax-internally. If the effects of MoM are real, and if access to the lexicon proceeds in the same chunks that are relevant to Transfer, it may be possible to derive the effects of MoM like those in (328) from Transfer alone.

### 7.3.2 Independence at the interfaces

In Chomsky's early writings on phases, much was made of the purported interface motivations for their existence. On the meaning side, Chomsky (2000:106) writes that phases are "the closest syntactic counterpart to a proposition: either a verb phrase in which all $\theta$-roles are assigned or a full clause including tense and force." On the form side, Chomsky (2001:12) argues that phases "have a degree of phonetic independence". While these ideas are somewhat vague, they are discussed and given substance in subsequent work, for example in Legate 1998, 2003, Matushansky 2005, Citko 2014. PF independence is thought to be detected by movability, isolability, ellipsis, and Nuclear Stress Rule (NSR) assignment. As the previously mentioned quote from Chomsky indicates, the main LF property of phases is that they should be "propositional"; this has been extremely difficult to make precise, as we will see below. To the extent that an LF phasehood diagnostic can be made precise, it is usually thought of as the ability to be the target of Quantifier Raising or the ability to reconstruct in an intermediate movement position. I discuss each of these diagnostics briefly, pointing out the issues associated with them along the way. A general issue with this type of diagnostic is that it is not clear how to reconcile the idea that phases are independent PF/LF objects with the implementation of Phase theory via the PIC, which spells out the phasal complement to the interfaces. As we will see, most of the diagnostics in this section show effects that apply to the entire phase, but it is not clear why these effects should arise if phasal complements are the targets of spellout.

I will exemplify the PF phase diagnostics using *v*P. For example, *v*Ps can be fronted (331a) or right-node raised (331b), and isolated in so-called "Mad Magazine" sentences (331c). It has furthermore been argued that only complements of phase heads undergo ellipsis (Holmberg 2001, Gengel 1992, 2009, van Craenenbroeck 2010, Rouveret 2012, Citko 2014, Park 2017), as illustrated for the VP complement of *v*P in (331d).[4] Furthermore, the examples in (331e-f) are argued by Legate to show that NSR applies at the level of the (*v*P) phase. Specifically, the underlying object of create in (331e) is *what*, and in (331f)

[4] Bošković 2014 argues that either complete phases or phasal complements undergo ellipsis. Empirical qualms aside, note that this optionality does not follow from Phase theory in any way.

it is *what suffering*. Bresnan 1972 shows that indefinites like *what* cannot bear primary phrasal stress even when final in the verb phrase. Instead, the primary stress lands on the rightmost element which can bear stress. In (331e) this is the (bolded) verb *create*, but in (331f) it is the DP object *what suffering*. The idea is that the primary stress is assigned when the DP *what suffering* is in its underlying position, before movement, and arguably at the *v*P phase.

(331) a. John said he would eat worms, and [*v*P $t_i$ eat worms] $he_i$ did! (Legate 1998:5)

b. John should and Bill won't [*v*P read a new article on RNR]. (Citko 2014:63)

c. Me teach physics?! (Legate 1998:6)

d. A macaw ate a nut and a cockatoo did [*v*P *v* [VP e] ], too. (Citko 2014:64)

e. The parable shows what (suffering men) can create. (Bresnan 1972:79)

f. The parable shows (what suffering) men can create.

Each of these diagnostics can be shown to be problematic in some way. For movement, isolability, and ellipsis, the main issue seems to be that they apply to a much wider range of constituents than those one would want to treat as phases based on syntactic locality effects. For example, TPs can be fronted (332a), right-node raised (332b), and isolated as stand-alone answers (332c).

(332) a. [TP To become a top syntactician], Mary is very likely. (Citko 2014:62)

b. Mary wonders when, and John wonders why, [TP Peter left]. (Bošković 2002:fn.18)

c. (What does Mary want?) –[TP To go to the cinema].

As acknowledged in Legate 1998, the NSR diagnostic does not distinguish between NSR assignment at *v*P or TP. The conclusion that it happens at *v*P is only warranted on the assumption that NSR assignment is phase-bound, and that TP is not a phase, neither of which has been conclusively determined. Furthermore, Aelbrecht (2009) argues in detail that the constituents that undergo ellipsis are not the same as those that trigger/undergo spellout; I will not review the arguments in detail here, the interested reader is referred to Aelbrecht 2009:119-132. Recall also the BCS ellipsis facts from chapter

6. The structure of the nominal phrase in BCS is as in (333). In chapter 6, I showed that ellipsis in the nominal domain in BCS can target the intermediate nominal projection, stranding one of the modifiers outside the ellipsis site (334a). Ellipsis can also strand only the quantifier (334b), or both the quantifier and all modifiers, as in (334c). It is very unclear to me what kind of phase-based ellipsis licensing could account for these facts.

(333) [QP pet [NP [AP mojih] [NP [AP žutih] [NP haljina]]]]]
five my yellow dresses
'five yellow dresses of mine'

(334) Context: Marija je kupila pet mojih žutih haljina.
Mary COP.3SG bought five my yellow dresses
'Mary bought five yellow dresses of mine.'

a. Jovana je kupila osam tvojih <žutih haljina>.
Jovana COP.3SG bought eight your yellow dresses
'Jovana bought eight of yours.'

b. Jovana je kupila osam <mojih žutih haljina>.
Jovana COP.3SG bought eight my yellow dresses
'Jovana bought eight.'

c. Jovana je kupila osam tvojih zelenih <haljina>.
Jovana COP.3SG bought eight your green dresses
'Jovana bought eight of your green ones.'

Turning now to LF diagnostics, there is (i) "full argument structure", also called $\varphi$-completeness; (ii) Quantifier Raising, and (iii) reconstruction (for binding or scope). I have already argued that (ii) and (iii) have the wrong modal force: they only show movement to a particular position is possible. The movement may even seem obligatory because without it an intended meaning would be unavailable, but the obligatoriness cannot be shown to follow from locality considerations. Another issue that pertains to all LF diagnostics is that they do not seem to make a distinction between transitive/unergative *v*P on the one hand, and unaccusative *v*P on the other hand. Chomsky 2000 argues that only the former are phases. Recall that the strong PIC is the only one that can force successive-cyclic movement; however, unaccusative *v*Ps cannot then count as phases, because of sentences like (335). In (335), the

matrix T $\varphi$-agrees with the plural argument *students*, across three unaccusative *v*Ps. This should be impossible if unaccusative *v*P is a phase, since *students* should become inaccessible for any further syntactic computation after the most embedded *v*P is completed–hence, unaccusative *v*Ps are thought to be non-phasal.

(335) There **are** [$_{vP}$ likely to [*v*P appear to [*v*P be **students** in the garden. ]]]

This conclusion is problematic for all interface phasehood diagnostics, however. Epstein (2003:30) notes that the specification of a phase as having "full argument structure" cannot mean that all relevant $\theta$-roles are discharged. This would have the unintended result that raising TPs as well as passive and unaccusative *v*P are phases because all $\theta$-roles associated with the predicate are discharged. As far as I am aware, it has remained virtually impossible to give this diagnostic any substance. For QR and reconstruction, unaccusative (and passive) *v*Ps are again an issue. Legate 1998, 2003 convincingly shows that unaccusative *v*Ps can host QRing phrases and intermediate reconstruction sites to the same extent as transitive/unergative *v*Ps. If QR and reconstruction were phasehood diagnostics, and phases became opaque when they were spelled out, the ability of the matrix verb in (335) to agree with the argument embedded under three *v*Ps would be fully unexpected.

Finally, it is also unclear what makes unaccusative *v*Ps less 'PF-independent' than transitive or unergative ones. In fact, Legate 1998, 2003 shows that unaccusative *v*Ps are PF-independent to the same extent as transitive ones, in terms of movability, isolability, and NSR assignment.

Before I close this section, let me mention the following: Matushansky (2005) argues that applying the interface independence diagnostics on DPs gives conflicting results. Specifically, DPs pass the diagnostics for PF-independence, but fail various LF and syntactic phasehood diagnostics. The conclusion is that "either interface independence is not a diagnostic for phasehood or phases do not exhibit independence at both interfaces at once, which casts the whole notion into doubt" (Matushansky 2005:157). To this I add simply that so-called PF diagnostics for phasehood seem to often be about little more than detecting constituency, and LF diagnostics are either completely void of substance or

else show that an element can be interpreted in a certain position due to reasons that are quite possibly completely independent of phasehood.

### 7.3.3 V2 satisfaction by intermediate copies of movement

Van Urk (2015, 2018) and van Urk & Richards (2015) argue that the Dinka *v*P is V2: exactly one constituent precedes the verb in the *v*P. In a transitive clause, the object must precede the verb (336). In ditransitives, exactly one of the objects precedes the verb (337); no other word order permutation is possible. If there is no nominal object, the preverbal position is empty; non-nominal elements such as adjuncts appear postverbally (338). Crucially, A'-movement interacts with V2. Specifically, if the moving element is a DP, then every preverbal position along the movement path must be empty, (339).

(336) a. ɣɛ̋ɛn cɛ̤́ mìir tɪ̤̂ŋ (van Urk & Richards 2015:122)
I PF giraffe see
'I saw a giraffe.'

b. *ɣɛ̋ɛn cɛ̤́ ___ tɪ̤̂ŋ mìir
I PF see giraffe
'I saw a giraffe.'

(337) a. ɣɛ̋ɛn cɛ̤́ Ayén yiɛ̋n kìtáp (van Urk & Richards 2015:122)
I PF Ayen give book
'I gave Ayen a book.'

b. ɣɛ̋ɛn cɛ̤́ kìtáp yiɛ̋n Ayén
I PF book give Ayen
'I gave a book to Ayen.'

(338) a. wɔ̋ɔk cɛ̤́ ___ kɛɛ̋t dòm-íc (van Urk & Richards 2015:123)
we PF sing garden-in
'We sang in the garden.'

b. wɔ̋ɔk cɛ̤́ ___ kɛɛ̋t dòm-íc
we PF sing garden-in
'We sang in the garden.'

(339) a. yeŋà$_1$ cíi môc ___$_1$ yiɛ̤̀n kìtáp? (Van Urk and Richards 2015:125)
who PF.NS man.GEN give book
'Who did the man give the book to?'

b. *yeŋà$_1$ cíi môc Ayén yiɛ̈n ___$_1$?
who PF.NS man.GEN Ayen give
'What did the man give Ayen?'

van Urk 2015 and van Urk & Richards 2015 analyze this effect in terms of phases, the idea being that only the shifted object in (339a) is accessible for further movement (to spec, CP). In (339b), the argument goes, the movement would need to proceed in one-fell-swoop, but the *wh-* phrase is trapped inside the *v*P phase which has already undergone spellout.

As noted by Keine (2020:283-288), the analysis of the Dinka V2 facts in van Urk 2015, 2018 and van Urk & Richards 2015 makes crucial use of an EPP property on *v*, and this property is what does the necessary work of attracting the closest DP to the relevant position; the appeal to phases is not substantive. The question, then, is simply whether the ungrammaticality of (339b) with the attempted fronting of a *wh-* phrase over a filled preverbal position can be explained in other terms.

The answer seems to be 'yes'; the asymmetry can be explained by appealing to FRM. Specifically, the object that moves to satisfy the V2 requirement moves to some position above VP, call it FP in (340).[5] After the subject is moved to TP, only one DP can be leapfrogged over it. Per FRM, this must be the shifted object, and thus the preverbal gap with object questions is expected.

(340) [C... [*v*P SUBJ [FP OBJ$_1$ F [ApplP <OBJ$_1$> [ VP VERB [DP <OBJ$_2$>]]

Keine & Zeijlstra (2023) point out that the phase account has nothing to say about the PP/DP asymmetry observed in (336)-(338) above. Furthermore, as shown in (339b), a DP cannot undergo A'-extraction if the preverbal position is filled. PPs show no such restrictions, as shown in (341) which demonstrates movement of what van Urk & Richards 2015 analyze as a PP across a filled preverbal position (bolded). Since Van Urk (2015:218, 2018:949) argues that such PPs are generated *v*P-internally, and *v*P spellout is supposed to explain the ungrammaticality of (339b), the PP in (341) should be trapped within the *v*P phase in the same way, contrary to fact.

[5]The movement is string-vacuous in (340) but leads to a change in word order in monotransitives, where the single object moves across the verb to the spec, FP position.

(341) yeŋó$_1$ cíi yìn **kôoor** nɔ̂ɔ̀k ___$_1$?
what PF.NS you lion kill
'What did you kill a lion with?'

Keine & Zeijlstra argue for an intervention account of this asymmetry. If the C probe in (340) is specified for a [D] feature, and T enables a single DP to leapfrog over the subject (see 7.3.4 below), then Minimality ensures that only the structurally highest DP below the subject can leapfrog, as in (339a). PP adjuncts, on the other hand, are generated above the position occupied by the preverbal DP-object, as in (342). Hence, PP adjuncts will not be able to satisfy the V2 requirement nor will their movement require that a gap be left in the preverbal position.

(342) [vP SUBJ [PP ADJUNCT [FP OBJ ... [VP VERB [DP <OBJ> ]]]]]

Note, however, that PP-adjuncts are sufficiently nominal to be attracted by the (subject) leapfrogging probe and by C, both of which are specified to look for [D]. Evidence for the nominal character of these PPs comes from the fact that moved PPs, like the one in (341) in fact appear as bare DPs and lose their prepositional marking, unlike their counterparts in declarative sentences, cf. (343a-b).

(343) a. Bòl à-thɛ̰t **né tó̰o̰ny**. (van Urk 2015:105)
Bol 3SG-cook.SV P pot
'Bol is cooking with a pot.'

b. **Tó̰o̰ny** à-thɛ̰ɛ̰tḛ̀ Bôl.
pot 3SG-cook.OBLV Bol.GEN
'A pot, Bol is cooking with.'

We see that the V2 facts in Dinka do not force an analysis in terms of phases; the V2 property is independently analyzed as a requirement for a filled specifier, and the consequences for A'-extraction are better explained in terms of FRM-based DP-intervention.

### 7.3.4 Obligatory extraction marking patterns

Unlike the optional extraction-marking patterns I identified in 7.2, obligatory extraction-marking patterns can, in principle, be used to argue for obligatory successive-cyclic movement and, if certain pre-

dictions are borne out, for phase-based derivations. I will show that the most convincing such cases from the literature show behavior which seriously puts into question both of these conclusions. We will see that so-called complementizer agreement in Irish may not be tracking A'-movement at all, and that the famous extraction pattern from Dinka may receive a better explanation in an intervention-based system.

One of the classical arguments for successive-cyclic movement, later taken as an argument for phases, comes from the Irish complementizer system. As reported in McCloskey 1979, 1990, 2001, 2002, Irish has a distinct complementizer in declarative clauses (344a) and in clauses that can be shown to involve A'-dependencies (344b). I illustrate here with relative clauses, but the same pattern is observed in other A'-contexts such as with *wh*-questions. I will gloss the marker $a^L$ as C.EXT for "extraction complementizer", though both parts of this description have been brought into question, see below. Crucially, in cases of long-distance A'-dependencies, the extraction complementizer is found in every clause on the path of movement (344c). This fact was taken to argue for obligatory successive-cyclic A'-movement through spec, CP and later on for a phasal spellout motivation for said movement.

(344) a. Creidim [CP **gu-r** inis sé bréag]. (McCloskey 2002:185–186)
believe.1SG C.DCL-PST tell he lie
'I believe that he told a lie.'

b. an fhilíocht [CP **a$^L$** chum sí ___]
the poetry C.EXT composed she
'the poetry that she composed'

c. an t-ainm [CP **a$^L$** hinnseadh dúinn [CP **a$^L$** bhí ar an áit]]
the name C.EXT was-told to-us C.EXT was on the place
'the name that we were told was on the place'

However, Since both verbs are unaccusative, treating their *v*s as non-phasal means there is only one cyclic node between matrix T and the argument in the embedded clause. We should similarly get external sandhi effects across CP in such configurations, contrary to fact.Since both verbs are unaccusative, treating their *v*s as non-phasal means there is only one cyclic node between matrix T and the argument in the embedded clause. We should similarly get external sandhi effects across CP in such configurations, contrary to fact.assume for a moment that there are no phases (i.e., no absolute locality

domains for syntactic relations), and that spellout does not make syntactic structure invisible for further computation. With these assumptions in place, it is not clear that we have any clear evidence from the Irish pattern for successive-cyclic movement. As pointed out by den Dikken (2017:9): "Absent a cogent demonstration of the need, over and above an Agree relationship between the $a^L$ and the closest c-commanded member of the *wh*-chain, for a Spec–Head relation between $a^L$ and a *wh*-trace/copy in its own specifier position, [the facts] do not lend support for successive-cyclic movement via the specifier of $a^L$". In other words, the evidence is equally compatible with the claim that all the complementizers simply agree with a *wh*-copy they c-command, which could just as well be the *wh*-copy in the base-generated position.

Furthermore, the idea that the marker $a^L$ is a complementizer has been widely challenged in the specialist Irish literature (Harlow 1981, Sells 1984, Noonan 1992, 1994, 1997, 2002, Duffield 1995; see the response in McCloskey 2001). I will not go through all the arguments here; I simply point out one alternative analysis that seems to be compatible with the data to the same extent as the complementizer analysis. According to Noonan 2002, $a^L$ is an agreement particle which reflects argument-shift (see also Sells 1984, McCloskey & Sells 1988, Noonan 1992, 1994, Duffield 1995, Adger 1996). That is, $a^L$ appears whenever the subject or object DP has raised to or through a licensing position and is positioned to the left of the surface position of the verb. The main argument comes from the presence of the marker $a^L$ in infinitival clauses which display obligatory object-shift (345a), shown to pattern with A-movement. If the embedded infinitival verb is intransitive, there is no object shift and the marker does not appear (345b).[6] Additionally, while an $a^L$ appears with (long-distance) A'-dependencies involving arguments, a different particle $a^N$ appears with those dependencies that involve adjuncts (346). This is straightforwardly explained if $a^L$ is an argument-shift marker but not if it is a marker of (successive-cyclic) A'-dependencies. The long-distance pattern is then argued to arise because the embedded CPs themselves undergo argument-shift, thus explaining the presence of $a^L$; for a detailed development of

[6]Both the verb and the infinitival subject remain *in situ*, which is why $a^L$ does not surface in (345b); unlike finite verbs, the infinitival verb does not move to T, so the subject precedes it even in its base position. Irish has a mechanism for licensing subjects in small clauses in their base position (see Chung & McCloskey 1987).

this proposal, see Noonan 2002.

(345) a. Ba mhaith liom [Seán an caora **a**$^{\mathbf{L}}$ mheá ar an bhfeirm]. (Noonan 2002:270)
I would like Seán the sheep C.EXT weigh on the farm
'I would like Seán to weigh the sheep on the farm.'

b. B'éadoiche [iad cruinniú]
would-be-improbable them assemble
'It would be improbable for them to assemble.'

(346) an áit **a**$^{\mathbf{N}}$ raibh muid ___ (McCloskey 1979:171)
the place A$^{N}$ were we
'the place where we were'

There is an issue that is equally relevant for the complementizer analysis and the complement-shift-marker analysis, which is that $a^L$ also appears in quite a few contexts in which there is no indication that any movement has applied, as in (347). As acknowledged by McCloskey (2001:72): "If there is no believable analysis of such cases [...] in terms of movement, we will have to assume either that they reflect accidental homophony, or that the relation between *wh*-movement and the appearance of $a^L$ is indirect". This casts into doubt the whole enterprise of treating $a^L$ as a(n A'-)movement indicator.

(347) Is amhlaidh **a**$^{\mathbf{L}}$ bhí neart céad fear ann. (McCloskey 2001:71)
COP so C.EXT was strength hundred man in-him
'It is a fact that he had the strength of a hundred men.'

The goal here was simply to point out that there are various analyses that are as compatible with the Irish data as the phase-driven successive-cyclic movement analysis. Furthermore, even if the issues I brought up are satisfactorily resolved and $a^L$ is found to truly mark successive-cyclic movement, an intervention analysis of such movement has not been ruled out. For example, one would want to find out (i) whether the marker appears in intermediate movement that involves unaccusative predicates (i.e., when an external argument DP cannot be said to intervene and induce leapfrogging), as in *What did she say* ***happened*** *to be broken?*; and (ii) whether (embedded) CP has the features that are relevant for the matrix probe, such that leapfrogging (successive-cyclic movement) would be necessary for intervention reasons and not for phase reasons. I leave these questions for further research.

I will move on now to discussing an extraction marking pattern in Dinka, perhaps the most famous case of obligatory extraction marking at the *v*P edge (see van Urk 2015, 2018, 2020b, van Urk & Richards 2015, Keine & Zeijlstra 2023). With the exception of local external arguments, whenever a plural element is moved out of *v*P in Dinka, the element *ké* (or *kêek*) must appear to the left of every verb that is crossed by the movement. A'-extraction of external arguments triggers the appearance of *ké* in every *v*P except the one in which the argument originates (348).

(348) a. yeyíŋì yḛ́ ké tâak, [CP cíi Bôl ké tîŋ]? (vU & R 2015:128)
who.PL HAB.2SG PL think PF.OV Bol.GEN PL see
'Who all do you think Bol saw?'

b. ròo̤o̤r áa-yṳṳkṳ̀ ké tàak [CP cḛ́ **(*ké)** yîi̤n tîi̤ŋ]
men 3PL-be.IPL PL think.NF PF (*PL) you see.NF
'The men, we think have seen you.'

According to van Urk (2015, 2018) and van Urk & Richards (2015), this effect is attributable to *v*P phases. Recall that Dinka *v*P has a V2 requirement, satisfied via an EPP feature. Additionally, *v*P is arguably a phase, so the only way the object can move to matrix CP is if it first moves to the edge of every *v*P on its movement path. The *ké*-marking is argued to be an overt realization of an intermediate copy of movement at the edge of each crossed *v*P. As noted in Keine & Zeijlstra 2023, it is surprising on this account that the A'-extraction of a local external argument does not similarly leave such a copy in the specifier of *v*P. One solution alluded to in van Urk 2018 is to say that *ké* only appears for copies of elements that occupy spec, *v*P as a result of attraction by *v*, which raises questions about how the morphological realization of a copy can depend on its Merge history with another element. Regardless of whether a principled solution to this problem is possible, however, Keine & Zeijlstra note that the subject/object asymmetry in *ké*-realization does not follow in any way from Phase theory.

Keine & Zeijlstra also note that movement of an object out of an unaccusative does not lead to *ké*-marking (349a). While this may be taken to suggest that unaccusative *v*P is not a phase (as was done for English in Chomsky 2000), they note that extraction of an adjunct PP headed by a plural noun out of an unaccusative *v*P does trigger *ké*-marking (349b). The phase account cannot explain this distributional

dichotomy, regardless of whether unaccusative *v*P is taken to be a phase or not.

(349) a. yè pɛ̌ɛɛl-kó bɛ́ **(*ké)** dhuôoŋ? (Keine & Zeijlstra 2023:20)
Q knives-which FUT (*PL) break.NF
'Which knives will break?'

b. yè thɛɛ̌k-kó bíi̱ pɛɛ̌l **ké** dhuôoŋ? (van Urk 2015:168)
Q times-which FUT.OV knives PL break.NF
'At which times will the knives break?'

Keine & Zeijlstra offer an intervention-based analysis which accounts for the reported facts in a principled way. Assume that there is no *v*P phase in Dinka. Furthermore, the feature that triggers A'-movement in Dinka is a [D] feature on C. Per FRM, C can only attract the closest DP. It is precisely when C seemingly attracts a further away goal over a closer one (e.g., a DP object over a DP subject) that *ké*-marking appears. Keine & Zeijlstra argue that such cases involve leapfrogging, movement of the lower DP across the subject, which makes the object the most local goal to C.[7] They analyze *ké* as the reflex of the probe that gives rise to this leapfrogging, T[$\varphi$] in their analysis. Some support for the idea that the marker *ké* is essentially the spellout of $\varphi$-features on a probe is the fact that it appears only with the extraction of plural phrases, and it is homonymous with the 3rd person plural pronoun. In addition to accounting for the object-over-subject pattern, Keine & Zeijlstra's analysis is straightforwardly extended to *ké*-marking in long-distance movement (the embedded DP must leapfrog over each subject to be closer to the matrix C) and PP extraction (PPs, which can be probed by [D], recall (343b), must leapfrog over the (unaccusative) subject in TP on their way to CP, leaving behind the reflex *ké* on T); see den Dikken 2017 for further arguments against the *v*P-phase account of extraction marking in Dinka.

Before I move on, it is important to add that there are other languages where the extraction marking pattern in long-distance dependencies has been reported as obligatory: Chamorro (Chung 1982), Kinande, (Schneider-Zioga 2009), Kîîtharaka (Muriungi 2005, Abels & Muriungi 2008), Seereer (Baier 2014), and Selayarese (Finer 1997, 2003). It remains to be seen whether all of these patterns can be ex-

[7] The ditransitive examples we saw in (337) may look problematic in light of this analysis since either of the two internal arguments can seemingly move. However, the two ways to derive a question from a ditransitive come from two distinct underlying structures with the two internal arguments in distinct hierarchical positions, as argued for in van Urk 2015.

plained without appealing to Phase theory, a task I leave to further research; see den Dikken 2017 and Keine & Zeijlstra 2023 for a critical evaluation of some of the evidence.

### 7.3.5 Lexical choice effects at the "phase edge"

van Urk 2020a argues that another way to detect phase heads is by looking at what he calls "lexical choice effects at the phase edge". Essentially, if a phase head carries a feature-trigger for intermediate movement (i.e., an EPP feature), then this feature may be reflected in the exponent that is ultimately assigned to that head. In toy English, we could imagine a Vocabulary Item for English C looking like (350). While English C does not have a designated exponent for C that carries a [WH]/[EPP] probe, van Urk argues that such languages do exist and that these effects should be taken as evidence for phasehood.

(350) C → dat / F[WH]
→ if / F[Q]
→ that / elsewhere

Note however that such featural marking on exponents does not really provide any solid evidence for phases. We know that various "flavors" of a head can be attested and that their distinct featural composition may lead to distinct exponence. For example, English T has different exponents based on the valuation of its $\varphi$-probe (*-s* vs. *-∅*); Slavic case morphemes have different exponents based on the declension class of the element they attach to, and so on. It is not clear to me at all why such effects should be taken to diagnose phasehood. More specifically, while an exponent that (arguably) only appears in movement contexts may provide evidence that successive-cyclic movement has taken place (though recall the Irish complementizer case discussed in section 7.3.4), this by itself will not tell us anything about the motivation for said movement. A priori, we do not know if the movement is triggered for phase reasons, Minimality reasons, or neither. Still, I will walk through one such case brought up in van Urk 2020a because it is, in fact, better accounted for in Minimality terms.

In Russian, extraction is dispreferred out of so-called indicative clauses headed by *čto*, but allowed

out of so-called subjunctive clauses headed by *čtoby*, see (351) from Müller & Sternefeld 1993:466-7. According to van Urk, this suggests that *čtoby* realizes a C head with an extraction feature, while *čto* realizes a C head without an extraction feature.

(351) a. ?*Kakuju knigu$_1$ ty dumaeš' [CP **čto** Petr pročital ___$_1$]?
which book you believe that.IND Petr read
'Which book do you believe that Petr read?'

b. Kakuju knigu$_1$ ty dumaeš' [CP **čtoby** Petr pročital ___$_1$]?
which book you believe that.SUBJ Petr read
'Which book do you believe that Petr read?'

Put this way, it is clear that these effects are quite tangentially related to phasehood and in fact may be completely orthogonal to it. On the phase account, the crucial assumption that will preclude movement out of a *čto*-clause is the assumption that this kind of clause has no available specifier (i.e., no escape hatch). But it is then this fact (the lack of available specifier) that derives the observed effects, and not phasehood per se. If we can find an alternative reason for why long extraction of *wh*-phrases is blocked from a *čto*-clause but not from a *čtoby*-clause, then the phase account can be abandoned.

In fact, Bailyn 2020 offers such an account, building on the work of Zemskaja 1973. Zemskaja (1973:398–405) shows that extraction out of *čto*-clauses is possible, but only with non-*wh*-elements. Bailyn confirms that (long-distance) A'-scrambling out of *čto*-clauses is acceptable (352a), while *wh*-movement out of *čto*-clauses is degraded (352b). This pattern is mirrored with *wh*-extraction versus A'-scrambling out of embedded questions (353). The asymmetry in (352) is completely unexpected on a Phase theory account: If extraction out of *čto*-clauses is degraded because *čto*-CPs lack an available specifier and the A'-element is therefore spelled out inside this CP phase, we do not expect to see any asymmetries in the acceptability of extraction depending on the type of phrase that is being A'-moved.

(352) a. **Ogurcov$_1$** žal', [CP čto malo ___$_1$]. (Müller & Sternefeld 1993:467)
pickles too.bad that there.are.few
'Pickles, it's too bad that there are so few of [them].'

b. *__Čego__$_1$ žal', [CP čto malo ___$_1$]?
what too.bad that there.are.few
'What is it too bad that there are so few of?'

(353) a. Ty **doktor**$_1$ videl [CP kogda pod'ezžal ___$_1$ ]?
you doctor.NOM saw when was.arriving
'Did you see when the doctor was arriving?'

b. *__Kto__$_1$ ty videl [CP kogda pod'ezžal ___$_1$ ]?
who.NOM you saw when was.arriving
'Who did you see when (he) was arriving?'

Bailyn argues that the contrast between *wh*-movement and A'-scrambling in (352)-(353) should be attributed to the same factor. Specifically, he argues that *čto*-clauses are a type of *wh*-island. This analysis has some initial plausibility since the complementizer *čto* 'that' in Russian is homonymous with the *wh*-word *čto* 'what.NOM/ACC'. There are further parallels between *wh*-islands and *čto*-clauses, like argument/adjunct asymmetries characteristic of weak islands, as well as the ameliorating effect of D-linking; see Bailyn 2020:648-9. Bailyn offers an account of the extraction asymmetry in *wh*-movement/scrambling in terms of FRM. Specifically, he argues that *wh*-movement is driven by a [Q] (quantificational) feature, while scrambling is driven by a [Σ] (scrambling) feature. On the assumption that *čto*-clauses do not have available specifiers (i.e., do not permit leapfrogging), the observed differences between *wh*-movement and scrambling follow. Specifically, the probe looking for the [Q] feature responsible for *wh*-movement will always find a closer [Q] goal than the embedded *wh*-word in both the *čto*-clause (352b) and the embedded question (353b). On the other hand, these [Q]-elements will not serve as interveners for scrambling, driven by [Σ].[8] Hence, the [Σ]-probe will be able to find the scrambling phrase inside both *čto*-clauses and embedded questions. The FRM approach therefore accounts for asymmetries that are completely mysterious on a phase-based account, on which the identity of the moving element should be irrelevant for the locality effects we observe. See Bailyn 2020 for further evidence that *wh*-phrases and scrambling phrases are featurally distinct, since different elements count as

[8] The same contrast is observed with quantificational adverbs and focus phrases: they block long-distance *wh*-movement but not long-distance scrambling, presumably because they are closer goals for the [Q]-probe, but not the [Σ]-probe; see Bailyn 2020:652-4.

interveners for their probing.

### 7.3.6 Inversion patterns

It has been observed for a number of languages that the subject and the auxiliary must invert if movement targets the CP-edge (see e.g., Kayne & Pollock 1978 for French; Torrego 1984 for Spanish; Ortiz de Urbina 1989 for Basque). Illustrating with Spanish, adapted from Torrego 1984, the basic SV order (354a) is obligatorily transformed to VS in the presence of *wh*-movement (354b-c).[9] Importantly, the VS order must obligatorily appear in every clause crossed on the path of movement (354d-e), unlike in English. This has been taken as evidence for successive-cyclic movement through every spec, CP, originally accounted for in terms of Subjacency, and later adapted to Phase theory as the dominant theory of successive-cyclicity.

(354) a. Juan vendrá con sus amigos hoy.
Juan come.FUT with his friends today
'Juan will come with his friends today.'

b. Con quién vendrá Juan hoy?
with who come.FUT Juan today
'With who will Juan come today?'

c. *Con quién Juan vendrá hoy?
with who Juan come.FUT today
'With who will Juan come today?'

d. **Qué$_1$** pensaba Juan [ que le había dicho Pedro [ que había publicado la revista ___$_1$?
what thought Juan that him had told Pedro that had published the journal
'What did Juan think that Pedro had told him that the journal had published?'

e. ***Qué$_1$** pensaba Juan [ que Pedro le había dicho [ que la revista había publicado ___$_1$?
what thought Juan that Pedro him had told that the journal had published
'What did Juan think that Pedro had told him that the journal had published?'

Torrego (1984: 105-6) also reports that inversion in Spanish is only obligatory with arguments

[9] The same inversion pattern is found in (long) topicalization, *though*-movement, exclamatives, and comparative clauses (but not in relative clauses).

and optional with adjuncts (*en qué medida* 'to what extent', *por qué* 'why', *cuándo* 'when', *cómo* 'how'), cf. the unacceptable argument questions without inversion in (354c)-(354e) and the acceptable adjunct questions without inversion in (355)-(356).[10]

(355) **Por qué** Juan quiere salir antes que los demás ___? (Torrego 1984:106)
for what Juan wants leave before than the others
'Why does Juan want to leave before the others?'

(356) **En qué medida** Juan había pensado [CP que Pedro le había (Torrego 1984:110)
in what way Juan had thought that Pedro him had
asegurado [CP que la revista se arriesgaria a publicar eso ___? ]]
assured that the journal SE risk to publish that
'To what extent had John thought that Peter had assured him that the journal would risk publishing that?'

The observed argument/adjunct asymmetry is quite surprising if successive-cyclic movement (which drives inversion) is triggered by the phasehood of CP. In particular, if the spellout of the CP complement is what triggers movement to each intermediate spec, CP, then arguments and adjuncts should be equally affected. However, if the successive-cyclicity effect is due to FRM-based intervention, then we can make sense of the Spanish data, in the way detailed below.

An appealing first suggestion is that we attempt to account for the observed effect by assuming that the movement probe on Spanish C is simply specified for the category of its goal (e.g., [D]), as in Dinka. Then, DPs–subject DPs in the examples above–will intervene for *wh*-DP extraction, forcing the leapfrogging (successive-cyclic) movement which leads to inversion. These same DPs will not be interveners for PP/adverb-extraction since the featural (i.e., categorial) makeup of these phrases is distinct. Furthermore, moving phrases are always able to stop in spec, CP, since CP has an available specifier, hence the optional inversion with *wh*-PPs). However, only intervention *forces* such stopovers in order to make the moving DP the closest DP to the probe on matrix C.

Torrego 1984 reports that Spanish allows sentences like (357), which are unacceptable in English because they constitute a *Wh*-Island violation. The approach I outlined above, where Spanish C

[10]Torrego specifically claims that one of the possible interpretations of (356) is the one where *en qué medida* 'to what extent' modifies the most embedded verb *arriesgar(se)* 'risk'.

is specified for the category of its goal–[D] in (357)–takes care of this finding. Specifically, if matrix C is looking for a [D] feature, then the adverbial *wh*-phrase *cuánto* 'how much' does not count as an intervener, and the movement of *quién* 'who' is possible.

(357) Quién no sabes cuánto pesa? (Torrego 1984:114)
who NEG know how-much weighs
*lit.* 'Who don't you know how much weighs?'

However, this account may need some tweaks in light of more recent experimental data on islands in Spanish. As shown in a line of experimental work culminating in Pañeda et al. 2024, the Spanish speakers who were tested have *Wh*-Islands which are independent of category. For example, their experimental results show that sentences like (358) have severely degraded acceptability, on a par with their English counterparts. This constitutes a clear divergence from Torrego's data above and is unexpected if the movement probe on Spanish C is specified only for category features.

(358) *Qué preguntó el cocinero cuándo modificarían?
what asked the cook when would-change
*lit.* 'What did the cook ask when they would change?'

An account that can explain both the inversion facts and the island facts is as follows: There is a complex probe on Spanish C, which looks for both a category feature (e.g., D) and a *wh-* feature. Then, the category of the (non-*wh*) subject will intervene for extraction only when the extracted phrase is a DP. Spec, CP is available for leapfrogging though, and the leapfrogging leads to inversion. In the case of *wh*-islands, the *wh*-feature of the already moved phrase in spec CP intervenes for extraction (regardless of the category of the two phrases), but Spanish CP like its English counterpart has only one available spec. The intervention effect cannot be circumvented and the extraction is impossible. By assuming complex probes, the FRM approach to locality can explain the Spanish data *in toto*. There may be two groups of Spanish speakers, whose grammars differ in the featural content of the C-probe.

Before moving on, I should note that den Dikken 2017 suggests inversion in Spanish may be a "*v*P effect" rather than the usually presumed "CP effect". Specifically, he suggests that inversion arises

as a result of the subject staying in situ rather than the auxiliary moving to a higher position over a TP-subject. Some evidence for this comes from examples like (354d) above, where the subject is found to the right of both the finite auxiliary and the participle (participles may move to AgrP, a low functional projection above *v*P, see Kayne 1989). While for den Dikken the inversion is then seemingly a result of successive-cyclic movement to spec, *v*P due to *v*P phasehood, it is still not clear to me how to obtain the argument/adjunct asymmetries on a phase account. Low adjuncts like *cómo* 'how' should presumably still trigger inversion if they must move to the edge of the *v*P phase on their way to a higher position. If inversion is triggered by the subject staying low when there is a *wh*-phrase in (outer) spec, *v*P, then inversion should presumably also be obligatory with (low) adjunct extraction, contrary to fact. If the subject indeed stays low, the intervention account still makes the right cut: only DPs are required to move successive-cyclically over the (low) subject, hence only DP-movement triggers obligatory inversion. The intervention approach based on FRM therefore allows us to make sense of the Spanish inversion data much better than the phase account.[11]

### 7.3.7 Long-distance Agreement (LDA)

The modern Generative conception of morphological agreement takes it to arise through a syntactic operation, namely Agree, as in (359).[12] If Agree is a syntactic operation, we then expect it, *ceteris paribus*, to be subject to the same locality constraints as other syntactic operations like movement. Of course, agreement and movement may be triggered by different features (e.g., [$\varphi$] vs. [WH]); in a system that incorporates FRM, we do then expect some differences in the observed locality effects in that some elements may count as potential goals (that is, interveners) for one probe but not for another. As we will see, (i) FRM is necessary in accounting for patterns of LDA, and (ii) Phase theory faces serious

[11] However, see Goodall 2011, 2021, 2022 for experimental evidence from satiation, which the author takes to suggest that the failure of inversion in the unacceptable Spanish sentences produces a processing difficulty rather than a syntactic violation. If the processing account turns out to be correct, then we need not appeal to Phases or FRM to account for this particular effect.

[12] Some have questioned this view and argued that agreement is post-syntactic (e.g., Bobaljik 2008), but in my understanding, the consensus is that agreement shows all the hallmarks of a syntactic operation (see Preminger 2014 for a particularly forceful argument to this effect).

challenges in accounting for the availability of Agree operations into what are supposed to be opaque domains. Phase theory should therefore be dispensed with as a theory of syntax-internal locality.

(359) Definition of Agree (Chomsky 2000:122):

Agree is a syntactic operation taking place between a probe P and a goal G in the domain of P, D(P), between which a Matching relation holds. G must (at least) be in the domain D(P) of P and satisfy locality conditions.

a. Matching is identity of feature attributes;

b. D(P) is the sister of P;

c. Locality reduces to "closest c-command."

As seen in (359), the original definition of Agree incorporates FRM, since a Goal (element with matching features) is accessible to an Agree Probe if it is the closest such element in its domain. If the locality of syntactic operations is also constrained by phases, then this makes additional predictions for the locality of agreement. Specifically, Phase theory is a theory of syntactic domains which become opaque to further syntactic computation due to cyclic spellout. The resulting opacity is not, as Phase theory stands, relativized in regard to distinct syntactic operations, and it is in fact difficult to see how this would be achieved.

So-called Long-Distance Agreement (LDA) is a phenomenon which shows that the conclusions reached in regard to the locality of (successive-cyclic) movement, and the formalization of those conclusions in Phase theory terms, cannot straightforwardly be carried over to Agree. In fact, Bošković 2007 uses LDA to argue that agreement, unlike movement, is not constrained by the PIC.[13] Let us then look at some of the attested patterns of LDA; note that I will be arguing for a conclusion distinct from that reached in Bošković 2007. It is not that movement and agreement are regulated by distinct locality principles, but rather that Phase theory should be abandoned as a syntactic locality theory in favor of

[13]The assumption that movement is constrained by the PIC is adopted without questioning in Bošković 2007, who writes: "I will take it for granted here that Move is subject to the PIC" (Bošković 2007:55). It is this assumption, along with detailed investigation of the locality of Agree, that leads him to the conclusion that the two are constrained by distinct locality mechanisms.

FRM. Note also that LDA is likely not a unified phenomenon and may sometimes be only apparent (see the discussion in Bhatt & Keine 2017).

LDA is traditionally understood as agreement that crosses clausal boundaries, as in the Chukchee example in (360a) from Bošković 2007:57, who cites Mel'čuk 1988. In (360a), the object-agreement marker on the matrix verb tracks the embedded object, across a finite clause boundary. In modern terms, we consider LDA to be any agreement relation that spans across a (presumed) phase boundary, for example the agreement into non-finite clauses in Hindi-Urdu (360b), which have been argued to be at least *v*Ps (Bhatt 2005). Various patterns of LDA are observed in typologically and genetically diverse languages, including at least Basque, Chukotko-Kamchatkan (Chukchee, Itelmen), Icelandic, Nakh-Dagestanian (Tsez), Indo-Aryan (Hindi-Urdu, Kashmiri), and Native North American languages (Blackfoot, Fox, Innu-aimûn); for an accessible overview of analytical approaches to LDA, see Bhatt & Keine 2017.

(360) a. ənən Ø-qəlɣiḷuləŋərkə-nin-**et** [CP iŋqun Ø-rətəmŋəv-nen-at **qora-t**].
he-INST 3SG-regrets-3-PL that 3SG-lost-3-PL reindeer-PL
'He regrets that he lost the reindeers.'

b. Raam-ne [*v*P **roṭii** khaa-nii] caah-**ii**. (Mahajan 1990:237)
Ram-ERG bread.F eat-INF.F.SG want-PF.F.SG
'Ram wanted to eat bread.'

Let us see exactly why LDA poses a challenge to Phase-theoretic locality. The Chukchee case is quite straightforwardly problematic, given the almost universal assumption that CP is a phase. If CP were a phase, then probes located on the outside of CP should not be able to see any material inside it, contrary to fact. Note, however, that FRM potentially gives us a handle on the observed facts. If there are no phases, then there are no absolute restrictions on probing, only the requirement that the probe find the closest relevant goal. There is at least one conceivable way to achieve the result in (360a), then. Note that the suffixal agreement marker on the matrix verb is specifically an object-agreement marker (subject-agreement being a prefix). It is possible that the object-agreement probe is a case-discriminating probe, such that only the object DP (but not the subject DP) of the embedded clause is considered a

potential goal. I do not have enough information on Chukchee to claim this conclusively; I simply point out that FRM provides a potential solution, while such a solution is not forthcoming on a Phase theory approach to locality.

I now turn to two patterns of LDA that can arguably be accounted for both in terms of Phase theory and in terms of FRM. We will see, however, that FRM has broader empirical coverage and still needs to be operative in the grammars of these languages even if Phase theory is adopted to account for the basic LDA facts. I will therefore argue, on economy grounds, that the Phase-theoretic explanation should be abandoned in these cases.

Tsez (Nakh-Dagestanian) has an interesting pattern of LDA, first analyzed in Polinsky & Potsdam 2001 and Polinsky 2003. Note first that only absolutive arguments in Tsez can control verbal agreement (361). The verb can also optionally agree with the absolutive argument in an embedded clause, but this is only possible for nominalized clauses (362a). Agreement with the embedded absolutive argument is impossible if the complement clause is a finite CP (362b). In (362b), the matrix verb obligatorily agrees with the entire embedded clause, giving class IV agreement (*r*-).

(361) eniy-ā **ziya** **b**-išer-si (Polinsky & Potsdam 2001:586)
mother-ERG cow.III.ABS III-feed-PST.EVID
'The mother fed the cow.'

(362) a. eni-r [užā **magalu** b-āc'-ru-łi]IV **b/r**-iyxo (P&P 2001:606)
mother-DAT [boy bread.III.ABS III-eat-PAST-N] III/IV-know
'The mother knows the boy ate the bread.'

b. eni-r [užā **magalu** b-ac'-si-ƛ**in**]IV ***b/r**-iyxo (P&P 2001:635)
mother-DAT [boy bread.III.ABS III-eat-PST-EVID.C III/IV-knows
'The mother knows that the boy ate bread.'

Polinsky & Potsdam discover an interesting correlation between topichood of the embedded absolutive argument and its ability to control LDA; this leads them to posit that the LDA pattern is available only if the absolutive argument moves to a Topic position at the edge of the embedded (nominalized) clause covertly (at LF). The movement must be covert since the absolutive argument is clearly not clause-peripheral in the surface string (362a). Now, while Polinsky & Potsdam do not appeal to

phase-locality (but rather to *head government*) to account for the contrast in (362a-b), they do claim that there is a CP layer in (362b) but not in (362a).[14] In Phase theory terms, then, the absolutive argument in (362b) would not be accessible to the matrix probe because its final (covert) landing site is below CP, and it gets spelled out at the CP phase (363). Note that Polinsky & Potsdam fully ignore the nominalizing layer in the clause in (362a) in their analysis, assuming that this clause is a bare TP/TopP. The LDA arises when the absolutive argument (covertly) moves to spec TopP. This cannot be quite right, as this movement would still place the absolutive argument below the *n*P, which itself triggers class IV agreement. Recall that class IV agreement is possible in (362a) but it is not the only option, as (364) would predict. If Minimality is still relevant in this system (as I show immediately below), then the structure in (364) does not make available the agreement with the class III absolutive argument. Instead, the argument would have to leapfrog over the *n*P, as in (365). Such leapfrogging is presumably not available over CP. While this is a stipulation, note that Tsez does not have any long-distance (i.e., cross-clausal) movement, which may give some weight to the conclusion that movement across CP is generally prohibited.

(363) [CP ƛin [IV] [TopP magalu [III] [TP ... ]]] (class IV agreement)

(364) [*n*P -łi [IV] [TopP magalu [III] [TP ... ]]] (class IV agreement)

(365) [FP magalu [III] [*n*P –łi [IV] [TopP magalu [III] [TP ... ]]] (class III agreement)

Independent evidence that Minimality is operative in the grammar of Tsez comes from superiority and intervention. Note first that Tsez exhibits classical superiority effects, in that a structurally higher (ergative) argument must front before the structurally lower (absolutive) argument (366). The classical explanation of these facts attributes the effect to the relative closeness of the two arguments to the landing site of movement. These effects are easily captured with FRM, but they are not within the

[14] The account of the contrast in terms of head-government presumably only works because Polinsky & Potsdam ignore the presence of the nominalizer in the causes that allow LDA. The nominalizer, which takes the TopP-headed clause as its complement should arguably block head-government by the matrix verb to the same extent that the presence of C does.

purview of Phase theory.

(366) a. łu šebi žek'-ā
who.ERG who.ABS hit-PST.INTERR
'Who hit whom?'

b. *šebi łu žek'-ā
who.ABS who.ERG hit-PST.INTERR
'Whom did who hit?'

Furthermore, the presence of a non-absolutive *wh*-phrase in the embedded (nominalized) clause blocks LDA with the absolutive argument (367). Polinsky & Potsdam argue that *wh*-phrases land in a position higher than the (moved) absolutive topic. It is the *wh*-phrase *łu* 'who.ERG' (which is not a suitable agreement goal, being ergative) that now occupies the edge position in the nominalized clause. The matrix probe can then only see the class IV features of the nominalized clause, since the absolutive *micxir* 'money.ABS' in (367) is buried too far away.

(367) enir [**łu** micxir b-ok'āk'-ru-łi] r/*b-iyxo (P&P 2001:634)
mother who.ERG money.III.ABS III-steal-PART-N IV/*III-knows
'The mother knows who stole the money.'

The goal in discussing the Tsez data was two-fold: to show that FRM can handle the LDA facts to the same extent as Phase theory, and to show independent evidence that FRM is operative in the Tsez grammar. See Bošković 2007 for additional arguments that Tsez LDA can be accounted for by appealing to Minimality rather than phasehood.

I now move on to discussing LDA in Hindi-Urdu. As already mentioned, Hindi-Urdu allows agreement into infinitival clauses (360b), repeated here as (368). On the other hand, agreement into finite clauses is prohibited (369); default masculine singular must be used instead (see Mahajan 1990, Butt 1995, Bhatt 2005, Chandra 2007, Keine 2016, 2017).

(368) Raam-ne [*v*P **roṭii** khaa-nii] caah-**ii**. (Mahajan 1990:237)
Ram-ERG bread.F eat-INF.F.SG want-PF.F.SG
'Ram wanted to eat bread.'

(369) lar.kõ-ne soc-aa/***-ii** [CP ki monaa-ne (Bhatt 2005: 776)
boys-ERG think-PF.M.SG/*-PF.F.SG that Mona-ERG
**ghazal** gaa-yii thii]
ghazal.F sing-PF.F.SG be.PAST.F.SG
'The boys thought that Mona had sung ghazal.'

Furthermore, Keine (2016, 2017) argues that LDA is possible across a potentially unbounded number of (infinitival) *v*Ps, even when all movement to the edge of the embedded clause is excluded.[15] Specifically, idiomatic readings of VO idioms are available only if the objects stays in situ (Keine 2017:179). Yet, even in these configurations the object can control agreement across two *v*Ps (370).

(370) raam-ne [bhains ke.aage **biin** bajaa-nii] caah-**ii** (Keine 2017:179)
Ram-ERG buffalo in.front.of flute.F.SG play-INF.F.SG want-PF.F.SG
*lit:* 'Ram wanted to play the flute in front of the buffalo.'
*idiom:* 'Ram wanted to do something futile.' (possible)

From this, Keine concludes that Hindi-Urdu CP is a phase, but *v*P is not. However, the attested facts can also be handled in a FRM framework.[16] In infinitival clauses, there are no interveners between the probe on the matrix verb and the embedded internal argument. In particular, even if some *pro*-like element occupies the external argument position in infinitives, there is no evidence that this element has $\varphi$-features. Agreement on the matrix verb is then freely established either with the actual embedded argument, or with the already agreed-with embedded verb which now carries $\varphi$-features (the latter avenue pursued for Hindi-Urdu in Butt 1993, 1995), see (371).

(371) [*v* want [$\varphi$] [*v*P *pro* [*v* ... [DP bread [F.SG] ... ]]]]

On the other hand, finite clauses always have overt subjects which have their own $\varphi$-features (ergative, in the case of *Mona* in (369)). Since agreement in Hindi-Urdu is (descriptively speaking) restricted to DPs without overt case marking, we may think that we are dealing here with a case of defective intervention, in the sense of Chomsky 2000. Specifically, the $\varphi$-probe may be specified to only be satisfied

[15] We cannot therefore appeal to covert movement to the edge of the phase, as in the Tsez case.

[16] FRM is independently needed in Hindi-Urdu, since agreement is always established with the structurally highest caseless DP (see e.g., Mohanan 1994).

and valued by DPs with a particular case value, but to terminate its search after encountering any DP at all (see Branan & Erlewine to appear for a discussion of such probes more generally). The ergative DP *Mona* in (372) will cause the $\varphi$-probe to halt its search without finding a suitable goal.

(372) [$v$ thought [$\varphi$] [CP that [TP **Mona[ERG]** ... [$v$P ... ghazal [F.SG] ...]]]]

While this analysis seems suitable for some languages like Tsez, where we clearly saw the effects of defective intervention, its predictions do not seem to be borne out for Hindi-Urdu. Specifically, if the presence of the (ergative) external argument were responsible for preventing the agreement relationship between the matrix verb and the non-case-marked argument in the finite embedded clause, we would expect that non-case-marked arguments of intransitive verbs should then be licit goals for agreement across a finite clause boundary. As illustrated in (373), this is not the case; the matrix verb cannot agree with the sole feminine argument in the finite embedded clause.

(373) us aadmii-ne soc-aa/*-ii [ki roṭii gir gayii] (Rajesh Bhatt, p.c.)
that.OBL man-ERG think-PF.M.SG/*-PF.F.SG that bread.F fell go.PF.F
'That man thought that the bread fell.'

Alternatively, it may be that CP itself has $\varphi$-features (as it does in Tsez), and that the probe must therefore agree with CP due to FRM. While this issue would need to be investigated in detail for Hindi-Urdu, it has been claimed to be the case for other languages that do not allow agreement into finite clauses, such as English (see Bošković 2007). The idea that finite clauses in English have $\varphi$-features, such that the clause is always a closer $\varphi$-goal than any of the arguments inside it, garners some support from examples like (374), discussed in McCloskey 1991 and Iatridou & Embick 1997. In (374) the subject is a conjunction phrase (ConjP) made up of two finite clauses. Agreement can be resolved to plural, as evidenced by the matrix verb *seem*, presumably because both clauses have [$\varphi$: SG], as in the more straightforward case of conjoined DPs in (375).[17]

[17] Thivierge 2021 offers an alternative intervention-based analysis of the Hindi-Urdu data. Her account depends on a distinction between inherent versus acquired $\varphi$-features. Only elements with inherent $\varphi$-features count as interveners, and only T in Hindi-Urdu has a $\varphi$-probe. On the other hand, $v$ does not have a $\varphi$-probe; its agreement features are parasitic on T (for arguments in favor of this position, see Thivierge 2021:151-5).

(374) [[That he'll resign] and [that he'll stay in office]] **seem** at this point equally possible.

(375) [[An apple] and [an orange]] **seem** to be on the table.

Finally, let me discuss a case of LDA in Serbian which supports a FRM approach to locality.[18] In Bešlin 2022, I argue that the verb *trebati* 'need' takes a TP complement headed by the T *da*. On a phase-based approach to locality, the inside of this TP should then be transparent for syntactic probing from the matrix clause (at least up until the first embedded *v*P). This is not the case, however. LDA with the subject of the embedded TP is possible only when that subject is at its edge, preceding T, as in (376a).[19] If the subject follows T, only the default neuter agreement is possible (376b-c).

(376) a. Ne bi treba-**o** [TP i-ko da to urad-i]. (Bešlin 2022:79)
NEG be.AOR.3SG need-PART.M.SG i-who.3M.SG DA that do-3SG
'No one should do that.'

b. *Ne bi treba-**o** [TP da i-ko to urad-i].
NEG be.AOR.3SG need-PART.M.SG DA i-who3M.SG that do-3SG
'No one should do that.'

c. Ne bi treba-**lo** [TP da i-ko to urad-i].
NEG be.AOR.3SG need-PART.NEUT.SG DA i-who.3M.SG that do-3SG
'No one should do that.'

This pattern is quite surprising in a phase-based locality system, but receives a natural explanation in a FRM system. Notice that the embedded clause is finite. I assume, following standard assumptions, that verbal $\varphi$-features originate on T. Then, what we have in (376b) is a classical case of $\varphi$-intervention. Specifically, as established in chapter 4, the BCS predicative active participle is an adjective, which needs to agree for the $\varphi$-features gender and number; it does not have person features, as indicated by the glosses in (376). The embedded DP subject has a complete set of $\varphi$-features: gender, person, and number (and case). Embedded T (in fact, any finite T) has only person and number features, which surface

[18] The switch from Bosnian/Croatian/Serbian in the rest of the dissertation to Serbian here is because embedded *da*-clauses are very common in Serbian (and some Bosnian) dialects, but almost non-existent in Croatian dialects, which use the infinitive instead.

[19] That the embedded subject is still in the embedded clause in (376a) is evidenced by the presence of the so-called *i*-NPI *iko*. This kind of NPI is licensed by superordinate negation, unlike *ni*-NPIs, which are licensed by clause-mate negation (for discussion, see Progovac 1991).

on verbs and the auxiliary. The matrix $\varphi$-probe cannot reach the embedded subject across T, presumably due to the presence of T's own $\varphi$-features, see (377). Embedded T cannot itself agree with the participial probe, as its features only partially overlap with those of the probe. Agreement fails, as evidenced by the default neuter in (376c). We may then be dealing here with a case of defective intervention (Chomsky 2000, i.a.). When the embedded subject is moved over T, it becomes accessible to the matrix probe, which can now agree with it for masculine singular (378).

(377) [ PART [$\varphi$: NUM, GEN] ... [TP **T[$\varphi$: 3SG]** ... [ SUBJECT [$\varphi$: 3SG.M] ]]]

(378) [ PART [$\varphi$: NUM, GEN] ... [TP [ SUBJECT [$\varphi$: 3SG.M] T[$\varphi$: 3SG] ... ]]]

In this section, I have shown that the existing evidence for phase-based locality is quite weak. I have shown for each diagnostic either that it is problematic or empirically unsubstantiated as it stands, or that a Minimality alternative can account for the observed data equally well or better. I have shown that Minimality is operative in the grammars in question more generally, and argued that Phase theory should be dispensed with on economy grounds.

## 7.4 The Minimality Alternative

In this section, I will go through (some of the) phenomena for which FRM needs to be invoked, showing along the way that a Phase theoretic account is not plausible.

### 7.4.1 EPP and classical Superiority effects

In the simplest case, FRM accounts for the fact that external, rather than internal, arguments are moved to satisfy T's need for a DP specifier in a language like English (379). We know that external arguments are generated higher than internal arguments inside the *v*P. When T probes for a [D] element, it must per FRM attract the closes DP, namely the external argument (380).

(379) a. Masha hid the pillow.

b. *The pillow hid Masha.

(380) [TP EXT.ARG T[D] [*v*P <EXT.ARG> *v* [VP V INT.ARG ]]]

There is no account for these facts in Phase-theoretic terms and the effect cannot be made to follow from any other existing part of syntactic theory.[20] In particular, while we could stipulate that the English facts arise because T requires that the DP in its specifier be marked for nominative case, this stipulation cannot be made into a general account of such effects. In Icelandic, for example, both internal and external arguments can be dative-marked (381). And yet, in both cases, the only available option is to raise the *external* (higher) argument to T; raising the internal argument to satisfy T's requirement results in an unacceptable sentence. This means that the probe on T cannot be case-discriminating in Icelandic the way it may be in English. Again, such effects are easily accounted for in a FRM approach to locality. T is specified to look for [D] and it can only find the closest element with the relevant feature, in this case the external argument.

The same kind of effect is observed with other A-movement phenomena, for example object-shift in Scandinavian languages. If only one object of a ditransitive verb moves to a higher position within the *v*P, it must be the structurally higher (indirect) object, see the Danish data in (387) (for discussion of the facts in Icelandic, see Collins & Thráinsson 1996, Richards 1997, Bruening 2001, i.a.).

(381) a. María hjálpar mér oft með heimavinnuna.
Mary helps me.DAT often with homework
'Mary often helps me with my homework.'

b. Henni höfðu leiðst þeir.
her.DAT had bored.at they.NOM
'She had found them boring.'

[20] While we could imagine that the strong PIC causes the complement of *v*P to be spelled out and the internal argument to therefore be inaccessible to the probe on T if it stays *in situ*, there is in principle no reason why the object could not leapfrog over the subject (to an outer spec of *v*P) and be accessible to T from this position. Furthermore, Phase theory has truly nothing to say about the facts of Scandinavian-style object-shift and passivization with ditransitives, discussed below.

The same structural height effect is observed with the passivization of ditransitive verbs; I use English to illustrate. In English, some ditransitive verbs have two internal DP arguments. As shown in (382a), the recipient argument *John* can bind the anaphor *himself* inside the theme argument. Linear precedence of the recipient argument therefore translates to (asymmetric) c-command. Then, if we attempt to passivize this sentence, we clearly see that only the structurally higher of the two internal arguments can become the passive subject in spec, TP (382b-c). This effect receives a natural account in a FRM approach to locality. The probe on T searches for a [D]-feature and it can only find the closest one–the [D]-feature on the recipient argument. Phase theory has nothing to say about these effects, since both objects are located *v*P-internally (below the hypothesized phase head, namely the head that introduces the external argument). If spellout happened at the *v*P phase and the PIC precluded T from searching within the *v*P, then both internal arguments should be equally inaccessible to it, contrary to fact. If passive *v*P is assumed to be non-phasal, the contrast between (382b-c) is equally as mysterious on a phase-based approach to locality.

(382) a. Mary gave John some pictures of himself.

b. John was given some pictures of himself (by Mary).

c. *Some pictures of himself were given John (by Mary).

We can also subsume under this general FRM label so-called *Superiority effects* (first discussed in Kuno & Robinson 1972, Chomsky 1973), effects in the A'-domain parallel to those in (379)-(382). In English, which allows only one *wh*-phrase to overtly raise to clause-initial position, the *wh*-phrase that raises must be the structurally highest one: the external argument rather than the internal argument (383a-b) and the DP complement rather than the PP complement in DP-PP ditransitives (383c-d).[21]

[21] I am glossing over many details here, for example the effect of so-called D-linking on Superiority. Pesetsky (1987, 2000) notes that D-linked *wh*-phrases are allowed to violate Superiority, as in *which book* in *Which book did which person buy?* (Pesetsky 2000:16). Bošković 2002 (2002: 360) argues for a focus-movement approach to *wh*-question formation, and proposes that D-linked *wh*-phrases are not inherently focused, since their range of reference is discourse given. Therefore, D-linked *wh*-phases do not undergo classical focus movement. This gives a plausible solution to the FRM problem to D-linked vs. non-D-linked *wh*-phrases; it could be that their movement is driven by distinct features, for example topic features versus focus features (see also Lipták 2001, Polinsky 2001, Boeckx & Grohmann 2004, Grewendorf 2012).

(383) a. Who did what?

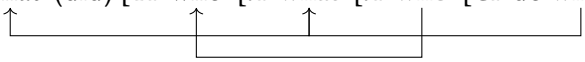
b. *What did who do?

c. What did you give to whom?

d. *Whom did you give what to?

Note that Phase theory says nothing about why such contrasts should obtain. Take a sentence where both the subject and the object are *wh*-phrases. If the only locality principle in the grammar were the PIC, a *wh*-object would simply move to an outer spec, *v*P (which it must do to avoid spellout if English *v*P is taken to be a phase), and from there over the *wh*- subject occupying the spec, TP position into spec, CP (384). We would then derive the ungrammatical **What did who do*?

(384) * [CP what (did) [TP who [*v*P what [*v*P who [CP do what ]]]]]



In order to preclude this derivation, FRM must be invoked. Specifically, a [WH]-probe on English C can only attract one *wh*-phrase, and FRM ensures that this is the structurally highest *wh*-phrase.

### 7.4.2 Intervention effects in A-movement phenomena

In this section, I discuss intervention effects in two types of A-movement: object-shift and subject-raising. These effects receive a natural account in a FRM approach to locality; Phase theory does not provide a suitable alternative.

DP objects in Scandinavian languages undergo object shift, A-movement to a relatively low position, below the verb in T, but above low VP-adverbs (Holmberg 1986; see Vikner 2006 for an overview). In Danish, only unstressed pronouns undergo (obligatory) object shift (Vikner 1990, Johnson 1991, i.a.). Both pronominal objects in a double-object construction undergo this movement (385).

(385) a. Peter viste jo Marie bog-en. (Vikner 1990:246-7)
Peter showed indeed Mary book-the
'Peter indeed showed Mary the book.'

b. Peter viste **hende den** jo.
Peter showed her it indeed
'Peter indeed showed it to her.'

Furthermore, double-object constructions have an interesting shifting pattern which shows all the hallmarks of FRM. First note that a reflexive anaphor within the theme argument can be bound by the recipient argument DP (386). This is taken to show that the recipient asymmetrically c-commands the theme. Next, object shift in Danish double-object constructions seems to only be possible if there is no intervening object that does not itself shift (387a-b). Specifically, the structurally higher recipient argument can shift even if the theme argument does not shift (387a), whereas the opposite is impossible (387b). According to Vikner 1989, there is no way to form a grammatical sentence in Danish if the recipient argument is a full DP and the theme an unstressed pronoun, as in (387b).

(386) ... i sine bestræbelser på at give **nation-en$_i$** (Herslund 1986:135)
in his-REFL efforts on to give nation-the
**sit$_i$** daglige fremmedord.
its-REFL daily foreign-word.
'... in his efforts to give the nation its daily foreign word.'

(387) a. Peter viste **hende** jo bog-en. (Vikner 1990:247)
Peter showed her indeed book-the
'Peter indeed showed her the book.'

b. *Peter viste **den** jo Marie.
Peter showed it indeed Mary.
'Peter indeed showed it to Mary.'

Vikner 1989 accounts for the Danish object-shift facts by appealing to Minimality. Bringing his analysis up to date, we can account for the fact as follows: The prevailing view in the literature is that the difference between full DPs and pronouns in Danish is that only the latter need to be case-licensed (only pronouns in Danish are overtly case-marked). Let us then assume that the object-shift probe (call it AgrO) is searching for [K] (case). Furthermore, the probe on AgrO is an *insatiable probe*, a probe that will attract as many [K]-bearing DPs as are in its search space (see Deal in press). In Deal's *interaction-satisfaction* theory of Agree (Deal 2015), this is a probe that is specified to interact with [K], but lacks a

satisfaction condition.[22] Then, due to Minimal search, the recipient object being the closer goal must be moved to AgrO first; the theme object can never move over the recipient object.[23]

(388) [AgrOP AgrO [int: K] ... [*v*P RECIPIENT [K] ... [*v*P THEME [K] ]]]

Next, in Icelandic, agreement with the nominative subject of an embedded infinitive is blocked by an intervening dative experiencer (389) (Watanabe 1993, Schütze 1993, McGinnis 1998, Holmberg & Hróarsdóttir 2003, Sigurðsson & Holmberg 2008, Kučerová 2016, i.a.). If the dative intervener moves to occupy the subject position, agreement is then possible with the embedded nominative subject (390).

(389) það virðist / *virðast einhverjum (Holmberg & Hróarsdóttir 2003:1000)
EXPL seem.3SG seem.3PL a.DAT
manni [TP hestarnir vera seinir].
man.DAT horses.NOM be slow
'The horses seem to some man to be slow.'

(390) Manninum virðist / virðast [TP hestarnir vera seinir].
the_man.DAT seem.3SG seem.3PL horses.NOM be slow
'The horses seem to the man to be slow.'

This pattern certainly seems like a textbook case of FRM: T can only establish an agreement relationship with the closest DP it c-commands–in (389), this is the dative experiencer. Moving the dative "out of the way" of the agreement probe on T allows T to establish an agreement relationship with the embedded subject which is now the closest potential goal. The optionality in (390) is also interesting–T can agree in singular or plural. I take this to suggest that T's [D] (movement) and [$\varphi$] (agreement) probes are not strictly ordered, such that either one can probe first. If [D] precedes [$\varphi$],

[22] As far as I can tell, there is no consensus on whether the object-shift positions are below or above the base position of the subject (spec, *v*P), see e.g., the discussion in Vikner 2006. If they are above this position, then the object-pronouns must first leapfrog over the subject (e.g., to an outer spec, *v*P) to be attracted to the object-shift positions while still obeying FRM.

[23] Note that the order recipient>theme is preserved in object-shift contexts, despite the fact that recipient must move first. We see here a classical case of "tucking-in", described first for multiple *wh*-movement in Bulgarian (Richards 1997, Richards 2001). An interesting generalization may be lurking here: Base hierarchical order is preserved when two or more phrases are internally Merged as specifiers of the same projection; this is crucially different from leapfrogging (successive-cyclic) movement. When a phrase is internally merged to a projection that already has an externally-merged specifier, the internally merged specifier is always structurally higher than the externally merged one. If this is correct, it would show that External Merge precedes Internal Merge, a principle that would presumably need to be encoded in the grammar. As far as I am aware, our current models have no way of predicting this pattern.

the dative intervener will move to spec, TP before T probes for agreement, making the argument in the embedded clause visible to the agreement probe. If [$\varphi$] precedes [D], the agreement probe will see and agree with the dative intervener before it moves to spec, TP.

A similar intervention effect is observed for some speakers of Italian, for whom raising-to-subject is blocked across an experiencer (391a-b) cf. (391c), see Rizzi 1986, McGinnis 1998. We may then similarly explain the grammaticality contrast in (391a-b) versus (391c) in FRM terms: the probe attracting the subject to spec, TP of the matrix clause is apparently unable to do so in (391a-b) due to the intervention of the (closer) experiencer (the same kinds of effects have been observed in a variety of languages, including Spanish (Torrego 1996), Greek (Anagnostopoulou 2003), and even English (Hartman 2011a,b); see Petersen 2016 for a detailed cross-linguistic investigation of raising across experiencers, and an account of the facts in terms of Minimality).

(391) a. *Gianni sembra a Pietro [TP aver paura di qualcose]. (Luisa Seguin, p.c.)
John seems to Peter have fear of something
'John seems to Peter to be afraid of something.'

b. *Gianni gli sembra [TP aver paura di qualcose].
John CL.DAT.M.SG seems have fear of something
'John seems to him to be afraid of something.'

c. Gianni sembra (sempre) [TP aver paura di qualcose].
John seems always have fear of something
'John always seems to be afraid of something.'

There are several peculiarities in the Italian case which distinguish it from the similar Icelandic case, and which deserve a mention. First, there is the caveat that the Italian verb *sembrare* 'seem' does not admit dative subjects, so unlike in Icelandic, the experiencer cannot move to spec, TP to avoid ungrammaticality in (391a-b). Hence, while in Icelandic there is merely a difference in the availability of plural agreement, in Italian there is a grammaticality contrast. Furthermore, when the intervener in Italian is not a clitic, it appears in an *a X* 'to X' phrase, which looks like a PP, and therefore does not seem (on the surface) to be of the same category as the raising DP. Questions then arise about why this phrase still seemingly counts as an intervener for subject-raising under FRM. Given that the effect is the

same with the *a*-marked experiencer (391a) and with what is clearly a pronominal (dative) clitic (391b), it seems reasonable to suggest that the *a*-marked experiencer is a DP despite superficial appearances (see Bárány 2018 for arguments that *a*-phrases in Romance are dative DPs).

Finally, moving the experiencer in (391a) sentence-initially, as in (392), does not lead to a significant improvement in acceptability. On the face of it, it looks like the strategy that works to avoid intervention Icelandic–moving the intervener out of the way–does not work in Italian.

(392) *A Pietro, Gianni sembra aver paura di qualcose.
to Peter John seems have fear of something
'To Peter, John seems to be afraid of something.'

The fact that we see this divergence between Icelandic and Italian is interesting and could be a matter of derivational timing. Note first that (i) in Italian the intervener is blocking (A-)movement while in Icelandic it is blocking agreement, and (ii) removing the intervention effect in Icelandic requires moving the intervener to an A-position (to spec, TP), while the Italian intervener can only undergo Topic-movement. Recall I suggested that the intervener in Icelandic can move to spec, TP before T probes for agreement, making the argument in the embedded clause visible. In Italian, on the other hand, raising the subject to spec, TP would always need to happen before the intervener can be moved to the topic position, which is above TP. There is therefore no way to remove the intervener in time. This account of the diverging behavior is plausible but speculative, and further research is needed to (dis)confirm it.

### 7.4.3 Agreement across unaccusative predicates

FRM also provides a natural account for why even English, which does not generally have LDA, allows LDA of T[$\varphi$] with arguments across an indefinite number of unaccusative predicates, as in (393). LDA is not permitted with transitive predicates in English, see (394) (unlike in Chukchee, recall (360a)). English T has a [$\varphi$]-probe, and per FRM, it can only agree with the closest $\varphi$-bearing element. Since the predicates in (393) have no $\varphi$-marked arguments, matrix T is able to agree with an indefinitely far-away

DP. In the case of transitive (and unergative) predicates, on the other hand, T will always find the (*v*P-internal) external argument of its own clause first, and therefore be unable to agree with any arguments in the embedded clause, regardless of that clause's structural size.

(393) a. There **are** [*v*P likely to [*v*P appear to [*v*P be **people** in your garden ]]].

b. [... T[$\varphi$] [*v*P ... [*v*P ...[TP to [*v*P be people[$\varphi$: PL] ...]]]]

(394) a. He consider*(s) [TP people to be loyal].

b. [... T[$\varphi$] [*v*P **he[$\varphi$:SG]** [TP people[$\varphi$: PL] to be loyal].

The alternative that has been proposed to account for the different agreement possibilities in (393)-(394) is that transitive and unergative *v* are phase heads, but unaccusative *v* is not. Then, unaccusative *v*Ps do not trigger spellout and the structure does not become opaque for further syntactic operations like Agree. While such a solution is, of course, possible, it is completely stipulative, and the diverging behavior of unaccusatives falls completely naturally out of a FRM account.

Also, as we have seen in section 7.2.2., unaccusative *v*Ps are equally as "interface independent" as transitive and unergative *v*Ps, so it is not clear that they do not trigger spellout in the first place. Tying up spellout as it is understood in the DM literature with the PIC, and adopting the idea that unaccusative *v*Ps are not phasal, would bring along certain undesirable empirical consequences. For example, if we define cyclic domains in the way that seems necessary to obtain the observed morphophonological effects, namely saying that spellout it triggered upon the merger of the second cyclic head, and if unaccusative *v*P is not a cyclic head, we then predict instances of syntactic and morphophonological operations across finite CP, even in English. For example, we predict agreement on matrix T to be possible with the embedded argument in (395). Since both verbs are unaccusative, treating their *v*s as non-phasal means there is only one cyclic node (C) between matrix T and the argument in the embedded clause. We should similarly get external sandhi effects across CP in such configurations, contrary to fact.

(395) It appear(*s) that the glasses broke.

### 7.4.4 Constraints on the extraction of ergative arguments

The idea of probing the closest DP has a life of its own in the literature on extraction restrictions in syntactically ergative languages. It has been observed that many languages with ergative case systems also observe restrictions on the A'-extraction of ergative subjects; I will call this phenomenon the ERGATIVE EXTRACTION CONSTRAINT(EEC), following Aissen 2017. I illustrate here one particular FRM-based proposal for the EEC in Mayan languages, comparing it to a previous proposal which invoked phase-locality. For other work arguing that the EEC arises due to minimality considerations, see for example Aldridge 2004, 2008, 2012, Erlewine & Lim 2023 on Austronesian, and Branan & Erlewine 2022, 2024 for a general discussion of some of these ideas and extensions beyond syntactically ergative languages.

I discuss here the intervention analysis of the EEC in Mayan offered in Coon, Baier & Levin 2018, building on the work of Levin 2018. As mentioned, many Mayan languages disallow the A'-extraction of transitive (ergative) subjects, exemplified for K'iche' in (396a); (396a) contrasts with (396b-c), which shows well-formed instances of direct object and intransitive subject extraction.[24] I illustrate here with focus-movement; the same restriction applies in relativization and *wh*-question formation.

(396) a. *Are ri ixoq x-∅-u-b'aq ri ch'ajo'n. (Can Pixabaj 2004:58)
FOC DET woman PST-B3SG-A3SG-scrub DET clothes
*intended*: 'THE WOMAN scrubbed the clothes.'

b. Are ri ch'ajo'n x-∅-u-b'aq ri ixoq.
FOC DET clothes PST-B3SG-A3SG-scrub DET woman
'THE CLOTHES, the woman scrubbed.'

c. Are ri ala x-∅-ch'aaw-ik.
FOC DET boy PST-B3SG-speak-S
'THE BOY spoke.'

Not all Mayan languages exhibit the EEC exemplified by (396a); as seen in (397), transitive sub-

[24] K'iche' is a head-marking language. The "A-marker" on the verb tracks the transitive subject (ergative), while the "B-marker" tracks the intransitive subject or the transitive object (absolutive).

jects in Chol undergo A'-extraction freely. There is a strong correlation between the presence of the EEC and the structural position of the absolutive argument (as also noted for other ergative languages in Aldridge 2004 and Legate 2008). Specifically, in Mayan languages that exhibit the EEC, the absolutive argument has been argued to raise to a high position. We can observe that the absolutive (B)-marker in K'iche', a language with the EEC, is found immediately after the Tense/Aspect marker, while in Chol it is found after the verbal root. The linear order of the marker has been argued to track the structural height in which the argument is licensed. Coon, Mateo Pedro & Preminger 2014 (henceforth CMPP 2014) argue the absolutive argument in languages with the EEC must raise to (an outer) spec, *v*P in order to receive absolutive case, while absolutive is assigned in situ (by *v*) in languages without the EEC.[25]

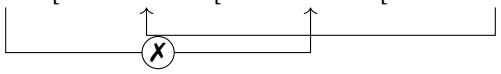
(397) Maxki tyi y-il-ä-yety? (CMPP 2014:193)
who ASP A3SG-see-TV-B2SG
'Who saw you?'

Coon, Baier & Levin 2021 (henceforth CBL 2021) argue that moving the direct object to an outer spec, *v*P in languages like K'iche' creates a configuration in which the direct object is an intervener for any further movement of the transitive subject. Specifically, they offer the generalization in (398); see the schematic representation in (399).

(398) MAYAN EEC GENERALIZATION (Coon, Baier & Levin 2021:271)

When an interpreted DP object structurally intervenes between the subject and the A'-probe on C, the subject is restricted from undergoing A'-extraction.

(399) [CP C ... [*v*P **ABS.ARG** [*v*P ERG.ARG [VP <ABS.ARG >]]]]



As is clear from (399), the account of the EEC is framed in terms of intervention. Following Levin 2018, CBL 2021 proposes that C has a complex probe, which probes simultaneously for [A'] and

[25] In addition to the argument from the linear position of the absolutive marker, CMPP also show that non-finite embedded transitive objects can only be licensed through special morphosyntactic means (e.g., the so-called "crazy antipassive" in Q'anjob'al) in languages with the EEC. This is expected if absolutive in those languages is licensed by T (which is absent in non-finite clauses).

[D] features. Hence, C will always target the first DP it encounters (or the first A'-element, whichever is closer) because all DPs bear a [D] feature. In languages where the configuration of transitive clauses is as in (399), the probe on C will always find the absolutive argument before it finds the ergative ergument. Hence, the movement of the absolutive argument to a high position in some Mayan languages and the specification of the probe on C conspire to derive the Mayan EEC. For a specific implementation of this proposal, see CBL 2021:sec. 3.1; I believe the same result can be achieved by having C in (399) simply probe for [D] as it does in Dinka, but I leave this issue for further research.

The intervention approach offered in CBL 2021 contrasts with previous work on Mayan which attributs the extraction restriction to *v*P phasehood. CMPP 2014 argues that the raising of the absolutive object to an outer spec, *v*P in languages with the EEC creates an issue for further movement of the ergative subject because *v*P is a phase which has only one available specifier ("escape hatch"), which is occupied by the absolutive argument. CMPP have to furthermore assume that there is a parametric difference with respect to the position of the *v*P phase: In languages like English, subjects are generated above the spellout domain of the first phase, whereas in Mayan they are contained within that spellout domain. Essentially, the entire *v*P in Mayan undergoes spellout, and the ergative subject remains trapped in its base position, which accounts for the EEC.

The two accounts make distinct predictions for the extraction of low-adjuncts; as I now demonstrate, the empirical landscape favors the intervention account. The phase account predicts that what I refer to as low-adjuncts—adjuncts generated in the first phase, typically those with instrument, locative, and comitative meanings—to be trapped inside *v*P to the same extent as the ergative subject. All material must vacate a phase through an escape hatch, and this position is filled by the absolutive argument. Hence, the extraction of low adjuncts should be ungrammatical. On the intervention account, this is not necessarily so. Say the probe on C can be specified for features other than [D], for example [P] or [Adv]. There is nothing conceptually problematic about this possibility. If this is possible in Mayan, and if the EEC is caused by the intervention of the absolutive DP argument, then low-adjunct PPs and AdvP should be able to freely undergo A'-movement.

As shown in (400) from K'iche', extraction of low-adjunct PPs and AdvPs is possible. That these are *v*P-internal adjuncts is confirmed by the presence of the particle *wi*; this particle does not appear in the extraction of high-adjuncts (Can Pixabaj 2015, Mendes & Ranero 2021); see also fn. 26.

(400) a. **Jawi** x-∅-u-paqchi-j wi ub'i le ak'aal? (Can Pixabaj 2015: 162)
where PST-B3SG-A3SG-push-ACT PRT DIR DET boy
'Where did s/he push the boy?'

b. **P-ulew** x-oj-ki-t'uyub'a' wi. (my fieldnotes)
P-foor PST-BIPL-A3PL-seat PRT
'ON THE FLOOR they sat us down.'

Hence, low-adjunct PPs and AdvPs can move out of the *v*P, unlike ergative subjects. This pattern is expected if the intervention approach to the EEC is correct; if the phase approach were correct, all low-adjuncts should be prohibited from vacating the *v*P to the same extent as the ergative subject.[26]

### 7.4.5 Hyper-raising patterns

Hyper-raising–raising out of finite CPs (Ura 1994)–has been observed to be possible in many languages, including Turkish (Moore 1998), Brazilian Portuguese (Nunes 2008, 2016, 2019 , Kobayashi 2020, Dias 2022), Jordanian Arabic (Farghal 2020), Mongolian (Fong 2019), Uyghur (Asarina 2011), P'urhepecha (Zyman 2017, 2018), Cantonese and Vietnamese (Lee & Yip 2024), and the Bantu languages (Zeller 2006, Carstens & Diercks 2009, Halpert 2019, Halpert & Zeijlsta 2024). I illustrate the pattern with an example from Brazilian Portuguese in (401). This kind of raising is impossible in English, cf. (402). There is good evidence that hyper-raising involves (A-)movement across a CP boundary, which should be banned in a phase-based approach to locality. On the other hand, there is evidence that hyper-raising obeys FRM: raising is only possible if there are no relevant interveners for establishing a

[26] As seen in all the examples with low-adjunct extraction, a particle, *wi*, appears in a low position; this particle is only found with low-adjuncts and only in A'-movement contexts. I follow Mendes & Ranero 2021 who argue that this particle is the overt realization of a copy of the adjunct in its base position (an instance of chain reduction via substitution). CBL 2021 suggest that the particle *wi* signals the movement of the adjunct to a high *v*P position in order to circumvent intervention by the DP arguments. I think this cannot be right. It is not clear to me why the adjunct would be able to move to a high *v*P position given that the the inability of the ergative subject to leave the *v*P was attributed to the *v*P having only one available specifier, which is filled by the absolutive argument.

probe-goal relationship between matrix T (the raising probe) and the element that raises.

(401) **Os meninos** parecem que <**os meninos**> comeram maçã. (Dias 2022:59)
the boys seem.3PL that ate.3PL apple
'It seems that the boys have eaten apples.'

(402) a. John seems [<John> to be smart].

b. *John seems [(that) <John> is smart].

While hyper-raising exhibits various idiosyncratic properties in different languages, I will present here one instance of this construction in the Bantu language Zulu, discussed in Halpert 2019 and Halpert & Zeijlstra 2024, which presents particularly striking evidence that a FRM analysis is to be preferred over a phase-based one. I do note that more detailed work is needed to see if the FRM treatment can generalize to all instances of hyper-raising; see, however, Halpert & Zeijlstra 2024 for a recent detailed study that argues for this conclusion.

Zulu shows are raising profile that is the exact opposite of English: raising is allowed out of finite CPs but disallowed out of TPs, see (403) from Halpert 2019:124. If, as argued for the English case, the locality of raising is regulated by Phase theory, this pattern is completely unexpected.

(403) a. uZinhle u-bonakala [CP ukuthi <uZinhle> u-zo-xova ujeqe].
AUG.1Zinhle 1SM-seem that 1SM-FUT-make AUG.1bread

b. *uZinhle u-bonakala [TP <uZinhle> uku-(zo-)xova ujeqe].
AUG.1Zinhle 1SM-seem INF-(FUT-)make AUG.1bread
'It seems that Zinhle will make steamed bread.'

Halpert argues that, in Zulu, T has both a [$\varphi$] (agreement) probe and an [EPP] probe, requiring a filled specifier. In addition to DPs, non-finite TPs can also occupy the spec, TP position, satisfying T's EPP requirement, see (404a) from Halpert 2019:144. On the other hand, CPs cannot do so, see (404b) from Halpert 2018:141. DPs, TPs, and CPs are all viable [$\varphi$] goals.

(404) a. [TP uku-(zo)-fika k-ubusika] ku-ya-bonakala.
AUG.17-(FUT)-arrive 17ASSOC-AUG.14winter 17SM-YA-seem
'Winter's arrival is evident.'/'We can tell that winter is coming.'

b. *[CP ukuthi w-a-thatha umhlala phansi] ku-ya-ngi-mangaza.
that 1SM-PST-take AUG.1sit down 17SM-YA-ISG.OM-surprise
*intended:* 'That he retired surprises me.'

Based on these (independently observable) assumptions, Halpert argues that the raising pattern in (403) arises as follows: Because C can satisfy only T's $\varphi$-probe but not its EPP-probe, T will continue to probe and find the closest DP in the embedded clause which can satisfy both. This produces (403a). On the other hand, both of T's probes can be satisfied by embedded TP, so T cannot find the embedded DP in this configuration; raising is not allowed (403b).

Before moving on, let me mention one more pattern observed in hyper-raising contexts in Brazilian Portuguese (BP). As observed in Kobayashi 2020, hyper-raising across CP in BP does not bleed *wh*-movement (405). Now, BP is a language in which only one *wh*-phrase moves to CP in multiple *wh*-questions, as well as a language which obeys *Wh*-Islands, both indications that its C has only one specifier. On a phase-based approach to locality, the grammaticality of (405) is very surprising. Specifically, if C is a phase, the subject *elas* 'they' must move through spec, CP in order to raise into the matrix clause. Then, the *wh*-phrase *quais livros* 'which books' should not be able to vacate the embedded CP, as it is competing to occupy the same specifier already filled by (a copy of the) the embedded subject. On a FRM approach to locality, (405) is derived straightforwardly. There are no phases, and matrix T is able to agree with and attract the embedded subject to its specifier position. Matrix C, on the other hand, is looking for *wh*-features. It is able to find the only phrase that contains these features, namely the object of the embedded clause. No intervention effect of any kind arises, and the derivation converges.

(405) **Quais livros elas** parec-em [que <**elas**> ler-am <**quais libros**>]? (Kobayashi, 2020:54)
which books they seem-3PL that read-PL
'Which books do they seems to have read?'

This section has shown a number of case studies for which FRM can be shown to make the correct predictions, in two types of cases: cases where the locality conditions are more stringent than those predicted on a Phase theory approach, and those where they seem laxer. The data clearly indicates

that syntactic locality arises due to intervention.[27]

## 7.5 Syntax-internal locality and Transfer to the interfaces

Recall that Phase theory was initially appealing because it promised to be a theory of syntactic locality and a theory of spellout to the interfaces. However, the previous two chapters have served to show that there are both conceptual and empirical reasons that seriously challenge the possibility of unifying syntactic and morphophonological locality effects under Phase theory.

In this chapter, I argued that Phase theory should be dispensed with as the theory of syntactic locality, FRM being the superior alternative. However, we have seen in the previous chapter that 'chunking' effects at the form interface come about as a result of spellout at specific points of the syntactic derivation (e.g, at *a* but not at *Deg*). In other words, whereas the mechanism that explains such facts as the locality of movement and agreement is not the same mechanism that regulates the spellout of structure to the interfaces, spellout points are still defined in syntactic terms. I should stress that I am not willing to claim at this point that categorial heads universally trigger spellout, and non-categorial heads do not. If this turns out to be true, it will be interesting to ask why this might be so. The fact is, however, that research is first needed on a vastly larger sample of languages, which will (dis)confirm this hypothesis. See chapter 8 for more discussion.

Still, I showed in chapter 6 that categorial heads in BCS define the points of spellout to the interfaces, while non-categorial heads do not. These labels are clearly syntactic labels, so the mechanism that regulates spellout needs to be stated over syntactic heads. Borrowing heavily from the ideas presented in

[27] There are many other effects that arguably arise due to FRM, and I cannot list them all here (see e.g., Rackowski & Richards 2005, Thivierge 2021, Ershova 2023, who all deduce certain kinds of "phasehood effects" from various kinds of intervention; Brattico 2014, who argues that certain instances of long-distance Case assignment obey Minimality, but violate phase-locality). One further family of potential FRM effects I don't discuss in any detail has to with binding. For example, it is well known that anaphors contained within complex DPs can be bound from the outside (*Mary$_i$ liked [DP these pictures of herself$_i$]*), but this is no longer possible if the complex DP contains a $\varphi$-feature matching possessor, which is itself a potential binder for the anaphor (**Mary$_i$ liked [DP Josie's pictures of herself$_i$]*). These effects were traditionally captured by complicating the definition of the "binding domain", so as to make DP possessors count as "subjects". An alternative is to say that like other syntactic operations, anaphor-binding is subject to FRM (cf. also *The boys$_k$ expect the girls$_j$ to like each other$_{j/*k}$*). Due to FRM, anaphors must be bound by the closest potential binder. See Hornstein 2001, 2009, 2024 for a particular implementation of the idea that Binding Theory is reducible to Minimality.

Embick 2010, we may then formulate the following condition, call it Transfer, to regulate the spellout of syntactic structure to the interfaces:

(406) Transfer:

In a structure [*y*P *y* [ZP Z [WP W [*x*P *x* [ROOT ]]]]], where *x* and *y* are cyclic heads, the domain of *y* is sent to the interfaces; this operation has no effect on the syntactic computation.

It is crucial that Transfer (unlike the PIC) does not impose any syntax-internal opacity. It is trivial to show that this kind of opacity is undesirable when we are dealing with the (often considerably smaller) locality domains relevant to the form interface. For example, an undesirable result of tying in spellout and opacity would be that movement of deadjectival nouns out of adjectival phrases would be predicted to be impossible, contrary to fact (407).

(407) Čije zainteresov-an-ost-i je Marija bila vred-n-a <čije zainteresovano.>?
whose interest-ADJ-N-GEN.SG be.3SG Mary was worthy-ADJ-FEM.SG
'Whose interest was Mary worthy of?'

Per (406), the adjectival portion of the denominal adjective is spelled out upon the merger of the nominalizer. The entire deadjectival noun is spelled out when the adjectivizer of *vredna* 'worthy' enters the picture. As we have seen extensively in chapter 6, BCS adjectives do not have an available specifier. If the spellout caused by the merger of the adjectivizer induced opacity, the deadjectival nominal would be frozen in place. This is clearly not a desirable result since the movement is, in fact, possible.[28]

Furthermore, as noted by Boeckx & Grohmann 2007, the computational system must be able to retrieve spelled-out material to provide a complete, coherent surface string. For example, correct pronominal binding depends on being able to 'see' multiple spelled out chunks. The determination of intonational patterns, which depend heavily on syntactic structure, possibly requires access to full clauses. All of this is problematic on the traditional Phase theory view that the computational system

[28]This kind of data is also problematic for any attempt to unify morphophonological locality effects found in the DM literature with a cyclic-linearization approach to Phase theory (Fox & Pesetsky 2005), since on this approach spellout is supposed to trigger linearization, which is argued to be inalterable at later stages of the derivation.

'forgets' about the spelled-out portions of structure. It is not problematic on the view espoused here, where spellout has no syntax-internal effects.

We may also want to ask about the motivation for the existence of these two locality principles, namely FRM and Transfer. There has been a move in psycholinguistic and biolinguistic circles to attempt to motivate FRM as a conventionalized property of the grammar that is functionally grounded as a response to memory and the exigencies of a cue-based retrieval parser constrained by similarity-based interference (see e.g., Ortega-Santos 2011) The literature on similarity-based interference in psycholinguistic experiments is vast, including Gordon, Hendrick & Johnson 2011, Van Dyke & Lewis 2003, Lewis & Vasishth 2005; for a recent review and meta-analysis see Jäger, Engelmann & Vasishth 2017. Similarity-based interference is also found extensively for memory in other cognitive domains, including motor (Adams 1987), visual (Chandler 1991), mental arithmetic (Campbell 1991). It may then be possible to situate FRM firmly in the bucket of 'third factors' in the sense of Chomsky 2005. Of course, while the motivation for FRM maybe in the realm of third factors, FRM is still firmly a linguistic principle, one that reference linguistic notions such as structural hierarchy, c-command, and syntactic features.[29]

Now, when FRM is understood as the only syntax-internal locality constraint (which does not and cannot have any effects at the interface), an answer to the question why something like Transfer should exist suggests itself: because of the need to have syntax communicate with other linguistic modules, namely the form module and the meaning module. Since syntax and its locality principles are self-contained, there is a need to decide at which point of the derivation the syntax should communicate with other linguistic modules. Something like Transfer is then logically necessary in order to establish the relation between syntax and form. In his most recent writings, Chomsky seems to suggest that the interfaces are dispensable, as he writes that "there is no need to postulate the interface levels; access can, in principle, take place at any stage of the computation." (Chomsky 2021:7) While this could,

[29]Rizzi (2002: 227) argues that similar FRM exists in phonology, quoting the following passage from Halle 1995:22: "A Coronal nasal assimilates the Coronal features from a retroflex consonant that precedes it... The nasal can be arbitrarily far away from the retroflex, provided that no Coronal consonant intervenes."

in principle, have been true (i.e., don't specify when Transfer takes place, do it whenever) we have pretty robust evidence that Transfer happens at certain points of the derivation and not at others, and that this process is entirely regular (and grammaticalized).

## 7.6 Conclusion

In this chapter, I argued that Phase theory should be abandoned as a theory of syntax-internal locality. I first showed that many of the commonly used phasehood diagnostics have the wrong modal force, showing that intermediate movement to some position is *possible*, rather than *obligatory*. This is the case for reconstruction (for binding and scope), QR patterns (including QR in ACD), parasitic gap licensing, quantifier float, optional agreement marking in long-distance movement contexts, optional inversion, and the spellout of intermediate *wh*-copies. The fact that these phenomena are either (i) optional or (ii) their obligatoriness feasibly stems from factors other than locality, makes them inadequate evidence for Phase theory, or indeed for any locality theory currently on the market, including FRM-based locality.

I then cast a critical lens at an array of other so-called phasehood diagnostics from the literature and concluded that they either do not stand up to scrutiny or are equally well or better accounted for via FRM, which I showed is independently needed in the grammars of the languages in question. The diagnostics included Merge-over-Move patterns, independence at the interfaces, V2 satisfaction by intermediate copies of movement, obligatory extraction marking patterns, lexical choice effects at the "phase edge", obligatory inversion, and patterns of Long Distance Agreement. Given that FRM is independently needed, I argued that Phase-theoretic explanations, even when possible, should be abandoned on economy grounds. As I noted in the chapter, this work has not been exhaustive as I have only been able to look at a couple of case studies in each of the categories. As some of the diagnostics have been applied to data in many different languages, further research is necessary to determine if my conclusions can be generalized across-the-board.

I furthermore discussed an array of phenomena for which FRM-based accounts are standardly assumed and/or clearly superior to the existing phase-based alternatives. These include a number of locality effects in the A-domain (the EPP, object-shift, and subject raising, including hyper-raising) and the A'-domain (traditional superiority effects and constraints on the extraction of ergative arguments), as well as in the agreement domain (specifically, agreement across unaccusative predicates). A locality theory with the ambitions to dispense with FRM in favor of Phase theory would need to provide alternative accounts for these observed effects.

Finally, since FRM is a syntax-internal locality constraint, I argued that some kind of an additional transfer mechanism is logically necessary in order for the syntax to communicate with other grammatical modules. Given the evidence from chapter 6 that the points of spellout are defined over syntactic heads (e.g., *a* but not *Deg* in BCS), I proposed a mechanism called *Transfer*, which achieves the desired empirical result at the interfaces and crucially has no effect syntax-internally. As it stands, *Transfer* is stipulative and the motivation for the way it is implemented is purely empirical. It remains to be seen whether a more principled account can be offered in its place; see chapter 8 for some discussion.

By way of conclusion, let me spell out what a possible strong piece of evidence for phases would look like, which could not be handled in a FRM framework. One would first want to find a case of opacity, expressed as either (i) the complete inability of syntactic operations to penetrate a certain domain, or (ii) demonstrably obligatory successive-cyclic movement. This opacity should then be shown to apply equally for a variety of (ideally, all) syntactic dependencies which cross the hypothesized phase head. This should be done in two ways. First, it should be shown that the opacity remains regardless of the features that trigger the relevant syntactic operations, where the role of those features should be independently shown. Second, the opacity should persist regardless of the presence or absence of potential interveners between the probe and the ultimate goal (excluding the hypothesized phase head).

The predictions are quite distinct in a FRM-based locality system. For FRM, intervention effects (and operations that circumvent them, like successive-cyclic movement) can in principle arise for any syntactic head, provided that two syntactic heads in the same derivation share features that a probe is

looking for. We then expect (and find) different intervention effects for different dependencies within a single language. On the face of it, FRM also imposes sometimes more permissive and sometimes stricter locality than predicted on a Phase theory approach, which is empirically desirable, as we have seen throughout this chapter.

# Chapter 8: Conclusions and future directions

The goal of this concluding chapter is twofold. First, I provide a summary of the dissertation's main findings and recurring themes. I also discuss some open questions and promising future directions of the work, both with respect to the specific phenomena dealt with in the preceding chapters, as well as to the plausible future targets of the broader research program.

## 8.1 Main findings and recurring themes

At the center of this dissertation were the goals of (i) simplifying the model of the grammar without sacrificing descriptive adequacy and (ii) where possible, searching for tenable functional grounding for some of its properties. These goals were motivated by general theoretical parsimony, learning considerations, and the issue of connecting the concepts of theoretical linguistics with questions about language evolution and its biological instantiation. With regard the the latter, I argued that the rich descriptions of linguistic phenomena arrived at during the Government & Binding era of Generative Grammar need rethinking if progress is to be made in studying (the human capacity for) language from a biological perspective. This is not to say, of course, that the faculty of language does not have domain-specific properties, or that the phenomena discussed here are fully explained by language-external mechanisms, but simply that the motivation for the existence of some grammatical primitives and principles may lie outside the language system itself.

In the first part of the dissertation, I argued that lexical categories (LCs) *noun*, *verb*, and *adjective* are purely formal, abstract categories which have a distributional role in the syntaxes of individual

languages, but which do not have a one-to-one mapping to any interpretive property. I furthermore argued against proposals that attribute universal syntactic or semantic properties to the specific LCs. In addition to discussing relevant data from a variety of languages, I provided two detailed case studies on mixed categories, specifically passive and active participles. I showed that all participles in the languages under discussion are deverbal adjectives, in every syntactic position they appear in and regardless of their interpretation. While participles may denote (predicates of) properties or eventualities, I argued that these different interpretations are not cross-linguistically associated with more or less verbal or adjectival structure. This reinforced my conclusion that a direct one-to-one correspondence between an item's LC and its interpretation does not exist. If correct, this proposal has significant consequences for our understanding of Universal Grammar. If there are no universal syntactic or semantic properties we can attribute to the LCs, then it becomes superfluous to assume that the individual LCs noun, verb, and adjective are part of the initial state of the learner. I proposed that the cross-linguistic tendencies we observe around LCs may stem from the way non-linguistic knowledge is organized in the mind/brain.

In the second part of the dissertation, we saw that there are in essence three competing locality theories currently in circulation within the field: Featural Relativized Minimality (FRM), Phase theory as it is understood in the relatively recent syntax literature, and Phase theory as it is understood in the DM literature. Despite recent attempts to devise a single, unified Phase theory which is responsible for both syntax-internal locality and interface locality, I argued that the unification is unfeasible because upon closer inspection their empirical desiderata are clearly different. Nonetheless, the original inventory of locality principles can be reduced if we assume that (i) syntax-internal locality is regulated by FRM, and (ii) interface locality is regulated by Transfer, a modified version of Phase theory which has no syntax-internal effects. I argued that this division of labor not only achieves the right empirical cut, but it also provides some hints as to why two distinct locality principles may be necessary in the grammar. Previously, the existence of both Phase theory and FRM in the syntax seemed inelegant, especially since the two often produce overlapping restrictions. However, if syntactic locality is constrained only via FRM, which regulates probe-goal relations and can by definition have no interface effects, the exis-

tence of a principle like Transfer becomes expected. There is still a need for the syntax to communicate with other linguistic modules, hence a separate Transfer mechanism becomes logically necessary.[1]

One of the main recurring themes of the dissertation was a strong endorsement of the Autonomy of Syntax thesis (Chomsky 1957). The primitives and principles of syntax are formulated in a way that does not directly reference other linguistic or non-linguistic cognitive modules (semantics, phonology, pragmatics, memory, attention, etc.). While some of the motivation for specific syntactic phenomena may lie in other components of the grammar or in aspects of non-linguistic cognition, the case studies presented in this dissertation provide strong evidence for the claim that the internal workings of syntax must be blind to outside interference. We saw this principle in action in both parts of the dissertation. The (syntactic) generalizations one can make over the distributions of LCs in a language are orthogonal to the generalizations one can make over the meanings of different types of predicates, for example. Moreover, the generalizations about syntactic locality are orthogonal to the generalizations about interface locality. Note that neither of these facts are necessary in order for the Autonomy of Syntax thesis to be maintained. For example, LCs could have had distributional groupings in the syntax and still have been mapped consistently to some meaning component at the semantics interface. Phase theory could have regulated syntactic locality and had direct consequences for locality at the interfaces. The fact of the matter is that we do not live in such a world, and the world we live in makes the case for the Autonomy of Syntax quite forcefully.

Another recurring theme has been the need to assume abstract morphemes in the syntax, advocated for in the more recent DM literature. The content of the Abstract Morpheme Hypothesis is as follows: Morphemes, which are the atoms of syntax, are fully abstract bundles of formal features. They receive their form and meaning at the interfaces, after (some) syntactic computation has taken place. The syntax-meaning dissociation was quite apparent in the case of participles, where it was shown that that participles, despite always being deverbal adjectives, may denote either (predicates of) properties or

[1]The focus in chapter 6 was on the form interface and I did not discuss the issue of whether Transfer applies to both the form and the meaning interface simultaneously. While this is the null hypothesis, there has been work suggesting that spellout is non-simultaneous (see Marušič 2005 *et seq.*).

of eventualities. The participles' meaning is determined in (syntactic) context, according to language-specific rules of interpretation. The abstractness of morphemes with respect to form was emphasized in the second part of the dissertation. The Transfer principle, which regulates morpho(phono)logical locality and has a role in determining phonological form, was shown to operate on complex syntactic structures and make reference to syntactic distinctions (specifically, the distinction between different kinds of syntactic heads: categorizers like *a* and non-categorizers like DEG). The form of a morpheme can only be determined by referencing its syntactic context.

Related to the previous two points, we have seen especially in the first half of the dissertation that meaning distinctions do not always line up neatly with distinctions in syntactic (especially, categorial) structure, as diagnosed by morphological and syntactic evidence. This issue often comes up in the work on mixed categories. The question is simple: What counts as evidence for syntactic structure? In earlier work on agent nominals, it was argued that eventive (episodic) interpretations, diagnosed by compatibility with adverbs such as *frequent*, went hand in hand with complement structure (408a). Nominals like (408a) were therefore thought to warrant a deverbal analysis (see e.g., Alexiadou 2001). This analysis was convincing since it relied on both semantic and syntactic evidence for said verbal structure. However, it is easy to come up with examples where episodic interpretations do not correspond to obligatory complement structure (408b); contrast this with *She frequently visited *(Marseille).* Moreover, the noun in (408c) does not correspond to an argument of a verb, yet it still has an obligatory complement.

(408) a. a frequent consumer *(of tobacco)

b. a frequent visitor

c. a frequent subject (*of Monet's paintings)

Faced with these issues, work on these topics increasingly favored semantic evidence in diagnosing syntactic structure. For example, Alexiadou & Schäfer (2010) propose an articulated verbal structure for both episodic (409a) and dispositional (409b) agent nominals. The former type are argued to have

event entailments, whereas the latter do not; one can be called a firefighter because they have been educated for that job, even if they have never engaged in the activity of fighting fires. Alexiadou & Schäfer propose that both types of nouns contain an articulated verbal structure, including *v*, Voice, and Asp; this structure is meant to account for the fact that *-er* nominals denote the (external) argument of the corresponding verbs. This is despite the fact that dispositional *-er* nominals do not admit any kind of event modification, nor do they ever have obligatory complement structure.

(409) a. a consumer of tobacco

b. a firefighter

The evidence offered for the extra syntactic structure in agent nominals becomes purely semantic and relies on entailment patterns. Using entailments to diagnose syntactic structure is a slippery slope, however. To give a couple of specific examples, it has sometimes been argued that the ambiguity of (410a), in particular the reading which allows the dancing to be characterized as beautiful, suggests that *dancer* is derived from the corresponding verb *dance*. Note, however, that the same ambiguity obtains in (410b-c), yet those nouns have no corresponding verbs to be derived from. A similar point pertains to (411): one can be *a just ruler* by virtue of being just in their ruling capacity (411a), where *just* then seemingly modifies the event of ruling. However, if this is taken as evidence that *ruler* contains verbal structure, then the same must be said for *king* in (411b): one can be just in their capacity as king without necessarily being a just person. While I am not saying that such an analysis is obviously wrong, I am simply pointing out its logical endpoint, one that I believe is less convincing for most.

(410) a. a beautiful dancer

b. a beautiful violinist

c. an elegant midfielder

(411) a. a just ruler

b. a just king

All of this is, of course, part of a broader point: entailments do not necessarily reveal the presence of hidden structure. Take (412) and the various entailments it licenses. For example, the truth of the statement that the child is blond can only be evaluated in virtue of the child having hair. However, this does not compel us to claim that (412) contains a hidden 'hair' argument that projects syntactic structure. A similar point can be made for the modifier *illegitimate* in (412). The agent nominal cases should be no different in this respect, as far as I can see.

(412) an illegitimate blond child

The takeaway from this discussion is the following: In the absence of clear syntactic evidence, we should consider more carefully what kinds of semantic arguments are valid in diagnosing the presence of syntactic structure. For example, while scope facts are a reliable semantic cue for the presence of syntactic asymmetries, entailment patterns are less convincingly so (see Williams 2015 for an extended argument to this effect).

## 8.2 Remaining questions and future directions

There are a number of questions that this dissertation raises but does not provide the answers to, and which are left to further research. The first and second half of the dissertation raise quite distinct issues, so I will divide my comments accordingly. Starting with the first half, recall the goal to reduce the inventory of LCs are much as possible. I argued that *participle* is not an independent category, but rather a derived category comprised of verbal and adjectival structure. This makes participles a mixed category, like gerunds and other types of deverbal nominals. Another group of elements that seem like a good candidate for a mixed category analysis are so-called *converbs*, which I illustrate for BCS in (413). These elements are clearly deverbal: they inherit the complement structure of their corresponding verbs and contain morphemes that are seen in the derivation of verbs. Like participles, they may denote eventualities (413a) or properties (413b). The morphological make-up of converbs includes verbal theme vowels and, somewhat mysteriously, what looks like the 3PL morpheme as it is found on present tense

verbs; see Klajn 2005.[2] The reason this is mysterious is two-fold: (i) mixed categories usually only contain the agreement markers of the "outer" categorizer and are the outermost suffix on BCS verbs, and (ii) the seemingly 3PL-marking persists regardless of the person/number specification of the main sentential predicate.

(413) a. **Prič-a-ju-ć-i** sa Marijom, shvatila sam da me voli.
talk-V-3PL-ADV-M.SG with Mary, realized.F.SG cop.1SG that me loves
'Talking to Mary, I (female) realized that she loves me.'

b. **let-∅-e-ć-a** veverica
fly-V-3PL-ADV-F.SG squirel.F.SG
'a flying squirrel'

Though traditional grammars of BCS refer to these elements as *glagolski prilog* 'deverbal adverb', their ultimate category is questionable. First notice that converbs appear in adverbial (413a) and adjectival (413b) positions. The initial morphological evidence is not too helpful, since the suffix *-ć* only appears on converbs and their concord/agreement properties are like those of adjectives and adverbs, based on their syntactic position. As seen in (414a-b), BCS adjectives assume the gender/number marking of the nouns they combine with. Adverbs, on the other hand, do not. As (414c) shows, the adverb based on the same root carries (the equivalent) of NEUT.SG marking, even though the subject is feminine. A similar pattern is observed in (413) above: the element in the attributive position assumes the $\varphi$-features of the noun it combines with, while the element in the adverbial position is marked M.SG despite the subject of the main predicate being feminine.

(414) a. Moja devojka je dobr-a.
my girlfriend.F.SG COP.3SG good-F.SG
'My girlfriend is good.'

[2]Despite 3PL present tense agreement morphology being quite irregular and changing depending on verb class, converbs consistently contain (what looks like) this exact morpheme, as seen in the paradigms below for the items in (413).

(i) a.

| | SG | PL |
|---|---|---|
| 1ST | priča-m | priča-mo |
| 2ND | priča-š | priča-te |
| 3RD | priča-∅ | priča-**ju** |

b.

| | SG | PL |
|---|---|---|
| 1ST | leti-m | leti-mo |
| 2ND | leti-š | leti-te |
| 3RD | leti-∅ | let-**e** |

b. Moje dete je dobr-o.
my child.NEUT.SG COP.3SG good-F.SG
'My child is good.'

c. Uradila je to dobr-o
did.F.SG COP.3SG that good-NEUT.SG
'She did that well.'

It is clear that further research is necessary to establish the exact syntactic structure of converbs and to understand the range of cross-linguistic variation with these elements. Nonetheless, a mixed category analysis seems like a promising avenue to pursue, and one that would contribute to the overall goal of reducing the hypothesized inventory of grammatical primitives.

Moving on, recall that I argued categorizers (*v, n, a*) do not have one-to-one mappings at the meaning interface. It is standard in the DM literature to assume the same for roots. On the other hand, categories like Tense and Aspect are commonly assumed to have straightforward interpretations: we take Tense to be informative as to the temporal interpretation of a predicate with respect to utterance time (or a relative reference point, on some theories), and Aspect to be informative as to the internal temporal organization of an eventuality-denoting predicate. Are those one-to-one mappings coincidental and simply arise as a subset of the (in principle possible) many-to-many mappings? If so, why do we observe the same tendencies in language after language? In other words, are there languages in which categorizers have one-to-one mappings to meaning, but Tense and Aspect do not? If not, why not? Note that we usually assume Tense to carry more specific features like [past] and many languages provide indirect evidence for such features through $\varphi$-agreement (e.g., *-s*/∅ vs. *-ed* in English). Categorizers are less often assumed to carry extra features, though such proposals do exist; for example, Folli & Harley (2005) propose different "flavors" of *v*: $v_{\text{DO}}$, $v_{\text{CAUSE}}$, and $v_{\text{BECOME}}$ (see also Jackendoff 1992). These distinctions still seem insufficient to explain the behavior of participles, where an element that contains verbal structure may yet denote a property. Possible avenues to pursue here include approaches to allosemy (contextually-conditioned special meanings) which appeal to distance from the root as a key mechanism in regulating the availability of special meanings (e.g., Marantz 2013, Wood 2023).

My conclusions in the first part of the dissertation also raise a learning question: If there are no syntactic or semantic universals in the realm of LCs, suggesting that there is no LC-related content in UG, then how exactly does learning in this domain proceed?[3] If nothing about the specific LCs is innate, then adult competence of LCs can be achieved through combining whatever (partly innate) knowledge enables the child to individuate morphemes from the speech stream and a bias to categorize those morphemes based on their distribution. There is some recent modelling work which suggests distributional evidence may be sufficient in learning syntactic categories, without the need to assume LC-specific inductive biases (see especially Liang, Marsala & Yang 2022). There is also research suggesting that children's input provides robust distributional cues for category learning (Mintz, Newport & Bever 2002), as well as evidence that very young children are able to use these distributional cues to form syntactic categories in experimental settings (Gerken, Wilson & Lewis 2005). This kind of work, especially work involving computational modeling of LC learning, is still in its infancy. It remains to be seen whether it leaves any aspect of LC knowledge unaccounted for. As I made clear throughout the dissertation, the view I have been pursuing states that such knowledge does not exist; the onus is on those who wish to claim otherwise. If it can be shown that there are aspects of LC knowledge that models cannot learn based on morpheme segmentation and distributional patterns in the input, that can provide clues as to what further inductive biases need to be assumed to guide the learning process.

A question that is relevant to the dissertation as a whole and has potentially far-reaching consequences is whether combining items in the syntax necessarily results in phrasal structure, as traditionally assumed. There is an interesting new line of work that argues some complex syntactic structures are built small, as complex heads (see Embick 2023, Paparounas 2023, Wood 2023). This work argues that there are both syntax-internal and interface effects supporting the small structure analysis. First, these small structures do not combine with any phrasal material in the syntax, as that would necessarily lead to phrasal projection. Hence, the material interior to these complex heads cannot combine with any

[3]Note that assuming innate knowledge about LCs does not solve the learning problem, it simply raises a different (heretofore unresolved) one, the so-called *linking problem*. The linking problem for LCs can be formulated as follows: Assuming innate LCs, how does the child link them up to the distributional patterns they observe in the input?

complements or adjuncts (with some caveats, see the cited work). Furthermore, complex heads are argued to not undergo phasal spellout in the same way as phrases. The data in the above-mentioned work involves deverbal structures, and the evidence for small structures comes from phenomena such as argument introduction, modification asymmetries, and the existence of root-specific interpretations across intervening categorizers, among others. While I argued, based on syntactic evidence, that resultative passive participles in BCS involve phrasal verbal structure, it has been recently claimed that resultative passive participles are small in some other languages (see Embick 2023 for English and Paparounas 2023 for Greek). The morphophonological and semantic evidence in chapter 6 of the dissertation suggests that *a* in BCS deadjectival nominals projects a phrase which is spelled out, namely *a*P. Specifically, root-conditioned allomorphy and morphological tone assignment are impossible over an intervening *a*, and the meanings of the resulting nouns are derived compositionally from the meanings of the underlying adjectives. On the other hand, Wood (2023) argues that some Icelandic deverbal nominals are small, allowing *n* to influence the meaning of the root across *v*. If it is true that some complex structures can be built as complex heads, it becomes important to understand what regulates the availability of such small structures. Cross-linguistic work on resultative passive participles in a broader sample of languages is likely to produce important results in answering this question.

Another issue raised in the latter half of the dissertation and one which certainly merits further research is the following: It seems that the categorizing heads (*v*, *n*, *a*) play a crucial role in determining when syntactic structure is transferred to the form interface. This was shown to be the case in the BCS case study from chapter 6, but also in other work that has dealt with the morphophonological effects of spellout (see in particular Marvin 2002, Embick 2010, Ingason & Sigurðsson 2015). In BCS, I showed that *a* serves as a locality boundary for morphophonological processes (allomorphy and morphological tone assignment), but non-categorial heads like Deg and Neg do not. Of course, more work needs to be done on a broader sample of languages, in order to understand if these results are broadly generalizable. All of the work on this topic thus far has focused on various Indo-European languages; work beyond this language family is necessary to understand whether the reported pattern represents only

one of the possible options and is in essence a historical accident, or whether it speaks to a fundamental principle of the grammar. If the results generalize, the next question we may want to ask is why the categorizers, and not any other syntactic head, should have this role. I have little to say about this issue at this point, except that giving categorizers this role may be efficient inasmuch as every extended projection will have *at least* one.[4] Still, a system that specifies only categorizers to trigger spellout is not the simplest conceivable system for the syntax to communicate with other linguistic modules. Instead, the simplest system would transfer syntactic structure to the interfaces at every Merge, as envisioned, for example, in Epstein & Seely 2006 for Minimalism or in the Nanosyntax framework as a whole (see Baunaz & Lander 2018 for an overview). On the face of it, the empirical picture that emerged in chapter 6 of this dissertation poses a problem for such conceptions of spellout (see also Newell & Noonan 2018 for recent discussion). It remains to be seen whether the data at hand could be reanalyzed in a spellout-at-every-Merge system and what kind of consequences this would have for the complexity of the computations and/or representations that result in such a system. While Epstein & Seely do not develop this part of their proposal in detail, work in the Nanosyntactic framework has implemented it by assuming constant rewriting of previous output. Even if such rewriting is allowed, it is not clear that the framework as it stands can have an explanatory account for the special role of categorizers. A potential work-around would involve an assumption that the representations of the categorial heads are somehow special. Are the categorizing heads more complex than other syntactic elements and could this be the driving force behind the apparent locality effects they induce? The consequences of this view have not yet been worked out.

Finally, I should emphasize the exploratory nature of the work undertaken in chapter 7. For example, I showed that certain patterns of extraction marking can be fruitfully analyzed in terms of FRM-based locality, without invoking Phase theory. As I noted there, there is a significant number of other languages where the extraction marking pattern in long-distance dependencies has been re-

[4] The same may or may not be true for roots. It has been argued that certain 'light' members of each category may be the spellout of a categorial head that does not select a root (see e.g., Marantz 2001 for such an approach to English *be*).

ported as obligatory: Chamorro (Chung 1982), Defaka (Bennett, Akinlabi & Connell 2012), Kinande, (Schneider-Zioga 2009), Kîîtharaka (Muriungi 2005, Abels & Muriungi 2008), Seereer (Baier 2014), and Selayarese (Finer 1997, 2003). It remains to be seen whether all of these patterns can be explained without appealing to Phase theory, a task I leave to further research (though see den Dikken 2017 and Keine & Zeijlstra 2023 for an existing critical evaluation of some of the evidence). The same is true for patterns of LDA and hyper-raising. The main goal in the latter part of the dissertation has been to understand the empirical desiderata for a theory of locality and suggest why it may be valuable to attempt to reduce syntactic locality to a single mechanism. I have laid the groundwork for a particular implementation of the single-mechanism approach in terms of FRM. Only further work can reveal if that implementation stands up to scrutiny.

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