Minimalist Syntax is psychologically real: Lessons from (counter)cyclicity

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with Madeline Bossi

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Preface

Michael Diercks

This project grew out of moments that I am sure all syntax professors share, at least of those working in the Minimalist framework. For an entire semester, my students will have engaged the generative theoretical enterprise with me, working through Carnie's (2013) textbook. They survive formal discussions of c-command and binding, they internalize the structures of X'-syntax, they explore illustrations of the depth of complexity that occurs in the extended projections of clauses. And then … I introduce the Minimalist Program. Some students love it, namely, the ones who love abstraction, who love generalizations, and (especially) who are not cognitive psychology majors. But for anyone outside those demographics, the questions come fast and hard about bottom-up structure building, about the degrees of abstraction, about the idea of 'computational efficiency' in a model that is not modeling performance. In general, I have found that that point is where their ability to suspend their disbelief gets strained to the point of breaking.

It's not that the students doubt the existence of syntax, or the benefit of theorizing in this way: rather, the mounting cognitive dissonance between abstract theory and tangible empirical realities outside acceptability judgments becomes difficult. How can we continue to present such narrow, detailed analyses of syntactic structures, and still not be able to make any direct correlations between those findings and other aspects of cognition? This monograph grew out of my own attempts to answer the excellent questions that my students raise at this point in their education. The core idea of this manuscript (I quickly learned) is one that's been floating around the field for a long time (that syntactic structure is acquired from bottom-up, with respect to the syntactic structure). The focus on counter-cyclicity grew out of the observation that it might offer a new perspective on a theory of syntax-acquisition correspondences, and furthermore, might make sense out of some troublesome theoretical issues in syntax at the same time.

I have found responses to these ideas to be quite mixed, in the few opportunities we've had to get feedback so far. We received one especially lengthy, detailed, and gentle critical review on a conference abstract; that anonymous reviewer deserves a lot of credit for taking our abstract so seriously (especially when they thought it was so wrong), and for teaching us many things about acquisition.[1] As this monograph shows, we persisted with the idea nonetheless, though our approach has been significantly shaped by that single (quite generous) review. The reviews overall on that conference abstract were the most mixed I have ever encountered on a piece of work. Aside from the aforementioned reviewer, one reviewer found the ideas semi-palatable but

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[1]Ironically, they were reviewer #2.

overall unconvincing: "As the author recognises, the proposal is a rereading of earlier proposals regarding the development of structure in children's syntax, now framed under a minimalist setting. By associating bottom-up derivations with incremental structure building observed in children's syntax, the author is able to provide a unifying approach to earlier analyses. However, associating unexpected patterns to look-ahead or late merge is too long a stretch that only highlights the limits of the bottom-up approach." Another reviewer thought the ideas needed move development but could eventually succeed: "This is a very ambitious and interesting concept, which could be improved through more grounding in acquisition data ... With this additional grounding, this would be a strong presentation at a future conference." Yet another reviewer seems to assume that our proposal was already widely accepted in the field: "There is no discernible way to evaluate the merits - or lack thereof - of this proposal, because the desiderata of the DMS shares much overlap with mainstream tenets of the MP since its inception. In sum, this abstract provides a proof-of-concept that was generally shared by most adherents of the MP."

From that one round of review, some reviewers thought this idea is interesting, some that it is untenable, others that it is actually already baked into the field as a whole. And as we've continued to consider and discuss the ideas sketched in this monograph, we find the responses to be similarly mixed from a broad range of sorts of linguists and cognitive scientists. We're not sure what to make of that, and there are enough unsettled questions in this monograph to ensure that we have gotten a number of things wrong. Nonetheless, the core ideas are intriguing enough that we thought they were worth exploring.

We were disheartened by that initial conference review in a number of ways, and set the project aside for a while. But I have simply not been able to shake these ideas, to the point where I have started interpreting most syntactic analyses I encounter through this lens. It became clear to me that I needed to organize and systematize the ideas, for myself and for my students, if for nobody else. Worst case scenario, this is just wrong (which, as a theoretical syntactician, I have a relatively high comfort level with). Best case, it has the promise of offering a bridge out of our intellectual silo, connecting the findings of the Minimalist enterprise with research in other domains. But before that's possible, we have to see if the ideas can hold up. Which is why we've written this up here, as a step toward evaluating their viability.

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Claremont, CA
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Acknowledgements

Michael Diercks

A comment from Brent Henderson is what first set me on this path (though apparently it was so fleeting that he does not even remember saying this to me). Specifically, we were discussing the state of generative syntax and its degree of isolation from other work on language and cognition, and wondering where points of connection might be. Brent suggested (paraphrased here) that the proposals in the Minimalist Program might actually simply be a formalization of the grammaticalization mechanisms that Construction Grammarians generally assume (but about which they usually have few proposals on the fine-grained details). This core idea is pervasive in this volume, and should not be attributed to any of the authors here, but instead to Brent. He just had the enduring wisdom to not let himself become obsessed with the idea for a decade. Nonetheless, he had nothing to do with how we took this idea and ran with it, so he shouldn't be held responsible for any of the proposals here, only credited with whatever wisdom arose from his initial speculation.

This work has benefited from interactions with a broad range of colleagues, including Galia Bar-Sever; Travis Major; John Gluckman; Ruth Kramer; Franco Liu; Rodrigo Ranero; Jenneke van der Wal; John Beavers; multiple anonymous NELS reviewers. In addition, multiple generations of Pomona students have listened to and engaged with these ideas critically, aiding their coherency considerably.

Part of chapter 4 is an edited version of a discussion that first appeared in Diercks et al. (2020) (namely, the overview of the phase reference model in §3). We gratefully acknowledge the co-authors on that discussion, and especially Marjo van Koppen for her contributions in developing those ideas and editing that text as it originally appeared in Diercks et al. 2020. It is republished here, with edits as appropriate for the current task, in accordance with Language Science Press's Open Access policies and with permission of the co-authors of that work: Michael Diercks, Marjo van Koppen, and Michael Putnam.

Madeline Bossi was central to the development of these ideas. In her junior and senior years at Pomona College, in discussions together, we took the initial pieces of my ideas and developed them into a workable theory. She also performed the first (extended) foray into the acquisition literature to test whether these ideas were viable at all, and to discover what precedent there was: I knew next to nothing about the acquisition literature before Madeline's work. Her undergraduate thesis (Bossi, 2017) was the first full write-up about DMS, and this manuscript would not exist without her (for both practical reasons, and intellectual reasons). The development from that

stage of the work into the full manuscript as it currently stands was done mainly by myself, though the first draft of multiple portions of the manuscript were directly copied from her thesis. After extended edits and revisions of the manuscript, we're not quite sure exactly what percentage of the final product here can be directly linked to the thesis, but whether most of those initial words got rephrased or not, Bossi (2017) is the foundation of this work here.

We have chosen to represent her contributions by describing authorship of "Diercks, with Bossi," and detailing a few particular chapters as co-authored (Chapters 1 & 2). Some of Bossi's thoughts and words may appear in the other chapters in smaller ways, but they are sufficiently minor in those instances to have restricted the authorship in this way to best communicate our respective contributions. I (Diercks) had final say in any disagreements. Whatever wisdom might arise from this work, Bossi certainly deserves shared credit. But she also ought not be held responsible for the most heretical portions of the proposals, especially given how much of this work was performed at a very early stage in her career. That is to say, I don't know whether she still believes in the approach advocated for here! Nonetheless, her contributions were substantive and substantial, and the authorship decisions are meant to communicate that.

As is clear from this work, I have been heavily influenced by work on Construction grammar and usage-based acquisition researchers, particularly by just how seriously they take acquisition facts in their conception of how Language works. The person who deserves credit for my openness to things I disagree with is Raffaella Zanuttini. She taught me that any well-educated person who has done intensive research on something has clearly noticed something about language. Even if we disagree with all of the conclusions, we can at least work to understand what it is that they noticed in the first place. Raffaella is deeply respectful of work she disagrees with, understanding that it can be deeply valuable, whether or not it's ultimately correct. I learned syntactic theory from Raffaella, I learned to be non-dogmatic from Raffaella, and I also learned to be courageous from Raffaella. As we chatted before my dissertation defense, she asked how I felt about the product, and I commented that I knew the work wasn't groundbreaking, but that I was proud of it nonetheless. Her response was: "how do you know it's not groundbreaking? We don't get to know that until later." Of course, it turns out that it wasn't, but the lesson stuck with me: do the work, and let it stand for what it is. Don't prejudge it. I probably never would have been the kind of linguist I am if it were not for Raffaella. She doesn't even know this manuscript exists as I write this, but she taught me to be the kind of linguist who can entertain these kinds of questions.

Finally, I especially owe a debt of gratitude to Tegan, who tolerated my distraction and obsession with this project, who encourages me to work at things that interest me, and who could care less what comes of my syntactic theories. I'd live my whole life down my rabbit holes if it wasn't for her.

Contents

Chapter 1

Introduction

Michael Diercks
Madeline Bossi

1 Derivation by Phase and Countercyclicity

1.1 Structure building in Minimalist syntax

For over two decades generative syntacticians operating in the Chomskyan tradition have been following the general framework laid out by Chomsky (1995, 2000a, 2001) that is known as the Minimalist Program, an iteration of the generative syntax tradition. By many measures, this has been an extraordinary success, both in terms of the range of empirical phenomenon that our theoretical mechanisms now capture, as well as the depth of detail that those same mechanisms allow us to describe and explain with precision within a given grammatical construction.

On the Minimalist approach, syntactic structures are built from bottom-up, with a verb (or more abstract root) merged with a complement to form a basic predication relation, and all additional syntactic structure is built on top of this in a cyclic operation where new material is merged with the root of the existing structure. We schematize this in (1), where we informally use the 'plus' symbol to annotate a Merge operation.[1]

(1) $V + DP \rightarrow [_{VP} \ V \ DP \ ] + v \rightarrow [_{vP} \ v \ [_{VP} \ V \ DP \ ] \ ] + ...$ (etc)

As (1) shows, a verb merges with a noun phrase (DP), resulting in a VP, and the resulting VP is then a candidate for merging with another phrase.[2]

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[1] The audience for this paper is Minimalist syntacticians (or those in adjacent fields), and as such we assume broad familiarity with Minimalist syntactic theory, only explaining core issues that are relevant to our concerns.

[2] Strictly speaking, on current assumptions Merge does not result in the labels assigned to the resulting phrasing (VP and vP in (1)): a labeling algorithm of some sort is assumed to apply to clarify the properties of the resulting phrase (Chomsky 2013, Bauke and Blümel 2017). This is largely tangential to our current concerns, and as with other work where the labeling algorithm is tangential, we simply label the resulting structure appropriately and don't address the question of how such labels arise derivationally.

This creates a strict cycle of structure building: successive applications of Merge build hierarchical syntactic structures step-by-step. Specifically, that standard implementation of this follows what is usually referred to as the "Extension Condition," a requirement that applications of Merge extend the root of the structure. This has also been expressed as a "no tampering" condition: i.e. a requirement against modifying/tampering existing structure; new operations only add structures, they cannot remove or modify the existing structure in the course of a derivation. "A natural requirement for efficient computation is a 'no-tampering condition' (NTC): Merge of X and Y leaves the two SOs [syntactic objects] unchanged. If so, then Merge of X and Y can be taken to yield the set X, Y, the simplest possibility worth considering. Merge cannot break up X or Y, or add new features to them. Therefore Merge is invariably 'to the edge' " (Chomsky, 2008, 138).

This Merge operation does not alone account for all of syntactic phenomena (there are other posited components of UG, and other empirical aspects of morphosyntax), but it is where we direct our attention for the moment. Focusing on that core structure-building operation, this requires a strict cycle that is monotonic, with each new cycle extending the root of the structure without any changes to the existing structure at that point of the derivation.

1.2 Persistent Countercyclicity

As an operating framework of analysis, the Minimalist Program has been quite successful, facilitating a large amount of analytical and theoretical work in a broad range of languages. That said, what might be viewed as a bit of a black mark is the persistence of counter-cyclic analyses of empirical phenomena in the literature. By "counter-cyclic" here we refer to step in the derivation that does not proceed according to the strict cyclic Merge described above. On the way we are using the term here, this includes an operation that happens *later* than it ought to according to the strict cycle: a classic instance of this is Late Merger, where some head/phrase is merged into a structure in violation of the Extension condition, inserting itself into existing structure (we describe this in more depth in §2). This is shown schematically in (2), where a structure like (2a) has been built, and only after the structure has been built does merger of LP occur in (2b), but instead of extending the root (XP), instead LP merges lower in the existing structure, resulting in the structure in (2b).

(2)   a. $[_{XP}$ X $[_{YP}$ Y $[_{ZP}$ Z $]\,]\,]$ + LP $\rightarrow$
        b. $[_{XP}$ X $[_{YP}$ Y $\boxed{[_{LP}\ L]}$ $[_{ZP}$ Z $]\,]\,]\,]$

Such operations are generally thought to be illicit, violating the Extension Condition by tampering with the root structure rather than extending it. Nonetheless, analyses like these do in fact appear to be well-motivated for certain empirical phenomena, and analyses dependent on Late Merger can be found again and again in the literature (examples follow in §2.1).

We also include (in our definition of countercyclicity) operations that happen earlier than they ought to according to the strict cycle of narrow syntax, and/or without clear motivation at the point at which they appear to occur. These are often termed look-ahead problems, where something happens derivationally before the supposed trigger of the operation has been merged

into the structure. A prominent example is that wh-movement raises through lower phase edges before the target position of movement has been merged into the structure (matrix CP).

(3)   What$_k$ do you think [CP ~~what$_k$~~ [C′ that Alex ate ~~what$_k$~~]]?

There is a broad range of empirical evidence arguing for intermediate stages of movement of a wh-phrase, shown in (3) at the edge of the embedded CP phase. The look-ahead problem arises in that (per the requirements of cyclic spell-out, i.e. derivation by phase) the wh-phrase moves to the edge of the embedded CP at the point that embedded C is merged; at this point in the derivation, however, the matrix C is not yet part of the structure, and there's no clear immediate motivation for *what* to move to the edge of the embedded CP (the matrix CP is marked as [+wh], the embedded CP is most plausibly [-wh]). This requires the computational system to "look ahead" in the derivation, foreseeing the eventual need to raise to matrix CP, and ensuring the exit from the lower CP to avoid being trapped in the spelled-out lower phase.

We discuss relevant accounts of look-ahead in more depth in §2.2. These of course are equally problematic (along with Late Merger) for a theory that purports to construct a computational system that builds outputs equivalent to the grammatical structures of a language: the system itself can't reference the outputs it is designed to create (and still stay a self-contained computational system, at least). Look-ahead poses a different kind of counter-cyclic problem for the Minimalist derivation of a sentence: just as Late Merger requires tampering with the existing structure (i.e. not obeying the Extension Condition), look-ahead (while not obviously violating some existing condition/operation) is simply not formulable in a (strict) Minimalist derivation, as the only syntactic structure that exists at an intermediate stage of a derivation of a sentence is that intermediate structure, within which there is no clear motivation for movement.

It is notable that despite the lack of large-scale theoretical incorporation of counter-cyclicity, as a field we are quite familiar with *both* look-ahead problems and instances of Late Merger, in addition to other more nuanced instances of counter-cyclic analysis, such as upward-probing Agree (Baker 2008, Bjorkman and Zeijlstra 2019) and delayed probing (Zeller 2015), among others. Despite their supposed illicit nature, these kinds of analyses occur frequently enough to be familiar to most of us.

Chomsky has recently spoken out against such countercylic analyses, which make up a large proportion of the "foundational issues which are unsettled and I think are rather troublesome, and that bear directly on a number of very important issues in current work and I think raise questions about the legitimacy of problems and challenges we have faced" (Chomsky, 2019, 264). Sportiche (2019) expressed similar skepticism in his article titled, "Somber prospects for Late Merger," arguing that the empirical benefits of the Late Merger analyses don't outweigh the potential overgeneration that such an operation introduces into the theory. Any syntactician who has submitted a counter-cyclic analysis for review has encountered the obligatory pushback, which is (of course!) deserved: the Minimalist Program has no widely-accepted theory of counter-cyclic operations, and based on foundational assumptions deducing the properties of language from interface conditions (e.g. Chomsky 2000a, 2001) there is not good reason that Late Merger or look-ahead ought to exist. Apparent counter-cyclicity is, from a standard Minimalist perspective, always an analytical problem that is perhaps only tolerated because no possible alternative (cyclic) derivation is apparent (to both the researcher and reviewers/editors) that might

be available to explain the patterns. Chomsky himself expressed it recently in this way:

> Over the years, whenever some descriptive device has been introduced, and whatever it is (PSG, transformations, X-bar theory, parameters, phases, whatever it might be), almost always it tends to be used pretty extravagantly, well beyond the basis, of any solid foundation for the rule … the advantages were that it led to a lot of discoveries, there were lots of insights about language that came out of it. They're not solutions, they're problems, and it's good to have problems, and led to explorations of new domains that hadn't been looked at. All of that's positive, and that's commonly true for the promiscuous use of devices that are invented. The negative aspect is that it doesn't lead us to the goal of trying to understand UG and the language faculty, and it's also misleading in that it tends to present problems, which are interesting problems, as if they were solutions, and they are not solutions: they are ways of stating a problem that we have to look at. (Chomsky, 2019, 270-272)

His overall perspective here seems undeniable: if a theoretical mechanism is made available, analysts will make use of it, whether or not it is deservedly in our theory. That is the nature of research, and it has positive effects, as it often opens us up to exploration in new theoretical areas. But if those mechanisms are in fact ultimately not well-founded, what do we do with the resulting empirical puzzles?

The core question this monograph investigates can be phrased like this: why does counter-cyclicity exist? The glib answer, of course, is that the analysts couldn't think of a better idea: it is one of these promiscuously-used analytical mechanisms that actually doesn't belong in the theory, so apparent counter-cyclic analyses are simply a persistent empirical problems to be solved. On this approach, counter-cyclic processes are necessarily only apparently counter-cyclic, because (by assumption) Merge is the only structure-building operation.

But as we've mentioned, certain kinds of counter-cyclic analyses have managed to persist in the literature with the dogged determination of a recalcitrant empirical reality, despite the relatively widely-accepted theoretical assumptions that counter-cyclicity ought not exist. How do we resolve this collective cognitive dissonance? In this monograph, we are going to assume that counter-cyclicity is real as an empirical phenomenon (broadly-speaking), and take that to argue that inclusion of counter-cyclic mechanisms in our conceptualization of Universal grammar. That said, we aren't arguing here for a specific technical account for countercyclic mechanisms (presumably, versions of the already-proposed mechanisms would work for the constructions they have been developed for). Rather, we propose an extra-grammatical motivation for counter-cyclic operations, correlating the derivation of sentences in adult competence to sequences of language acquisition. It is on the basis of this extra-grammatical evidence that we believe derivational counter-cyclicity to be a real property of human language, which Minimalist theories of UG must accommodate.

1.3 Developmental Minimalist Syntax

Developmental Minimalist Syntax (DMS) is a relatively simple proposal about a possible interpretation of the findings of the Minimalist Program. In short, DMS claims that adult knowledge of the syntax of a speaker's native language(s) is well-modeled in a bottom-up fashion precisely

because bottom-up structure building mimics the ontogenetic (i.e. organism-internal) timeline of grammaticalization of syntactic structures during child language acquisition. That is to say, the general model of the acquisition of syntax is that 1) structurally lower elements are acquired before structurally higher element and 2) later stages of syntactic knowledge incorporate previous stages of knowledge (i.e. as a general rule, later-acquired syntactic structures are added to existing knowledge, rather than replacing that existing knowledge). The result is that earlier stages of knowledge are retained in our adult grammatical representations.

DMS is at its core an interpretive principle; it is a proposal that the Minimalist syntactic analysis of adult syntactic structures has direct implications for our understanding of the ontogenetic development of language. The step-by-step derivation of a sentence using Minimalist principles (e.g. Merge, Agree, phases)—which DMS adopts without alteration—mirrors the acquisition timeline for a child acquiring those linguistic structures:[3]

(4) **Developmental Minimalist Syntax (an interpretive principle)**
The Minimalist derivation of adult language structures recapitulates the ontogenetic development of those same syntactic structures.

DMS essentially proposes a correlation between empirical domains—that structural hierarchy, syntactic displacement, etc, and the mechanisms which generate it in adult language (e.g. bottom-up structure building, syntactic movement via Copy + Internal Merge, etc) are directly correlated with the progression of language development.

More specifically, if (4) is on the right track, the theoretical constructs and derivational processes proposed to account for observed syntactic phenomena in effect also model the mechanisms and pathways of child language acquisition. For example, DMS leads to the claim that the early stage of the derivation of a sentence—in which a subject, verb, and object all exist in their base positions in vP—corresponds to an ontogenetic stage wherein the components of vP are the full extent of a child's (grammaticalized) syntactic knowledge of their language; higher structures are systematically *added* on top of existing structures as a child gains more knowledge about the target language. In other words, the widely-accepted vP-internal subject hypothesis—in which all clausal subjects have an underlying base position within vP regardless of their surface position—is a remnant of an earlier stage in child language acquisition when the child represented the subject in that vP-internal position. Such remnant structures can be thought of as *ontogenetic fossils* (i.e. fossils from an earlier state of that organisms language knowledge), a term (and a concept) inspired by work on the evolution of human syntax in Progovac (2015) (discussed in §2.4). In this way, movement is always an instance of adding a new grammatical representation of the position of an element, but without demolishing the initial grammatical representation: adult grammatical knowledge encodes not only the final word order, but also the previous stages of grammatical knowledge.

Stepping back from the theory for a moment helps to put this interpretive principle in perspective. The fact that Minimalist syntactic models build structures bottom-up—starting first

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[3]Though he does not remember this conversation (and even if he did he wouldn't connect this evolution of the idea to his original comment), this idea was first suggested to Michael Diercks by Brent Henderson in a casual conversation, namely, that Minimalist theory might actually be a theory of grammaticalization structures assumed in Construction Grammar. Taken to its logical conclusion, we landed on DMS.

with verbs and objects—is quite unintuitive from certain perspectives. There is no obvious way that this bottom-up structure building relates to the psychological reality of language processing; perception of most of the world's languages clearly cannot proceed in that fashion—or else garden path sentences could not exist—nor can language production proceed bottom-up: a speaker can certainly begin a sentence without knowing how they are going to end it. Non-specialists and new linguistics students often find these theoretical constructs quite unintuitive given the lack of obvious correlation to more surface-evident language patterns like perception and production.

Generative syntacticians generally do not assume that a model of language must have a psychological reality related to language processing; per the now-famous distinction proposed in Chomsky (1965), syntacticians are modeling speakers' knowledge about the syntax of their language (i.e. 'competence') rather than their ability to speak or perceive sentences (i.e. 'performance'). Given that Minimalist models of syntax work quite well to model a wide range of constructions in a large number of the world's languages spanning a disparate range of language families, a crucial question arises: why does bottom-up structure building model grammatical knowledge so well as a process, a sequence of apparently 'mental' operations, when it clearly does not relate to language processing or production? DMS represents an attempt to answer precisely that question by claiming that there a psychological reality to bottom-up structure building, as it directly encodes the pathway by which children arrive at adult-like syntactic generalizations about the target language. In other words, bottom-up structure building models adult language so well because adult linguistic knowledge is essentially a crystallized history of the successive generalizations that children make when learning language.

Similar ideas were discussed and heavily debated in the 1990s (e.g. Rizzi's 1994 Truncation Hypothesis, Radford's 1990 Small Clause Hypothesis, and the work from Harald Clahsen and colleagues (e.g. Clahsen et al. 1993/1994; Clahsen 1990/1991; Clahsen et al. 1994, among others). Lebeaux (1988, 2000) in fact sketches a theory quite similar to what we discuss here; while his initial proposal of late merger operations has certainly taken hold among syntacticians (albeit somewhat controversially), it's fairly clear that his ideas about connections with language acquisition have not. We do find some general cursory comments in the literature about some aspects of his proposals, they haven't led to a holistic conception of syntax-acquisition links in the way that we propose here. In part, this may be due to Lebeaux's (2000) insistence that some of his proposals were in fact radically different than Chomsky's Minimalist proposals. At least insofar as the Minimalist Program has evolved in that time, in our minds they are not so incompatible as Lebeaux (2000) suggests (see discussion in §5).

Significant recent work in this direction is the work of Anne Vainikka and Martha Young-Scholten on their theory *Organic Grammar* (focused mainly on second language acquisition, but also explicitly discussing first language acquisition) (e.g. Vainikka and Young-Scholten 2007, 2011, 2013). We discuss their claims at length below so we won't go in depth here, but perhaps the most succinct way to explain the difference between our proposal and theirs is that Vainikka and Young-Scholten propose Organic Grammar as an alternative theory of human language (that has parallels to the Minimalist Program as a generative syntactic theory). We, on the other hand, are not proposing an alternative to Minimalist syntax, but rather proposing an interpretive principle to systematically correlate Minimalist analyses of adult language structures with empirical do-

mains outside adult grammars. Organic Grammar shares many similar claims, which we discuss in depth below, but our wholesale adoption of Minimalist syntax opens the door to exploration of more extensive correlations between Minimalist syntactic analyses and acquisition processes. Another recent research program that shares many similarities with Organic Grammar comes from the suggestions about language acquisition advanced in Progovac (2015) as part of a discussion of language evolution, and the follow-up work on acquisition itself from Rakhlin and Progovac (2017, 2020). We also discuss this work at length in what follows.

DMS effectively brings Minimalist theories of Universal Grammar back to their historical roots: UG as a Language Acquisition Device. As we lay out in more detail below, this readily allows for many acquisition processes to proceed based on statistical learning, and to proceed in an item-based fashion, as it has become clear that acquisition does. But at the point when grammaticalization occurs (i.e. the child generalizes beyond specific examples to a grammatical rule, structure, or generalization), that these generalizations take a very specific form. Specifically, the Minimalist proposals about UG, on this approach, are proposals about how children grammaticalize a pattern. We argue below that the canonical operation for forming a new grammatical generalization about the structure of a sentence is Merge: essentially, new knowledge of their language's structure is incorporating by merging a new functional head atop the child's existing grammatical structure of a sentence.

The claim that children acquire language in some kind of bottom-up syntactic fashion has always been appealing (and, as we will summarize below, boasts relatively strong empirical support), and hence there is a host of generative work that has proposed this. But there have been persistent problems with such claims, specifically, empirical domains where obvious predictions of such accounts are not upheld. What we want to show, however, is that there are compelling correlations between (on the one hand) the empirical domains that have proven to be problems for a bottom-up theory of acquisition and (on the other hand) the empirical domains in adult language grammar that are problematic for a strictly cyclic derivation of syntactic structure.

For example, some presumably CP-level structures are acquired by children before they "ought to be" according to a bottom-up acquisition theory. Likewise, some lower-level structures seem to be acquired later than they "ought to be." Rather than calling these weaknesses of this account, though, we claim that such divergences from the core structure building process in fact correlate with long-standing anomalies within Minimalist syntactic analyses: namely, countercyclic operations. We will show that "early" acquisition (i.e. earlier than predicted per a strict bottom-up acquisition process) correlates with look-ahead and with otherwise unmotivated movement processes (like most head movement of verbs). In contrast, "late" acquisition (i.e. later than predicted on an account of strict bottom-up acquisition) correlates with constructions in adult language that require Late Merger operations.

So our proposal in ways attempts to reinvigorate an old idea, but also claims that advances in the Minimalist Program can give it new life; syntax is generally acquired bottom-up, but not exclusively so. On the flip side, we suggest that facts about acquisition can offer some explanatory basis to the persistence of counter-cyclic analyses in the field. Moreover, we claim that the Minimalist model of analysis uncovers this specific kind of grammatical knowledge (and acquisition phenomenon), rather than countercyclicity being a problem to be eliminated from the Minimalist Program.

The rest of this introductory chapter covers other central background information. Chapter 2 lays out the central claims that we make about Developmental Minimalist Syntax (1), discusses the theoretical precursors (§2), and empirical support for the overall concept (§3). §4 sketches the pathways of acquisition as they would proceed on a DMS approach (which is largely, in fact, a report of the constructionist claims about language acquisition, with some "new details" on how grammaticalization proceeds, namely, according to a Minimalist derivation). The final main part of chapter 2 is a discussion of parallels between nominal and verbal/clausal acquisition, adopting the approach proposed by Wiltschko (2014) and Ritter and Wiltschko (2014) for an abstract set of universal structural domains.

Chapter 3 overviews some major counter-cyclic processes specifically, showing that they adhere to the predictions of DMS ("early" and "late" in adult grammatical derivations correlating with "early" and "late" in acquisition). We do this for late merger of relative clauses (§1) and look-ahead in wh-movement (§2). We also use the well-studied phenomenon of Zulu object marking to consider a claim of Chomsky's, that counter-cyclic processes necessitating non-canonical theoretical mechanisms like late merger are exotic sorts of constructions that are largely inaccessible to children in acquisition. §3 shows that a morphologically transparent construction can itself show the same counter-cyclicity properties (and, while evidence is limited, there is at least initial evidence suggesting that it is acquired later than similar kinds of verbal morphology).

Chapter 4 proposes a new interpretation of phases. Specifically, we propose that phase domains in adult grammars are ontogenetic fossils of acquisition workspaces: we propose that acquisition proceeds according to a few major domains (namely, those proposed as part of the Universal Clausal Spine by Wiltschko 2014 and Ritter and Wiltschko 2014): this is laid out in §4. This is built on a relatively recent interpretation of the properties of phases by Wolfram Hinzen and colleagues (§3). This helps to explain why acquisition is not as neat and clean as we might expect if it exactly mirrored a sequence of acquisition matching adult syntactic structures, and yet nonetheless does appear to have identifiable stages. It also helps to explain why in adult grammars phases are likewise not as neat and clean as the citation form of the theory might suggest. So we discuss the evidence for phases having variable sizes (§6), and for apparent simultaneity of operations in phases (§7), suggesting that both fall out from the approach to phases as acquisition workspaces. And we suggest it offers a solution to the long-standing tension between the assumptions that phases exist in part to facilitate a more efficient computation (on the one hand) with the fact that the MP is an account of competence and not performance (on the other) (§2).

2 Counter-Cyclic Processes in Minimalist Analyses

In this section we overview some prominent instances of empirical phenomena that benefit from counter-cyclic analyses, and the kinds of proposals that have been advanced about them. These will serve as case studies regarding our major claims about counter-cyclicity, chosen both based on their impact on the literature on counter-cyclicity but also based on the availability of child language acquisition data to test the predicted correlations. In this section we only overview the relevant background, explaining two major kinds of counter-cyclic analyses in the literature. Chapter 3 discusses the DMS-style analysis of counter-cyclicity. Given how pervasive the is-

sues are throughout the volume, however, we want to be sure to clearly state our background assumptions here.

2.1 Late Merger, Wholesale Late Merger, and (anti-)reconstruction

It has long been established that A-movement and A'-movement behave differently with respect to reconstruction (i.e. interpretation of the lower position of a moved element with respect to binding interactions): A'-movement is known to obligatorily reconstruct, whereas this is not the case for A-movement. Nonetheless, reconstruction puzzles persist. For example, (5) shows a distinction in reconstruction effects between A'-movement of a complex DP with a noun complement clause (5a) as opposed to a relative clause (5b). Both of the examples below set up a potential Condition C violation in the base position of the extracted argument, such that the pronoun *she* binds the R-expression *Rebecca* in the base position of the A'-moved object (marked here by a trace). But this condition C violation only appears in (5); it would be expected to also appear in (5b), but does not.

(5)   a. ??/* [Which argument [that Rebecca$_k$ is a genius]]$_i$ did she$_k$ believe t_i ?
      b. [Which argument [that Rebecca$_k$ made]]$_i$ did she$_k$ believe t_i? (Fox, 1999)

(5a) is thought to be unacceptable as a result of Condition C, as well as the obligatory reconstruction of A'-movement: because the *which argument* DP is necessarily interpreted in its base position for the purposes of binding, the R-expression *Rebecca* is illicitly bound by the subject pronoun *she*. This follows straightforwardly from the Copy Theory of Movement (Chomsky, 1993, 1995): A'-movement leaves a copy in its base position that is subject to binding conditions. The puzzle that arises, however, is how the R-expression *Rebecca* inside the relative clause in (5b) is able to escape this same fate. The R-expression *Rebecca* inside the relative clause (inside the A'-moved object) is only interpreted in its surface position, and does not reconstruct (an anti-reconstruction effect).

   To address these facts, Lebeaux (1988) proposes that relative clauses are merged into the structure countercyclically, after the rest of the structure has been built (*late merger*). (5b) appears to bleed a Condition C effect because there is actually no underlying Condition C violation to avoid—the relative clause is merged at the structurally higher position of the DP, not in the lower position. Lebeaux's original proposal made a complement/adjunct distinction: only adjuncts undergo late merger.[4,5] We can see this pattern perhaps more clearly in the examples in (6). The circled phrase in (6a) is an adjunct and assumed to be Late-Merged, therefore there is no copy of of the R-expression *Picasso* in the base position of the wh-phrase, as the PP adjunct is only merged after A'-movement has taken place. In (6b), in contrast, the wh-phrase contains a complement clause, which is argued to *not* be Late-Merged, and therefore occurs in the base position of the wh-movement.

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[4]Lebeaux's explanation rested on an interpretation of the Projection Principle, that the arguments of a lexical item must be present throughout an entire derivation.

[5]Sportiche (2019) makes a valuable comment in terms of the continued empirical investigation of these patterns: "Even though they have been recently questioned (see Adger et al. 2017, Bruening and Al Khalaf 2017), such asymmetries are robust for many speakers (including me) in many languages, but not for all speakers (in any language?); this suggests the presence of at least one uncontrolled variable."

(6)   a.   Which villages [near Picasso_k's estate] did he_k visit t ?
      b.   *Which pictures [of Picasso_k] did he_k sell t?
           (Sportiche, 2019, 416)

As Sportiche notes, the Late Merger account of adjuncts allows (6a) to be derived in one of two ways: merger of the adjunct structurally low (hence, undergoing wh-movement with its head noun wh-phrase "which villages," or alternatively, the adjunct can undergo Late Merger, merged with the nominal only after the nominal has undergone wh-movement, as shown in (7b).

(7)   a.   [Which villages near Picasso_k's estate]_i did he_k visit ~~which villages near Picasso_k's estate_i~~?
      b.   [Which villages]_i near Picasso_k's estate did he_k visit ~~which villages_i~~?
           (Sportiche, 2019, 417)

The presence of a copy of the adjunct PP in (7a) would result in ungrammaticality (similar to (6b)), because A'-movement reconstructs, and when the lower copy is interpreted, it results in a configuration where the pronoun *he* c-commands the R-expression *Picasso*, a violation of Condition C. In contrast, the Late Merger derivation in (7b) completely lacks a lower copy, making available a structure where the R-expression is never bound, and therefore examples like (6a) are acceptable. Key to Leabeaux's analysis, and that of many others, is that adjuncts necessarily are merged late, whereas this is not the case for arguments.[6] This explains the difference between (6a) on the one hand and (6b) on the other: arguments cannot be late merged (on this account), and therefore a copy of the R-expression *Picasso* in each of these is present in the base position of the noun phrase, where it is bound by the pronoun *he*, resulting in a condition C violation.

(8)   *[Which pictures of Picasso_k]_i did he_k sell [~~which pictures of Picasso_k~~]_i?

Takahashi and Hulsey (2009) (henceforth, T&H) expand this notion of late merger to also include A-movement reconstruction (see (10)), proposing a process of *Wholesale Late Merger* (WLM): "late merger is permitted whenever an output representation can be interpreted in the semantic component (henceforth, the *LF interpretability approach*). A consequence of the LF interpretability approach is that, in addition to adjuncts, a restrictor of an operator/determiner can undergo late merger" (T&H, 388). The idea here is that the NP content of every DP is potentially Late Merged. This is illustrated in (9) (T&H, 388).

(9)   a.   Every argument seems to be correct.
      b.   Base Structure:
           [_XP [every] correct] $\rightarrow$
      c.   Main clause merged:
           [_YP seems to be [_XP [every] correct]] $\rightarrow$
      d.   Movement of Determiner:
           [_ZP [every] [_YP seems to be [_XP [every] correct]]]

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[6] Bruening and Al Khalaf (2017) argue that there is no empirical difference between arguments and adjuncts, but it nonetheless does appear (as noted by Sportiche 2019) that the distinction does exist for a number of speakers.

     e. Merger of Restrictor with higher copy of Determiner:
       [$_{ZP}$ [every [argument]]] [$_{YP}$ seems to be [$_{XP}$ [every] correct]]]

While a relatively radical proposal, T&H are able to explain a fuller range of reconstruction and anti-reconstruction effects than was previously possible. For example, A-movement can also exhibit reconstruction effects; in (10) a bound reading of the variable is possible despite the fact that the DP containing the variable c-commands the quantifier after A-movement (based on Fox 1999, 161, Takahashi and Hulsey 2009, 391).

(10)   [Someone from her$_k$ class]$_i$ seems to [every professor]$_k$ t_i to be t_i a genius.

Here the lower copy of the A-moved subject is interpreted in order to achieve the appropriate structural configuration for the bound reading; the availability of reconstruction with A-movement suggests that A-movement (like A'-movement) also leaves a copy of the moved element in its base position. Yet, A-movement still bleeds Condition C:

(11)   [John$_k$'s mother]$_i$ seems to him$_k$ [~~John$_k$'s mother~~]$_i$ to be wonderful.
     (Lebeaux, 1988, 23)

WLM can account for A-movement's bleeding of Condition C effects by allowing the restrictor of the quantifier (NP complement to D°) to be merged counter-cyclically, as sketched in (9). No Condition C effect emerges because the relevant R-expression is never bound by the pronoun.

A common characterization of these effects (apart from the late merger proposals) is that A-movement has the option between leaving a contentful copy or leaving a content-less trace of movement, whereas A'-movement always leaves a contentful copy (Fox, 1999; Sauerland, 1998). T&H claim that this disjunctive analysis can be avoided: all movement leaves a full copy of whatever content is moved, but *wholesale late merger* applies: sometimes DP-content is merged countercyclically, after movement has occurred. Their claim is that A-movement bleeds Condition C because the lower copy of the DP consists only of the D head; the R-expression in question is not merged until later in the derivation, and therefore no copy of the R-expression is subject to the illicit binding configuration.

But why does A'-movement necessarily reconstruct? T&H claim this is due to Case-licensing: an entire D-NP composite requires Case (not just a D head), and therefore NP complements of D must be merged structurally low enough that they can receive Case. WLM can in principle occur at any point in a chain, provided that that position is lower than (or equivalent to) a Case-licensing position. This explains why chains terminating in Case positions (A-movement) can bind from those positions, but chains terminating in non-Case positions (A'-movement) must obligatorily reconstruct: merger of the NP complement of DP must happen before the final landing site of A'-movement, because Case-licensing necessarily happened below that position.

At present, we simply present these major existing proposals in order to introduce the relevant phenomena and the analyses that have been proposed. We discuss the field's response to such proposals (and our proposals about counter-cyclicity) in Chapter 3 below. The core of our claim will be that Late Merger in adult grammars corresponds to "late" acquisition of those same structures ('late' only in the sense of being grammaticalized well after surrounding grammatical structures). We don't offer a claim to somehow derive counter-cyclic empirical properties in

adult grammars from strict Merge-based structure-building; rather, we offer empirical arguments (from acquisition) that counter-cyclicity is real, and our theory ought to incorporate it even if the demands of the SMT suggest otherwise.

2.2   Look-ahead in wh-movement

A fairly famous example of problematic derivation via bottom-up structure-building via Merge comes from quite simple instances of wh-movement. As we showed in (3)—repeated here as (12)—in long-distance wh-movement, wh-phrases move through the edge of intermediate phases (illustrated for the embedded CP here).

(12)   What$_k$ do you think [CP ~~what$_k$~~ [CP that Alex ate ~~what$_k$~~]]?

A well-established outcome of the generative enterprise is that long-distance dependencies (which we model by movement) in fact are not a single long-distance dependency, but a series of shorter dependencies. This is despite the fact that there is no obvious motivation for movement to the edge of the embedded CP in (12). Bošković (2002) argues that the same property holds of successive-cyclic A-movement, where movement of the subject from the embedded clause proceeds in smaller steps, rather than one single movement to the matrix subject position.

(13)   The students$_k$ seem [~~the students$_k$~~ to have ~~the students$_k$~~ liked French].

Successive-cyclic movement has been captured in various ways over the years. Here we replicate Bošković's (2007) summary of the literature, focusing on the core alternatives that led to his influential proposal to account for the look-ahead problem. In the Minimalist approaches to successive-cyclic movement, there are two major contrasting approaches. One (emerging from the early minimalist period) is that the requirement is imposed on entire movement chains: Chomsky and Lasnik's (1993, 546) Minimize Chain Links Principle (MCLP), implemented by Takahashi (1994) to explain successive-cyclic movement. "For Takahashi, successive-cyclic movement is not a result of feature checking. Rather, it is a result of the requirement that all chain links be as short as possible. This requirement forces element X undergoing movement of type Y to stop at every position of type Y on the way to its final landing site independently of feature checking" (Bošković, 2007, 592). On the MCLP analysis, there is no necessity for feature-checking in intermediate positions of movement, as those shorter links of movement are necessitated by a global principle. This approach assumes that movement is feature-motivated, but more specifically that the operation Form Chain is feature motivated, not the formation of particular chain links. As a result, the moving XPs in (12) and (13) do not begin moving until the target of their movement are merged into the structure (matrix interrogative C and matrix finite T, respectively).

This kind of MLCP analysis is not (directly) formulable in the Derivation by Phase (DbP) evolution of Minimalist theorizing (see Chomsky 2000a, 2001, which has been the (relatively) stable) operating framework of Minimalist syntacticians for over 20 years. In DbP, Agree is a component of movement, meaning that all all movement involves feature checking. Moreover, the derivation is expected to proceed by phase, meaning that a smaller portion of the syntactic structure is constructed, after which that portion is completed and left alone while subsequent portions of the derivation are constructed.

(14) Phase Impenetrability Condition (Chomsky, 2001, 13) For strong 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.

Per Chomsky's Phase Impenetrability Condition, on the assumption that (at least) C is a phase head, once the embedded CP is merged and additional structure is built, the complement of C is expected to be inaccessible to subsequent operations, meaning that movement of a phrase from inside of a phase is impossible once the phase edge has been (derivationally) passed. This rules out a MLCP-style analysis of successive-cyclic movement, however, necessitating that movement of the wh-phrase in (13) must be initiated by some feature at the level of the embedded CP.

However, there is no obvious wh-feature at embedded CP in (13) that might be motivating movement to that intermediate position. Furthermore, Boeckx (2003); Bošković (2002, 2007) provide a number of empirical and conceptual arguments against the feature-checking in intermediate positions. One empirical argument comes from ellipsis, which has been argued to only be licensed by heads that have undergone a feature-checking operation. The lack of feature-checking is why the non-agreeing functional heads *the* and *that* cannot license ellipsis.

(15) a. John liked Mary and [$_{\text{IP}}$ Peter$_i$ [$_{\text{I}'}$ did t_i ~~like Mary~~]] too.
      b. John's talk about the economy was interesting but [$_{\text{DP}}$ Bill [$_{\text{D}'}$'s ~~talk about the economy~~]] was boring.
      c. *A single student came to the class because [$_{\text{DP}}$ [$_{\text{D}'}$ the ~~student~~]] thought that it was important.
      d. John met someone but I don't know [CP who$_i$ [$_{\text{C}'}$ C ~~John met t_i~~]].
      e. *John believes C/that Peter met someone but I don't think [$_{\text{CP}}$ [$_{\text{C}'}$ C/that ~~Peter met someone~~]].

Bošković (1997) shows that intermediate C heads cannot license ellipsis, as shown in (16); based on the pattern above, this suggests that intermediate C does not participate in any feature checking.

(16) a. *John met someone but I don't know who$_i$ Peter said [$_{\text{CP}}$ t_i [$_{\text{C}'}$ C/that ~~John met t_i~~]].
      b. *I know who Mary said C/that John met, but I don't know who$_i$ Peter said [$_{\text{CP}}$ t_i [$_{\text{C}'}$ C/that ~~John met t_i~~]].

We don't fully summarize the all of the arguments against intermediate feature-checking: we refer reader to the cited literature for details. But Bošković concludes based on the ellipsis evidence (among a range of other arguments) that there is in fact no feature-checking at the intermediate CP, and likewise that there is not intermediate feature-checking in nonfinite TP in instances of successive-cyclic A-movement. Based, on this evidence, "we cannot accept Chomsky's (2000a; 2001) theory of successive-cyclic movement, which relies on intermediate feature checking. The available alternative is Takahashi's (1994) approach, which does not use intermediate feature checking, but relies on the operation Form Chain [along with the MLCP], which is not needed under Chomsky's approach" (Bošković, 2007, 607). Furthermore, any standard approaches to phases causes major problems for a MLCP-approach to deriving successive-cyclic

movement (given that movement needs to be initiated before the matrix CP is generated to avoid a PIC violation).

This clearly articulates the look-ahead problem raised by standard instances of successive-cyclic movement, going back to our initial example.

(17)   What$_k$ do you think [CP ~~what$_k$~~ [C′ that Alex ate ~~what$_k$~~]]?

Assuming the embedded CP is a phase, *what* must have raised to Spec,CP at the point in the derivation immediately upon merging of the embedded C. But at this point, there is no feature checking at C motivating that movement, and there is no interrogative matrix C merged yet. So the only reason that *what* moves there is because it must be in that position in order to be accessible once the interrogative matrix C is merged. But the computational system as constructed cannot accommodate this, as this requires *what* to look ahead in the derivation and to move based on what will eventually be necessary, rather that what structures have been built at the time that it moves.

Bošković (2007) does offer a solution here, which has been influential but is by no means universally adopted. Specifically, he claims that uninterpretable features on moving elements (for example, [uQ] or [uWH] on the wh-phrase) is what motivates a syntactic object to move, and *not* the properties of the target position of movement (in contrast to prevailing assumptions). Specifically, uninterpretable features cannot be valued as the Goal of an Agree relation, but must always serve as probes, c-commanded their valuer. As a result, any structurally lower phrase undergoes "greedy" movement in search of valuation. In the end, he is able to discard a number of previously-proposed UG principles, most of which go beyond our core concerns here. But the point is that "[t]he resulting system is characterized by strong restrictions regarding when a feature is checked by Move and when by pure Agree, Move being moving-element driven and Agree target driven" (Bošković, 2007, 634).

In some ways this clearly does solve the look-ahead problem, without needing to adopt a whole-sentence operation like Form Chain: in (17) it is evaluatable within the embedded CP phase whether or not the features of the wh-phrase will be checked. Since they will not be, movement of the wh-phrase is motivated, as movement operations are driven by properties of the moved elements. This still raises questions, of course, about the complexity of the computational system; in a target-drive system like the widely-adopted assumptions of Chomsky (2001), the only computational operations that are necessary are Merge and Agree: the necessity of feature-checking is established at the point of Merging new functional heads, and features that fail to be valued cause the derivation to crash. In contrast, a system like Bošković's that requires persistent evaluation of whether features have been checked introduces its own computational complexity, as that "checking for unchecked features in the relevant domain" either is performed constantly, or performed at the phase edge, both of which are their own kind of counter-cyclicity, albeit not a look-ahead problem.

In practice, many syntacticians still assume target-driven movement. Likewise, people often adopt the working assumption that there is in fact checking of features in intermediate positions. Van Urk (2015) argues quite directly that "the right analysis of Dinka [successive-cyclic movement] requires that intermediate movement is established by the same mechanisms as movement to the landing site: Merge triggered by an Agree relationship with a probe" (20).

But even if a particular language (like Dinka) does provide evidence for intermediate feature-checking, this leaves the hard question of why intermediate positions should require feature-checking at all, if movement to those positions is largely driven by the requirement to derive sentences by phases.

There are many excellent ideas about how to cope with the look-ahead problems posed by successive-cyclic movement; we think it accurate to say that the field has not collectively settled on any single explanation as consistent and reliable, however. And look-ahead problems pop up with some frequency; they are not contained to this particular empirical puzzle. This is simple a prominent and commonly-recognized one. So just as we have the "too-late" counter-cyclicity of Late Merger, we also have not escaped the "too-early" counter-cyclicity of look-ahead problems.

3   What is (counter)cyclicity, then?

This brings us back to our main question: what is counter-cyclicity, and why does it exist? As we've said previously, rather than reject counter-cyclic analysis on the grounds of the lack of compliance with the strictest version of the SMT, in this monograph we entertain the possibility that counter-cyclic operations are a real property of human language syntax. Most centrally, they are not a failure of analysis, but moreso, we are also going to claim that they need not be derived from previously-accepted first principles (cyclic Merge and Agree).[7] This does not deny the existence of bottom-up structure building: without that as a theoretical construct, the idea of counter-cyclicity is informulable. Instead, we want to say that a real property of human language is both cyclic processes and counter-cyclic processes.

The question of what counter-cyclicity is, once one accepts its empirical existence (even if only for the sake of argument), raises equally the opposite question: what is cyclicity, then? On the strict Chomskyan approach (Chomsky, 2000a, 2001, 2019; Hauser et al., 2002), structure-building by cyclic merge is the essence of UG itself, so there's no need for additional explanation. An approach that allows counter-cyclic analysis re-raises the question, however, of what role Merge-based structure building has in the first place (if it is not the only mechanism for building structure and initiating operations).

As will be the case with most of our proposals in this paper, we don't attempt to re-analyze most existing analyses. Rather, our proposal is to reinterpret them. So, the question here is: assume Late Merger is possible (with a formulation along the lines as has previously been proposed) and suppose that look-ahead operations do exist, either in the literal "look-ahead" sense, or in a sense more like that of Bošković (2007), where there is no look-ahead beyond the current phase, but the moving element (or the computational system) is able to evaluate the lack of feature-checking of that element, and institute a Move operation. Are there any constraints on

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[7]These problems have been known for the entire 20 years we have been operating within this version of the Minimalist Program. Scientific fields have lived with similar-appearing intractable problems for much longer than that, but we are willing to accept at this point that the operations of Merge and Agree as currently widely accepted are not going to be able to generate these counter-cyclic constructions, and furthermore, that any solutions that are on offer for a given construction (even if they solve that construction's particular issue of counter-cyclicity) will most likely necessarily complicate the model in violation of the SMT.

these operations? What determines which structure-building operation occurs, and when?

Our claim will be that, similar to core Minimalist theorizing, that Merge is the canonical structure building operation. Counter-cyclic operations are instances where for one reason or another there is a mismatch in acquisition timeline between the counter-cyclic structure and the core phrase structure, and these mismatched timelines, due to the nature of acquisition, are retained in adult grammatical knowledge. In what follows, we won't propose a specific theory restricting these counter-cyclic operations (though we do present some ideas). Rather, what we will show is that we can systematically correlate instances where they occur to a parallel set of empirical phenomena (timelines of acquisition by children). This provides two things: first, it provides extra-grammatical confirmation that counter-cyclicity is a real syntactic phenomenon, and 2) it provides a direction of analysis for restricting the application of counter-cyclic processes.

Beyond that, it is also "the exception that proves the rule," in the sense that counter-cyclic processes demonstrate that a bottom-up approach to acquisition has value, but even beyond that, they provide a strong argument for systematic correspondences between acquisition timelines and adult grammatical knowledge, which we articulated as (4), repeated here as (18).

(18) **Developmental Minimalist Syntax (an interpretive principle)**
      The Minimalist derivation of adult language structures recapitulates the ontogenetic development of those same syntactic structures.

The next chapter builds the argument that (4)/(18) is reasonable and well-founded based on both empirical evidence, and previous theoretical proposals. Our discussions of counter-cyclicity then follow in Chapter 3.

Chapter 2

Developmental Minimalist Syntax

Michael Diercks

Madeline Bossi

As we introduced in chapter 1, we refer to our core proposal (Developmental Minimalist Syntax, DMS) as an interpretive principle. It is not a new theory of syntax. The central goal of DMS is not to propose changes to Minimalist syntactic theorizing, as the Minimalist Program successfully serves as a unifying theoretical framework and offers heuristic devices for a broad range of research within and between languages. Rather, DMS offers a new way of looking at existing Minimalist analyses, and Minimalist syntactic theory in general. We propose that Minimalist analyses (merge-based derivations) of adult language actually encode (i.e. preserve and exemplify) the sequence in which children draw generalizations about their native language while acquiring it. The proposal centrally relies on several 'stable postulates' of current Minimalist theorizing, some of which predate the MP. These stable postulates are the components of Minimalist generative models that apply so broadly that there is little-to-no theoretical contention surrounding them, to the point that most Minimalist theoreticians simply take them for granted:[1]

(19)   a.   Syntactic structures are built via recursive Merge in a bottom-up fashion (Chomsky 1995, 2000a, 2001). Therefore the model of a sentence is not just a single structure, but a sequence of operations that builds a structure.

        b.   Broadly speaking, VP < vP < AspP < TP < CP.[2]

        c.   More specifically, a universal (or nearly-universal) hierarchy of functional categories exists cross-linguistically (Cinque 1999; Ramchand and Svenonius 2014; Wiltschko 2014).

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[1]We of course don't mean to imply that there are no remaining questions related to any of these topics: particularly, the 'how' and 'why' of phases are still under healthy debate. What we mean to say, though, is that these assumptions are extraordinarily stable, given the extremely fluid state of theorizing in many other areas.

[2]A significant caveat to this is that in real terms this isn't actually true: the more accurate claim is that most of those projections are language-specific realizations of domains that are themselves more abstract, and more general, as proposed by Ritter and Wiltschko (2009, 2014) and Wiltschko (2014). For the time being we focus on these particular instantiations as they are the most broadly used among Minimalist syntacticians.

d. Syntactic movement is *upward*, as modeled by the Copy Theory of Movement (Chomsky 2000a; Nunes 2004).

e. Syntactic structures are derived by phase (Chomsky 2001, 2008).

These stable postulates could possibly be viewed as theoretical conveniences; that is to say, they may or may not have any direct connection to real world phenomena, they could just make our theory work well. This is the general assumption underlying the 'metaphor story' often given to students or non-specialists about why prevailing syntactic models build structures bottom-up or why phases are related to computational efficiency (even though syntacticians model competence, not performance). If these aspects of the model are essentially metaphor, they provide an internally-consistent story as to how different aspects of human language syntax relate to each other, but they do not need to have any extra-grammatical correlates.

On the other hand, we might interpret the stable postulates in (19) more literally, assuming that they do indeed relate to grammar-external phenomena in some direct way. The claim that we advance is that all of these stable postulates so in fact have an additional empirical correlate: the timeline of child language acquisition.

1   Introducing Developmental Minimalist Syntax (DMS)

1.1   Core principles of DMS

This section outlines our claims about Developmental Minimalist Syntax. In the next section we will outline how some of these principles have direct precursors from previous work on acquisition, but for ease of exposition we first focus on the framework of assumptions that we adopt. We will claim the following: 1) theories of UG describe how children grammaticalize the patterns they are learning, and therefore 2) acquisition of syntax proceeds unidirectionally with new structures added on top of existing structures (with a few principled exceptions), with the result that 3) the sequence of bottom-up structure building per the MP correlates with the timeline of acquisition of those structures in ontogeny. That is to say, adult syntactic structure building (evident in the derivation of a sentence adult syntactic knowledge) necessarily recapitulates acquisition pathways; the nature of UG as a Language Acquisition Device necessitates this, because (crucially) previous stages of knowledge are incorporated, not forgotten. As we will see, this has consequence for how we think about aspects of Minimalist syntax, e.g. movement and phases, among other things.

Recall from (4) above the Interpretive Principle defining DMS, repeated here as (20).

(20) **Developmental Minimalist Syntax (an interpretive principle)**
The Minimalist derivation of adult language structures recapitulates the development of those same syntactic structures in ontogeny.

We have claimed that this is an interpretive principle; what we are proposing is that there is a systematic correlation between the Minimalist derivation that composes an adult's knowledge of a grammatical sentence in their language. But why would such a correlation hold? We outline

what we believe must be true in order for this to hold in the principles that follow below. This will clarify our assumptions about why (4)/(20) might hold, but will also allow us to be precise in stating the predictions of DMS.

(21) DMS Principle #1:
      The theory of Universal Grammar (composed of at least Merge and Agree) is effectively a description of the nature of grammaticalization in language acquisition.

Principle #1 in (21) makes clear why acquisition must proceed in this fashion. While UG has been traditionally thought of as the Language Acquisition Device, we should perhaps be more precise here and refer to it as the Grammar Acquisition Device.[3] The statistical learning that is clearly at play in language acquisition does not undermine the existence of a grammar-forming operation (Lidz and Gagliardi, 2015). It is our understanding that most usage-based, constructionist approaches to syntax assume that after stages of learning patterns using general statistical learning abilities, that there is a step from this pattern-based knowledge wherein a generalization is formed, and the structure is 'grammaticalized' into an abstraction (Tomasello, 2003). In effect, what we are claiming is that a good many of the acquisition pathways described by Tomasello (2003) can hold in directly the manner he proposes. That doesn't eliminate the need for generalizing to an abstract structure, however, and we claim that this generalization happens in a very specific way, one that has been under investigation by generative syntacticians for a good 60 years: the operations inherent in UG. We discuss correlations and distinctions with constructionist accounts in §4.[4]

(22) DMS Principle #2:
      In acquisition, new syntactic structures typically incorporate existing structures (with some principled exceptions).

Principle 2 directly follows from Principle 1, assuming a standard approach to Merge and Agree (i.e. one which hews relatively closely to those proposed by Chomsky 2001). We claim that the standard method of acquiring (i.e. grammaticalizing) additional syntactic structure is one of expansion rather than replacement. This contrasts starkly with the method of acquisition proposed in Tomasello (2003); per Tomasello, children pass through stages of language acquisition, but new generalizations effectively replace older generalizations. In this respect, then, DMS adopts a similar approach to that in Progovac (2015) about the evolution of human syntax (see §2.4). In brief, according to this conception of language acquisition, newer structures are built upon the foundation of older structures and tampering with these developmentally older structures is either forbidden, or restricted: we will claim it is the latter (cf. Chomsky's 2000a Extension Condition).

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[3]We will note that while we continue to use the 'UG' terminology, as this is appropriate for the proposed (universal) theoretical constructs underlying syntactic grammar, nothing in our proposal here requires UG to be language-specific. It is not implausible, for example, to think that Merge and Agree might be language-specific applications of general cognitive abilities. As far as we can tell, nothing here rests on the 'language-specific' component of traditional theories of Universal Grammar.

[4]As we mentioned elsewhere, while he doesn't remember his offhanded comment, this partial-synthesis between Constructionist approaches and Minimalist theorizing was first suggested to the first author by Brent Henderson many years ago.

(23)   DMS Principle #3:
       The sequence of structure building in the Minimalist derivation of a sentence correlates
       with the timeline of acquisition.

The principle in (23) likewise follows from (21) and (22). If UG drives grammaticalization of syntactic structures in acquisition, typically following the 'expanding, not revising' pathway necessitated by typical assumptions within the MP regarding the nature of UG (i.e. Merge/Agree), we necessarily end up with adult syntactic structures that themselves realize not only the final sequences of words and morphemes, but that knowledge is also composed of a sequence of stages, stages that are historical records of earlier stages of the child's knowledge. This is why we introduced the term *ontogenetic fossils* above, adapted from Progovac's (2015) use of the fossilization metaphor: those structurally lower structures (including unpronounced copies of various syntactic objects) are a fossilized remnant of the organism's earlier stages of syntactic competence. This claim makes clear, testable predictions about the relative order of acquisition of different types of syntactic structures, which we show in §3 are largely supported.

(24)   DMS Principle #4:
       Syntactic movement is reanalysis.

As we've stated above, different stages in the derivation of a sentence represent different stages in the development of a child's linguistic competence. This offers a direct explanation for the nature of syntactic movement, as proposed in the Minimalist Program and its antecedents. For example, the base position of an agentive subject is widely assumed to be in Spec,*v*P, even though in most languages its final (surface) position is somewhere higher than this. The account developed here proposes that this cross-linguistic generalization is possible precisely because Spec,*v*P is the first place the child grammaticalizes (agentive) subjects, while the position of the subject after movement is a revised generalization about the subject position that the child reaches upon acquiring higher syntactic structures. This derives the *v*P-internal subject hypothesis (Koopman and Sportiche 1991) without additional stipulation.

The *v*P-internal subject hypothesis becomes thoroughly unsurprising if children acquire verbs and their participants first (regardless of the target language)—which §3 shows is the widely reported pattern—the universality of the assignment of theta roles to a specific *v*P structure is derived from more foundational principles. This account fits nicely with the Copy Theory of Movement (i.e. moved constituents are actually copied in different positions and the constituent's phonological features are usually deleted in all but one case: Chomsky 2001; Nunes 2004). Given this formalization, under DMS, movement can be viewed as re-grammaticalization of an developmentally-previous generalization into a new generalization. As children grammaticalize additional syntactic structure and are exposed to more and more unambiguous primary linguistic data which indicate that some constituent (e.g. a subject) ought to be structurally higher than where it was first grammaticalized, they form a new generalization. The result, as is well-known in modern syntactic theory, is that the subject of the sentence is represented in two positions in adult grammatical knowledge: in Spec,TP and in Spec,*v*P.[5]

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[5]Again, this is a blatant oversimplification, but it serves our specific purposes here and is accurate insofar as our current concerns.

Interestingly, this way of interpreting movement offers some explanation for the claim made in older acquisition literature that movement is "harder" for children to grasp and is therefore acquired later (see, e.g., Klima and Bellugi 1966). Although the empirical finding still holds, rather than relying on some conception of cognitive difficulty to account for this delay, DMS in fact predicts movement to become productive only after the appearance of constituents that are targeted for movement (i.e. the tails of movement chains). And movement itself can only occur when children grammaticalize the syntactic structure that serves as the landing site of the movement process; in other words, if children have not yet grammaticalized TP, movement of the subject from Spec,*v*P to Spec,TP obviously cannot occur. In this way, DMS predicts that subjects of predicates ought to appear before subject A-movement to Spec,TP. So movement is not necessarily more cognitively burdensome (therefore slowing acquisition), but rather it simply requires more existing knowledge, making it a subsequent stage of acquisition. The DMS interpretation of movement also derives the 'mid-level result' noted in Svenonius (2016) that movement is upward; that is to say, a constituent's base position is structurally lower than its target position. Because newer structure is built on top of older structure, the directionality of movement (i.e. re-grammaticalization) is always upwards.

A significant aspect of the proposals here is that we assume, like many Minimalist syntacticians do nowadays, that many properties of human language are emergent (i.e. not encoded in UG), including syntactic categories (25) and parameters (26).

(25)   DMS Principle #5:
       Syntactic categories are emergent (Wiltschko, 2014)

(26)   DMS Principle #6:
       Parameters are emergent (Roberts 2019, among others)

We don't discuss these implications in much depth in this monograph, though they are certainly worthy of discussion and they are at odds with what is often claimed by and/or attributed to generative language acquisition researchers (see Ambridge et al. 2014 for a discussion, especially pages e53-e55). Given the fluidity of generative syntactic theoretical claims, it's no surprise that there is large variance at what acquisition researchers (both generativists and non-generativists) assume to be a part of theories of UG. For our part, we work hard to stay close to the main goals of this paper: to argue for a specific interpretive principle of Minimalist analyses of adult grammar, one which makes predictions about child language acquisition, and which certainly becomes a theory of acquisition of syntax. But there are many large debates especially about what is universal, and what is innate, and what is not, and we are ill-prepared to make strong contributions to that debate, for the most part. Rather, what we intend to so is argue for the systematic correlation of acquisition sequences and Minimalist analyses of adult grammars (as implemented by the everyday working grammarian). We are relatively agnostic about the canonical polemics about UG, especially regarding how much of the cognitive mechanisms building syntax is language-specific, and how much is domain general. We will do our best to be cautious about such claims. Nonetheless, principles like (25) and (26) fall out rather directly from the proposal that we sketch here.

1.2 Previewing some nuances in DMS

As we will discuss briefly in §3 of this chapter and in more depth in Chapter 3, there are a number of facts about acquisition that don't directly align with the proposed correlations in DMS Principles 1-6. What we will propose, however, is that many of these non-alignments are not arbitrary, and in fact have direct correlations in adult grammars:

(27)   DMS Principle #7:
        Counter-cyclic phenomena in adult language grammars correlate to counter-cyclic acquisition processes.

(28)   DMS Principle #8:
        Counter-cyclic syntactic operations exist.

The implication of (27) is of course (28): empirically-speaking, counter-cyclic constructions themselves exist too (and are not simply failures of analysis).

In addition, we will propose another systematic correlation between adult grammars and child acquisition pathways: specifically, we claim that children proceed by a specific set of referential tasks that occur in a specific sequence. These referential tasks are what (in adult grammar) end up correlating to phases (Chomsky, 2001).

(29)   DMS Principle #9:
        Phases are acquisition workspaces.

In this way, phases themselves are ontogenetic fossils as well. Each of Principles 7-9 will take extensive evidence and argumentation to construct and defend; furthermore, they are necessarily dependent on the proposals in Principles 1-6 themselves being viable. As such, there is a long path to defending these.

Recall, however, the specific phrasing of our central claim in this work, the definition of the interpretive principle of Developmental Minimalist Syntax (first in (4) and repeated in (20)): The Minimalist derivation of adult language structures recapitulates the ontogenetic development of those same syntactic structures. Even though Principles 7-9 complicate the picture presented by just Principles 1-6, they all adhere to (4) quite directly. If these proposals hold up, the outcome will be that there are many direct correlations in the grammar of adult languages and the timelines of acquisition; children's acquisition mechanisms are of a character such that adult's grammatical structures encode the pathways (cyclic, and counter-cyclic) by which they arrived at those grammatical conclusions as children.

1.3 Reflections on the SMT

It's worth reflecting on the ways in which this complicates (and the ways in which it doesn't complicate) the foundational desiderata of the Minimalist Program.

The shared goal is to formulate in a clear and useful way—and to the extent possible
to answer—a fundamental question of the study of language, which until recently

> could hardly be considered seriously and may still be premature: to what extent is the human faculty of language FL an optimal solution to minimal design specifications, conditions that must be satisfied for language L to be usable at all? We may think of these specifications as "legibility conditions": for each language L (a state of FL), the expressions generated by L must be "legible" to systems that access these objects at the interface between FL and external systems—external to FL, internal to the person. (Chomsky, 2001, 1)

The interfaces in question are PF (Phonological Form) and LF (Logical Form), roughly, the interfaces with the system for pronunciation and the system for meaning (semantics), respectively.[6] The Minimalist conjecture, then, is that the simplest proposed system that can make language interpretible at these two interfaces is the best candidate for UG (Universal Grammar), the genetically-endowed cognitive mechanism that generates human language.

> The strongest minimalist thesis SMT would hold that language is an optimal solution to such conditions. The SMT, or a weaker version, becomes an empirical thesis insofar as we are able to determine interface conditions and to clarify notions of "good design." (Chomsky, 2001, 1)

Chomsky (2001) continued:

> Tenable or not, the SMT sets an appropriate standard for true explanation: anything that falls short is to that extent descriptive, introducing mechanisms that would not be found in a "more perfect" system satisfying only legibility conditions. If empirical evidence requires mechanisms that are "imperfections," they call for some independent account: perhaps path-dependent evolutionary history, properties of the brain, or some other source. It is worthwhile to keep this standard of explanation in mind whether or not some version of a minimalist thesis turns out to be valid. (Chomsky, 2001, 1)

Often, "descriptive" here is taken by syntactians to mean something like, "not yet the full explanation," that is, descriptive of a phenomenon but not of UG. We differ here: certainly, our proposals here to allow counter-cyclic operations are a divergence from the SMT as developed by Chomsky, and as such can be classified as "descriptive." But our stance is that they are in fact descriptive of UG, descriptive of the core grammaticalization mechanisms. And while they might not be the idealized UG that Chomsky sketched in Chomsky (2001), Chomsky himself noted that "the SMT cannot be seriously entertained" (Chomsky, 2001, 1). The analytical problem, of course, is when to accept a solution we've arrive at as final, vs. when to probe whether there is a deeper explanation.

Our read of the landscape is that, despite the field's best efforts, counter-cyclicity is here to stay. And specifically, it's here to stay because it is in fact a part of FL. Counter-cyclicity, on this approach, is a theoretical and empirical reality about human language. That said, in some senses, the SMT as Chomsky presents it is probably spot-on. In many senses, the model we develop here does not diverge at all from core Minimalist theory (which is why our proposal is framed as an interpretive principle: it is not a new theory of syntax). It does, in fact, appear that children,

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[6]In other works Chomsky referred to these as the Sensorimotor interface (SM) and the Conceptual-Intentional interface (CI).

in acquisition, are optimizing form-meaning correspondences. The main complicating factor is that SMT cannot hold for adult grammars as a whole, as practicioners of the Minimalist Program tend to assume. Rather, the SMT seems more likely to be descriptive of children's assumptions in approaching language: that direct form-meaning correspondences can be discerned, and can be learned. This is why the Minimalist approach to syntax appears to model acquisition so well in these respects: the minimal operations that have been proposed capture most of grammatical structures precisely because they model UG well. The assumption that needs to be added is that these mechanisms are in fact grammaticalization mechanisms, so the applications of the SMT are necessarily dependent on what information is available to a child at the point that a piece of syntactic structure is being acquired.

Essentially, what is missing is what generative syntax is often derided for: where is the human? where is the mind? Minimalist syntax deals mainly in abstraction. But it is our claim that in that abstraction we have found hierarchical patterns of logical dependencies that, in fact, directly derive from sequences of acquisition.

2   Precedents for DMS

2.1   Introduction to Organic Grammar

In this section we briefly sketch the main conceptual claims of *Organic Grammar* (Vainikka and Young-Scholten, 2007, 2011, 2013), a proposal that shares many properties with our own, here, and which supplies some necessary theoretical constructs that we ourselves will also adopt. As far as we can tell, these proposals have received limited attention in the acquisition literature and no attention in the syntactic literature, which we think to be in error. Our proposals for DMS hew more closely to Minimalist syntactic theories; Organic Grammar has been proposed as a separate theory of language, which we believe to be an unnecessary step. Nonetheless, we will see that there is strong theoretical precedent (as well as empirical support in §3) for our proposals about Derivational Minimalist Syntax.

It has long been observed that a simplistic, highly specific theory of Universal Grammar makes much too restrictive predictions about children's acquisition pathways, which are highly variable and highly affected by input (Ibbotson and Tomasello 2016 and Tomasello 2003 offer some arguments to this effect). And the successes of statistical approaches to modeling child language acquisition are well known (Tomasello, 2003; Lidz and Gagliardi, 2015; Pearl and Goldwater, 2016).

> Children certainly vary in their pace of development, and they also vary in their vocabulary size, volubility and articulation. Yet when it comes to core grammar - to basic syntax - even though the paths which children take are indirect, numerous studies have pointed to stepping stones along these paths (children's early and non-adult grammars) which are predictable for a given language also share characteristics across languages. (Vainikka and Young-Scholten, 2011, 2)

> The basic idea of [Organic Grammar] is that the child (or second language learner, as we shall see) begins with the 'core' of the sentence, the VP (the verb phrase: the

main/thematic verb and its arguments, e.g. a direct object). The child then acquires further segments of the tree during development. Once all of these have been acquired - around the age of three or so - the child's (subconscious) grammar can be said to represent the full adult structure. Since each segment of the tree (or technically, 'functional projection') that is acquired remains in the tree unchanged when the next piece is added, there is a straightforward connection between the child's acquisition process and the final tree. This connection is fundamental to Organic Grammar[.] (Vainikka and Young-Scholten, 2011)

This approach shares many similarities with DMS, specifically the direct correlation between acquisition and bottom-up structure building in adult grammars. Vainikka and Young-Scholten nonetheless consistently present Organic Grammar as an alternative to Minimalism, rather than compatible with it.

Part of why they do so is that they assume as a tenet of Minimalism some of the more extreme versions of the cartographic enterprise, i.e. that UG consists of a fixed, fully articulated syntactic structure that is innate. This is (perhaps most dramatically) realized in Cinque's functional hierarchy of the clause, which was proposed on the basis of a a variety of strong evidence for a universe hierarchy of adverb positions cross-linguistically. Cinque proposed that the structure in (30) represents a universal, innate set of syntactic structures.

(30)   The universal hierarchy of clausal functional projections (Cinque, 1999, 106)
[*frankly* Mood$_{speech\,act}$ [*fortunately* Mood$_{evaluative}$ [*allegedly* Mood$_{evidential}$ [*probably* Mod$_{epistemic}$ [*once* T(Past) [*then* T(Future) [*perhaps* Mood$_{irrealis}$ [*necessarily* Mod$_{necessity}$ [*possibly* Mod$_{necessity}$ [*possibly* Mod$_{possibility}$ [*usually* Asp$_{habitual}$ [*again* Asp$_{repetitive(I)}$ [*often* Asp$_{frequentive(I)}$ [*intentionally* Mod$_{volitional}$ [*quickly* Asp$_{celerative(I)}$ [*already* T(anterior) [*no longer* Asp$_{terminative}$ [*still* Asp$_{continuative}$ [*always* Asp$_{perfect(?)}$ [*just* Asp$_{retrospective}$ [*soon* Asp$_{proximative}$ [*briefly* Asp$_{durative}$ [*characteristically(?)* Asp$_{generic/progressive}$ [*almost* Asp$_{prospective}$ [*completely* Asp$_{SgCompletive(I)}$ [*tutto* Asp$_{PlCompletive}$ [*well* Voice [*fast/early* Asp$_{celerative(II)}$ [*again* Asp$_{repetitive(II)}$ [*often* Asp$_{frequentative(II)}$ [*completely* Asp$_{SgCompletive(II)}$

Vainikka and Young-Scholten (2011) distinguish the approach of Organic Grammar from the proposal of universal innate structures like (30) specifically because they claim that syntactic structures are emergent, and that children's non-adultlike stages of grammar can in fact represent partial stages of target-like grammaticalization, in just the way we propose for DMS.

Of course, the assumption that UG is extremely rich in content (as in (30)) in many ways runs directly counter to the core assumptions of the Minimalist Program. And in fact, a large number of adherents to Minimalist theories find a proposal like (30) (as a part of UG) to be deeply troubling. Ernst (2014) suggests that these adverb hierarchies can be generated by the assumption that adverbs adjoin to points in the clause structure based on their semantics, with the cross-linguistic similarities in adverb ordering emerging on the basis of the relatively stable semantics of different clausal heights across languages. While itself reasonable, this raises itself the question of why there is such consistent clause structure across languages. We don't share the assumption of Vainikka and Young-Scholten (2011) that a Minimalist approach necessitates assumption of a content-rich Universal Grammar, and that in fact, quite to opposite effect, many practicioners assume that a universal hierarchy similar to (30) is real, but emerges from more fundamental

aspects of language (see Ritter and Wiltschko 2014, Wiltschko 2014, Ramchand and Svenonius 2014).

Vainikka and Young-Scholten (2011) argue that Organic Grammar is a theory that shares features of Minimalism, but which needs to be developed as a distinct entity. We are more optimistic that Minimalism (as widely practiced today) is actually wholly compatible with an emergent view of most grammatical structures, especially if considered in a perspective where acquisition and child development can be taken into account. This is why we have posited our proposal as an interpretive principle: Developmental Minimalist Syntax, on our proposal, is not a new syntactic theory, but is instead a way of interpreting Minimalist syntactic analyses with a broader cognitive lens.

2.2   Core Principles of Organic Grammar

Vainikka and Young-Scholten (2011) lay out 10 core assumptions of Organic Grammar, that establish the major principles necessary to understand the framework. We overview the first 7 here, which focus on the core issues of concern for us.

(31)   Organic Grammar Assumption 1
Each language has a Master Tree that includes all possible projections occurring in that language.

Organic Grammar assumes that when children are acquiring knowledge of their language, they are building a centralized repository of grammatical knowledge that makes clear the set of functional projections contained in their language. We adopt a similar assumption, though (as we discuss below) we will assume that it may be more appropriate to talk about an inventory of master trees for different constructions (minimally including noun phrases and whole finite clauses, and but plausibly including a large range of additional constructions).[7]

(32)   Organic Grammar Assumption 2
All and only those projections occur in the Master Tree for which there is evidence in the language.

As we mentioned above, while Vainikka and Young-Scholten (2011) considered this a departure from Minimalist assumptions, we suspect most Minimalist syntacticians assume something quite similar to this in their own work already. We assume the same, as a part of DMS.

(33)   Organic Grammar Assumption 3
Universal Grammar provides the tools for acquiring the Master Tree, based on input.

This again is directly parallel to DMS, though we are inclined to make the even stronger claim that the entire content of UG is the cognitive mechanisms for grammaticalizing input, i.e. taking input and forming generalizations that build the Master Tree(s). This is our assumption

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[7]As we discuss in §5, this is largely equivalent to the Construction Grammar theoretical equivalent of the lexicon that is designated for constructions, the 'constructicon.'

that UG is essentially a Grammar Acquisition Device, the mechanisms responsible for grammaticalizing input.

(34) Organic Grammar Assumption 4
      The Master Tree is acquired from the bottom up.

(35) Organic Grammar Assumption 5
      The Acquisition-Syntax Correspondence: syntax mirrors acquisition.

Again, these concepts are familiar to the reader from our introduction to Developmental Minimalist Syntax. As we have articulated it, adult language knowledge specifically contains earlier stages of knowledge, precisely because of the nature of Merge that constrains the nature of generalizations that children make about syntactic structures. Therefore we share these assumptions, but they arise specifically due to the properties of Merge (and its role in acquiring and grammaticalizing language).

(36) Organic Grammar Assumption 6
      Actual instantiations of the tree are projected from the bottom up, based on the Master Tree.

This is essentially the claim that knowledge of individual sentences has the same grammatical properties as knowledge of the Master Tree. What an "instantiation" of a tree is is an important question (presumably, the knowledge a speaker constructs/reference in offering an offline judgment of the acceptability/grammaticality of a sentence in their language).[8]

(37) Organic Grammar Assumption 7
      Partial trees may be projected for constructions which do not involve the full Master Tree structure.

This, again, is a quite common Minimalist assumption, including the presence of defective clauses, nonfinite clauses, ECM complements, small clauses, etc. In short, particular constructions within a language ought to be consistent with the Master Tree(s), but need not always include the entire Master Tree(s).

While we don't explore second language acquisition here, Vainikka and Young-Scholten (2011) specifically claim that UG functions the same across the lifespan of an organism, and that Organic Grammar functions as a theory of (naturalistic, non-instructed) second language acquisition, in addition to first language acquisition. We don't explore this possibility here, but they offer extensive evidence and argumentation in support of this claim.

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[8]This raises many questions in our minds, specifically about the relationship between knowledge of grammar and processing (perception/production) of language, as well how specifically speakers use existing (general) knowledge of their language to generate syntactic judgments about particular sentences. This goes far outside our domains of expertise.

2.3   (Brief) history of generative ideas about Acquisition of Syntax

There is a long history of ideas in the generative literature on language acquisition that share properties with our own proposals. We summarize the main relevant ideas here, albeit briefly.

The Small Clause Hypothesis (SCH) suggests that children's early grammars contain only maximal projections of lexical heads—that is, small clauses. Children then gradually add the functional categories DET, INFL, and COMP, which are subject to developmental maturation. In his conception of the SCH, Radford (1990, 1992) divides the acquisition process, at least for English-speaking children, into exactly two distinct periods: an earlier lexical stage and a later functional stage. During the lexical stage, children's grammars consist exclusively of maximal projections of lexical categories (e.g. noun, verb, preposition, adjective), whereas in the functional stage, children acquire all functional material essentially simultaneously (i.e. INFL is grammaticalized at the same time as COMP). Although Radford is a main proponent of the SCH, other interpretations of this theory exist (see Guilfoyle and Noonan 1989 and Lebeaux 1988), but they do not necessarily align with the claim that the grammaticalization of INFL and COMP occur simultaneously (see also Meisel and Müller 1992a and Clahsen et al. 1993/1994).

The Full Competence Hypothesis (FCH)—described and defended for child German in Poeppel and Wexler (1993)—holds that a child's grammar is in essence adult-like; production differences between child and adult language result from some type of underspecification of a functional projection. In other words, children's developing grammars contain all the functional projections associated with adult grammar, but the features associated with each functional projection may not be fully acquired. For instance, a child's grammar might contain each and every adult-like functional category (e.g. VP, *v*P, TP, CP), but it might not recognize all the features associated with these projections (e.g. a child might not realize that tense, subject-verb agreement, and nominative Case assignment are all associated with T).

The FCH has been pursued most extensively for early child German, a V2 language in which VP, *v*P, and TP are thought to be head-final and the verb is thought to raise to C (though the head-final status of TP in German has been questioned in, e.g., Zwart 1997 and Vainikka and Young-Scholten 2011). The primary support presented in Poeppel and Wexler (1993) for the FCH arises from the early appearance of V2 patterns in child German. Under the assumption that verbs in second position have raised to C—if VP, *v*P, and TP are all head-final, C is the only possible verb landing site that generates V2—the appearance of V2 structure must entail that the CP level is operative. If the verb were not raising to C (i.e. if it remained in its original position in VP or raised to *v*P or TP), it would be expected to surface clause-finally. As this is not the case, Poeppel and Wexler conclude that children speaking early child German must therefore have "full knowledge of the universal principles and processes that underlie clause structure" (Poeppel and Wexler 1993: 29).

The Agreement Tense Omission Model (ATOM)—proposed by Schütze and Wexler (1996)—represents an analysis that falls between the SCH and the FCH, in that it involves the absence of only certain functional categories. Schütze and Wexler divide the inflectional projection into a series of sub-projections, thus separating issues of tense from issues of agreement. This model of children's early grammars was designed with a specific child language phenomenon in mind, namely the root default phenomenon (i.e. root infinitives). As will be discussed at length in

§3, during the root default stage, children learning a variety of genetically diverse languages optionally mark finiteness on main verbs. Crucially, under an ATOM approach, a higher layer of grammatical structure can exist even if a lower layer is missing; that is to say, CP can still be present in a child's developing grammar, even if the AgrP or TP projection is underspecified (i.e. missing).

Also emerging from that time period was another prominent model of child grammar that likewise falls between the SCH and the FCH with respect to the amount of structure assumed to exist is the Truncation Model proposed in Rizzi (1994). Rizzi assumes that in adult language there is an operative principle that mandates that CP be the root of a clausal structure, ensuring that a sentential structure will always culminate with a CP layer.

The Truncation Model proposes that this root=CP principle is not fully operative in child language. Instead, children can truncate structures at any point below CP. In other words, any category below CP can serve as the root in early child grammar. One would then expect potential truncation sites at any level of structure: non-adult-like truncation can occur at VP, *v*P, TP, or any additional functional projection in between. As Rizzi explains, "If CP is not the compulsory starting point in early grammars, we would expect children to use a much wider variety of root categories, i.e. simple NPs, PPs, APs, (nonfinite) VPs, different kinds of uninflected small clauses, etc." (Rizzi 1994: 165). This prediction is indeed upheld with respect to a variety of children's early linguistic behaviors (e.g. the root default stage, children's first one-word utterances, a.o.). Under the Truncation Model functional projections cannot be omitted from the middle of a structure; rather, all syntactic structure that would surface above the truncation point in adult grammar must be omitted.

Perhaps the closest precedent to our claims is that advanced by Lebeaux (1988), which was later published as Lebeaux (2000) (we restrict our discussion to the published work). While published at the beginning of the Minimalist era, most of Lebeaux's (2000) theoretical structure is from the heart of the Government and Binding era.[9] Nonetheless, Lebeaux lays out a set of claims that are quite closely aligned with DMS as we've presented it here. Lebeaux (2000) proposes the *General Congruence Principle* in (38), which quite similar conceptually to DMS.[10]

(38) **General Congruence Principle**: Levels of grammatical representation correspond to (the output of) acquisitional stages.
(Lebeaux, 2000, 47)

"In acquisition, the grammar is arranged along the lines of *subgrammars*. These grammars are arranged so that the child passes from one to the next, and each succeeding grammar contains the last" (Lebeaux, 2000, xiii). Lebeaux (2000) specifically argues that the entire sentential grammar cannot be acquired bottom-up, however, requiring intermingling of structures: in this way, Lebeaux argues that the then-incipient Minimalist approach to structure building (via Merge) would be insufficient, instead suggesting a collection of alternative operations in adult

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[9]As we will discuss in section 5.3, Lebeaux (2000) argues—erroneously, in our opinion—that his claims are incompatible with a Minimalist approach. However, that may partly be due to the development of those ideas in the earliest stages of the Minimalist Program: it was Chomsky (2001) that truly crystallized the Minimalist approach into the set of assumptions that are largely still used today.

[10]As mentioned in our preface, DMS was developed independently of Lebeaux's work (which we only discovered after refining a lot of our ideas) so it is striking how similar they are.
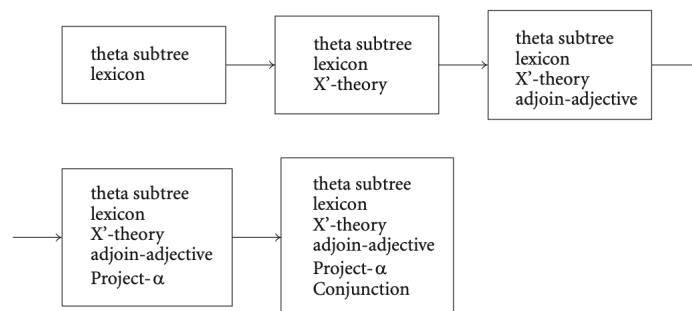
Figure 2.1: The Growth of Children's Grammar, adding operations (Lebeaux, 2003, 288)

grammars, that become operative in a child's grammar in a specific sequence, as illustrated in Figure 2.1. "The acquisition sequence is the addition of licensing relations modelled by generalized transformations, added to the grammar" (Lebeaux, 2003, 288).

Lebeaux (2000) claims that Lebeaux (1988) "played a major role in the coming of the Minimalist Program" as conceived of in the early stages (Chomsky, 1993, 1995), those works abstract away from the acquisition discussion, simply engaging the resulting proposals about the structure of grammar as abstract questions about the theory (mainly around the relative non-cyclicity of adjuncts). And reference to Lebeaux's work is absent in the foundational documents of the contemporary Minimalist framework (Chomsky, 2000a, 2001). There are certainly some references to Lebeaux (2000) and Lebeaux (1988) in more recent acquisition work (e.g. Avram et al. 2015), but from what we can find this tends to reference his discussion of subgrammars, and not a full engagement with his General Congruence Principle.[11] Lebeaux (1988) is extraordinarily broadly cited in reference to questions of Late Merger (see §2 and Chapter 3), but certainly in broader syntactic theorizing the link between acquisition questions and the theoretical proposals of Lebeaux seems to have either been forgotten, or largely set aside. In general, while Lebeaux's General Congruence Principle is conceptually highly similar to DMS, his assumptions about both syntactic acquisition and the structure of adult grammar were (and are) relatively non-standard, which may explain some degree of the lack of impact of the General Congruence Principle on syntactic theorizing. Our claim, however, is that advances in the Minimalist Program have fulfilled the promise of Lebeaux's General Congruence Principle, which can now be articulated relatively transparently as a principle like DMS.

A long history of work by Tom Roeper has drawn careful connections between Minimalist theoretic elements and acquisition as well: Yang and Roeper (2011) offers a good overview. The ideas proposed there include conceptualizing of (Asymmetric) Merge as an acquisition operation: "The important point here is that Asymmetric Merge allows an immediate representation of a child's first utterances and, more importantly, an abstract analytic instrument that enables a child to attack in a simple way what is a very complex set of inputs, before projecting the full array of functional categories (which is not to say that the capacity is absent). The significance of this point should be underlined: a virtue of the abstraction of minimalism is that it reduces the

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[11]A search on Google Scholar for "General Congruence Principle" among papers citing either Lebeaux (1988) or Lebeaux (2000) yields two results, one of which was a paper from Lebeaux himself that was the published version of a chapter of his dissertation.

Primary Linguistic Data problem by giving the child representational tools that allow first-stage efforts to represent linguistic forms whose full feature system has not yet been identified. In that sense, minimalism predicts that Stages can exist" (Yang and Roeper, 2011, 565-566). In a sense, DMS is the logical conclusion of a long line or reasoning from this basic foundation.[12]

2.4 An evolutionary parallel

Aside from Organic Grammar, one of the closest correlates to DMS is not actually found in the acquisitionist literature, but instead in the evolutionary linguistics literature. In her 2015 monograph, Progovac puts forth a gradualist analysis of the evolutionary development of human syntax, which operates on certain principles that are also central to DMS. This section outlines Progovac's argument, drawing attention to the parallels between her conception of the evolution of human syntax and the present developmental proposal.

Progovac divides the evolutionary history of human syntax into four rough stages: the one-word stage, the proto-syntax stage, the proto-coordination stage, and the specific functional category stage. Rather than adopting an all-of-a-sudden, single mutation-style analysis—in which human language sprung from a single genetic mutation—Progovac suggests that modern, hierarchically complex syntax is the result of a series of complementary evolutionary developments.

The one-word stage—as its name suggests—is characterized by single-word utterances such as *Run! Snake! Out!* which stand alone to convey a full communicative intention. In this way, there is no combinatorial power in language and therefore no bona fide syntax at this point in the evolution of human syntax. Although Progovac does not classify this period as a genuine stage in the development of syntax—as it is defined by its lack of syntactic structure—it nevertheless represents an important foundation for the development of more complex utterances.

The next stage in the evolution of human syntax is the proto-syntax stage, in which the first sentences consisting of multiple linguistic elements surfaced. These multi-element structures were paratactic, meaning that they were non-hierarchical, non-headed combinations of two lexical items (e.g. a verb and a noun). In these proto-syntactic structures, the operation Conjoin—rather than the MP's Merge—combines constituents into a single utterance. Conjoin—unlike Merge—generates neither headedness, nor hierarchy which are not thought to exist at this evolutionary stage.[13] Progovac cites exocentric (i.e. non-headed) verb-noun compounds like *cry-baby*, *hunch-back*, *rattle-snake*, etc. as present-day fossils of this stage in the evolution of human syntax. At this stage, only prosody indicates that two constituents or clauses were in fact Conjoined. This proto-syntactic—also known as paratactic—grammar, as well as the grammars of the following stages of language evolution, can operate both clause-internally and clause-externally. This means that during the paratactic proto-syntax stage, two words can be Conjoined or two

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[12]Though it's worth noting that we explicitly do not assume that a child's first utterances are based on a grammar represented via Merge, only the final grammaticalization.

[13]Note the parallelism here between the constructions generated during Progovac's proto-syntax stage and the pivot schemas proposed in Tomasello (2003). Both sorts of constructions are non-hierarchical, non-headed combinations of two terms, which are representative of a relatively primitive stage in development (either evolutionary or ontogenetic development).

(two-word) clauses can be Conjoined; for example, expressions like *Him worry?!* and *Easy come, easy go* were both possible at this stage in the development of human syntax. With respect to expressions like *Him worry?!* Progovac holds that small clauses in the paratactic stage have no structural mechanism for subject Case assignment, causing the subject to surface in its default form (accusative in English). This explains why present-day fossils of the paratactic stage (e.g. *Him worry?!*) show accusative Case marking of the subject.

The next development in the evolution of human syntax was the appearance of proto-coordination. During this evolutionary stage, in addition to prosodic indicators, conjunctions or linkers provided the segmental glue to hold the utterance together. In this way, the evidence for the Conjoin operation is now dual: both prosodic (retained from the previous proto-syntax stage) and segmental (in the form of lexical linkers best approximated by the present-day *and*). Nevertheless, structure in this stage is still syntactically flat, meaning that it involves no hierarchical structure building. Therefore, Move cannot yet occur, since there is no higher structural position for Move to target. This is corroborated by the finding that even today coordination structures constitute islands for Move.

Finally, Progovac posits the the specific functional category stage, which represents the current state of syntactic evolution. It is in this stage that the structural hierarchy (i.e. VP < vP < TP < CP) emerges. In this hierarchical stage, functional categories become available, thus providing "specialized syntactic glue for constituent cohesion" (Progovac 2015: 13). The introduction of hierarchy allows for the activation of a wide array of syntactic phenomena including tense, syntactic embedding, movement, etc. all of which require layered structures. This functional category stage includes all the attainments of the previous stages but adds another: the ability to use the linker—whether prosodic or lexical—to identify the type of the constituent created by Conjoin (now comparable to the MP's Merge due to the introduction of structural hierarchy). In this way, then, Progovac proposes two distinct structure building mechanisms during the specific functional category stage: Conjoin and Merge.

The movement from one evolutionary stage to the next represents a progression from least to most syntactically elaborated (i.e. from no syntax, to paratactic syntax, to coordinated syntax, to hierarchical syntax). Crucially, though, "the advent of a new stage does not obliterate the previous stage(s), but rather ... the older stages continue to co-exist, often in specialized or marginalized roles, in addition to being built into the very foundation of more complex structures" (Progovac 2015: 2). That is to say, structures developed and used during an earlier stage in the evolution of syntax are not simply discarded at the advent of a new stage; rather, these more antiquated structures are incorporated into the newer, more complex structures, both as foundational elements and in additional specialized roles. For instance, the verb-noun small-clause structures typical of the proto-syntax stage are not abandoned, but rather form the basis for future stages in the evolution of human syntax. Structures from earlier evolutionary stages, therefore, constitute syntactic foundation that later structures are built directly on. The notion of building upon the work of previous evolutionary stages—which is central to Progovac's proposal—is in essence what DMS proposes on a developmental, rather than evolutionary timescale.

In this vein, Progovac remarks: "A modern sentence (TP) is built upon the foundation of a proto-syntactic small clause, as if the building of a modern sentence retraces its evolutionary steps" (Progovac 2015: 208). Since under Progovac's model evolutionarily newer structure is

built directly onto evolutionarily older structure, one should therefore be able to see vestiges of these older syntactic stages in more recent developments. In Progovac's proposals, structurally lower (often unrealized) syntactic structures are older: vestiges of an earlier time in evolutionary history of language, which she refers to as *evolutionary fossils*. As mentioned above, Progovac's evolutionary fossils are parallels to the ontogenetic fossils posited under DMS. We think it important to mention that we are not claiming substantive connections between child acquisition and evolutionary development of language: we have nothing to say about the evolution of language. But our work benefited from Progovac's parallel ideas (and Progovac herself suggests that acquisition pathways may be relevant to her claims), so we note these connections here.

2.5   Extending the Evolutionary Account

Progovac's central claims are the same notions that underlie DMS: 1) older syntactic constructions form the foundation for newer syntactic constructions and 2) as a result, the building of a modern sentence retraces its developmental progression. Progovac mentions this parallelism between language evolution and language acquisition: "Let me also point out that in my proposal language evolved through scaffolding/layering, in such a way that the lowest layers served as necessary foundation for the higher layers. The prediction of this proposal is that child language, to the extent that it emerges in stages, has to observe the same scaffolding" (Progovac 2015: 50). In this way, DMS extends the underlying principles of Progovac's theory of the evolution of human syntax to the domain of language acquisition, which Progovac (2015) only comments on, without fully developing. DMS, therefore, offers a more thoroughly principled account of this potential ontogenetic "side effect" of Progovac's approach to human language evolution.

In more recent work, however, Progovac has explicitly extended these concepts more systematically to child language acquisition (Rakhlin and Progovac, 2017, 2020). That account assumes something similar to what we argue for directly here, that there is a systematic bottom-up sequence of syntactic acquisition. That work is less a systematic theory of acquisition, though, than an extensive investigation of the correlations between acquisition of levels of syntactic structure and the parallel development of relevant non-linguistic cognition. We discuss that work extensively at various points in what follows but below we have included several of their main claims that are most relevant at this point in the discussion:

> The approach we advocate does not imply that syntactic development should be analyzed in terms of fully discrete stages, with children being limited to certain types of grammatical structures at each stage. Syntactic development does not seem to involve sharp qualitative shifts from the absolute small clause to transitivity or finiteness. Rather it involves a gradual increase in complexity: first, with a progressively greater proportion of children's utterances containing VPs relative to single word utterances, with a gradual increase in argument structure, and greater and more consistent use of higher layers. Even during the earliest combinatorial stage, children's utterances are not limited to VPs or any two-word combinations, but contain many single-word utterances, along with some two-argument clauses. To account for this gradual nature of change, we conceptualize syntax acquisition as a sequence of partially overlapping phases, with the onset of each subsequent phase occurring

during the earlier phase(s), allowing for the elements of more than one to coexist. Before a syntactic layer is fully established, the elements that are associated with that layer are used sparingly, inconsistently, and are error prone. Their frequency, consistency, and accuracy gradually increase. (Rakhlin and Progovac, 2020, 4-5)

This approach can account for the inconsistent presence of functional categories in child language (e.g., a single functional projection in Clahsen et al., 1993). TP starts to emerge during the SC [small clause] phase, but does not get fully established until later on, its frequency and accuracy gradually increasing at the expense of SCs. This view also accounts for individual variation in the rate of syntactic acquisition: rapid progress through phases in some and prolonged in other children, particularly those with developmental language disorders. There may also exist cross-linguistic differences in how rapidly new functional layers are added–based on how robustly and consistently each layer is instantiated in the input. Thus, German-acquiring children may show evidence of finiteness earlier than English-acquiring peers, who are exposed to a profusion of bare stem finite forms, whereas in German, most verb forms (including infinitives) are overtly inflected, plus the finite/non-finite distinction is reflected in word order[.] (Rakhlin and Progovac, 2020, 5)

We only became aware of the work by Rakhlin and Progovac (2017, 2020) at the late stages of developing this manuscript, and it is thrilling to discover others working the same lines of reasoning as ourselves. Rakhlin and Progovac (2020), especially, lays out not only the clear argument for this developmental link between generative syntactic findings and language development, but also draws very direct correlations with corresponding non-verbal cognitive development as well. A similar approach is signaled in the work of Potts and Roeper (2006), emphasizing the initial role of small clauses (and discussing ways that childlike small clauses can persist into adulthood).

This volume contributes in several ways to the proposals of Rakhlin and Progovac (2020). While some of the evidence we present overlaps with their overview of acquisition timelines, our larger monograph format here allows for some more extended discussion of these facts. Furthermore, our proposals here do make a much more specific claim: rather than focusing solely on the core structural hierarchies, we make the much stronger claim that acquisition timelines track with the Minimalist model of sentence grammar much more closely. This allows for a more systematic proposal of the links between derivations in syntax in Minimalist Syntax and acquisition processes, and therefore a more precise/falsifiable theory). For example, the specific formulation of (4) allows us to see what lessons can be learned from counter-cyclicity, both as an affirmation of the link between acquisition timelines and Minimalist syntactic derivations, but also as a potential explanation for the existence of counter-cyclicity at all (from the adult-syntactic perspective).

2.6 Intermediate Summary: Theoretical precedent

It should be clear at this point that, in some ways, the core ideas of DMS have been floating around the field for decades. They appear to have never taken hold, fully: part of this (in our opinion) is that they were most thoroughly explored before the advanced of the Minimalist Program came

along. And while nobody has put the full range of ideas together that we do here, many of the foundational ideas of Developmental Minimalist Syntax (as first articulated by Bossi (2017), and as further developed here) find very strong parallels in Vainikka and Young-Scholten (2011) and Rakhlin and Progovac (2020), from Lebeaux (1988, 2000), Roeper papers, Progovac (2015), and Rakhlin and Progovac (2017, 2020). We believe these ideas deserve more attention than they have received, however, not just as a way of grounding and correlating Minimalist work to cognition outside adult grammar alone, but also as a way of addressing core theoretical issues in the Minimalist Program itself.

3 Empirical support for bottom-up acquisition

In this section we briefly summarize the kinds of empirical support that are available for bottom-up acquisition of syntactic structures, mirroring the bottom-up derivation of structures in adult grammars. This summary is necessarily highly truncated so we can focus on the core claims of this volume. But in some senses, this is a broadly accepted pattern. As noted in §2.3 a broad range of generative acquisition researchers in the 90s posited versions of this thesis, built on the observation that lexical categories are acquired before functional categories. The specific claims correlating bottom-up structure building in the Minimalist sense with bottom-up acquisition has been defended more recently, in more depth, by Vainikka and Young-Scholten (2011) and Rakhlin and Progovac (2017, 2020). We outline some of the major patterns in this section, in a highly summarized form.

Broadly speaking, DMS predicts that the cross-linguistically attested functional hierarchy (at least, the portions attested to be universal) ought to be be acquired (universally) in the same sequence. (39) gives a simplified version of the core functional hierarchy that most syntacticians take to be universal (Wiltschko, 2014; Ramchand and Svenonius, 2014).[14]

(39) **Sequence of syntactic structures, and of acquisition**
VP < vP < AspP < TP < CP.

The direct prediction of DMS is that constituents on the left in (39) ought to be acquired before constituents to their right. So the VP level of structure (verb plus complement) is acquired before full vPs (full events, predicates + arguments), which are acquired before inflectional categories like tense and aspect (along with case and agreement, which are also associated with that domain), which are then in turn acquired before CP-level structures.

This section outlines initial evidence showing that these broad predictions are in fact upheld. It is important to note from the outset that we do specifically mean the *broad* predictions are upheld. On a fine-grained level, a lot of questions are raised, especially given the range of

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[14]A caveat is that the specific grammatical categories as described in (39) is not accurate, as this largely reflect the structures of Indo-European languages. What is actually universal is something more abstract (and more cognitively plausible). §5 initiates this discussion, which the continues fully in Chapter 4. For the present discussion we retain the more familiar grammatical categories (vP,TP,CP) both because they correspond to the relevant discussions in the existing syntactic literature (and acquisition literature) but also because the alternative requires some introduction, which we won't do until the aforementioned sections.

Table 2.1: Examples of early pivot schemas / small clauses in Western European languages (Rakhlin and Progovac, 2020, 3)

English:	Eve climb. Go truck. See Adam. Put baby. Hurt doggie. Kittie go home.			
German:	Weint die Katze.	Macht das Baby.	Schlafen mein Bruder.	Mache ich auch
	cry.3sg the cat	make.3sg the baby	sleep.inf my brother	do.3sg I too
French:	Ouvre la porte.	Monter Grégoire	Mangé (l)e chien	Est tombé voiture
	Open.3sg the door	climb.inf Gregoire	eat.3sg the dog	have-3sg fallen car
Russian:	Ubasku simat'.	Midet' idjot	Osik kusiit.	
	shirt.acc take.off.inf	bear.nom go.3sg	donkey.nom eat.3sg	

variability that occurs in child acquisition (both gradual and overlapping processes in the individual, variation between individuals, and cross-linguistic variation as well). But the point that we will make as the monograph proceeds is that it is precisely the instances where the sequences in (39) appear to be least supported that DMS (as articulated in (4)) is most supported, because it is precisely these exceptional instances that we see exceptional derivations in adult grammars. This is the task of §3; first we engage the broad pattern of lower structures being acquired before higher structures.

3.1 Acquisition of predication

Symbolic language begins around 12 months with the one-word stage lasting from 12-18 months and the two-word stage lasting from about 18-30 months. Though nouns predominate during the one-word stage, verbs play a central role in the transition to more adult-like language. Early speech is "telegraphic," in the sense that it is often marked by the lack of functional categories (e.g. auxiliaries, prepositions, etc.). Nonetheless, there are aspects of children's production that matches input precisely: for example, head-complement ordering is fixed based on the parameters of the target language very early.

The earliest stage of grammar appears to be a basic predication structure, combining a predicate of some sort (often a verb) with an object of an underspecified thematic role: Tomasello (2003) calls these *pivot schemas*; Rakhlin and Progovac (2020) follow on Progovac (2015) in referring to these as small clauses. Table 2.1 shows a collection of these early constructions across English, German, French, and Russian.

Rakhlin and Progovac (2020) note that there is an absolutive-like quality at this stage, where there is a clear preference for objects and intransitive subjects as the single argument of the predicate, though there is often ambiguity as to the role of the single argument (subject/object), as (for example) in English subject-like postverbal arguments and object-like preverbal arguments both appear at this stage. Therefore the central grammar being formed seems to simple be that of a predication relationship, associating an individual entity with an action or state (e.g. the famous *allgone sticky*). Progovac (2015) suggests that a basic predicate-complement relationship underlies all clauses (which is often annotated as a verb phrase with a verb and argument: [$_{VP}$ V *argument*]), which she describes as a small clause. She posits this to be the evolutionarily basic linguistic structure, and suggests that it may likewise be so in child language development (this

suggestion is pursued in more depth in Rakhlin and Progovac 2017, 2020, works that are discussed repeatedly throughout this volume).

This is further supported by the conclusions of Theakston et al. (2015); they construct a novel approach to measuring grammatical productivity, which includes measures of flexibility of constructions (i.e. how many different items can occur in a slot of a construction) but also creativity (how novel the combinations are that the children are producing, compared to the input). What they found in looking at two distinct stages of English-learning children between the ages of 2 and 3 is that objects were flexible and creative even at the first stage they considered (T1), whereas subjects were much more restricted in their distribution. At the second stage (T2) subjects have become more productive, but was still more restricted than objects. Theakston et al. (2015) conclude that objects become approach adultlike usage earlier than subjects do. As relevant for our concerns here, this supports the idea that the internal argument is fully grammaticalized before subjects are.

3.2   Acquisition of verb phrases

If the pivot schema stage (early predications) correlates to the acquisition of lexical heads and their complements (VP, in the verbal domain), then the next broad structure that we expect to appear would be agents: the *v*P level. There does appear to be some asymmetry between subjects and objects as the grammar develops; this was the conclusion of Theakston et al. (2015), as described above. Furthermore, children tend to omit subjects freely until about 2;6 years of age regardless of whether the target language allows null subjects. These omitted subjects have various characteristics including: they co-occur most frequently with root nonfinite verbs, they are often in clause-initial position, children learning null-subject languages show different usage patterns than children learning non-null-subject languages, and subjects are omitted while obligatory objects are not (Rizzi 1994; Hyams and Wexler 1993; Valian 1991; Wang et al. 1992; Valian 2016). This is suggestive that agentive subject are in fact acquired later than the core verb-argument small clause (pivot schema) predication that seems to be less specified in its thematic properties.[15] Rakhlin and Progovac (2017) examined data from typical language acquisition in English, German and Russian (CHILDES), as well as atypical development in English (SLI and Down syndrome) finding that "at the stage when a child's MLU is ~2.0, 50%–75% of verbal utterances were single-argument SCs, over 80% of which were absolutive-like, i.e., a single argument (either an object or an intransitive subject)" (Rakhlin and Progovac, 2020, 4). Notably, also at this stage Rakhlin and Progovac (2017) found "a conspicuous lack of structures unequivocally associated with TP and CP layers (e.g., auxiliaries, wh-words associated with argument positions), except for rare and restricted (formulaic) auxiliary use with specific verbs (e.g., 'is gone')" (Rakhlin and Progovac, 2020, 4). This does not mean a lack of subjects overall: as noted above, there are certainly subjects included in the pivot schema stage. But rather, "at the onset of combinatorial language (MLU ≈ 2.0), when, according to our theory, children's grammar lacks a formal means for expressing transitivity, children's utterances appear to refer to events without an explicit

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[15]We assume this lack of specification in pivot schema thematic properties is related to the relative underspecification of the thematic role THEME/UNDERGOER as it is treated in generative syntax/semantics, as compared to other roles like AGENT and RECIPIENT.

expression of agency/ causation or, frequently, without individuating the agent" (Rakhlin and Progovac, 2020, 6). This results in small-clause-type utterances as in (40):

(40)   Adam: "Adam fell down", "ball go", "tummy hurt", "see truck", "spill rug", "drop nut."
       (Rakhlin and Progovac, 2020, 6)

However, Rakhlin and Progovac (2020, 6) note that "[a]round 2 years of age, children undergo a cognitive shift when they begin to refer explicitly to causes of events, instead of reporting events as simply happening." This is perhaps most evident in the causative errors that appear around this stage, where children over-generalize the transitive schema to produce errors as in (41).

(41)   Daddy go me around (2;8); I come it closer so it won't fall (2;3); Mommy can you stay this open (2;6); Drink me (3;1); I am going to fall this on her (2;9).
       (Rakhlin and Progovac, 2020, 6)

While errors like (41) clearly show errors in lexical knowledge (i.e. *go, come* are not transitive verbs), they in fact are very strong evidence for the acquisition of transitivity and agentivity in general. The key for us, here (as for Rakhlin and Progovac 2020), is that these kinds of errors in fact reflect acquisition of the grammatical notion of agentivity, and specific correlation of that with particular syntactic sequences. This addresses the concern of a reviewer on earlier stages of this work, that children make mistakes like these (*don't giggle me*!) much later than when they are otherwise acquiring verb-phrase-level structures. This is to be expected: children are acquiring new lexical items throughout all of their development, as we do throughout our entire lifetimes, and it is not surprising that errors may occur about the proper structure of a particular lexical item. Our claims are about children's acquisition of grammatical knowledge, and the examples in (41) clearly show the presence of a transitive grammar.

It is well established that children often follow a U-shaped curve in how adultlike any given construction/pattern may be, that specifically correlates with acquisition of grammatical patterns: Cournane (2019) offers an excellent discussion of this phenomenon: see also Tagliamonte and D'Arcy (2009); Biberauer (2019). This emerges most clearly in morphological patterns, but sometimes appears in syntactic patterns like (41) as well. So, before children acquire a rule their utterances adhere quite closely to the input for any particular utterance, as they are at a stage that is largely mimicking what they've been exposed to, rather than generating novel utterances based on grammatical knowledge. But there is not a consistent, gradual approach to the target; rather, once children acquire a rule, for a period of time their utterances appear *less* adultlike, as they apply the rule quite broadly, including over-generalizing to instances where it does not in fact apply. This results in transitive frames for intransitive verbs, as in (41), and also can result in over-regularization of morphological paradigms, such as common child utterances like *I go-ed* instead of *I went*. So it is important to note that we are not claiming that VP-level errors might not persist to later stages of development, in fact, the expectation is that they will. The key question (and one which is not often easy to ascertain) is at which point a child can be considered to have adult-like knowledge of a particular abstract syntactic structure.

3.3 Transition from *v*P to TP

There is a stage in acquisition that is well documented children use some sort of non-adultlike inflection for their sentence-like utterances (see Grinstead 2016 for an overview). That is, this is a stage where children are consistent in producing verbs with their arguments in systematic ways, but do not inflect those verbs appropriately for tense/aspect/etc. This stage has variably been referred to as the root infinitive stage, optional infinitive stage, or root default stage, the latter term being the one we will adopt. Although children occasionally make use of finite verbs during the root default stage, this stage is characterized by the frequent use of nonfinite forms and/or default forms in finite contexts. As (42) shows, children acquiring a range of genetically distinct languages exhibit characteristics of the root default stage (adopted from Legate and Yang 2007 and Kallestinova 2007):[16]

(42)   a.   Papa have it.   English

        b.   Thee drinken.   Dutch
           tea   drink-INF

        c.   Dormir   petit bébé.   French
           sleep-INF little baby

        d.   Mein Kakao hinstelln.   German
           my   cocoa   out-INF

        e.   Lashevel al ha-shulxan.   Hebrew
           sit-INF   on the-table

        f.   Mama   spat'.   Russian
           mommy sleep-INF

     Note that unlike in examples (42b) through (42f), which feature full morphological infinitives, in English the root default (RD) is simply a bare stem. We adopt the term "root default" here (Vainikka and Young-Scholten, 2011), capturing the core aspect of the pattern: during this stage, no matter the specific inflectional realization, children are inflecting verbs in inconsistent and non-adult-like ways. A good example of this is Swahili; as an agglutinative language, verbs never/rarely appear uninflected. Given children's tendency to stay close to the input in their utterances, it is not surprising that we find tense and agreement morphology on verbs in children's utterances that we would suspect to be during the root default stage. The key, however, as pointed out by ud Deen (2001), is that in Swahili this stage is typified by frequent omission of either tense, or agreement, or both. (43) gives the minimal template of a Swahili verb form (in adult grammar).

(43)   Swahili verb form: Minimal verbal template
       SUBJ.AGR-TENSE-verb.root-MOOD

As ud Deen (2001) shows, children in the root default stage who are acquiring Swahili will (in addition to inflecting verbs correctly at times) variably omit either subject agreement (44), tense

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[16]There is a quite large range of research on this topic; Grinstead (2016) provides a relatively recent overview.

marking (45), or both (46). For each, we give the child utterance and the equivalent adult form. Each of the child examples in (44)-(46) come from the same child.[17]

(44)   a.   ta- tap -a   Swahili (2;1)
              Ø– T– V –IND
              'I will slap (you).'

       b. ni-   ta- chap -a   Swahili (adult form)
          1SG.SM- FUT- hit   -IND
          'I will slap (you).'
          (ud Deen, 2001, (17))

(45)   a. a-   timam -a   hapa   Swahili (2;1)
          3SG.SM– Ø– V   –IND here
          'He has stood up here.'

       b. a-   me- simam -a   hapa   Swahili (adult form)
          3SG.SM– PRF stand.up -IND here
          'He has stood up here.'
          (ud Deen, 2001, (17))

(46)   a.   lal -a   tini   Swahili (2;1)
          Ø– Ø– V -IND down
          'I am lying down (here)'

       b. ni–   na– lal   –a   chini   Swahili (adult form)
          1SG.SM– PRS lie.down -IND down
          'I am lying down (here)'
          (ud Deen, 2001, (17))

We interpret all of this as evidence for the bottom-up acquisition of structures, because it points to stages where verbal argument structure is well-established (at least, for core transitive structures), but aspects of tense and agreement (inflection) are still unsettled and non-adult-like.

The length of the root default stage is not constant across different languages. Spanish, for instance, exhibits a very short root default stage (ending around 2;0), whereas English exhibits a prolonged root default stage (until around 3;5). The length of the root default stage for French-speaking children falls between that of Spanish and English-speaking children (around 2;8) (Legate and Yang, 2007)). Broadly speaking, children learning morphologically rich (i.e. highly-inflected) languages like Spanish display a shorter root default stage than those learning less morphologically rich languages like English (presumably a result of differing kinds of empirical evidence that children are receiving for the relevant inflectional patterns).

The use of finite forms at these stages is not simply inflectional, however: as Clahsen et al. (1993/1994) show, in V2 language (e.g. German) default/infinitive verb forms surface clause-finally, while finite verbs appear in a more adult-like second position in the sentence. Likewise,

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[17]Here, the child examples (the (a) examples) are glossed according to the source; the adult examples are glossed by the first author, adapted to the glossing conventions of ud Deen (2001).

unlike their finite counterparts—which surface to the left of negation— nonfinite verb forms in utterance at the root default stage surface to the right of negation particles in German and related languages. So we do begin to see syntactic effects of finiteness here in ways that closely mimic input, but which are nonetheless non-targetlike.

The presence of root default stages has played a significant role in many theories of acquisition of syntax, including Lebeaux (1988), Rizzi (1994), Wexler (1994), Schütze and Wexler (1996), and Roeper (1996). Some accounts assume a maturational account of root defaults, wherein children have not developed the cognition necessary to include tense and agreement in their grammars. Vainikka and Young-Scholten (2010) make the argument, however, that a root default stage occurs across all types of acquisition situations, including instances of delayed acquisition in instances of Specific Language Impairment, as well as in contexts of naturalistic adult second language acquisition.[18] This would suggest, then, that it is a feature of naturalistic language acquisition in general and not directly connected (only) with cognitive shortcomings of children at that stage.[19] From all of this we can see, therefore, that there is a period of time during which TP material is being acquired, but is non-targetlike.

3.4   Transition from TP to CP

Recalling the predictions above, CP-level structures (across languages) are consistently structurally higher than TP and *v*P, and are predicted to be acquired later, sequentially-speaking. Empirically, there does appear to be a consistent, cross-linguistic tendency for CP structures to be acquired later than TP structures. Nonetheless, acquisition of CP material poses some of the largest challenges to a DMS account because there are consistent overlaps of acquisition of TP-level structures with acquisition of CP level structures. Specifically, the early acquisition of wh-questions in English and the (apparent) early onset of V2 patterns are problematic.

In many—though certainly not all—languages, children begin producing interrogative structures including yes-no and *wh*-questions early in the acquisition process (e.g. as early as 1;6 years old for English-speaking children). Although these early questions generally do not appear adult-like, they are clearly discernible as interrogatives, either through their syntactic structure or intonation. Nevertheless, children pass through a phase in which correctly formed questions co-exist alongside questions that display various types of inversion errors. *Wh*-question formation in English—as in many other *wh*-ex-situ languages—involves two related movement processes: the *wh*-element moves via A'-movement to Spec,CP while the verb—or more specifically the auxiliary—raises via head movement to C. Ambridge et al. (2006): 522) show that inversion errors for English-speaking children generally take the form of either non-inversion, (what they call) raising, or double-marking:

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[18]In fact, the bulk of Vainikka and Young-Scholten (2011) is devoted to the investigation of the Organic Grammar approach in adult second language acquisition.

[19]As an aside, root default structures in child language often correlate with non-adult-like pronominal case marking of subjects. Specifically, in languages with default accusative Case, early subjects often surface in the accusative rather than the nominative. Errors of the opposite direction (i.e. direct objects receiving nominative Case marking) are less common cross-linguistically. Again, this is to be expected if the higher structures (including TP, which licenses subject cases) are acquired later.

Table 2.2: L1 Acquisition of IP and CP in 12 languages ([Vainikka and Young-Scholten](), [2011](), 77)

Language	IP-elements acquired earlier [*before or around age 2*]	CP-elements acquired later [*after age 2*]
English	tense	relative clauses
	auxiliary verbs	sentential complementation
Polish	tense/aspect	relative clauses
		complex sentences
Scandinavian	negation	relative pronoun
French	clitic pronouns	subordinate clauses
	tense	relative clauses
	negation	
Hebrew	tense	relative clauses
	negation	causal and temporal linking
	agreement	of clauses
Turkish	verb inflections	conjunctions
Georgian	agreement inflections	two-clause constructions
Mandarin	modals	topicalisation
	aspect marking	discourse particles
Japanese	verbal inflection	relative clauses
Kaluli	tense	discourse particles
Sesotho	tense/aspect	relative clauses
		topicalisation
K'iche 'Maya	aspect	yes/no question particle
	negation	

(47)   a.   What she does like?   Non-inversion

        b.   What she likes?   Raising

        c.   What does she does like?   Double-marking

Crucially, English-speaking children struggle with subject-auxiliary inversion long after the close of the root default stage; Ambridge et al. (2006) observe all three types of inversion errors during the entirety of their study, which follows English-speaking children aged 3;6 through 4;6 years old. Though inversion errors are likewise present throughout the entirety of their study (2;3-4;10 years), Rowland and Pine (2000) note that the proportion of inversion errors peaks around 3;8 years of age.

The presence or absence of subject-auxiliary inversion in question formation is often cited as evidence in support of the existence or absence of the CP system in children's developing grammars, since this type of inversion operates at the CP-level in adult English. However, "the assumption that inverted auxiliaries are positions in C might seem to be called into question by the observation in Radford (1987) that inverted auxiliaries seem to be acquired several months before overt complementizers" (Radford 1992: 47). That is to say, English-speaking children first attempt subject-auxiliary inversion around 24 months of age, while overt complementizers appear in their earliest instantiations only later around 30 months of age. Although English-speaking children at 24-30 months of age are still firmly within the root default stage, it is important to note that these early instances of subject-auxiliary inversion and overt complementizers are not productive, adult-like structures, but rather represent children's initial attempts with these CP-level phenomena. In this way, this slight timeline disconnect noted by Radford is not particularly troublesome for DMS or its sister approaches (Vainikka and Young-Scholten, 2011; Rakhlin and Progovac, 2020).

Turning away from English, in which *wh*-questions first surface quite early in a child's acquisition process, German-speaking children produce essentially no argument or adjunct *wh*-questions until around 2;6 years old (Clahsen et al. 1993/1994). Although there is the occasional, highly formulaic exception (e.g. *Wo ist X?* 'Where is X?'), *wh*-questions are exceedingly rare in early child German. Crucially, though, the first *wh*-questions surface after the acquisition of verbal inflection, which German-speaking children master by around 2;4 years old. When young German children begin generating *wh*-questions, approximately 30% of their questions contain errors (Schmerse et al. 2013). The most common type of error is omission of the clause-initial element, which is generally the verb or the *wh*-phrase. Most of the children's non-inversion errors—which are significantly less common for German-speaking children than for English-speaking children—involve a finite verb in clause-final position (Schmerse et al., 2013).

Looking more specifically at V2, a consistent question for "bottom-first" acquisition is that German-speaking children appear to acquire V2 (and/or V1) word order relatively early, i.e. before CP structures would be expected to be acquired. German-speaking children begin to use V2 productively around 2;6 years old, shortly after the end of the root default stage for speakers of Standard German (Penner 1992). According to Roeper (1992), though, German-speaking children do not allow object preposing in their earliest V2 structures, even though this word order is entirely acceptable and, in fact, extremely common in adult language. Instead, it is always the subject that precedes the verb in children's early V2 structures. However, this claim is not uncontested; Poeppel and Wexler (1993) report that although SVO word order is by far the most

Table 2.3: Word order frequencies in early child German (adopted from Poeppel and Wexler 1993: 15)

Partial word order	Number of occurrences
SV	147
OV	19
Adv-V	31
Total	197

common choice in early V2 utterances, other types of constituents can be positioned before the verb. These findings are summarized in Table 2.3.

Poeppel and Wexler interpret the existence of non-canonical (i.e. non-SVO) word order as evidence for the existence of the CP system. However, Clahsen et al. (1993/1994) point out that the subject (Andreas) in Poeppel and Wexler (1993) had a very high degree of correct verbal inflection, and also produced V2 sentences at about the same rate as adult German, suggesting that Andreas was at a later stage of development. Clahsen et al. (1993/1994) show that there is a stage in German acquisition before this where, despite the presence of some structure above the verb phrase, it is clearly not CP, as they show a number of patterns from V1/V2, modal and auxiliary constructions, and negation showing that the predictions of a CP-level analysis do not hold during the first stage of acquisition that they examine, despite V1/V2 orders being variably available. Notably, also, they show that there is a lack of wh-questions, subordination, and overt complementizers at this stage.

To illustrate one of their arguments, consider the acquisition of German negation. On the assumption that negation is located at the edge of the verb phrase, Clahsen et al. (1993/1994) note that VS-Negation word order in children's utterances would indicative of an adult-like grammar in which the verb is in CP. To generate VS-Negation word order both the verb and the subject raise around negation, surfacing in C and Spec,TP, respectively. On the other hand, V-Negation-S word order is indicative of a grammar lacking CP. To generate V-Negation-S word order the verb stops at an intermediate functional category—immediately above negation—while the subject remains in its base generated position in Spec,vP.

In the earliest stage of productive V2 (circa age 2;6), Clahsen et al. report no instances of VS-Negation word order; negated structures at this point in development show V-Negation-S word order. Even a few months later in development, when Clahsen et al. report six instances of VS-Negation word order, the majority of negated sentences fall into the V-Negation-S mold (Clahsen et al. 1993/1994: 423):

(48) Darf nich Julia haben.   German
      may not   Julia have-INF
      'Julia may not have that.'

Given these facts, Clahsen et al. (1993/1994) conclude that there is only a single functional projection above the verb phrase at this stage of development. These observations suggest, then, that German-speaking children do not grammaticalize CP until after 2;6 years of age. In this way, CP

grammaticalization actually takes place later than the apparent-V2 evidence suggests; although V2 becomes productive around 2;6 years of age, this productivity is not necessarily indicative of acquisition of the CP-level. Crucially for the current discussion, these grammaticalization processes all occur after grammaticalization of TP.

Given these observations, the use of productive V2 word order seems to precede the acquisition of CP. How, then, to account for V2 patterns without CP? Meisel and Müller (1992a) (a.o. including Clahsen 1990/1991) adopt a split-inflection hypothesis, in which verbal inflection relies on two distinct, but related projections: TP and AgrP (with TP subordinating AgrP). Per Meisel and Müller, children originally analyze TP—rather than CP—as a (head-initial) possible landing site for finite verbs. In this way, V2 word order arises through verb movement to T rather than to C. This also accounts for the preponderance of SVO word order in early V2 structures. In early SVO structures, the subject raises to Spec,TP where it receives nominative Case and satisfies the EPP. This subject movement to Spec,TP is operative in the target grammar as well, rendering the disjoint between child and adult language less significant. In the rare instances of early OVS structures, the object—rather than the subject—raises into Spec,TP.

However, even if (for the sake of argument) V2 constructions *do* show up early in acquisition using a CP-level functional head, there are a variety of mitigating factors with respect to the prospects for DMS as articulated here. First, it is clear that other CP-level constructions lag behind this point, meaning that a fully-general analysis of the CP domain has not yet been reached by those children. And per DMS (following Vainikka and Young-Scholten 2011) the "timeline" under consideration is the construction of the clausal "master tree," not instances of usage of a CP level head. As we discussed previously, it is quite common for individual verbs to receive their own construction-specific analysis from children, even for extended periods, before those various sub-generalizations are resolved into master-tree-level generalizations.

Vainikka and Young-Scholten (2011) take a somewhat different approach to the early presence of V2 in German, one which attempts to reconcile the early child grammar with adult grammar. Similar to Mikkelsen's 2015 treatment of Danish, they analyze the position of the verb in subject-initial V2 clauses as in Spec,TP rather than Spec,CP. This is less straightforward than it is in Danish, however, as German is head-final apart from CP. Therefore, they assume that TP can in fact be flexible in its headedness, such that TP may be head-final or head-initial, depending on context.

(49) *German(ic) Headedness Generalization:* German is a head-final language, but the first functional projection in a sentence is head initial.
(Vainikka and Young-Scholten, 2011, 53)

This assumes, therefore, that subject-initial matrix clauses are TPs, rather than CPs, and TP is realized as head-initial in those instances. This allows for a much more direct correlation between, one the one hand, early child V2 in which subject-initial forms predominate (presumably once they have acquired TP but not CP) and on the other hand, adult grammars where CP has obviously been acquired. Despite, this, it is the broad conclusion of a wide range of researchers that—despite the use of V2-like structure—true CP-level structures are not present at this stage in development: there are no overt complementizers, no subordinate clauses, no object topicalizations, and no wh-questions (Vainikka and Young-Scholten 2011, who also cite Clahsen 1988,

1990/1991; Clahsen and Penke 1992; Clahsen et al. 1994; Ingram and Thompson 1996; Clahsen et al. 1996; Meisel and Müller 1992b; Poeppel and Wexler 1993; Tracy 2002; Rothweiler 2006: we will see this trend repeatedly in the discussions that follow as well). Instead, this appears to be an instance of an intermediate grammatical stage that mimics V2, but does not have adult-like structures for CP more broadly. Researchers differ on their explanation for the V2-like appearance: Vainikka and Young-Scholten (2011) claim that subject-initial V2 is in fact not a CP, even in adult grammar; Clahsen et al. (1993/1994) argue for an underspecified functional projection where the finite verb appears, others suggest it may simply be the result of mimicry and not grammar (Tracy, 2002).

What we see, then, is that despite some early use of CP-level constructions like wh-phrases and V2, there is consistent evidence that the full scope of CP properties is not acquired until later than both TP and *v*P level phenomena. Diessel (2004) shows that full subordination of CPs (e.g. complement clauses, relative clauses) are some of the last core syntactic structures to grammaticalize, lasting well into the preschool years (see also de Villiers 2007).

If a theory of syntax-acquisition correlations requires production of any CP-level construction to require the child to have acquired CP (or, to possess CP innately) then some of these patterns are certainly problematic: obviously, children acquire some of these patterns early. However, it is the claim of DMS that what correlates to adult syntax is the point of grammaticalization: i.e. the point that a child posits their final adult-like grammatical structure for any particular syntactic structure. It is these points of grammaticalization that we claim to proceed in a particular sequence.

3.5   Caveats

We just want to take a moment to mention a couple of caveats. The description above glosses over a massive range of variability both in individual differences between children, and cross-linguistic variation in acquisition tendency. It is our assessment (and that of others: Vainikka and Young-Scholten 2011, Rakhlin and Progovac 2020) that these broad tendencies do in fact hold cross-linguistically, despite the variation in details. Beyond the obvious impossibility of addressing an extraordinarily large empirical domain in this short section (all children's acquisition of any natural language), a more pressing issue is raised by the literature: despite the broad tendencies above, there are challenging counter-examples to the pattern described above. Namely, some structurally high grammatical constructions (esp. involving CP) appear to be acquired, at least in part, by children much earlier than inflectional structures (wh-movement in English, V2 in German). We will show, in fact, that the issue is broader than that: in the face of an attempted bottom-up analysis of acquisition of syntactic structures, there appear to be all kinds of "out-of-sequence" acquisition processes (at least, divergent from the predicted sequence). This is of course the heart of monograph, to argue that these "out-of-predicted-sequence" acquisition patterns in fact correlate to "out-of-predicted-sequence" derivations in adult grammars. So it is our claim that the apparent problems with claims of bottom-up acquisition in fact are some of the best evidence for correlating bottom-up structure building in syntax with bottom-up acquisition processes (though, of course, we need to have a theory that allows for such counter-cyclic processes). Furthermore, the approach to phases taken up in Chapter 4 is meant to address overlappy

and gradual acquisition.

4 Developmental pathways of acquisition of syntax, per DMS

In this section we outline our assumptions about the developmental pathways of language acquisition, as compatible with DMS. We find the most plausible discussion for the pathways for acquisition of syntax to be rooted in Tomasello's work on Construction Grammar, which on its face is staunchly anti-Chomskyan. As we will show, however, in many ways the deeply minimal theories of Universal Grammar that are now most prominent among practitioners of the Minimalist Program are sufficiently compatible with a constructionist approach to language; what is needed is the assumption (encoded in DMS) that what Minimalist syntacticians are modeling are the grammaticalization mechanisms that children employ in acquisition (grammaticalization) of syntactic structures in their first language. This does require some core differences with standard constructionist assumptions (mainly, regarding what is a possible construction). We will clarify these differences below.

Tomasello adopts a theory of usage-based linguistics for his study of children's language acquisition. Under the usage-based model, children are not born with an innate UG, but instead acquire language through domain-general cognitive processes—such as intention-reading and pattern-finding—that are not necessarily specific to human language (Saffran et al. 1996, Marcus et al. 1999, Kirkham et al. 2002, a.o.). These studies (among many others) collectively indicate that children are incredibly adept at recognizing patterns, a crucial skill for both the comprehension and production of human language. According to Tomasello—and construction grammarians more broadly—children use these general cognitive skills to analyze their linguistic input, first generating lexically-specific schema (i.e. constructions that revolve around specific words) and ultimately progressing to more abstract schemas. As Ambridge et al. (2006) explain: "Under functionalist, construction-based accounts of language acquisition, children acquire a structured inventory of grammatical constructions, which become increasingly abstract as development proceeds" (Ambridge et al. 2006: 525). For example, a child might begin with a lexically-specific WANT-X schema, in which a variety of other constructions can be inserted into the slot following *want*. As children hear more and more exemplars of the WANT-X construction—as well as other constructions of the VERB-X format—they generalize across these exemplars and develop the more abstract SUBJECT-VERB-OBJECT (SVO) transitive schema. Per usage-based linguistic models these specific schemas are meaningful in and of themselves; in this way, both the linguistic units (e.g. words) and the underlying linguistic structures (e.g. abstract constructions like SVO word order) take on meaning and contribute to the child's understanding of his or her language.[20] Constructionist approaches therefore conceive of the 'construction' as the core unit of linguistic competence, with variability in how abstract it is. Constructions can be highly concrete (e.g. a word, with a particular meaning) or they can be highly abstract (schemas for declarative clauses, questions, etc). They can be anywhere in between, as well: idioms (e.g. *kick the bucket*, *the cat is out of the bag*) and partially productive constructions (e.g. *let alone: I won't eat cheese, let alone pizza*). But crucially, they all qualify as constructions: '[a] construction is prototypi-

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[20]We do not take the stance that constructions themselves are meaningful, at least in any sense that is different from the standard Minimalist approach to how to build syntactic structures of any sort.

cally a unit of language that comprises multiple linguistic elements used together for a relatively coherent communicative function, with sub-functions being performed by the elements as well" (Tomasello, 2003, 100).

Adult competence within the usage-based framework is simply a structured inventory of these meaningful constructions. As Tomasello describes: "In usage-based linguistics, the linguistic competence of mature speakers of a language is characterized not as a monolithic grammar—as in generative grammar—but rather as a 'structured inventory of symbolic units' in the minds of its speakers" (Tomasello 2003: 105). In other words, the adult endpoint of language development is not a formal grammar based on rules, constraints, operations, and parameter setting—as is the case traditionally in generative frameworks—but instead a hierarchically ordered network of related constructions. Just as the exact composition of UG is not universally agreed upon in generative circles, the exact structure of this network of constructions remains under debate. A full summary of the range of existing analyses is outside the scope of the current work, but see Tomasello (2003), Goldberg (2006), Croft (2001), Hilpert (2014), and Hoffmann and Trousdale (2013) for a more detailed discussion of the structure of adult grammar within the usage-based framework.[21] Importantly, though, under usage-based analyses the gap between child language and adult language is rather easy to bridge; if children's early language is representative of the beginning stages of the grammaticalization process—which is essentially the usage-based proposal—a child's ability to arrive at adult competence is quite comprehensible, as this adult competence is merely a more refined inventory of grammaticalized structures of that early language.

This kind of approach has the advantage of essentially being a what-you-see-is-what-you-get (WYSIWYG) approach, where in many ways the apparent knowledge of children at different developmental stages is taken at face value, instead of assuming deep extensive language knowledge that they are not yet showing evidence of. But usage-based approaches tend to be relatively non-precise about the properties of adult grammar. They also tend to be relatively non-specific about the nature of grammaticalization and the resulting constraints on constructions. The prevailing assumption appears to be that there aren't such constraints on constructions, and any string can be grammaticalized.

Tomasello divides children's acquisition of grammar into four general stages, each of which is marked by the use of a specific type of construction: holophrases, pivot schemas, item-based constructions, and abstract constructions. Holophrases are expressions that display holistic, undifferentiated communicative intentions: even if they repeat strings that have complex structure in adult grammar, for the child they are unanalyzed, a memorized string. Tomasello holds that holophrases do not contain syntax, but instead represent unparsed adult utterances (e.g. *I-wanna-do-it*, *are-you-ok?*, *what-doing?*). From these holophrases, children develop pivot schemas, in which a constant structural term combines with variable terms of elaboration (e.g. More X schema: *More juice*, *More grapes* or Go X schema: *Go store*, *Go home*). Although Tomasello insists that they do not have syntax (e.g. the ordering of constituents does not matter), pivot schemas represent the earliest form of syntactic abstraction in that they are organized around a constant term with slots that can be filled by a variety of words. Arguably, pivot schemas are

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[21]Rowland et al. (2005)—construction grammarians themselves—point out that the usage-based theory is often lacking in specifics and must, therefore, move towards increased specificity. Our aspiration is that the detailed work within the generative framework on adult grammars can be highly influential in this process.

in fact the first grammatical abstraction that children make, a predicate-argument schema which contains a completely underspecified argument (i.e. having no specific thematic role), correlating to the structure syntacticians model as the minimal VP [$_{VP}$ V Obj]. This is the claim that we advanced in §3, and follows on similar claims by Rakhlin and Progovac (2020). It is these PREDICATE-X constructions that we claim become grammaticalized (in the case of verbs) as the Verb-Internal.Argument structure that is at the base of most clauses: [$_{VP}$ V Obj]. Critically, it is not until the verb-object relation is fully productive and abstract that we assume that 'Merge' has occurred, in the Minimalist sense.

But, at least at early stages, these are item-specific and not a broad PREDICATE-X construction. That is to say, a child will have a construction "More X" but they do not automatically generalize that to any new predicate, instead proceeding through a stage where their knowledge of small clauses is restricted to a smaller set of predicates.

> Tomasello et al. (1997) demonstrated more systematically that these pivot schemas are indeed productive in this way. They found that 22-month-old children who were taught a novel name for an object knew immediately how to combine this novel name with other pivot-type words already in their vocabulary. That is, when taught a novel object label as a single-word utterance ("Look! A wug!"), children were able to use that new object label in combination with their existing pivot-type words in utterances such as "Wug gone" or "More wug." This productivity suggests that young children can create linguistic categories at this early age, specifically categories corresponding to the types of linguistic items that can play particular roles in specific pivot schemas (such as "things that are gone," "things I want more of").
>
> However, children at this age do not make generalizations across the various pivot schemas; each is a constructional island. Thus, Tomasello et al. (1997) also tested the idea that children who use pivot schemas can come to a new scene and already know how to partition it by means of a pivot word and some other word. But they found that children cannot do this. When taught a novel verb as a single-word utterance for a novel scene (for example, "Look! Meeking!" or "Look what she's doing to it. That's called meeking"), 22-month-olds were not then able to talk about the event in a more differentiated way that included reference to a participant in the event based on some generalized knowledge of how other events are partitioned in the English language (for example, they did not create a slot for the newly learned verb by saying "Ernie meeking!"). (Tomasello, 2003, 115)

As Tomasello discusses, that item-specific characteristic of their knowledge proceeds for some time, even beyond the pivot schema / small clause stage. Around two years of age, children's linguistic production progresses to what Tomasello (2003, 117) calls item-based constructions (e.g. *Where's the X*, *I wanna X*, *X on there*) which extend beyond pivot schemas "in having syntactic marking as an integral part of the expression." Importantly, these item-based constructions are also lexically-specific (by predicate), meaning that they are limited to how children have heard lexical items used previously (i.e. children do not yet generalize across exemplars). Tomasello (1992) referred to this as the Verb Island Hypothesis, extended the island metaphor for constructions. In general, before children reach higher levels of abstraction, their learning is item-specific, and for sentential utterances, the item in question is the verb.

Around 36 months of age, children begin to grammaticalize abstract constructions that are less lexically specific than their predecessors (e.g. trans-SUBJECT trans-VERB trans-OBJECT). Although early abstract constructions do not necessarily resemble adult language—they are limited to particular functions in particular contexts—the use of abstract constructions marks the beginning of children's development of adult-like syntactic capabilities. On the DMS account, we assume that the acquisition of syntax up until this stage proceeds according to how Tomasello (2003) suggests. There is relative agreement among researchers working on language acquisition (and other aspects of child development) about the large role of statistical learning (e.g. Lidz and Gagliardi 2015, Pearl and Goldwater 2016), and there is good evidence for other aspects of these proposals as well. Specifically, Tomasello's verb island hypothesis reflects the early bias of children to adhere very closely to the data that they have already observed (Sugisaki and Snyder, 2013; Snyder, 2011). That is to say, it appears that children are quite conservative in their generalizations; rather than quickly generalizing from one example to a general pattern, children appear to learn specific examples/instances (of linguistic constructions, in our cases) and only generalize after encountering some threshold level demonstrating a pattern, at which point they will apply a principle much more broadly (Yang, 2016; Cournane, 2019). This is reflected in Tomasello's (1992; 2003) verb island hypothesis, which posits that each verb (and the constructions it generates) is acquired distinctly, before (eventually) more abstract generalizations are acquired, e.g. intransitive verbs, ditransitive verbs, etc.

Importantly, though, as we discussed in §3, there are specific sequences in which structures achieve fully productive abstraction. V+OBJ is productive before full verb phrases, complete events (predicate and arguments) are grammaticalized before tense is, and so on. Full nominal and clausal constructions don't achieve grammaticalized status as a holistic gestalt, instead progressing from bottom to top (in broad terms, *v*P before TP before CP).

With these added observations, Tomasello's (2003) outline of the pathways of language acquisition seems reasonable and well-founded, from the DMS perspective. Of course, Tomasello is in ways the standard-bearer for the anti-UG movement (Ibbotson and Tomasello, 2016; Tomasello, 2009).[22] Part of the trouble is that generative studies start at the point where Tomasello (2003) largely stops: grammaticalization. As we mentioned above, we conceive of the Minimalist program as a detailed look at the nature of syntactic grammaticalization in the process of language acquisition.

We don't rehash the entire discussion of §1, but at this point, when a child reaches a generalization about a particular abstract syntactic construction (or as is usually the case, a contained portion of a larger structure, e.g. *v*P), we propose that this small learning leap of moving from item-based sub-patterns to a more general abstract pattern is implemented via a cognitive grammaticalization mechanism, modeled within the Minimalist program via Merge (and Agree, and the other proposals about Universal Grammar within the Minimalist Program). Despite the Minimalist UG's broad inheritance of a claim of language-specific innate properties from the history of generative linguistics, we are aware of nothing within the Minimalist framework as currently practiced that requires such a stance. Rather, we can view Minimalist models of "Universal Grammar" (UG) as models of the cognitive mechanisms of grammaticalization in acquisition, irrespective of whether these mechanisms are specific to language or not (which, while important and

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[22]An illustration is the title of Tomasello (2009), *Universal Grammar is dead.*

certainly interesting, is not here a central issue, and doesn't impact our claims). In this way, we use the term "UG" to mean, simply, stable postulates that Minimalist syntacticians have ascribed to UG, specifically Merge and Agree (and, as we will comment below, a few other operations as well). A better term than "Universal Grammar" is likely "Grammaticalization Mechanism," because this is how we view UG, and it avoids some of the baggage of (non)innatist polemics.[23]

Crucially, we are not assuming that children's initial item-based constructions are internally represented via a Merge structure. At early stages, we assume them to be holophrase-like—unanalyzed—such that new items are simply concatenated with whatever structures have previously been grammaticalized. An example of this comes from Akmajian and Heny (1973) via Roeper (1998), where the auxiliary *are* is used by a child in a non-targetlike fashion as a yes/no question marker as shown in (50).

(50)   a. are you put this on me
       b. are you get this down
       c. are you know Lucy's name is
       d. are you got some orange juice
       e. are you don't know Sharon's name is
       f. are you sneezed
       g. are you help me
       h. are you want one
       i. are this is broke

In instances like this, the child is showing a fair degree of accurate language knowledge about the structure of a sentence (e.g. *you got some orange juice*) but is still combining that with non-targetlike knowledge about question-asking. It has been suggested by Roeper (2011) that a non-recursive conjunction operation proceeds true subordinating Merge operations for children; Progovac (2015) suggests the same for language evolution, suggesting a potential extension to acquisition as well based on the rest of her reasoning about potentially extending her evolutionary account to acquisition. On this approach, pre-grammaticalized structures are not formed via Merge, but rather are simply conjoined/concatenated, before the Merge-based grammar is deployed (*contra* Yang and Roeper 2011).

In the pathway of acquisition, then, we expect overlaps. At some point, children reach target-like, abstract, general knowledge of the VP 'small clause' structure. It can be tricky to know *when* exactly this is (the discussion in §3 engages that question) but key for our claims is simply that this point is reached *before* target-like knowledge of full *v*P structures is reached (i.e. the root default stage). But, we do expect to see item-based transitive constructions (e.g. a verbal utterance using subjects and objects) to appear before the VP itself is grammaticalized in its target-like form: this is unproblematic for DMS. The key for DMS is that grammaticalization itself proceeds in a stepwise fashion, not that the entire acquisition process of each hierarchical structure occurs in a stepwise fashion. Figure 2.2 schematizes this progression, showing overlaps

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[23]Perhaps the best description of our stance about *both* Chomsky's UG and Tomasello's Construction Grammar can be borrowed from Eric Reuland's comment (about a separate issue): they are both "right about too many things to be completely wrong, and wrong about too many things to be completely right."
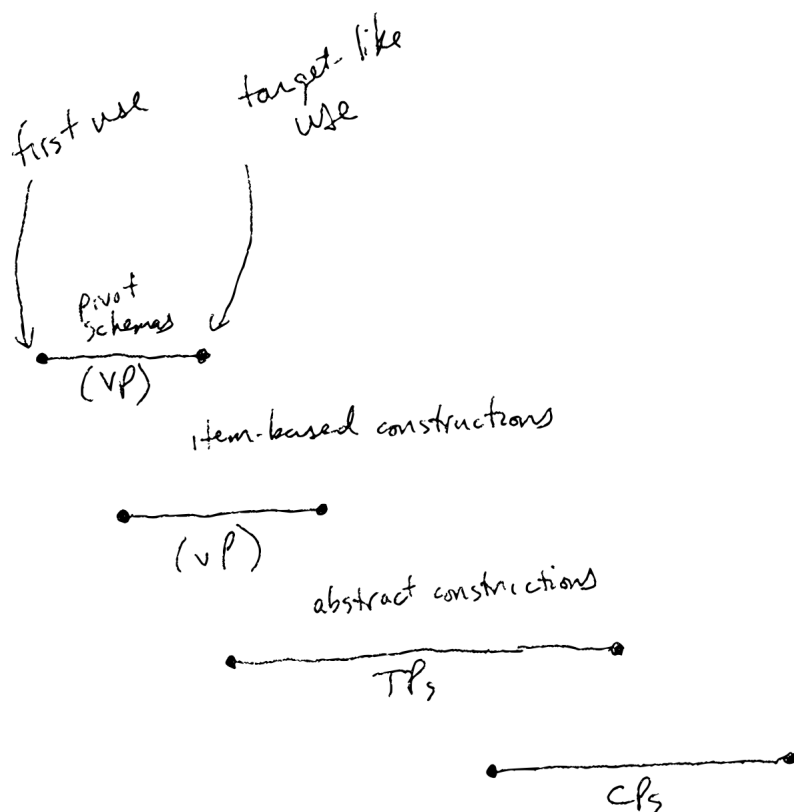
Figure 2.2: Schematic of overlaps in acquisition (but stepwise grammaticalization) of hierarchical structures in syntax

between successive periods of grammatical development, but also the stepwise progression of the right-side points of each line: the point where that specific structure reaches a target-like state of knowledge. The crucial point for us is that these right-side points proceed in the sequence, of hierarchically lower structures before hierarchically higher structures.

Again, this schema taken on its own presents a simple picture, whereas children's acquisition itself is highly variable, and overlappy. The overlaps between the timelines of the successive structures in Figure 2.2 is meant to communicate that, but it matters because it is no simple matter to empirically demonstrate when a structure has been acquired in a targetlike fashion. The best evidence, as discussed in §3, is overgeneralization errors: when children demonstrate grammatical knowledge by over-extendeding that knowledge to instances where it does not apply. Not all grammatical structures lend themselves to such instances (i.e. it requires some degree of irregularity, instances where a new correct generalization does not apply). But as §3 discusses, there are various points where these successive stages of grammaticalization are visible in various languages, and they do in fact show the sequence predicted by DMS.

Importantly, the claim here is not that the UG-based mechanisms are in fact the *learning* mechanisms: we have nothing to say about the learning mechanisms, despite that being an important and fascinating area of investigation (for just a few recent works, see Yang 2016, as well as the overview chapters in Part V of Lidz et al. 2016). In general, we suspect that statistical and

analogical mechanisms may play a major role here, though there may well also be cues that are specific to language that children come prepared for innately. Again, this goes far outside our expertise. Our claim is that the UG-based mechanisms are in fact either descriptions of the cognitive Grammaticalization Mechanisms themselves, or precise formalizations of the outcome of grammaticalization. Either way, these are descriptions of the outcome of the right-side points of each line in Figure 2.2, not of anything to the left of those points on each line.

This is where a point of disagreement Tomasello (2003) is clear (and to our knowledge, a disagreement with construction grammarians in general): Tomasello assumes that any sequence of linguistic elements can participate in a construction, and assumes no specific constraints/limits on what can be grammaticalized. While it is completely reasonable to assume (based on the demonstrable conservatism of child learners) that item-based acquisition leads children to arrive at a grammar that is relatively close to that of the language that they are acquiring, this fails to explain the broad syntactic generalizations that have been arrived at across languages in their adult grammatical structures. That is to say, the UG-based mechanisms of the Minimalist Program have proven adept at analyzing (and discovering) fine degrees of grammatical detail across the world's languages. Again, while sidestepping the questions of language-specificity of UG, the key here is that adult-like grammatical constructions show consistent properties in the precise (adult) grammatical structures and relations between structures (see Svenonius 2016 for list of some of those properties). This is completely an accident of history on the constructionist account, without a more precise theory of the properties of grammaticalization. On the DMS approach, the Minimalist Program provides that more precise theory of grammaticalization: it is what Minimalist syntacticians have been calling 'UG.'

An important point is that we borrow the concept of the "master tree" from Vainikka and Young-Scholten (2011): what is being grammaticalized here is a generalization, a representation of a canonical hierarchical tree structure, and not each and every sentence individually. This should be relatively noncontroversial, but it is valuable to make specific what is being acquired. And in fact, as we will explain below in §5, it is more appropriate to talk about acquiring a master tree inventory of the grammatical structures of a language: essentially, an inventory of constructions, similar to how Tomasello (2003) and Goldberg (2006) discuss grammatical knowledge of adults as inventories of constructions.[24] More on this in §5.

By no means do we want to suggest that a wholesale synthesis between Construction Grammar and the Minimalist Program is possible, nor do we attempt one. DMS does have some clear divergences from the approach advocated by Tomasello (2003). Crucially, per Tomasello, the transition from holophrases to pivot schemas to item-based constructions to abstract constructions is not one of addition of structure, but rather of replacement of a newer generalization for an older one. In other words, the advent of a new linguistic stage is not necessarily built upon the previous stage(s), as is proposed under DMS. Construction Grammar approaches do at

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[24]In many ways, Minimalist syntacticians already operate on a construction basis: depending on the grammatical construction under investigation, syntacticians assume specific functional heads and lexical items to be present in certain constructions that are absent in others. The goal of Minimalist theorizing, of course, is to build a theory (small-scale within a language, and larger scale for Big-L Human Language) that can account for what constructions are possible and what constructions are not. By speaking of a master tree inventory here, while explicit for the sake of discussion acquisition, we think it is in fact not far from what Minimalist syntacticians have to assume about grammatical knowledge anyway.

times assume existing generalizations to be the foundations of later generalizations, but this tends to revolve around verb classes, such as 'give' being the template for later-acquired change-of-possession verbs. As for acquiring sentential grammar itself, although certain lexical similarities might exist between semantically related holophrases, pivot schemas, item-based constructions, and abstract constructions, Tomasello construes these four types of constructions as fundamentally distinct. In this way, earlier stages of children's language acquisition processes no longer exist in later stages of language knowledge. This is a central difference, as we understand the existence of ontogenetic (developmental) fossils as central to explaining aspects of adult grammatical knowledge. There are surely additional differences as well.

That said, what is striking about the story we are telling here is that many, if not most, properties of languages can be considered emergent, with very little language-specific content proposed to be innate (which, again, largely reflects standing assumptions among Minimalist syntacticians, rather than any major innovation from us). Rather than any particulars about language being emergent, it is simply the operations by which grammatical generalizations are drawn (paired, perhaps, with aspects of cognitive development playing a key part) that result in the shared properties of language that generativists have attributed to Universal Grammar. Of course, there are many aspects of syntax (and language more broadly) that we aren't addressing here, so this is not a claim that all properties of syntax are emergent. For the ones that can be argued to be so, however, this is a welcome result and may perhaps help show some of the value of Minimalist approaches to usage-based grammarians.

5   The "Universal Spine" and a Master Tree Inventory

In this section we consider a line of reasoning that falls out of some core assumptions about the Minimalist program, offering some clarity on the whole picture of acquisition under DMS, and which also create some interesting areas for future investigation. When structures are built bottom-up, any new phrase (whether argument or adjunct) that is added to the structure must already be constructed before it is merged into the sentence structure. So when a DP subject is first-merged into Spec,vP, the subject DP itself must have already been constructed.

(51) $[_{v\text{P}}\ v\ [_{\text{VP}}\ \text{V}\ \text{DP}_{theme}\ ]\ ] + \text{DP}_{agent}\ \text{–>}\ [_{v\text{P}}\ \text{DP}_{agent}\ [_{v\text{P}}\ v\ [_{\text{VP}}\ \text{V}\ \text{DP}_{theme}\ ]$

The metaphor that was originally suggested by Chomsky and which is widely assumed by practitioners of the Minimalist Program is that there are separate 'workspaces,' that substructures (e.g. arguments, adjuncts) can be built independently of each other before they are merged into the main structure of the sentence being constructed. While this was a theoretical necessity to allow bottom-up structure building to work, to our knowledge there is no broadly accepted theory of what workspaces are in any real sense, other than a logical necessity of the theory. But if, per DMS, we are interpreting operations in adult syntax to be recapitulating acquisition pathways, we might also expect that different workspaces have some psycholinguistic reality related to acquisition. Our suggestion is that this is related to different generalizations that are being built by a child in acquisition, i.e. different master trees in an inventory of master trees.

5.1 DP-CP parallels

It is a long-standing observation of generative syntax, dating at least to Chomsky (1970) and proposed explicitly by Abney (1987), is that there are systematic correlations between the structure of clauses and the structure of noun phrases. We follow Wiltschko's (2014, 75ff) explication of the DP-CP parallels here. Consider the similarities between the constructions below: verbal predicates (52), gerund nominalizations (53), and the least verbal nominalizations in (54), borrowed from Wiltschko (2014, 75ff).

(52)   a. John is eager to please.
        b. John has refused the offer.
        c. John criticized the book. (Chomsky, 1970, 187)

(53)   a. John's being eager to please
        b. John's refusing the offer
        c. John's criticizing the book (Chomsky, 1970, 187)

(54)   a. John's eagerness to please
        b. John's refusal of the offer
        c. John's criticism of the book (Chomsky, 1970, 187)

There are noticeable similarities and differences between the constructions. Subjects appear to have a distinctive status of sort: they are marked nominative in clauses as in (52), but in nominalizations subjects are marked as genitives/possessives, as seen in (53) and (54). This is presumably because despite all of the constructions in (52)-(54) showing similar root predicates (whether verbal or nominal) and similar arguments structures (the Theme and Agent roles are retained in each case), there are differences in licensing of subject roles in particular in nominalizations vs. verbal constructions. Furthermore, the object role is distinguished between the two sorts of nominalizations: objects are licensed in the usual verbal way (presumably accusative case) in gerund nominalizations (53), but objects are not licensed by the predicate in true nominalizations, instead requiring the 'of' preposition (54). The usual explanation for this is that nominalizations are verbal at their structurally lower points, and that gerunds "nominalize" above the domain for licensing objects (hence the verb-like licensing of objects), but lower than the domain for licensing subjects (explaining the non-verbal subject licensing). A full nominalization construction, in contrast, includes neither the verbal structures for licensing subjects nor that for licensing objects. Wiltschko (2014) points out in Table 2.4 that while thematic properties like agent and theme are preserved between verbal constructions and deverbal nominalizations, grammatical roles like subject and object are variably lost.[25]

Despite these differences, there are certainly parallels in what it means to be a "subject," whether in a verbal or a nominal construction (e.g. Abney 1987, Szabolcsi 1994). One relevant point is that similar to how subjects in sentences can bear various thematic roles (e.g. Agent in a transitive, or Theme in a passive), genitives in nominals can bear many thematic roles as well:

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[25]Whether they are 'lost' or expressed differently in the nominal domain is besides our point here, the point being that thematic properties are retained but grammatical properties are not.

	Verbal clause	Gerund nominalization	Nominalization via derivation
THEME	✓	✓	✓
AGENT	✓	✓	✓
OBJECT	✓	✓	✗
SUBJECT	✓	✗	✗

Table 2.4: Patterns of nominalization (Wiltschko, 2014, 77)

(55) a. Piccasso's painting (Picasso=Agent)

b. The cake's baking (Cake=Theme)

c. The student's books (Student=Possessor)
(Sutton, 2017, 11)

Beyond this, a number of inflectional parallels between DPs and CPs have been noted, though this is obviously subject to crosslinguistic variation. Some of the most famous examples come from Szabolcsi's (1984) work on possessor agreement: in Hungarian, nouns bear agreement morphology sharing features with their possessors (56a), identical to the subject agreement that appears on verbs (56b).

(56) Hungarian Possessor Agreement

a. a   te   kalap-od
    the 2SG.NOM hat-POSS.2SG
    'your hat'

b. te   rúg-od a   fiú-t
    you.NOM hit-2SG the boy-ACC
    'You hit the boy.'
    (Sutton, 2017, 12)

As Abney (1987) points out, Yupik shows a similar pattern of possessor agreement on nouns mirroring subject agreement on verbs.

(57) Yupik Plural Possessor Agreement

a. angute-t kuiga-t
    man-PL   river-AGR
    'the men's (pl) river'
    (Abney, 1987, 39)

b. angute-t kiputa-a-t
    men-PL   buy-OM-AGR
    'The men (pl) bought it.'

(58) Yupik Dual Possessor Agreement

a. angute-k kuiga-k
    man-DU   river-AGR
    'the men's (dual) river'

b. angute-k kiputa-a-k̲
   men-DU buy-OM-AGR
   'The men (dual) bought it.'
   (Abney, 1987, 39)

We can see, then, that some languages provide direct evidence of inflectional patterns within nominals that mirror the patterns that appear in clauses.

5.2   Adverb(ial) hierarachies

These kinds of parallels between DPs and CPs are just one instance of the consistent cross-linguistic structural hierarchies that syntacticians have uncovered. As is well-known among generative syntacticians, across languages there is astonishing similarity in the hierarchy of functional projections that occurs (Cinque, 1999; Ernst, 2014; Ramchand and Svenonius, 2014). We have been discussing this so far in this monograph as the pattern of CP dominating TP dominating VP cross-linguistically. To most syntacticians this is transparently obvious to the point of not being interesting. As Ramchand (2018) points out, however, this is not a logical necessity:

> Consider a hypothetical language spoken on the planet Zog. The planet Zog is a world very different from our own, inhabited by many strange creatures, one species of which has acquired symbolic thought and speaks its own form of language: Zoggian, which has properties found in no human language. In particular, Zoggian displays the bound morpheme /fub/, which denotes roughly 'the process of dissolving into a green slimy puddle.' In addition, it includes the bound morpheme -ax-, which has the semantics of PAST, and the bound morpheme ilka, which has the semantics of CAUSE. Like human languages, Zoggian works by generating hierarchical symbolic structures with predictable interpretations. However, unlike the Human PAST morpheme, the Zoggian PAST morpheme always occurs hierarchically closer to the conceptually rich part of the verbal meaning than the CAUSE morpheme does ... Suppose further that there are many Zoggian language families but that, with very few exceptions, CAUSE appears external to temporal information. This is no problem for a compositional semantics. Indeed, it is no problem for the semantics developed for Human languages either. (Ramchand, 2018, 4)

Ramchand's point, of course, is that human languages don't work like Zoggian. So "[w]e could imagine things to be otherwise, but they never are" (Ramchand, 2018, 6). She uses this observation to argue for an alternative approach to the compositional semantics of the clause that captures this generalization (CP>TP>VP). Here we simply want to observe that it's not a logical necessity that languages work like this. But they do.

Another way that this universal structural hierarchy emerges transparently is that there is a quite consistent cross-linguistic hierarchy of adverbials, organized according to their semantics.

(59)   Discourse-Oriented > Evaluative > Epistemic > Subject-Oriented (> Neg) > Manner
       (Ernst, 2014, 109)

So, taking an English example, an evaluative adverb like *unfortunately* necessarily precedes the epistemic adverb *probably*:

(60)   a. Albert **unfortunately** has **probably/obviously** bought defective batteries.

       b. *Albert **probably/obviously** has **unfortunately** bought defective batteries. (Ernst, 2014, 110)

More obvious distinctions appear for manner adverbs, which show up much farther right in an English sentence, structurally lower in the clause.

(61)   a. This orchestra plays even the soft sections **loudly**.

       b. The committee arranged all of our affairs **appropriately**.

       c. She faced her fears bravely. (Ernst, 2014, 111)

As pointed out by Cinque (1999); Ernst (2014) these patterns are familiar across a broad range of unrelated languages.[26] So in the Bukusu examples below, for example, evaluative adverbs appear high (to the left of subjects, or between subjects and verbs, as shown in (62)) and manner adverbials appear structurally low, at the right side of the sentence, as shown in (63).

(62) Bukusu *wakana* 'perhaps,' 'maybe'

      a. **Wakana** Wafula a-lakat-a   e-khafu
          perhaps 1Wafula 1SM-will.slaughter-FV 9-cow
          'Perhaps/maybe Wafula will slaughter a cow.'

      b. Wafula **wakana** ?(,) alakata ekhafu

      c. ??Wafula alakata **wakana** ekhafu

      d. ??Wafula alakata ekhafu **wakana** (Wasike and Diercks, 2016)

(63) Bukusu *kalaa* 'slowly'

      a. Ba-a-sakhulu ba-nywe-changa ka-ma-lwa **kalaa** (preferred)
          2-2-elder   2SM-drink-HAB 6-6-beer   slowly
          'Elders usually drink beer slowly.'

      b. Basakhulu banywechanga **kalaa** kamalwa

      c. *Basakhulu **kalaa** banywechanga kamalwa

      d. ***Kalaa** basakhulu banywechanga kamalwa (Wasike and Diercks, 2016)

As pointed out above in §2.1, one analysis for these generalizations is Cinque's (1999) hierarchy of functional projections: the proposal here is that adverbs sit in specifier positions of functional heads dedicated to the relevant semantics of that adverb. Crucially, on Cinque's account this functional structure is assumed to ba a part of UG.

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[26]For another entirely genetically and typologically distinct example, see Pearson (2000); Malagasy adverbs don't display the same linear order, but reveal the same hierarchical effects in unexpected ways.

(64) The universal hierarchy of clausal functional projections (Cinque, 1999, 106)
[*frankly* Mood*speech act* [*fortunately* Mood*evaluative* [*allegedly* Mood*evidential* [*probably* Mod*epistemic*
[*once* T(Past) [*then* T(Future) [*perhaps* Mood*irrealis* [*necessarily* Mod*necessity* [*possibly* Mod*necessity*
[*possibly* Mod*possibility* [*usually* Asp*habitual* [*again* Asp*repetitive(I)* [*often* Asp*frequentive(I)* [*intentionally* Mod*volitional* [*quickly* Asp*celerative(I)* [*already* T(anterior) [*no longer* Asp*terminative* [*still*
Asp*continuative* [*always* Asp*perfect(?)* [*just* Asp*retrospective* [*soon* Asp*proximative* [*briefly* Asp*durative* [*characteristically(?)* Asp*generic/progressive* [*almost* Asp*prospective* [*completely* Asp*SgCompletive(I)* [*tutto* Asp*PlCompletive*
[*well* Voice [*fast/early* Asp*celerative(II)* [*again* Asp*repetitive(II)* [*often* Asp*frequentative(II)* [*completely*
Asp*SgCompletive(II)*

This kind of cartographic research has been hugely influential and has led to a lot of empirical discovery. But there are various empirical and theoretical critiques. First, as pointed out by Wiltschko (2014) and Ernst (2014), while the broad patterns reported in (59) are affirmed crosslinguistically, the finegrained predictions of (64) cannot be replicated with the same consistency. And as Ernst (2014) suggests, proposal of a rich innate structure of categories like in (64) is not necessary: if we expect certain semantic properties associated with different structural heights within a clause anyway, the locations of adverbs can derive from these independent facts, without require. That said, the broader generalization of the (apparently) universal functional hierarchy itself is something in need of explanation, as explored by Ramchand and Svenonius (2014), Ramchand (2018), Ritter and Wiltschko (2014), and Wiltschko (2014). We largely rely on the proposals of Ritter and Wiltschko (2014) (and related work), which we outline here first.

5.3   Non-universal grammatical categories

As argued by Wiltschko (2014), despite the strong evidence for a universal functional hierarchy in the clause, as discussed above, there is nonetheless plenty of evidence that this universality cannot be universal in its details, but rather in more general domains. This builds from her work showing that grammatical categories themselves ought not be considered universal, which is also a common refrain among typologists (Haspelmath, 2010; Dryer, 1997; Croft, 2001; Haspelmath, 2007; Cristofaro, 2009). As Wiltschko (2014) points out, English (like many Indo-European languages) requires tense in matrix clauses: though there are unmarked forms, they have obligatory tense-related interpretations, such that even the absence of morphological tense-marking is informative as to the tense of the clause. As Dryer (1997) suggests, this kind of obligatory interpretation is suggestive that a category of Tense exists in English (and other European languages).

(65)   Tense marking on verbs in English
    a.   Yoshi play-ed with his ball yesterday.
    b.   *Yoshi play-s with his ball yesterday.
    c.   *Yoshi play with his ball.
    d.   *Yoshi playing with his ball.

As argued by Wiltschko (2014), Ritter and Wiltschko (2009), Ritter and Wiltschko (2014), many North American languages in fact lack tense as an obligatory category. Therefore no tense

Language-specific categories *c* are constructed from a universal set of
categories (κ) and language-specific UoLs ($c = \kappa + $ **UoL**)
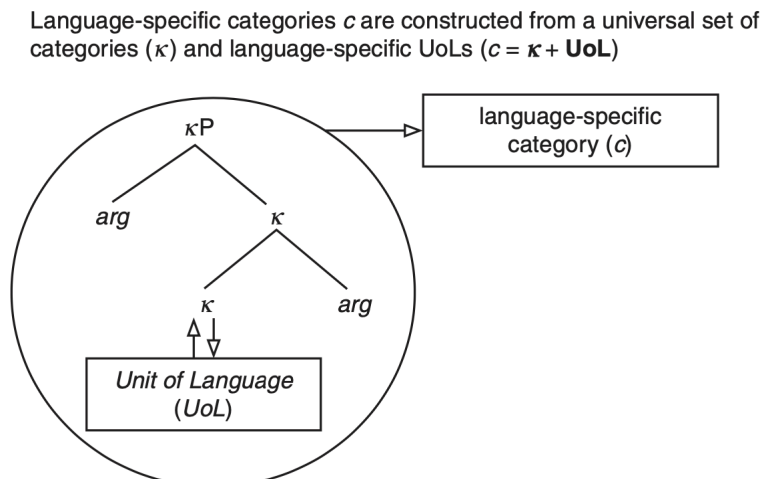
Figure 2.3: (Wiltschko, 2014, Figure 1.6 (i)). Images are screenshots for draft monograph, will be redrawn in final work.

is marked in (66) and, critically, there is no specific tense interpretation arising with the unmarked tense.

(66) Blackfoot, tenseless
ann-wa   Mai'stoo-wa isttso'kiniwa.
DEM-PROX Raven-PROX hungry.AI-PROX
'Mai'stoo is hungry.' OR 'Mai'stoo was hungry.'
(Wiltschko, 2014, 13)

Instead of an assumed inventory of universal grammatical categories, Wiltschko (2014) argues that what is universal is more abstract. That is to say, actual grammatical categories are constructed, language-specific, emergent. What is instead universal is the mechanism for constructing a category, and what she refers to as the "Universal Spine," an abstract set of domains within which language-specifical functional projections are constructed. Figure 2.3 illustrates her κP, which is the universal categorization mechanism. She uses the term *Unit of Language* to refer to the language-specific linguistic object that gets associated with a particular category to instantiate a grammatical category in a given language. So in English, for example, tense is the *Unit of Language* that is categorized (i.e. associated with a universal category), becoming T° and TP.

If grammatical categories aren't universal, what explains the hierarchy effects that are documented across the world's languages? Wiltschko (2014) and Ritter and Wiltschko (2014) argue that there is a *universal spine*, that is, an abstract set of domains that wherein units of language are associated with specific grammatical properties, or functions (using those descriptors in non-technical senses).[27]

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[27]Here, we use the Universal Spine proposal advanced by Ritter and Wiltschko (2014): Wiltschko's (2014) includes one additional domain (point of view) that sits between *anchoring* and the thematic domain. Wiltschko (2014) also refers to the thematic domain as 'categorization:' we retain the more familiar term 'thematic,' as used by Ritter and Wiltschko (2014).
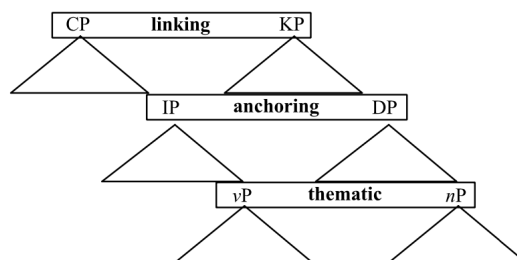
Figure 2.4: The "Universal Spine," defined by functional domains rather than grammatical categories (Ritter and Wiltschko, 2014, Example (5)) Images are screenshots for draft monograph, will be redrawn in final work.

Wiltschko (2014) proposes that the properties of the universal spine also underlie the DP-CP parallelisms that we began this section discussing. Because there is one Universal Spine, lexical categories (namely, nouns and verbs) will therefore share similarities in their extended projections precisely because the same abstract universal structure-building mechanisms underlie both: the universal spine and the κP categorization mechanism. As discussed by Wiltschko (2014), the *thematic domain* is where the event is introduced (i.e. predicate + participants). The *anchoring domain* anchors the event to the utterance, and discourse roles are introduced in the *linking domain*, linking the existing structure to the discourse.

On this approach, specific grammatical categories are not universal: instead, just as the realization of anchoring can be different between nominal and verbal projections, Wiltschko (2014) and Ritter and Wiltschko (2014) argue that it can be different across languages. So in many Indo-European languages, 'tense' is the anchoring category. So tense is obligatory (in matrix clauses at least) and is associated with core grammatical properties like agreement and case. They argue extensively that this is not in fact universal: in some languages person is the anchoring category, in others location: in such languages, tense is in fact (grammatically) option and not associated with the core grammatical effects that it is in Indo-European languages. What is universal is the anchoring function (in this instance).

For the sake of this discussion, let us adopt the Universal Spine hypothesis as proposed by Ritter and Wiltschko (2014) in Figure 2.4. Assume, therefore, that acquisition ought to proceed structurally upward by domain: thematic domain precedes anchoring, which precedes linking.[28] In broad senses, this is what we already showed to be true (in broad strokes) for acquisition: vP/VP is acquired before TP, which is acquired before CP. But the link to the more abstract domains of Wiltschko (2014) makes some additional predictions.

First, given this assumption and as discussed by Wiltschko (2014), this gives a natural explanation for DP-CP parallelism (which, for its broad recognition in the field, hasn't been given a strong explanation before Wiltschko 2014, to our knowledge). This is illustrated in Table 2.5. This of course makes a prediction: functional projections within similar domains ought to be acquired at similar points in time, whether in nominals or in verbal constructions.

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[28] As we will outline below, this corresponds broadly to distinct phases, which we think is no accident: see Chapter 4.

Verbal Spine	Universal Spine	Nominal Spine
CP	**linking**	KP
IP	**anchoring**	DP
*v*P	**thematic**	NP

Table 2.5: The universal spine in applied to nominal and verbal domains, adapted from Ritter and Wiltschko (2014, 1334)

It is of course well known that lexical categories (nouns and verbs, per our current concerns) are acquired first by children (Radford, 1990): this results in the so-called 'telegraphic' speech of early learners, lacking functional elements (including only lexical categories). But is it true, for example, that the anchoring domain is acquired simultaneously in nominals and in clauses? In fact, this is precisely what has been documented by Sutton (2017) for L1 acquisition in Estonian, Hungarian, and English. Sutton (2017) shows precisely this kind of parallel sequence of development, though it's important to note that nothing is immediate in acquisition. In both Hungarian and Estonian the verbal morphology preceded the onset of nominal morphology, but both occurred during the same period of acquisition, before higher-level aspects of acquisition (e.g. CP level material).

This suggests that there are parallel structures being built in acquisition: (at least) the nominal extended projection and the verb extended projection. As mentioned above, Vainikka and Young-Scholten (2011) propose the idea of a "master tree" - the overall generalization of sentence structure that a child is acquiring. We adopt a similar idea here, however, alongside the acquisition of sentential structure, children are also acquiring the "master tree" for nominals. As a result, it is likely improper to conclude that children are acquiring a single "master tree" as implied by Vainikka and Young-Scholten (2011): it is more precise to talk about acquiring the *master tree inventory*: the CP master tree (i.e. the extended projection of verbs), the DP master tree (the extended projection of Ns), and while we don't engage this here, likely also the master trees for other lexical categories, e.g. the AdjP master tree (the extended projection of Adjs), etc. We think it likely that there may be master trees for exemplars of major syntactic constructions as well, e.g. questions, passives, imperatives, etc. This is a direct parallel to the 'constructicon' proposed by Construction Grammarians (e.g. Goldberg 2006), the parallel to the lexicon but for constructions.

We don't think it's accidental that the theoretical metaphors developed for the Minimalist Program necessitate "building" noun phrases and sentences separately from each other. Consider for example the Merge operation below, which adds a DP to the existing *v*P structure to form a complete event, a *v*P with a agent/theme.

(67) $[_{vP}\ v\ [_{VP}\ V\ DP_{theme}\ ]\ ] + DP_{agent} \rightarrow [_{vP}\ DP_{agent}\ [_{vP}\ v\ [_{VP}\ V\ DP_{theme}\ ]$

Notice, here, that when the agent DP is merged into the structure, it is already built. The necessity for 'already-built' structures in the derivation of a sentence led Chomsky (1995, 2000a, 2001) to posit the notion of 'workspaces:' structures can be built independently of each other before being merged together into a clause. It is not just DPs that require this, of course, but in fact Minimalist assumptions require that any phrasal structure that is merged with the extended

projection of a verb must have been constructed separately before being merged. On our assumptions this is due to the fact that what is 'merged' into an adult grammatical structure is a structure drawn from the master tree inventory: e.g. the structure of a DP. And this is grammaticalized as part of a distinct structure from the clausal master tree, we argue in a parallel acquisition process that is driven by the same mechanisms that drive acquisition of of grammatical structures in the clausal domain: a categorization mechanism that can be associated with units of language (Wiltschko's 2014 κP), along with the domains making up the Universal Spine. We discuss what this looks like in more detail in §5.3 of Chapter 5. The metaphor of 'workspaces' captures the reality that children grammaticalize mental representations of noun phrases separately from their representation of clauses.

6 Summary: Developmental Minimalist Syntax

This chapter has sketched the main ideas of DMS (Developmental Minimalist Syntax), proposing that there is a systematic correspondence between the Minimalist derivation of a sentence and the timeline of acquisition of those structures. On this approach, Minimalist theorizing about 'UG' (Universal Grammar) is conceived of as articulations of the Grammaticalization Mechanisms that humans employ as they acquire their language. Categories are emergent, acquired by children based on evidence, and grammatical structures are acquired based on evidence as well. Operations like Merge and Agree are precise descriptions of the relationships among units of language that emerge as a result of grammaticalization (or, descriptions of the mechanisms themselves: the distinction between these options is immaterial for our current purposes). The universal properties of clause hierarchies are proposed to fall out from the Universal Spine (Ritter and Wiltschko, 2014; Wiltschko, 2014).[29] Following Wiltschko (2014), this allows for an explanation of the degree of similarities across languages will allowing for differences in both grammatical categories and in fine-grained details in functional hierarchies. Likewise, consistencies in phrase structures and dependencies across languages are explained by a shared mechanisms for grammaticalizing structures (Merge, Agree). The result is "upward" building of structure, which is parallel in both children's acquisition timelines and in adult grammatical knowledge.

In a way, if this turns out to be correct, this allows Minimalist theorizing to proceed as it currently operates—this is not a new theory of syntax. On the other hand, the subtle shift in perspective to considering UG mechanisms to be Grammaticalization Mechanisms does in fact change our perspective on the computational system. UG is considered a "blind" computational system, as standardly practiced it is understood to generate outputs (sentences) from inputs (lexical items in the Numeration). The perspective shifts with DMS: it may still be a context-blind system in the sense that the core UG mechanisms can be independently formalized, but in this case it now takes sentences as input (a child's linguistic input) and generates grammatical structures as outputs. The result is that the same mechanisms can describe adult grammars (as, of course, it was these mechanisms that built the adult grammars). But we might expect that the mechanisms overdetermine possible outputs in the adult grammar that aren't realized, which

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[29]Wiltschko (2014) suggests the possibility that the Universal Spine itself could fall out from more independent properties of cognition, emergent based on properties of cognitive development. Rakhlin and Progovac (2020) explore this same kind of possibility.

should be unproblematic, as the language-particular system was of course generated in order to capture particular targets that were available in the in put to begin with.

We have shown that these kinds of ideas have been persistent within work on language acquisition, though never having reached any kind of broad acceptance. We sketched both the precursors to these ideas in the literature, and the range of empirical evidence that suggests that the broad predictions of this account are upheld: roughly speaking, it does appear that children acquire syntactic structure from bottom to top. That said, there are a range of empirical complications: for example, wh-words at the left edge of a sentence are one of the earliest constructions that English-speaking children acquire, not the latest (as a strict correlation of acquisition-timeline-to-structural-height might imply). Recall again, however, that the core proposal is not, in fact, that syntactic structures are acquired from bottom-to-top. Rather, the claims of (4) is that the derivation of a sentence, as constructed according to Minimalist assumptions, mirrors the acquisition timeline. So when there are divergences from the strict bottom-up acquisition time-line, we would expect these divergences to remain encoded in adult grammars as well. This is what we claim is the foundation for the presence of counter-cyclic syntactic operations in adult grammars.

Chapter 3

Deriving Counter-cyclicity

There are important caveats to the description of DMS in Chapter 2, which come back to the core focus of this paper. The preceding claims are novel in the sense that they themselves have not been articulated in this manner (to our knowledge), nor has there been made such an explicit, pervasive link between Minimalist derivations and acquisition timelines as we've articulate as part of DMS in (4). That said, what we've argued for so far can be considered a rather direct modernization and refinement of previous proposals for syntax-acquisition correspondences (see §2.3). It is our claim, however, that there is an *additional* syntax-acquisition correspondences that has not been previously noted in this discussion, namely, counter-cyclic processes.[1] Recall the formulation of DMS in (4), which we repeat here as (68).

(68) **Developmental Minimalist Syntax (an interpretive principle)**
The Minimalist derivation of adult language structures recapitulates the ontogenetic development of those same syntactic structures.

Centrally, we are not simply claiming a structural-directional correlation (timeline of acquisition to structural height). Rather, we are proposing that a Minimalist derivation of a sentence is itself what correlates to acquisition. Of course, in most instances, this is a straightforward 'bottom-up" derivation (at least, per standard Minimalist assumptions). But our claim in this work is that counter-cyclic operations are in fact real parts of syntax (both empirically, and theoretically) and therefore a Minimalist derivation is not, in fact, as strictly "bottom-up" as the citation version of the theory suggests.

DMS Principle #7 (69) articulates this claim specifically, which adds nothing new from (68), but makes explicit the stance about counter-cyclicity. And (69) in turn entails (70).

(69) DMS Principle #7:
Counter-cyclic phenomena in adult language grammars correlate to counter-cyclic acquisition processes.

(70) DMS Principle #8:
Counter-cyclic syntactic operations exist.

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[1]As we mention in §2.3 of Chapter 2, Lebeaux (2000) does propose that counter-cyclicity has relevance in this kind of way, but that discussion appears to have been largely overlooked by the field.

(69) summarizes our claim (substantiated empirically below) that there are instances where adult-like acquisition of a grammatical element is not available to a child. This can be for various reasons. In instances of so-called "early" acquisition of some element, this will correlate with look-ahead. Our case study of these kinds of instances is wh-movement. The counter-cyclic puzzle is that wh-phrases (in adult grammar) appear to move before they should, i.e. before the final landing position has been added to the structure. This correlates to a prediction, per DMS, that children ought to acquire wh-movement "before they should," i.e. before they have acquired the correlating CP structures that are the target of wh-movement. We show in §2 that this is the case. In this instance, then, counter-cyclicity arises because the full adult structure has not developed yet, but their input (PLD, primary linguistic data) offers a large supply of overt evidence for wh-words being sentence-initial. Therefore, children easily conclude that wh-words are sentence-initial at stages earlier than the final grammaticalization of CP.

Likewise, we propose that there are instances where children acquire structures "late," meaning, a pattern is acquired that is located a specific structural height in a tree, but only after additional structures beyond that point have been acquired. We claim that this does not eliminate the possibility of merging at that position, but instead that "late merger" operations of the sort discussed in §2.1 are in fact available grammaticalization operations.

But why would Late Merger be necessary in the first place? Why could children not simply acquire the relevant structures on the strict cycle of Merge alone? This is where we think the link between adult structures and child acquisition may in fact be quite informative in broader research on cognition. In all of the areas where we have seen plausible syntactic proposals, the late-merged element is a structure of relevant complexity that is either plausibly (or obviously) not grammaticalized in acquisition until after subsequent, more complex clause structures are acquired. We outline the empirical arguments for this below, but in short, we have seen proposals for late mergers of DP-content (noun phrases exist early in acquisition, plausible, but fully complex DPs are not acquired until late), adjuncts (modifiers/ornaments of existing clause structures), adjunct clauses/relative clauses (requiring acquisition of full clause structures) and constructions bearing on discourse/pragmatic structure (e.g. topic/focus): all of these are plausibly XPs that are not grammaticalizable as Merged into the structure at the point on acquisition where they occur, because they contain a complexity of structure or complexity of semantics that the child has not yet acquired sufficient background to acquire, or which is cognitively inaccessible to a child.

In short, counter-cyclicity emerges when a child does not grammaticalize a pattern that they are encountering on the usual cycle of strict structure-building via Merge. If they can access the data patterns themselves (e.g. wh-questions) but they do not yet have the grammatical knowledge to fully grammatically incorporate the data pattern they observe, a look-ahead problem is created: aptly named, because the child can in fact "look ahead" to what the final structure looks like (to some degree, depending on the interpretability of the data to them). This is because DMS is not the blind computational system that the Minimalist itself is: under DMS, children are in fact "looking ahead," as the UG-mechanisms are being employed to grammaticalize the patterns they are observing.

Conversely, a child may be unready to grammaticalize a pattern not because they don't have the requisite grammar knowledge built to incorporate the pattern, but because they instead

don't yet have the cognitive ability to analyze a pattern to be grammaticalized in the first place. We will suggest in §3 that this is what is at work in instances like Zulu conjoint/disjoint and object marking constructions.

Of course, the predictions DMS makes are vast, covering grammatical constructions in every language, the vast majority of which we can't possibly begin to address. As such, we take a case study approach. We chose case studies on the basis of their prominence in *both* the syntactic literature and the acquisition literature, with the exception of Zulu object marking, which was chosen as a clear illustration of counter-cyclic phenomena in morphologically transparent contexts, relevant for a discussion of the viability of Late Merger from an acquisition perspective.

1 A DMS account of anti-reconstruction effects

1.1 Recap: Late Merger

As a reminder, the anti-reconstruction effects we are discussing is specifically the structure of relative clauses. It is well-known that A'-moved elements can behave (for the purposes of binding) as if they are in their base position. So the example in (71) is expected to be unacceptable because the pronoun binds the R-expression *John*, which violates Principle C.

(71)   *He_k avoided Mary's examination of $John_k$.
       (Sportiche, 2017, 2)

The outcome in (72) is less expected: here, the pronoun no-longer c-commands the R-expression *John*

(72)   *Whose examination of $John_k$ did he_k avoid?
       (Sportiche, 2017, 2)

The long-standing analysis of this is of course that the base position of the moved phrase is in object position, and in that position, the pronoun does c-command the R-expression.

(73)   *Whose examination of $John_k$ did he_k avoid ~~Whose examination of John~~$_k$?

This simply illustrates the long-standing observation that A'-moved elements necessarily 'reconstruct,' i.e. are interpreted in their base position. Of course, the puzzling factor is that sometimes such reconstruction doesn't occur, resulting in anti-reconstruction effects (i.e. instances where we would have expected reconstruction, but don't see its effects). So while the unacceptability of (74a) is expected, the lack of reconstruction in (74b) is not. So, if the relative clause in (74b) reconstructed in the same manner as the complement clause in (74a), we would expect a Principle C violation to arise in (74b) as well, but it does not.

(74)   a.   *Which report [that $John_k$ was incompetent] did he_k submit?
       b.   Which report [that $John_k$ revised] did he_k submit?
            (Freidin, 1986, 179)

The pattern in (74b) is termed an anti-reconstruction effect, as we expected the see the effects of reconstruction, but we don't. The distinction between (74a) and (74b) is interesting and has received much discussion in the literature (as well as some denying a real distinction between the two, claiming anti-reconstruction effects hold for complement clauses as well), but our concerns here are mainly on the existence of anti-reconstruction in the first place, so we don't concern ourselves with that debate here.

As we discussed in §2.1, the standard analysis for such issues has been to claim that the relative clause is Late Merged: it is only adjoined to its head nominal after that nominal has undergone A'-movement. We discussed this in more detail in §2.1 (as well as the critiques offered by Chomsky 2019 and Sportiche (2019), among others). At least for the approach that attempts to capture the distinction between reconstruction effects for complement clauses vs. relative clauses, the argument is that relative clauses are Late Merged (after A'-movement has occurred) whereas this is not the case for complement clauses. We refer to our overview of the facts in the introductory chapter (§2) for more background.

1.2 Late Acquisition of Relative Clauses

According to DMS, late merger analyses make very specific predictions. Namely, the late-merged constituent ought to be acquired later than the grammatical structures it is late-merged to. Here, we expect that relative clauses ought to be acquired later than wh-movement.

So what does the acquisition pathway for relative clauses look like in actuality? Compared to complement clause constructions—both nonfinite and finite—relative clauses are infrequent in early child speech. Diessel (2004) observes the earliest relative clauses at 1;9 years of age, which almost always take the form of predicate nominative relatives containing a single proposition (e.g. *Here's the tiger that's gonna scare him*). Over 75% of children's early relative clauses are exemplars of this type of presentational relative construction (Tomasello 2003). In fact, Diessel (2004) notes minimal development in relative clause structure (i.e. these structures do not become any less formulaic) up through 5;1 years of age. However, around the fifth birthday, relative constructions including two distinct propositions begin to account for a higher proportion of the data.

Interestingly, studies that probe children's knowledge of relative clauses demonstrate that these types of structures are difficult for children to comprehend even after six years of age (Guasti 2016). This is particularly striking, since comprehension generally precedes production. In this way, it is not unreasonable to posit that children might not productively generate novel types of relative clauses until well after they reach six years old, and may not fully grammaticalize relative clause structures until that point. The interpretive difficulty of relative clauses is particularly pronounced with center embedded relative clauses (e.g. *The cow that jumped over the fence kissed the pig*). In a similar vein, children often incorrectly interpret the relative clause in sentences like *The pig bumps into the horse that jumps over the giraffe* (i.e. an OS sentence, since the relative clause modifies the object and contains a subject gap) as a modifier of the matrix subject (Guasti 2016), though some degree of this trouble is surely attributable to processing effects.

Although early relative clauses are not frequent in English-learning children, the ones that do occur differ from the adult targets in two primary respects: pied-piping is absent and resumptive pronouns are common. Because spoken English does not necessarily require pied-piping of

prepositions with relative clauses (e.g. *The man to whom I sent the letter lives in Canada*), it is not shocking that pied-piping is avoided in production and rejected in comprehension by three to six year old English-speaking children (McDaniel et al. 1998). However, certain languages (e.g. Italian, French, and other Romance languages) mandate this type of pied-piping. Nevertheless, it is rarely found in the early speech of learners of these languages; instead, children replace the preposition-relative pronoun complex with the complementizer (Guasti 2016: 298):

(75)   le   couteau que [avec lequel] le   monsieur coupe le   sapin   French
         the knife   that [with which] the man   cuts   the fir-tree
         'the knife with which the man is cutting the fir-tree'

The problem with pied-piping in relative clause constructions does not reside in an inability to pied-pipe per se, since children at this age produce pied-piped interrogatives (Labelle 1990).

On the other hand, resumptive pronouns are abundant in early relative clauses (e.g. *The one that he lifted it*) even though these constructions are not grammatical in the target language. This observation holds cross-linguistically at least for continental French, European Portuguese, Spanish, Serbo-Croatian, and English (Guasti 2016). That is not to say, however, that resumptive pronouns in relative clauses are a unique feature of child language; many languages including Irish, Palestinian Arabic, Welsh, nonstandard English, and certain colloquial varieties of Romance allow for resumptive relatives. These non-adult-like patterns (i.e. the avoidance of pied-piping and the use of resumptive pronouns) persist until around nine years of age, at least for children acquiring Romance languages (Guasti 2016). In this way, then, the acquisition of adult-like relative clause constructions shows significant acquisition delays, even compared to other relatively late linguistic acquisitions like finite complement clauses. Notably, wh-questions are acquired long before this. While there are distinctions between languages on the details, children are generally acquiring wh-movement quite early (and in English, where our anti-reconstruction evidence comes from, children are beginning to use wh-questions even during the root infinitive stage, see §2).

In fact, the link between Late Merger and Late Acquisition was part of the original proposal about Late Merger from Lebeaux (1988). While the specific mechanisms and acquisition processes that he proposed have not been retained in either the acquisition or syntax literature (to the extent we can determine, at least), embedded in his discussion is the assumption that children acquire target-like relative clauses relatively late. Recall from our discussion in §2.3 of chapter 2 that Lebeaux proposes a maturational account, where different structure-building operations "come online" at different times. His discussion of relative clauses is extended and has a complex set of operations, but the key observation is that he likewise recognizes the late grammaticalization of relative clauses and seeks to explain the apparently acyclic structure of relative clauses as connected with this acquisition timeline.

We interpret this as strong support for the DMS approach. Not only does acquisition appear to proceed bottom-up in terms of the broad structures of clauses, but we also seem to find alignment between exceptional instances of counter-cyclic adult processes, and acquisition that maps onto that counter-cyclic process. Here, in adult grammars we see that relative clauses behave as if they enter the structure late in the derivation, late merged into a noun phrase after it has undergone A'-movement (despite this being largely unacceptable per standard Minimalist

assumptions). We think it is no coincidence, then, to find that relative clauses are themselves acquired late. That relative clauses are acquired late is not surprising given their relative complexity (and, the fact that they are embedded clauses, which of course necessarily requires acquisition of clause structures more generally before the embedding of those clause structures can be acquired).

1.3 Summary: DMS account of Late Merged Relative Clauses

The claim that we have proposed is that there is a systematic correlation between late merger operations and "late" acquisition processes. To be clear: we are not claiming these are instances of *delayed* acquisition in any developmental sense, but assuming a strict bottom-up canonical method of grammaticalization, these occur outside that idealized timeline. The main claim of DMS as state in (4) is that derivations of adult grammatical representations recapitulate acquisition pathways. The idea is that adult grammar knowledge retains earlier stages of knowledge, with new grammatical knowledge added to existing knowledge. In canonical instances, this results in monotonically building the structural hierarchy: more recently acquired functional projections are structurally higher.[2] There are systematic exceptions to the "later-is-higher" prediction, however: the claim in (4) is not actually that structurally higher material is acquired later, but that the sequence of structure-building in the derivation itself is a record, in a sense, of the sequence of structure-building in acquisition. This means that operations like Late Merger in fact find a place in DMS (where in standard Minimalist analyses they struggle to do so): operations that happen later in the derivation of a sentence (on this proposal) are simply those that were acquired at that same late stage of syntactic development in childhood. As for a syntactic analysis of Late Merger, we have nothing new to propose: we can simply accept that whatever the outcome of the ongoing syntactic discussions of these constructions are will suffice. Our contribution is that these kinds of empirical realities are in fact expected on a DMs approach, so there is no need to attempt to exclude counter-cyclic operations from our model of syntax (perhaps all we need to do is posit more systematic, explicit constraints on their application).

There are other areas where this test case can be examined further, but which we leave for future work. For example, a prominent analysis of degree clauses in comparatives posits that the degree clauses are late merged to degree heads in comparative constructions like the underlined phrase in *Maisha ate more rice than Alex (did)* (Bhatt and Pancheva, 2004). As Syrett (2016) lays out, comparatives are quite complex semantically in addition to syntactically, and "given all of these conceptual and linguistic components, it is perhaps not surprising that children are known to produce comparatives that differ markedly from the adult form up through at least six years of age" (464). Therefore even at later stages of syntactic development, various kinds of errors are still present in children's comparative constructions. The examples below are drawn from Syrett (2016, (2)-(6)).

(76) a. (be)cause it's gonna be **more dirtier** # huh Ma ?   Sarah 4;10

b. Put it **more further** away   Olga 4;3

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[2]This is certainly an over-simplification, which we discuss at length in §4; but it suffices for our discussion at this point.

c. I wan(t) (t)a make the **prettiest than** the whole wide world. Adam 5;2

d. That's **orange than** my room. Rachel 2;10
   (Rachel pointing to a card that is about the same shade of orange as the wall in Rachel's room)

e. It's **bigger just like** the truck Adam 3;5

Evaluating DMS is much more difficult in this context, given the semantic complexity of comparatives. And while there is a host of research on acquisition of comparatives, there is much more on the relevant semantic aspects of the constructions and less on the components of the syntax themselves. So more work is needed, but what is known is at least consistent with DMS: late-acquired structures show counter-cyclic properties in adult syntax.

Another prominent area where late merger accounts are proposed is for adjuncts in general (Stepanov, 2001; Nissenbaum, 1998; Sauerland, 1998; Abe, 2018; Haddad, 2019; Zyman, 2020). To our knowledge most of those claims center on clausal adjuncts rather than nominal adjuncts (and conversely, it appears that most of the acquisition work on adjuncts is on nominal adjuncts, as opposed to clausal adjuncts). Adjectives and adverbials may be another potential testing ground for correlations between counter-cyclic adult grammatical structures and delayed acquisition of adjunct structures (delayed with respect to the acquisition of their sites of adjunction, not delayed in any other sense).

2 Input-driven movement: a DMS account of look-ahead in *wh*-movement

2.1 Reminder: Wh- look-ahead in Minimalist derivations

Recall the core look-ahead phenomena that we have been concerned with, wh-movement in English. The basic example we were referencing above is repeated in (77):

(77) $[_{\text{CP[WH]}}$ What$_k$ do $[_{\text{TP}}$ you ~~do~~ think $[_{\text{CP}}$ $\boxed{\text{what}_k}$ $[_{\text{CP}}$ that Alex ate ~~what$_k$~~]]?

The problem here for a Minimalist derivation (on standard assumptions) is that on the bottom-up derivation of a sentence, the embedded wh-object *what* must raise to Spec,CP of the embedded clause in order to escape spellout in the embedded clause and to be accessible to move to the higher clause (the circled copy). But there is no clear motivation for the wh-phrase to make its initial movements, as at the time that it must move, the matrix CP has not yet been merged (the matrix C head being the one with the relevant Q/WH features to motivate movement.

The problem is even present in a simple object question, once the *v*P phase is considered. So in (78) the wh-phrase moves to the edge of *v*P despite *v*P not being the target of the wh-movement, and before the matrix C is merged.

(78) $[_{\text{CP}}$ What$_k$ did $[_{\text{CP}}$ Alex ~~did~~ $[_{v\text{P}}$ ~~what$_k$~~ $[_{v\text{P}}$ ~~Alex~~ eat ~~what$_k$~~?

Therefore we see that wh-movement to the left edge of intermediate phases occurs before the

derivational stage at which the movement would be syntactically motivated. This look-ahead has attracted plenty of attention from syntacticians, as discussed in §2.2.

2.2   Evidence for early acquisition of wh-questions

So what are the predictions of DMS? The stance that we take is that counter-cyclic operations are real, and are reflected in acquisition pathways. What this predicts for look-ahead in English wh-movement, then, is that English-learning children ought to demonstrate knowledge of wh-movement well before there is strong evidence that they have acquired CP. And in fact, wh-questions do emerge quite early in English-speaking children, long before they would be expected to if acquisition proceeded in a strict bottom-up fashion. English-speaking children begin producing questions quite early in development (circa 1;6 years), meaning that wh-questions appear frequently during the root default stage of acquisition, before even the TP domain is grammaticalized, let alone the CP domain:

(79)   a.   Where daddy go? (Adam 2;3)
       b.   What I doing? (Eve 2;0)
            (Thornton 2016, (3), attributed to Guasti 2002)

If we assume at this point that children either have (or are) grammaticalizing a *v*P domain, what limited wh-generalization children have acquired here would necessarily place the wh-phrase at the left edge of *v*P. It does seem clear that children in wh-movement languages are never tempted to place wh-phrases in the argument's true *in situ* argument position, instead always surfacing a wh-phrase at the left edge of their utterance.[3]

(80)   WH ∧ [$_{vP}$ ...]

It is probable that children's grammars do not include the full CP-level when they first begin producing questions. This is probably even just based on the relative timelines, but at this stage of development errors with subject-auxiliary inversion persist. Likewise, full subordinate clauses, which would signal full grammaticalization of CP, are some of the latest constructions acquired (especially those containing complementizers and containing an independent proposition from the main clause) (Diessel, 2004). We similarly see issues with subject-object inversion persisting to late stages of acquisition (see Thornton 2016 for an overview).

We see at least two ways of analyzing this in the child's developing grammar. One possibility is that (along the lines of Roeper 1992) children adjoin unanalyzed *wh*-phrases to the front of the clause, producing apparently CP-level structures without CP. Such an approach is not dissimilar to the usage-based proposal of Rowland et al. (2005), who claims that a wh-aux sequence is used as an unanalyzed chunk (a pivot, like children's initial pivot schemas) together with a variable, where the variable in this instance is their already-grammaticalized sentence structures (so, a combination of unanalyzed material and analyzed material). Alternatively, we could assume something along the lines of Clahsen et al. (1993/1994), who propose that German children utilize an underspecified functional projection (FP) as a target of movement while they

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[3]With the exception of long-distance wh-movement, more on that below.

are acquiring structurally higher patterns. In this case, wh-phrases could be considered simply as moving to the edge of FP, a projection children are hypothesizing and using but have not fully analyzed yet. We don't attempt to provide a detailed theory of intermediate stages of knowledge here (the aforementioned accounts may not be mutually incompatible), though we do discuss this at some length in Chapter 4.

Separate from the question of the precise nature of the intermediate grammar children are developing, key for our point here is that children have internalized a wh-movement construction that can apply at the root default/root infinitive stage, before CP level structures are added. It is our claim that this directly corresponds to the counter-cyclic 'early' movement of wh-phrases in adult grammars.

The early appearance of both yes-no and *wh*-questions in English is not necessarily surprising, as these constructions are both common in children's linguistic input and important for children's communicative needs. As such, by "early" acquisition we do not mean that something astonishing is happening in acquisition, but rather that there is sufficient evidence available to children in child-directed speech and in other data they have access to that wh-phrases are displaced at the left edge of a sentence, that they are able to replicate this displacement in their emerging grammar without fully grammaticalizing CP structures. And this is key to the DMS account: all other things being equal, we would expect structure-building (in acquisition, and therefore in adult grammars) to proceed in a strict bottom-up cycle based on Merge. Divergences from this cycle should be for reasons. In instances where operations happen *late*, this is presumably because the operations and/or the data were inaccessible to the children. On the other hand, instances where an operation happens *early* should be instances where the children have ready access to data that demonstrates the pattern.

In the case of wh-questions, this is certainly true. as Cameron-Faulkner et al. (2003) show, in two separate studies questions were shown to make up significant proportions of child-directed speech: 21% (Wells, 1981) and 32% (Cameron-Faulkner et al., 2003), respectively. Wells (1981) yes/no questions appeared more frequently (13% of utterances) than wh-questions (8%), whereas Cameron-Faulkner et al. (2003) found that yes/no questions and wh-questions appear at roughly the same rate (15% and 16%, respectively).[4] In fact, in both studies questions were more frequent in child-directed speech than SV(O) declaratives (18% in both studies). It is clear, then, that children are exposed to questions at very high rates.

2.3   Long-distance wh-movement

Some of the best illustrations of successive-cyclic wh-movement come from instances of wh-copying; in some languages a wh-phrase may be pronounced in multiple positions in the clause, specifically, at the edge of CP phases (see Felser 2004 for discussion of examples from a range of languages).

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[4]Cameron-Faulkner et al. (2003) suggest that the relatively high rate of questions in their study was an artifact of the design.

(81) ⟦Wen⟧ glaubst Du, ⟦wen⟧ sie getroffen hat? (Adult) German
 who think you who she met has
 'Who do you think she has met?'
 (Felser, 2004, (1a))

It was originally more indirect evidence that led syntacticians to positing long-distance movement through CP edges, but as cross-linguistic research has expanded, it has become clear that there is robust evidence of grammatical representations of long-distance-moved elements at the edges of CPs (and at phase edges more broadly).

Even in languages that don't allow for such wh-copying, similar constructions have been documented in child language despite their non-target-like character. Thornton (1990) discovered that children learning English will produce such examples:

(82) a. ⟦Who⟧ do you think ⟦who⟧ Grover wants to hug? (TI 4;9)
 b. ⟦What⟧ do you think ⟦what⟧ Cookie Monster eats? (KM 5;5)
 c. ⟦What⟧ do you think ⟦what⟧ the baby drinks? (MA 3;3) (Thornton, 2016, (18))

The literature on long-distance wh-movement in children is complicated, as despite their ability to produce long-distance questions, children struggle to interpret them in adult-like ways long past the time when they produce versions of them themselves (an interesting inversion of the normal receptive/expressive distinctions in child language knowledge, and one which also emerges with relative clauses: Diessel 2004). We refer the reader to Thornton (2016) and Roeper and de Villiers (2011) for excellent overviews of the relevant literature. Beyond this, there are a range distinct proposals analyzing the nature of children's intermediate knowledge at this stage. Detailing the full range of the literature goes far beyond our concerns here. What is necessary to demonstrate the DMS points on counter-cyclicity is simply to show that, at the point when the (apparently) unmotivated wh-movements are occuring, children in fact have good evidence for those movements and there is good reason to think they have acquired the requisite knowledge to at least mimic the surface patterns.

Ranges of proposals have been set forward to account for the patterns in (82), and related patterns. Some have relied on notions of switching parameters (McDaniel et al., 1995), others proposing intermediate (non-adult-like) grammars that are stepping stones on the way to the target grammar (Thornton, 1990; Roeper and de Villiers, 2011). Grolla and Lidz (2018) claim that accounts depending on non-target-like grammars are unable to account for the variability that they document among children and even within an individual child, suggesting instead that wh-copying results from a kind of perseveration error, where activation of features of the matrix wh-element leads to the erronious conclusion of additional wh-elements later in the sentence. They build on Dell's (1986) "spreading activation model for sentence production, which postulates that items that will enter the derivation are selected among those with high level of activation" (Grolla and Lidz, 2018, 293). This model accounts for things like slips of the tongue, for example, *blue bug* may be erroneously pronounced *blue blug*,as the incorrect [bl] onset is more active than the correct [b] onset in *bug* and is incorrectly inserted.

On this account of wh-copying constructions in child language, as in (82), what occurs is a failure to inhibit pronunciation of the wh-element at the edge of the embedded CP. Therefore,

they link these doubling constructions with the limited executive control of children, rather than to a non-target-like intermediate grammar. Grolla and Lidz (2018) suggest that this only occurs in positions where they are otherwise allowed in the language. Notably, these kinds of doubling constructions (here, and elsewhere, for example auxiliary doubling in child language) only occur not just in positions where the doubled element could plausibly occur, but rather, they only occur in positions where Minimalist syntactic accounts propose that a copy in fact occurs in the target grammar of those constructions. We find it plausible that this is in fact the result of an inhibition error, but that what the children fail to inhibit is pronunciation of the wh-element that is in that position.

No matter what the account, what is consistent among all of the accounts is that, even before the target grammatical construction has been acquired, children obviously have some kind of a fledgling grammatical representation wh-elements at the edge of the embedded CP. This is reasonable in general in that embedded clauses in general are fully acquired after matrix wh-questions, such that any child producing a structure like this has also acquired matrix questions (also evidenced by the examples themselves, where the consistent pattern is a fronted wh-phrase, with the intermediate structures still being sorted out, much as is the case in the acquisition of matrix questions with delayed auxiliary inversion).

The point being, of course, that 'look ahead' in long-distance wh-movement can be readily accounted for on a DMS approach. An example like (77) (displaying successive-cyclic movement through the intermediate CP) can be explained if, at the point that children are acquiring the CP-level structures that make up the intermediate CP, they have evidence for movement of wh-phrases to CP-level positions already and matrix CP specifically. And, of course, they do: long-distance wh-constructions are themselves only grammaticalized after the acquisition of simpler wh-constructions is already well underway. And in the process of acquiring long-distance questions, we can posit that they are utilizing their knowledge of two different construction frames: a matrix question, together with an embedded clause that is a construction with a gap: namely, a wh-like construction. The puzzle of look-ahead in successive-cyclic movement comes in two pieces. Why must movement proceed by phase edges, for one, and second: how does the computational system know that the wh-phrase must move, before the matrix CP is merged? We will see that both questions fade somewhat looking at what knowledge children already possess at the point that they are grammaticalizing long-distance wh-constructions.

Work from Diessel (2004) sets the stage for understanding the transition to these kinds of complex embeddings. Diessel suggests that this same kind of pattern is how embedded clauses come about at all. He argues that the first (apparent) embedded clauses are not truly embedded at all, in the sense of representing a distinct proposition from the matrix clause. Instead, the apparent "matrix clause" in early productions of clause-embedding verbs is highly restricted: it only occurs with a few predicates at first, is extremely short, with either no overt subject or a pronominal subject (often first person), and with little-to-no evidence that the matrix 'clause' is a distinct proposition from the lower clause. That is to say, the matrix clause of initial apparent clause-embedding structures will be short, present tense, and will not serve to communicate any separate proposition—instead, what is found is a high frequency of a small number of identical or highly similar clause-introducing formulaic statements:

(83)   Sarah (first 15 utterances including *think* plus S-complement)

<u>I think</u> I'm go in here.	3;1
And <u>I think</u> (pause) we need dishes.	3;2
<u>Think</u> some toys over here too.	3;3
<u>I think</u> I play jingle bells . . . with the record player.	3;5
<u>I think</u> he's gone.	3;5
Oh (pause) <u>I think</u> it's a ball.	3;5
It's a crazy bone (pause) <u>I think</u>.	3;5
<u>I think</u> it's in here.	3;5
<u>I think</u> it's in here . . . Mommy.	3;5
<u>Think</u> it's in there.	3;5
<u>I think</u> I don't know that one.	3;6
I'm get my carriage (pause) <u>I think</u>.	3;6
<u>Think</u> it's in this.	3;6
<u>I think</u> that your hands are dirty.	3;6
<u>I think</u> my daddy took it.	3;7
(Diessel, 2004, 92)	

Diessel (2004) takes the "matrix' clause of utterances as in (83) to be formulaic: that is, the true main clause the child is uttering (the main point of their utterance) is the lower clause.

Even as clausal embedding develops further, their appears to be a successive stage where the "matrix" clause is less formulaic and more substantial, but still does not appear to communicate an entirely separate proposition, instead simply serving as an evidential-like statement: "rather than denoting a mental state or an act of perception, they function as epistemic markers, attention getters, or markers of illocutionary force" Diessel (2004, 77).

(84) Development of Sarah's matrix clauses including *think*, with # of instances in the corpus in parentheses (adapted from Diessel 2004, Table 5.4)

- 3;1 I think _ (68)
- 3;7 Do you think _ (5)
- 4;0 I thought _ (7)
- 4;2 I'm thinking _ (1)
- 4;3 They think _ (1)
- 4;4 What do you think _ (2)
- 4;8 I don't think _ (2)
- 4;10 I'll think _ (1)

Diessel (2004) notes that such uses (which he terms 'performative') are in fact a part of adult speech as well, where the main clause does not seem to really be offering a separate proposition as much as providing descriptive/evidential information about the embedded clause. Consider the possible answers to the question in (85):

(85) Who was Louise with last night?
- a. She was with Bill.
- b. Henry thinks/I think that she was with Bill.
- c. Henry believes/I believe that she was with Bill.

     d. Henry said that she was with Bill.

     e. Henry suggested that she was with Bill.
        (etc)
        (Simons, 2007, (2))

As Simons (2007, 1036) points out, "[r]esponse (a) is a straightforward assertion which answers A's question: Louise was with Bill. [The other responses] are ways of proferring that same answer, but with some degree or other of limited certainty. The answer, though, is in these cases the content of the embedded clause. So here, the content of the embedded clause, not the main clause, constitutes the main point of the utterance." There are of course other kinds of clausal embedding (even complement clauses) where the matrix clause is the main point of the utterance. This does illustrate that even in adult language such utterances exist, and Diessel (2004) makes a plausible case for an earlier stage of child 'embedded' clauses where the embedding is only apparent. The pathway that he proposes is shown in (86), where a formulaic matrix clause precedes a pattern more similar to (85) (where this is more flexibility in the structure, but still where the "embedded" clause is the main point of the utterance). True subordinate clauses (paired with a true subordinating clause) only appear later.

(86)   formulaic matrix clause > performative matrix clause > assertive matrix clause
      (Diessel, 2004, 112)

As for the final stages of development, as Diessel (2004, 114) also notes, "the child must be able to understand that reality and mental representation do not always match and that different people might have different beliefs about the same state of affairs in order to use mental verbs in performative and assertive matrix clauses." But children don't fully pass false belief Theory of Mind tasks until around the age of 4: de Villiers (2007) argues that the development of theory of mind and the final grammaticalization of embedded complement clauses are linked. The causal relations are not our concern here: our point is that these are grammaticalized much later than children are actively using (apparent) embedding structures. Diessel doesn't claim that every lexical item moves along this pathway: lexical items acquired later, building on the generalizations built from earlier constructions, can be acquired more directly in their target-like grammars. But he does claim that this is the pathway to acquiring clausal embedding in the first place.

What this suggests, however, is that children are utilizing their knowledge of the master tree of a clause to generate early embedding structures: in fact, the earlier 'embedded' structures don't appear to be really embedded at all, but instead an instance of a more formulaic (potentially multiword) sequence combining with a lower grammaticalized structure, presumably a TP-level structure at the points being considered here. So, assuming that brackets demarcate grammaticalized constituents, we will see utterances looking like (87), where we use the logical connective '∧' to annotate this kind of agrammatical combination of structures in an utterance:

(87)   *formulaic 'matrix clause'* $\wedge$ [$_{TP}$ *sentence*]

In a structure like (87), the clause that is embedded in the target is plausibly analyzed (at least functionally, perhaps even grammatically) as the "main" clause.

Note that this is directly parallel to what is proposed by Rowland et al. (2005) for simple matrix questions at the point that children are still making errors with subject-auxiliary inversion

(e.g. *what can he can do?*, *what does he likes?*). She proposes that children learn lexically-based frames, rote constructions, that assist with acquiring wh-questions. On this analysis, children utilize a relatively small number of high-frequency sequences of wh-phrase + auxiliary (e.g. *what did ...*, *who does...*, etc) which they combine with some sentential element (X in (88)) that they have already grammaticalized.

(88)   WH+AUX ∧ [X]

These of course share a lot of similarities with children's earliest grammatical constructions, what Tomasello (2003) referred to as *pivot schemas*, such as *more X* and *allgone X*. The difference here is that the variable slot in the pivot schema is here filled with already-grammaticalized syntactic structures rather than simple nominals as at earlier stages.

(89)   WH+AUX ∧ [$_{TP}$ *sentence*]

Of course, these WH+AUX frames will eventually be decomposed (compositionally analyzed), but this kind of analysis to both wh-questions and long-distance wh-questions appears reasonable based on the evidence, plausible given the use of this frame-based strategy at many points in acquisition. What this suggests is somewhat unsurprising: in both questions, embedding, and therefore also in long-distance questions, children are using their existing grammatical knowledge of smaller units of language to leverage their way into understand the most complex forms of sentences.

In long-distance extraction specifically, for example, we do see evidence that the lower clause is given primacy in interpretation. As Roeper and de Villiers (2011) report, children make a consistent error until very late stages of development (ages 6-7, even) where long-distance questions, and matrix questions accompanying embedded indirect questions, are interpreted according to the properties of the embedded clause, not the matrix clause.

(90)   How did she say what she bought?
       Adult answer: "She whispered it"
       Child answer: "Cake"
       (Roeper and de Villiers, 2011)

Likewise in a long-distance question, children will appear to interpret the matrix question as if it were a question restricted to the structurally lower clause.

(91)   Context: she bought a cake but said she bought paper towels
       Question: "What did she say [t_{what} she bought t_{what}]?"
       Child answer: paper towels
       (based on Roeper and de Villiers 2011, 233)

"The error here is that children answer with the 'truth' *what she bought* ("paper towels"), not *what she said she bought* (*a birthday cake*) (Roeper and de Villiers, 2011, 233). This is consistent with the developmental pathway described by Diessel (2004), where lower clauses are given primacy (grammatically, and also therefore in interpretation) and that a structure of true embedding is only reached over time (even when children can produce structures that appear to be long-distance extractions, albeit not without errors: Thornton 1990).

The entire point of discussing the parallel developments of clause structure, wh-extraction, and long-distance movements is to demystify the look-ahead problem. In a Minimalist analysis, allow wh-phrases to move to the edge of a phase before their (higher) final target position has merged into the structure is problematic (and even if solutions are proposed, to our knowledge every proposed solution has complicated our assumptions about the computational system). Yet, there is strong cross-linguistic evidence that such cyclic movement to the edge of phases exists. When acquisition is considered, however, the look-ahead problem dissipates because children are in fact "looking ahead" at the target structure: the Minimalist computation is the sequence of grammaticalization, but the target is not unknown. Beyond that, it appears that there are in fact earlier stages of knowledge where children represent wh-phrases in those intermediate positions as natural stages in the growth of their grammars. At the root default stage where children have grammaticalized vP (and plausibly nothing else), children are quite capable of uttering questions in obligatory wh-movement languages like English, and from the start they produce questions at the left edge: notably, at this point, the left edge of their utterance is likely the left edge of a vP.

Similarly, with long-distance extraction: it looks like children utilize their knowledge of main clauses to produce their early (apparent) embedded clauses. And even when they start using (apparent) long-distance wh-questions, their comprehension of these lags behind production (which is atypical for developmental receptive/expressive mismatches), and it appears that they actually interpret wh-phrases mainly as associated with the lower clause (in addition, of course, to making wh-copying errors in production where they produce wh-phrases at the edge of the lower clause, despite that being never present in the input in languages like English). All of this is suggestive that children, in instances of long-distance wh-movement, have developmentally early contentful representations of wh-phrases at the edge of the "embedded" CP. Therefore, we can see that the gradual growth of grammatical structures would necessarily results in these intermediate structures of movement (more on derivation/acquisition by phase in Chapter 4.

2.4   An aside: On the lack of wh-copying *in situ*

The DMS approach also suggests an explanation for a somewhat curious effect: for all the evidence (in adult languages) of pronunciation of multiple copies of wh-phrases in languages with wh-movement (especially, those without wh-in-situ), there is, to our knowledge, no evidence of pronunciation of copies *in situ*, only in intermediate positions (Spec,vP; Spec,CP).

(92)   a.   [Wovon] glaubst du, [wovon] sie träumt?   **(Adult) Colloquial German**
        of.what   believe you of.what she dreams
        'What do you believe that she dreams of?' (Felser, 2004, 549)

    b.   *[Wovon] glaubst du   dass sie [wovon] träumt?
        of.what   believe you that she of.what dreams (Felser, 2004, 551)

This is somewhat curious, as we might expect lower copies of movement to be pronounceable, at points, in both base positions and intermediate positions, if it is possible at all to pronouns lower copies of moved phrases. But it is of course absolutely reasonable given the DMS account: children in wh-movement languages (i.e. languages without *in situ* wh-questions) from their first

attempts to grammaticalize wh-phrases place them at the left edge, as they posit potential landing sites. If the instances of wh-copying and partial movement of wh-phrases in adult languages are built from acquisition is not just "pronounce lower copies" but instead "pronounce a copy that was previously the analysis of the position of wh-movement," we predict exactly these effects.[5]

What we find, then, is that instead of early movement of wh-phrases being a problem for theories of bottom-up syntactic acquisition, and instead of look-ahead being a problem for Minimalist syntax, that both are solved if we equate a Minimalist derivation of a sentence to its acquisition pathway, per DMS.

2.5   Input-based movement

It is a relevant point that movement must not necessarily require an Agree relation and feature-valuation/feature-checking in order for it to be acquired. This much seems obvious given the wh-movement facts (at least, if we are to retain the derivation-by-phase model): there is not evidence of feature-checking of intermediate wh-phrases in most adult grammars (see §2.2). But the field of syntax as a whole has gone back and forth on this point endlessly: is feature checking required for movement? At points researchers assume that movements are motivated by feature-checking, at other times they assume that movement is either free or is driven by an "EPP" feature, a feature whose sole purpose is to require something to be merged in a specific position (which is considered by many researchers to be nothing more than a placeholder analysis, as it does little more than restate the empirical fact that a phrase was merged in that position). Chomsky himself has gone back and forth on this point, most recently commenting, "The easy answer, which is in my recent papers, is simply to drop the condition that Internal Merge (Movement) has to be triggered, so it's free, like External Merge. In fact, that's an improvement, we should never have had that condition" (Chomsky, 2019, 268).

On the DMS approach we are developing, however, there are no truly unmotivated movements. All movement operations should either be driven by feature-checking operations (feature-checking operations themselves being an annotation of a link between two distinct properties, e.g. question operators and movement), or by direct empirical evidence that is readily available to children at the relevant developmental stages. Therefore, we would propose that in addition to *feature*-motivated movements (Agree-driven operations), that there are also input-motivated movements, driven simply by the empirical data, which makes it unsurprising that a child would posit a movement.

(93)   There are no unmotivated movements in syntax: all movements are either feature-driven or solely input driven.

It is our suspicion that there may be a large degree of overlap between movements that are plausibly input-driven movements, and those that have been analyzed as simply movement to

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[5]It is also tempting to correlate the lack of D-linked wh-phrases in wh-copying (in both adult and child grammars) to the relatively late acquisition of D-linked wh-phrases in general. If wh-copying derives from wh-copies in intermediate positions, but D-linked wh-phrases are themselves late merged at the final site of movement, we would expect them to never occur in intermediate positions. More work is necessary to confirm these potential correlations, however. See Felser (2004) and Thornton (2016).

check an EPP feature. That is a question we must set aside for future work, however. So in some sense, Chomsky (2019) is right that as a matter of UG itself, movement need not be triggered by an element of the computational system (i.e. it is 'free' in this sense). But the re-orientation to UG as a grammaticalization mechanism means that the computational system is not applied blindly, but rather is employed to model input.

It is our suspicion that many instances of verb movement across languages may likewise be considered an input-driven movement. While the empirical evidence for verb movement has long been considered irrefutable (verbs in different languages behave as if they are at different structural heights), it has long troubled syntacticians to come to any reliable featural motivations to capture when languages have verb movements, and when they do not. Of course, the reason why verb movement has been considered uncontroversial (empirically-speaking) is that it is, well, obvious: surface word order usually makes quite clear what structural position the verb must be in. But if this is the case, then this is surely the case for a child acquiring the language as well. Therefore, we ought not expect to find a child positing levels of abstraction (i.e. abstract features unifying otherwise unrelated things) when a simple surface-driven generalization suffices.

2.6 Intermediate Summary: DMS and Look-Ahead

Again, the prediction of DMS as stated in (4) is that derivational sequences in adult language should mirror acquisition timelines. Largely, this would appear to result in bottom-up acquisition in a cyclic fashion (phrase by phrase), a kind of proposal that has been around generative acquisition circles for decades. But the derivational approach to adult grammars has revealed various kinds of sequencing puzzles: some things happen before they "should," some things after. The DMS solution is not to deny the countercyclicity, but rather, to suggest that the sequencing in adult grammars exists because adult grammatical knowledge contains previous stages of knowledge. And if evidence is available to a child for an operation to occur "early" (i.e. before the target-like structures are fully formed), such initial operations are acquirable. Likewise, if an acquisition target is unavailable to a child during a given period of acquisition (e.g. if it is too complex to be acquired at early stages) it can be acquired later: though this, likewise, will be recorded as such in the adult grammar.

3 Is counter-cyclicity "exotic"?

We speculate here, but it's possible that one of the reasons that counter-cyclic analyses persist in the literature despite counter-cyclic theoretical proposals being largely only-marginally acceptable (at best) in the minds of most theoreticians is that the original constructions they have been invoked to explain are largely narrow and/or uncommon. Chomsky laid this out as an explicit reason to reject non-standard structure-building operations like countercyclic Merge, among others:

> Counter-cyclicity is about the same as Late Merge, so this critique holds for everything that is done with what's called Late Merge: it's completely unacceptable, because it involves operations that are complex, unmotivated, they have nothing to do

with the goal we think we ought to obtain, something like the Strong Minimalist Thesis (SMT). These considerations become far more significant in the case of what are sometimes called exotic constructions, those which have virtually no evidence, maybe none, available for the child; things like Antecedent-Contained deletion or Across-the-Board movement, Parasitic gaps … It's simply impossible to propose a new principle for those, it can't be. The child has no evidence for them if he has to understand them. It must be the application of principles that are available for simple, easy, normal cases. So, in fact every kind of construction is in fact pretty exotic of the kind that Charles (Yang) was talking about but some are extremely so thus leading to the invocation of operations like counter-cyclicity, Late-Merge … completely unacceptable. (Chomsky, 2019, 267)

Chomsky's point is a good one: if the only reason we need a dramatically non-standard theoretical mechanism is for a cross-linguistically rare sort of construction that is also (likely) statistically rare in child-directed speech, and in the entire range of primary linguistic data available to a child acquiring a language, then it is methodologically questionable to make those non-standard mechanisms available in the theory.

Now, obviously, we take a quite different stance on counter-cyclicity. But this is also an empirical question: are all purported instances of counter-cyclic operations such "exotic" constructions that a child would have little-to-no access to? This is highly relevant, because it is another way of asking, "How seriously do we have to take this apparent counter-cyclicity?" So here we entertain the question: are all such constructions "exotic," to use Chomsky's descriptor? Or to use a different one, are they at the periphery of our grammatical concerns? The answer to this is a definitive 'no.' Another clear instance of counter-cyclic empirical phenomena come from conjoint/disjoint and object marking constructions across a large range of Bantu languages. We focus on Zulu here as a particularly well-researched example, though the same argument can be replicated in a number of languages: there are relevant parallels in Bukusu (Sikuku and Diercks, 2020) and Sambaa (Riedel, 2009), just to name two. We assume the Zulu situation is not extremely familiar to the likely readership of this monograph, and as such we lay out the empirical background in some depth to make the argument clear.

3.1 Zulu Conjoint/Disjoint

Across a broad range of Bantu languages, there are languages that possess forms of verbal marking that correlate with the presence/absence of postverbal material: the forms are known in the Bantu language literature as conjoint forms and disjoint forms.[6] Conjoint forms on a verb show a closer connection between a verb and what follows, and disjoint forms are used when there is a looser connection with what follows, or when nothing follows the verb (van der Wal and Hyman, 2017). In Zulu, the predominant analysis of conjoint/disjoint is that the distinction tracks the presence of morphosyntactic content inside vP: conjoint is used when a constituent is inside vP, and disjoint is used when vP is empty.

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[6]The literature also refers to these as short and long forms of verbs, along with other language-specific terms.

(94)   Conjoint-disjoint generalization in Zulu:
       Conjoint (Ø): appears when *v*P contains material (after movement)
       Disjoint (ya): appears when *v*P does not contain material (after movement)
       (Halpert, 2016, 122)

So (95a) occurs with a conjoint form with an overt object, but when there is no overt object the conjoint form is unacceptable in (95b); this circumstance instead requires the disjoint form (95c).[7]

(95)   Zulu Conjoint/Disjoint (Zeller, 2015, 19)

       a. U-mama   u-phek-a   i-n-yama   (conjoint)
           AUG-1a.mother 1SM-cook-FV AUG-9-meat
           'Mother is cooking the meat.'

       b. *U-mama   u-phek-a   (conjoint)
           AUG-1a.mother 1.SM-cook-FV
           Intended: 'Mother is cooking.'

       c. U-mama   u-ya-phek-a   (disjoint)
           AUG-1a.mother 1.SM-DJ-cook-FV
           'Mother is cooking.'

In Zulu the *v*P is a focus domain (see below), and as a result there is a link between conjoint/disjoint and information structure, though the predominant analysis for Zulu is that this link is indirect. Others have claimed that the conjoint/disjoint alternation in other languages directly references information structure (see Nshemezimana and Bostoen 2017 and van der Wal and Hyman 2017 for discussion), but the consensus for Zulu is that it is a syntactic pattern, and only indirectly pragmatic.

A central aspect of the conjoint/disjoint distinction is that it is surface-oriented, and it appears to be category-neutral. That is to say, it doesn't matter *what* is in *v*P, as long as something is: in these instances, the conjoint form is used. So we can see that it's not just the presence/absence of object from above, but if a subject occurs *in situ* postverbally, the (unmarked) conjoint form must be used:

(96)   ku-(*ya)-pheka uSipho   *v*P]
       17SM-(*DJ)-cook AUG.1.Sipho
       'Sipho's cooking.'

Crucially, however, the parallel example above in (95c) shows that preverbal subjects do not require conjoint forms, instead occuring with disjoint verb forms (when nothing else follows the verb). So we can see that the surface position of the *v*P-internal constituent is what is relevant. This is further demonstrated by object marking constructions, which is spelled out in the next section.

Beyond the fact that it is the surface position that matters, it is also striking that the exact content of the *v*P-internal constituent is largely irrelevant. So, for example, *v*P-internal argu-

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[7]Zulu verbs raise out of *v*P, usually assumed to be to an aspect projection in the middlefield of the clause.

ments readily require conjoint forms (and disallow the disjoint form), but low adverbials do the same.

(97) Ngi-(*ya-)cul-a   kahle.
      1SG.SM-(*DJ-)sing-FV well
      "I sing well." (Buell, 2006, (21))

A similar pattern is evident in the relative clauses in (98), both of which require a resumptive pronoun for the extracted phrase (circled in the examples below). Both of these verb forms require the conjoint form of the verb, based on the presence of the resumptive pronoun postverbally.

(98) a. i-ndawo   lapho ngi-cul-e   ⟨khona⟩ (conjoint)
          DET-9.place there 1SG.SM-sing-FV 17.PRO
          "the place where I sang"
      b. i-sikhathi   engi-cul-e   nga-⟨so⟩ (conjoint)
          DET-7.time REL.1SG.SM-sing-FV at-7.PRO
          "the time when I sang"
          (Buell, 2006, (21))

The precise analysis of the conjoint/disjoint distinction is deeply interesting, but aside from the main point here (we refer the reader to van der Wal and Hyman 2017 for a recent overview and specific studies). Instead, it is a valuable diagnostic of whether postverbal material appears internal to the *v*P or not, which for our purposes is crucial for understanding Zulu object marking patterns.

3.2 Zulu Object Marking

There is a long history of research on Zulu object marking (OMing) (Adams, 2010; Buell, 2005, 2006; Cheng and Downing, 2009; Halpert, 2012; Van der Spuy, 1993; Zeller, 2012, 2014, 2015, among others). Jochen Zeller's (2012; 2014; 2015) recent research aggregates and synthesizes this work well; our summary here mainly relies on his work. Zulu only allows a single object marker on the verb form: attempts to pronominalize both objects via OMs on the verb are unacceptable, as (99) shows.

(99) Zulu (Zeller, 2012, 220)
      a. U-John   u-nik-e   a-ba-ntwana i-zi-ncwadi
          AUG-1a.John 1SM-give-PST AUG-2-child   AUG-10-book
          'John gave books to the children.'
      b. *U-John   u-⟨ba-⟩⟨zi-⟩ nik-il-e.
          AUG-1a.John 1SM-2OM-10OM-give-DJ-PST
      c. *U-John   u-⟨zi-⟩⟨ba-⟩ nik-il-e.
          AUG-1a.John 1SM-10OM-2OM-give-DJ-PST

Instead, if one wants to pronominalize both objects, only one may be represented via an OM on the verb, and the other must be a free pronoun following the verb. As (100) shows, however,

the grammar does not restrict *which* object may be an OM vs a free pronoun. Instead, either object may be represented as an OM.

(100)   Zulu (Zeller, 2012, 220)

    a.   U-John   u-ba- nik-e   zona.
               AUG-1a.John 1SM-2OM-give-PST 10PRON
               'John gave them to them'

    b.   U-John   u-zi- nik-e   bona.
               AUG-1a.John 1SM-10OM-give-PST 2PRON
               'John gave them to them.'

This turns out to be a quite general property of Zulu OMing, that it is what has often been termed "symmetrical" in the literature on Bantu languages. Following on a long line of research (cf. Bresnan and Moshi 1990, and much that follows) it has become clear that there is a range of variation between languages (and between constructions within languages) about the properties of objects in multiple object constructions. Specifically, some languages only allow a single object (the structurally highest object, e.g. a benefactive in a benefactive applicative) to carry object properties like being OMed on the verb, or being promoted to subject in a passive: these are termed asymmetrical languages. Symmetrical languages, on the other hand, allow multiple objects to do so. With respect to object marking, then, Zulu is symmetrical, illustrated again in (101) with lexical DP objects.

(101)   a.   U-Langa   u-phek-el-a   u-mama   i-nyama.
              AUG-1a.Langa 1SM-cook-APPL-FV AUG-1a.mother AUG-9.meat
              'Langa is cooking meat for mother.'

    b.   U-Langa   u-m -phek-el-a   i-nyama   (u-mama) .
              AUG-1a.Langa 1SM-1OM-cook-APPL-FV AUG-9.meat AUG-1a.mother
              'Langa is cooking meat for her (mother).'

    c.   U-Langa   u-yi -phek-el-a   u-mama   (i-nyama) .
              AUG-1a.Langa 1SM-OM9-cook-APPL-FV AUG-1a.mother AUG-9.meat
              'Langa is cooking it for mother (the meat).'
              (Zeller, 2012, 227)

This symmetricality ends up playing a major role in Zeller's (2014; 2015) analysis of Zulu OMing, which we will see below. There are some restrictions on what kinds of objects may be OM-doubled: unaugmented noun phrases (i.e. negative polarity items) cannot be OMed, and the same is true for wh-phrases, as (102) shows. *v*P is well-established to be a focus domain in Zulu, and (as we will see below) object marking objects is connecting with those object leaving the *v*P focal domain.

(102)   Zulu (Adams, 2010, 42–43)

    a.   A-ngi-(*m)-thand-i   mu-ntu.
              NEG-1SM-1OM-like-NEG 1-person
              'I don't like anyone.'

    b. U-zo-(*m)-qabul-a   bani?
       1SM-FUT-1OM-kiss-FV 1a.who
       'Who will he kiss?'

OM-doubling in a transitive requires the disjoint verb form in Zulu. So a simple mono-transitive sentence without an object marker appears in the unmarked conjoint form in (103a), but when an OM appears on the verb (even with the lexical DP object still appearing postverbally) the verb must be in a disjoint form (103b).[8]

(103)   Zulu (Zeller, 2012, 222)

    a. Ngi-theng-a   le   moto.
       1SG.SM-buy-FV 9DEM 9.car
       'I'm buying this car.'

    b. Ngi-*(ya)-[yi]-theng-a le   [moto].
       1SG.SM-DJ-OM9-buy-FV 9DEM 9.car
       'I'm buying (it) this car.'

The predominant analysis has been to propose that overt postverbal objects that are OMed on the verb are in fact dislocated from their base position, appearing outside the *v*P (Van der Spuy 1993 et seq.). This explains the conjoint/disjoint facts, presuming that conjoint marking is taken to indicate that a constituent remains inside *v*P, and disjoint marking is when *v*P contains no argument or adjunct constituents on the surface (Van der Spuy, 1993; Buell, 2005, 2006; Halpert, 2016, 2017). Zulu shows a lot of consistent evidence that affirms this analysis. For example, OM-doubled arguments occur to the right of manner adverbs. In (104a) we see that the canonical position of a manner adverb is to the right of the direct object. In (104b) we see that OM-doubling is unacceptable with canonical word order; instead, when an OM appears on the verb the corresponding lexical object must appear to the right of the manner adverb (104c).

(104)   a. Si-bon-a   i-n-kosi   kahle.
       1SG.SM-see-FV AUG-9-chief well
       'We are seeing the chief well.'

    b. *Si-[yi-]bon-a   [i-n-kosi]   kahle.
       1SG.SM-9OM-see-FV AUG-9-chief well

    c. Si-[yi]-bon-a   kahle [i-n-kosi].
       1SG.SM-9OM-see-FV well   AUG-9-chief
       'We are seeing him well, the chief.' (Zeller, 2015, 20)

Assuming that manner adverbs adjoin at the edge of *v*P, this suggests that OM-doubled arguments move outside *v*P:

(105)   . . .siyibona kahle]$_{vP}$ . . . inkosi = (104c) (Zeller, 2015, 20)

    Double object constructions show the same pattern: when the indirect object is OM-doubled,

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[8]Several of the examples below are in the past tense/aspect *-ile/e*, which shows a conjoint/disjoint distinction in those two allomorphs, rather than in the *-ya-* marking used above.

the indirect object DP cannot appear in its canonical position (106b), instead occurring to the right of the direct object (106c). This is consistent with a right-dislocation of OM-doubled objects.

(106) a. U-John   u-nik-a   a-ba-ntwana i-mali.
           AUG-1a.John 1SM-give-FV AUG-2-child AUG-9.money
           'John is giving the children money.'

       b. *U-John   u-ba -nik-a   a-ba-ntwana i-mali.
           AUG-1a.John 1SM-OM2-give-FV AUG-2-child   AUG-9.money

       c. U-John   u-ba -nik-a   i-mali   a-ba-ntwana .
           AUG-1a.John 1SM-OM2-give-FV AUG-9.money AUG-2-child
           'John is giving the children money.'

Another diagnostic comes from the properties of focused phrases in Zulu. It is well-established that focused phrases in Zulu must appear *v*P-internally. This is shown for exclusive focus with *kuphela* 'only' in (107) and for wh-elements (assumed to be inherently focused) in (108). In both examples, a focused subject cannot appear in the canonical preverbal subject position, instead appearing *in situ* (and triggering conjoint forms of the verb, confirming the *in situ* position within *v*P.)

(107) a. Ku-fik-e   u-Sipho   kuphela.
           17.EXPL-arrive-PST.CJ AUG-1a.Sipho only
           'Only Sipho arrived.'

       b. *U-Sipho   kuphela u-fik-ile.
           AUG-1a.Sipho only   1SM-arrive-PST.DJ

(108) a. Ku-sebenz-e   bani?
           17.EXPL-work-PST 1a.who
           'Who worked?'

       b. *U-bani   u-sebenz-ile?
           AUG-1a.who 1.SM-work-PST
           (Zeller, 2015, 20)

If it is true that OM-doubled phrases must be outside *v*P, however, it is predicted that OM-doubling focused phrases would be impossible, which is confirmed below:

(109) a. Ngi-bon-e   u-Sipho   kuphela]$_{vP}$.
           1SM-see-PST AUG-1a.Sipho only
           'I saw only Sipho.'

       b. *Ngi-m -bon-ile]$_{vP}$ u-Sipho   kuphela .
           1SM-1OM-see-PST   AUG-1a.Sipho only
           (Buell, 2008, (6))

(110) a. U-cul-e   i-phi   i-n-goma]$_{vP}$?
           2SM-sing-PST 9-which AUG-9-song
           'Which song did you sing?'

b. *U-[yi] -cul-ile]$_{vP}$ [i-phi   i-n-goma] ?
2SM-9OM-sing-PST 9-which AUG-9-song
([Buell], 2008, (5))

Zeller (2015) argues that the appropriate analysis of these patterns is that object markers arise via an agreement relation with a functional head on the edge of *v*P, which triggers movement of the relevant object to a right-facing specifier of the functional projection. (111) sketches this analysis.

(111)   Zeller's (2015, (65)) analysis of Zulu object marking
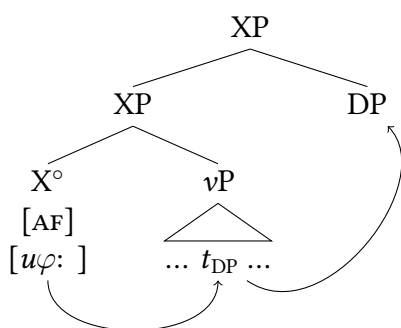
Zeller (2015) argues that the relevant feature triggering this Agree relation is an A'-feature related to the discourse status of objects that undergo this short right-dislocation. Zeller proposes that it is an *anti-focus* feature that accomplishes this in the syntax. This explains, among other things, why Zulu object marking is 'symmetrical' (i.e. either object of a ditransitive can be OMed): the anti-focus Probe doesn't necessarily find the closest DP, but instead finds the closest anti-focus-marked DP. In (112a) the indirect object *uSipho* is the discourse-familiar object and is dislocated to the right of the direct object. In (112c) the direct object is OMed and dislocated, though less obvious in this case because direct objects appear canonically to the right of indirect objects.

(112)   a. Ngi-[m] -theng-el-a   u-bisi   [u-Sipho] .   (conjoint)
       1SG-1OM-buy-APPL-FV AUG-11.milk AUG-1a.Sipho
       'I'm buying him milk, Sipho.'

     b. *?Ngi-[m] -theng-el-a [u-Sipho]   u-bisi.   (conjoint)
       1SG-1OM-buy-APPL-FV AUG-1a.Sipho AUG-11.milk

     c. Ngi-[lu] -theng-el-a   u-Sipho   [u-bisi] .   (conjoint)
       1SG-11OM-buy-APPL-FV AUG-1a.Sipho AUG-11.milk
   'I'm buying it for Sipho, the milk.' (Zeller, 2015, 23)

The pattern in (112a) and (112b) is what is expected based on the preceding discussion: if OMing is connected with a (short) right-dislocation, we expect an OMed indirect object to appear to the right of other objects that remain in *v*P. Notice in all of the examples in (112) the verb appears in the conjoint form: in each of these instances, only one object is dislocated, and therefore one remains inside *v*P, requiring a conjoint form (on the empirical analysis discussed above, that the Zulu conjoint/disjoint distinction tracks whether there is surface content inside *v*P).

3.3 Counter-Cyclic Object Marking

We are now at the point where the Zulu patterns become relevant to our main concerns here. Zeller tackles a puzzle about the implementation of his account above. Crucially, external arguments (canonical subjects) are generated in Spec,*v*P, before being raised to Spec,TP. A central aspect of Zulu syntax, however, is that preverbal subject position is incompatible with focused subjects, instead requiring focused subjects to remain postverbally. This suggests that T is also an anti-focus probe, as it can only find an anti-focus subject as a Goal (similar accounts have been proposed/assumed by a number of researchers working on southern Bantu languages). That said, when subjects remain postverbal, they are clearly internal to *v*P, as they require conjoint marking on the verb. Postverbal subjects in Zulu are widely assumed to be in Spec,*v*P (see Halpert 2016 for extensive discussion). This raises a crucial question, however: the base position of subjects clearly aligns with standard assumptions, being in Spec,*v*P.

This means, however, that in even the most basic object marking sentences, canonical subjects are marked anti-focus and appear in Spec,*v*P before being the target of an [AF] Agree relation from T. But in constructions with object markers, this poses a hard paradox. If the anti-focus probe on X° is merged atop *v*P, it ought to *always* find the subject in Spec,*v*P and never find the object (perhaps also ruling out T° finding the subject as a Goal of its own Agree relation, if Agree with X° in this hypothetical scenario would also deactivate the subject DP, as we would expect). This is of course the opposite of the normal pattern, where T agrees with subjects and X° with objects. These cris-crossing Agree relations are illustrated in (113):

(113) Zulu counter-cyclic object marking (failure of subjects to intervene)
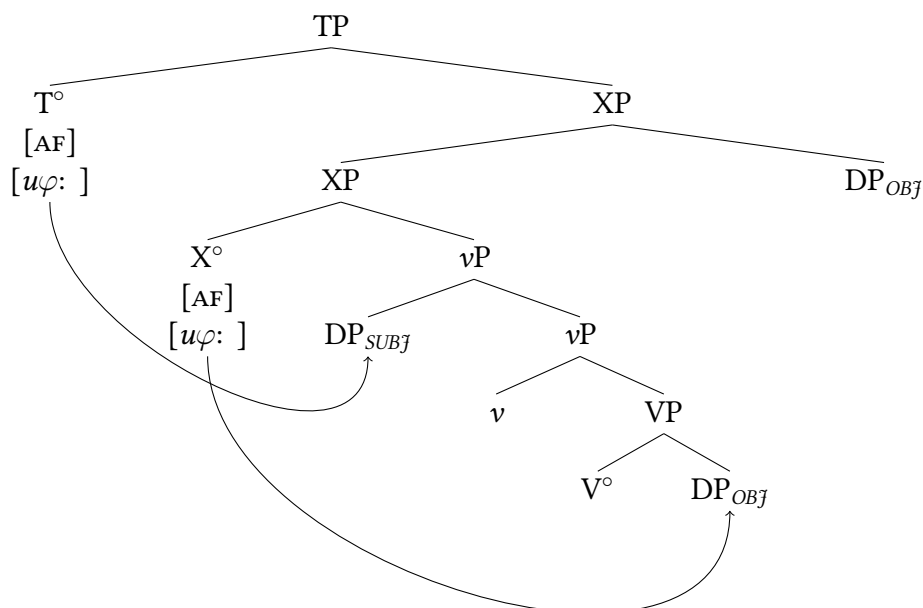
Zeller's solution to this is to stipulate that T always probes first, and that lower copies of moved

elements are not relevant in the calculations of locality, here, irrelevant as a potential Goal for Agree.

(114)   The "T Always Probes First'' principle (TAPF) (Zeller, 2015, (68))
         The first *v*P-external PROBE-GOAL relation in a derivation must involve the uninterpretable features of T.

While we find it reasonable that the copy of the moved subject would not intervene (Riedel (2009) must assume something similar for object marking contexts), (114) is pure stipulation. It should be pointed out that object marking (and conjoint/disjoint constructions) raise this same problem across a large number of Bantu languages, with scholars taking various solutions. For example, Sikuku and Diercks (2020) have the same countercyclic problem of subjects not intervening in the relevant Agree relation between the head generating object marking and the object. Rather than a delayed agreement analysis, they instead propose a Late Merger of the head generating object marking (the equivalent of XP here), after T° has already probed the subject in Spec,*v*P. The point is not the details, of course, but that this kind of derivational paradox is not simply an artifact of Zeller's analysis of Zulu; the head that generates the OM is above *v*P and probes for anti-focus features, but necessarily does not ever find anti-focus subjects as Goals, despite them clearly being the most accessible structural argument. Similar countercyclicity puzzles questions also arise in the analysis of Zulu conjoint/disjoint itself, which (similar to Zeller) Halpert (2016) resolves by stipulating the ordering of probing features that are responsible for moving the subject and tracking *v*P content, respectively.

Again, the point of raising this puzzle here is not simply to add to the list of countercyclic constructions: the list of such proposed analyses is much too extensive to try to catalog here. Rather, this addresses the suggestion by Chomsky (2019, 267) that divergences from standard cyclic Merge tend to be necessitated by "exotic" constructions which a child would have little-to-no direct access to in the PLD. Quite to the contrary, of course, conjoint/disjoint distinctions and object marking are the definition of morphological transparency, showing explicit morphology on verbal forms in most basic sentences (in the relevant tenses/aspects, which are not themselves uncommon). There is no reason to think that a child would have limited access to relevant data for constructions like these. Nonetheless, the Zulu constructions (and similar constructions across various Bantu languages) pose hard problems for basic cyclic structure building.

3.4   Explaining Counter-Cyclicity in Zulu OMing

What does the DMS-approach predict in the case of Zulu OMing? For the sake of argument, let us assume that the Zulu pattern is derived via Late Merger of XP, rather than by a delayed agreement.[9] This counter-cyclic analysis of object marking makes very clear predictions according to DMS: Zulu object-marking is predicted to be acquired later than (at least) TP in Zulu. Unfortunately, there is very little acquisition data available for Zulu or for any Bantu language.[10]

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[9]Perhaps worth noting, in order to derive the rightward movement of objects, if T probes first, a Late Merger is still required under Zeller's analysis, of the DP object to XP; it seems likely that a Late Merger analysis would instead categorize the problem alongside other similar problems, as opposed to the stipulative TAPF in (114).

[10]Perhaps best-studied in this respect is Swahili, but Swahili object marking doesn't (obviously, at least) share these same counter-cyclic properties that we are concerned with here, different in many ways from Zulu, Bukusu,

There is one study this is suggestive the object marking is acquired later than structurally higher morphology like tense and subject markers, however. Suzman (2002) provides two case studies of language impairment from Zulu (alongside one control). Language impairment is a useful domain to study acquisition in, as (among other things) the protracted timeline of acquisition for some children offers an extended window to observation acquisition processes and sequences. Suzman (2002) found variable access to morphology in the case studies, finding that while the control subject acquired a relatively broad range of verbal and nominal morphology, only some of these were acquired by the language-impaired children. Crucially for our purposes, object marking was still rare even after subject markers and tense were acquired.

(115)

Grammatical Morphology	Typical Zulu	Impaired Zulu
Selected Noun classes	✓	✓
Subject Markers	✓	✓
Adjectives	✓	✓
Tense	✓	✓
Object Markers	✓	Rare
Relative Clause Markers	✓	Rare

(*Partial Replication of Table 9 from* Suzman (2002))

This is consistent with the predictions of DMS, but hardly sufficient as an empirical support for our claims. But if more language acquisition work occurs in Zulu, it will open the door to test these predictions more thoroughly. But if the early results from Suzman (2002) hold, we would see here again that counter-cyclic properties are possible but arise because of counter-cyclic acquisition.

We have suggested that counter-cyclicity emerges when a child is not ready to grammaticalize a pattern that they are encountering on the usual cycle of strict structure-building via Merge. This may well emerge because the child does not yet have the requisite grammatical structures necessary to grammaticalize the input, but Zulu and other counter-cyclic object marking contexts are not obvious examples of that. Zulu offers a child overt morphology on the verbal form which ought to be readily available to children from the start.

What we suspect is at play here is that, as in other instances, rather than the requisite grammatical structures having yet to emerge, in an instance like this, children instead don't yet have the cognitive ability to analyze the pattern that is to be grammaticalized in the first place. As as noted above, these Zulu patterns are integrally related with information structure and the structure of discourse: these kinds of semantic and pragmatic notions are quite plausibly unavailable to children at the point when they are acquiring core predicates and argument structures or tense/inflection. While work isn't available from Zulu, more broadly, there is certainly research showing that acquisition of target-level proficiency of grammatical information structure can be *much* later than acquisition of argument structures and/or tense/inflection. As Dimroth and Narasimhan (2012) report, some aspects of adjusting discourse strategies according to the pragmatics of discourse do begin to be used quite young. But

> despite evidence of early sensitivity, children do not always use the linguistic forms in the input in appropriate ways from an early age. For instance, children's use of

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Sambaa, etc.

> pronouns is not influenced by the listener's co-presence and ability to perceive the target event although the immediate discourse context does exert a significant influence. German-speaking children's use of word order in phrasal conjuncts demonstrates their sensitivity to the distinction between 'new' and 'old' information, yet their propensity to order 'new' information first is not adult-like. Children's use of the topic marker *ne* in Chinese also suggests that children do not use linguistic forms in the same way as adults do despite their sensitivity to the relevant distinctions. (Dimroth and Narasimhan, 2012, 332)

In fact, von Stutterheim et al. (2012) show that L1 learners of German have often not acquired adult-like structuring of information by age 14 in narratives.

> The grammatical underpinnings and syntactic boot strapping into discourse organisation entail the fusion of syntactic, semantic and pragmatic knowledge in language-specific terms—a system of knowledge which takes years to uncover as well as discover. Grammatical form provides the key for the child in tracking down the knowledge they have to acquire when organising information in text functional terms. No one will deny of course that there are also cognitive prerequisites that have to be met so that the child can begin and continue to acquire more complex systems. (von Stutterheim et al., 2012, 582)

So in a way we do agree with Chomsky that these counter-cyclic structures are largely unavailable to children, but not because there is evidence lacking in the input, but we would instead suggest that it is because the cognitive skillsets necessary to acquire Zulu conjoint/disjoint and object marking patterns are unavailable to the children at the time that they are acquiring other verbal morphology at a similar structural level. (The skillsets that are necessary here being whatever enables tracking of discourse participants and interlocutor Common Ground in order to use focus/background structures appropriately.) This, of course, is exactly what is predicted on a DMS-style interpretation of Minimalist analyses. But these patterns are not entirely unavailable to children, of course—there is no need to require the counter-cyclicity of Zulu object marking patterns to emerge directly from the properties of UG as Chomsky suggests for constructions like parasitic gaps, as there is robust direct evidence of object marking and conjoint/disjoint available to children in the input. What we suggest, however is that due to the necessary cognitive prerequisites, the pattern is unavailable to children at the developmental point when it would need to be grammaticized if it was going to happen via Merge; as such, when it is acquired it is via Late Merger, i.e. a substitution operation that does *not* obey the Extension Condition, instead tampering with the existing structure.

To reiterate, the acquisition evidence we have for this (from Zulu) is quite limited, though it does support these conclusions. The main contribution here is to show that counter-cyclic constructions can in fact be both central to the grammar of a language, and immediately available in surface-evident morphology, making it hard to marginalize the counter-cyclic to the periphery of the constructions we allow to structure our theory.

4   Summary: Deriving Counter-cyclicity

Our claim is that counter-cyclic constructions are the exceptions that prove the role of Developmental Minimalist Syntax. If Minimalist derivations of adult grammatical structures recapitulate developmental pathways due to the nature of our UG mechanisms as grammaticalization devices, we would largely expect acquisition to proceed in a 'bottom-up' fashion, as this is the canonical mode of structure building in adult grammars. But as a field we have not rid ourselves of counter-cyclic derivations in adult grammars. Rather than these being black marks for the theory, we claim that they in fact further demonstrate DMS: counter-cyclicity in adult structures arises because of parallel counter-cyclic acquisition pathways. So, constructions that are acquired "earlier" or "later" than their immediate grammatical surroundings are expected to behave as derivationally "early" or "late" in adult grammar.

Again, for an important piece of clarity, we are not claiming there is anything atypical developmentally about this "earliness" or "lateness:" counter-cyclicity presumably appears in typical development for unsurprising reasons. Children can acquire patterns in their developing grammars that can't be fully incorporated yet, but which are obvious from the input they receive (e.g. wh-questions). Likewise, some parts of grammar may be sufficiently complex syntactically or semantically that they are not fully grammaticalized until later points in development, even if they appear structurally in the lower or middle zones of the clause. Again, our model of language need not rule these out, but rather simply recognize that adult grammars will reflect this timeline.

This does of course have implications for our theory overall: counter-cyclic operations must not be disallowed. We discuss this conclusion in Chapter 5. And it does raise important questions that will need to be resolved if this becomes a standard part of Minimalist assumptions, not least of which is what exactly constrains the use of counter-cyclic processes. Resolving these kinds of theoretical questions will be important next steps.

Chapter 4

A potential extension: The psychological substance of phases

If one were to summarize in a sentence or two the components of UG per current Minimalist theorizing, both Merge and Agree would be central, but so too would be *phases*, a concept which we've said quite little about to this point. Chomsky (2000a, 2001) argues that the derivation of a sentence is not as simple as cyclic merge operations: rather, there is good evidence that the derivation of a sentence proceeds by *phase*: particular sub-domains of a sentence are constructed, and then treated as complete units after that (complements of phase heads undergo "spellout" or Transfer to the PF and LF interfaces). So, for precision, consider the derivation of a basic sentence in (116). C° is generally considered a phase head, so upon merger of C°, the complement of C° (TP here) is spelled out (transfered to LF and PF).

(116) $[_{TP}$ sentence $]$ + C°–> $[_{CP}$ C° $[_{TP}$ ~~sentence~~ $]$ $]$

      Empirically, this can account for a range of domain-based phenomena, including (but certainly not limited to) the movement of wh-phrases through the edges of phases when undergoing long-distance movement. This ensures that a wh-phrase avoids being spelled out (and therefore be inaccessible) to higher operations. As we discuss in §2, this is commonly presented as a way of keeping computation of the grammatical structure more efficient.

      The result of this cyclic spellout is that phases ought to be impenetrable to higher operations.

(117)   **Phase Impenetrability Condition (PIC)**
      In phase α with head H, the domain of H is not accessible to operations outside α, only H
      and its edge are accessible to such operations. (Chomsky, 2000a, 108)

      The phase impenetrability condition defines the domain which is impenetrable: the complement of the phase head. Likewise, the edge of a phase (Spec,CP in (116)) is the 'escape hatch' of the phase: phrases within a phrase can move to that position to avoid being spelled out, and therefore continue to be accessible to higher operations. Chomsky also articulated another (weaker) version of the PIC (commonly referred to as PIC2)

(118) **Phase Impenetrability Condition (PIC2)**
The domain of H is not accessible to operations at ZP; only H and its edge are accessible to such operations.

According to (118), spellout does not occur when the phase head H is merged; rather, spellout of the complement of H only occurs when the next phase head Z is merged. This allows for more interactions between phase content and external material, as spellout is delayed. In general, we're assuming some familiarity with the structure of phases from our readership: for those unfamiliar, we recommend Citko (2014).

As (Bošković, 2014, 28) put it, "the question of why phasehood is picky (i.e., why only some phrases function as phases) has never been answered in a satisfactory manner." In this section we discuss multiple open questions relating to phase theory that we believe the DMS approach can make contributions to.

1   Phases as acquisition workspaces

The core idea that we set forward here is that the domain that is modeled as a *phase* in minimalist syntax, and the empirical effects that it captures, are the direct effect of a domain effect in acquisition as well. Specifically, we suggest that phases correspond to what we will call "acquisition workspaces," i.e. a period in time where children are working on the acquisition of particular subsets of syntactic/semantic properties. We will suggest that these domains correlate with the abstract functional domains that Ritter and Wiltschko (2014) and Wiltschko (2014) argue to be part of the "Universal Spine." The intuition here is that the 'freezing' effects of phases (encoded originally in the Phase Impenetrability Condition), wherein an already-completed phase is less accessible to syntactic operations, are the result of completed grammaticalization processes. The 'syntactic activity' that occurs during the phase that is being constructed in a syntactic derivation corresponds to the active acquisition/grammaticalization activity that a child is undergoing as they analyze units of language within specific domains.

If this proposed correlation can hold, it has many benefits. First, it can provide some clarity to the metaphor of "computational efficiency" that is often attributed to phases, which we discuss in the next section. Second, it can also provide some substance to the idea of a phase apart from the syntactic effects that it derives (successive cyclicity, phase impenetrability, etc). Essentially, distinct phases restrict the hypothesis space of what a child is investigating at any given point in the language acquisition process. Following the proposals of Ritter and Wiltschko (2014) and Wiltschko (2014) that we did above, we would expect three main 'workspaces': thematic, anchoring, and linking.[1] This of course aligns rather directly with the major clause-level instances of spell-out transfer: *v*P (thematic), TP/Inflection (anchoring), and the final transfer of the edge of matrix CP (linking).

We will also see in what follows that this can help address some standing puzzles about

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[1]Wiltschko (2014) adds a fourth—point of view, between anchoring and thematic/categorization for her—but we focus on the simpler proposal of Ritter and Wiltschko (2014) for our purposes here. Nothing that we are proposing here rules out inclusion of a fourth domain, though.

the nature of phases, namely, their variable size (§6), and the apparent simultaneity of operations within a phase (§7). In the end, then, we find this approach to phases promising on a number of levels. It explains why phases might exist in the first place, it offers solutions to some of the persistent syntactic puzzles around phases, and the syntactic work on phases likewise offers some clarity on modeling of acquisition as well.

§2 focuses on a core metaphor that has been used as justification for phases (and other aspects of the Minimalist Program): that the operations being proposed take the form that they do in order to maximize computational efficiency. We look at some of the claims/commentary on that point from Chomsky, as well as some critiques, and consider the implications of DMS for the issue.

§3 outlines a theory of phases from Wolfram Hinzen and colleagues that provides a foundation for our discussion here. We then outline an interpretation of this theory of phases in the context of DMS in §4. Given the breadth of consequences of these ideas, we can't empirically defend these proposals in the depth they will eventually require. But this is a potential extension of the DMS approach that follows relatively naturally and shows much promise, so we lay the groundwork here for that future work.

2   Computational efficiency of phases

From the beginning of the Minimalist program, questions of derivational economy and computational efficiency have been invoked in the quest for the simplest syntactic theory available. Chomsky (2001, 11) repeatedly refers to the Minimalist enterprise reducing "computational burden," and the question of reducing computational load is a constant point of evaluation in Minimalist theorizing; some of the foundational commentary is included here:

> Returning to SMT [Strong Minimalist Thesis], arguably restriction of computational resources limits n for Merge to two, as Luigi Rizzi suggests, thus yielding the "unambiguous paths" structure postulated by Kayne (1981). (Chomsky, 2008, 138)

> Phases should, presumably, be as small as possible, to minimize computation after Transfer and to capture as fully as possible the cyclic/compositional character of mappings to the interface. (Chomsky, 2008, 155)

> Proceeding further, MI [Minimalist Inquiries] proposes another reduction of computational burden: the derivation of Exp [Expression] proceeds by phase, where each phase is determined by a subarray LA, of LA [Lexical Array], placed in "active memory." When the computation L exhausts LA;, forming the syntactic object K, L returns to LA, either extending K to K' or forming an independent structure M to be assimilated later to K or to some extension of K. Derivation is assumed to be strictly cyclic, but with the phase level of the cycle playing a special role. (Chomsky, 2001, 11-12)

> Spell-Out seeks formal features that are uninterpretable but have been assigned values (checked); these are removed from the narrow syntax as the syntactic object is transferred to the phonology. The valued uninterpretable features can be detected with only limited inspection of the derivation if earlier stages of the cycle can be

"forgotten" – in phase terms, if earlier phases need not be inspected. The computational burden is further reduced if the phonological component too can 'forget' earlier derivation. These results follow from the Phase-Impenetrability (PIC)." (Chomsky, 2001, 12-13)

It has been a consistent stance of generative syntactic theory (and Minimalist syntactic theory more specifically) that our syntactic models are models of competence, and not performance. That is to say, we are modeling human knowledge of language, rather than any particular psycholinguistic processes.[2] The repeated references to computational burden and computational efficiency are therefore somewhat at odds with this stance, that we are not modeling processing in any way. The standard rationalization (often of syntax professors, to undergraduate students) is that this is one extended metaphor: that there are no literal computations at play, and this is simply a way of pressing for the simplest theory possible. That said, Chomsky himself seems to conflate the computation metaphor with actual neuro-biological computation, considering three factors in language development in an individual: "1) Genetic endowment, apparently nearly uniform for the species" (UG), 2) "Experience, which leads to variation," and 3) "Principles not specific to the faculty of language" (Chomsky, 2005, 6). The most minimal theory of UG, then, is one which can explain the most depending most on extralinguistic (i.e. "3rd") factors, rather than enriching UG. Along those lines:

> One natural property of efficient computation, with a claim to extralinguistic generality, is that operations forming complex expressions should consist of no more than a rearrangement of the objects to which they apply, not modifying them internally by deletion or insertion of new elements. If tenable, that sharply reduces computational load: what has once been constructed can be "forgotten'' in later computations, in that it will no longer be changed. That is one of the basic intuitions behind the notion of cyclic computation. The EST/Y-model and other approaches violate this condition extensively, resorting to bar levels, traces, indices, and other devices, which both modify given objects and add new elements. A second question, then, is whether all of this technology is eliminable, and the empirical facts susceptible to principled explanation in accord with the "no-tampering'' condition of efficient computation. (Chomsky, 2005, 11)

In short, the idea here is to limit the amount of stuff that syntax can do: largely, syntax operates on lexical items as they are combined, but on the Minimalist approach it is largely a combinatorial operation and not much else. The reasoning here, however, encroaches on the competence/performance distinction that syntacticians have long maintained. O'Grady (2012) offers a useful critique that is highly relevant to our concerns here:

> Despite the allusions to memory and forgetting, Chomsky is not proposing a model of how sentences are produced and comprehended: computational efficiency is not the same thing as processing cost. ... Such examples [Minimal Link Condition, phases,

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[2]This distinction is discussed or assumed in most of Chomsky's work on syntax; Chomsky (1965, 3) stated, "Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance."

copy theory] illustrate the extent to which the viability of computational efficiency in the Minimalist sense depends on a network of theory-internal assumptions about cost, copies, movement operations, and syntactic representations. Such assumptions raise questions about whether an independently verifiable third factor is really in play here at all. No such doubt arises with respect to processing cost, a performance-based notion whose effects are measured and tested through psycholinguistic experimentation. (O'Grady, 2012, 496)

In a sense, Minimalism (to anthropomorphize the field) has attempted to have it both ways: we theorize about so-called "3rd factor effects," but nonetheless do not make our theory in any way responsible for language processing (or any form of actual neurological/psychological computation): this has of course not gone unnoticed.

Take for instance the proposal that the derivation proceeds in 'phases', with vPs and CPs receiving a phonological and semantic interpretation immediately upon being formed, after which they are 'forgotten' and become inaccessible to later operations. Chomsky ... suggests that this 'minimize[s] computational load', but it is far from clear how this claim can be interpreted or evaluated psycholinguistically, given that the minimalist computational system that he employs builds structure from right to left—the reverse of what actual processors do. (O'Grady, 2008, 460-461)

We are inclined to agree with O'Grady - while there are perhaps important theoretical benefits to having a computationally efficient system, it's not clear what the "computation" is actually referring to, as it does not appear to align in any direct way with more familiar mental phenomena like psychologically synchronous processing costs. As it has turned out, a model designed to reduce computational burden in fact has been quite successful at what it attempts to do (model adult grammatical competence). This is surely a major reason why these theoretical mechanisms have been retained for over two decades. But despite some efforts to connect the model directly to processing in some way (Chesi 2015, for example), for the most part there has been no clear link to actual processing in any way.

DMS, though, suggests an alternative. While it seems relative clear that the 'reduced computational burden' that Minimalist theorizing attempts to capture is not related to language production or perception, we suggest that there are in fact real neuro-biological computational processes being modeled by Minimalist syntactic theories: it is simply that they are not synchronous processes in adults, but instead they are essentially the ontogenetic-historical documentation of the language processing that was involved in the course of language acquisition. If the DMS principles proposed in Chapter 2 hold, the resulting target adult grammatical structures not only represent a system of grammatical knowledge, but they actually encode the process of acquisition in the first place. If this finding holds, we would then *of course* expect to find principles of minimal computation at play, reflecting the same principles of economy and optimal mapping between sound and meaning that Chomsky idealized in his foundational reasoning about the Minimalist Program.

Therefore, on this approach, Merge could be considered the most computationally efficient structure-building operation, and the canonical, 'normal' procedure for a child grammaticalizing patterns they observe in the input they are receiving. We offer some specific thoughts on the theoretical mechanisms related to this efficiency later in this section, but those are admittedly

only initial speculations. The point at present, however, is to note that DMS provides a path to reconciling this persistent tension in Minimalist theorizing: we are modeling competence, yet using a computational system designed to minimize computational burden. Whose burden is it that is being minimized? In our opinion, the child's. But the same mechanisms model adult competence well precisely because new structures are added to existing structures in acquisition, retaining previous knowledge and preserve the pathway to the target, adultlike grammar.

3   Phases as a unit of semantic significance

In this section we sketch a DMS-style approach to phases, though there is more work to be done. This is built on the conception of phases introduced by Wolfram Hinzen, Michelle Sheehan, and colleagues. We don't assume broad familiarity with these ideas, so we describe the background in §3.1 before exploring the consequences for our approach in the subsequent sections.

3.1   Referential Phases as the heart of UG

Hinzen (2006, 2012) and Hinzen and Sheehan (2013) challenge the notion that the semantic ontology and semantic principles are independent of syntax.[3] This abandons the approach developed in a long history of Chomsky's work that claims that language is simply a tool to express thought, but that language and thought are fundamentally distinct (e.g. Chomsky 2000b). Hinzen adopts a framework that is in fact closely linked with the syntactic architecture of the Minimalist Program (Chomsky, 2000a, 2001, 2008) that claims that the syntactic derivation proceeds by phase, and each phase must necessarily be legible at the C-I (Conceptual-Intentional) interface (also known as LF, Logical Form). However, Hinzen contests the traditional syntax-semantics disjunct and instead claims that grammar is in fact the principle factor that allows for organization of meaning in language. Therefore, "rather than being 'autonomous' and merely 'interfacing' with the semantic component, ... grammar is a way of carving up semantic spaces" (Hinzen 2012:311). That is to say, grammar "creates the semantic ontology of language," such that grammar in fact is meaningful, and meaningful contribution of grammar is reference (Hinzen 2012:311). Specifically, the phase is the referential component of grammar, with different phases referring to different entities—DPs refer to individuals, *v*Ps to events, and CPs to propositions/truth (Hinzen and Sheehan, 2013; Sheehan and Hinzen, 2011). A phase's semantic contribution is to take the conceptual/predicational content of the phase (e.g. the concept of DOG, or BANANA) and to enable linguistic *reference* to relevant entities. Phases themselves are composed of a phase interior and a phase edge, as shown in (119).[4]

(119)   [EDGE [INTERIOR]]

(120)   [$_{DP}$ the [$_{NP}$ man]]

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[3]A version of this section first appeared in Diercks et al. (2020), and is republished here, with edits, in accordance with Language Science Press's Open Access policies and with permission of the co-authors of that work: Michael Diercks, Marjo van Koppen, and Michael Putnam.

[4]The formulation in terms of edge/interior presented here is adopted from Hinzen and Sheehan (2013).

A DP phase, for example, will refer to an object/individual. On the approach developed in this collection of work, the interior of a phase is the descriptive content of the phase and the edge of the phase (head+extended material) enables reference of varying degrees of specificity. In this sense lexical content cannot refer on its own – reference is only possible in grammatical contexts.

> Lexemes by contrast [to animal calls] not only can be used referentially in the physical absence of their referent, but are also very incomplete in their meaning. The word 'eagle' by itself does not denote anything in particular: not this eagle or that, not all eagles or some, not a kind of bird as opposed to another, not the property of being an eagle, etc. — things that it can denote only once it appears in the right grammatical configurations. It is also used for purposes of reference and predication, in addition to being used as a directive for action, and it again requires a phrasal context, hence grammar, when it is so used. (Hinzen and Sheehan 2013: 42-3).

On this approach, then, linguistic meaning is *reference* (to objects, events, and propositions), and reference is determined grammatically, via a syntactic derivation by phase. For ease of exposition, we will refer to this general framework as the Phase Reference (PR) model. In one sense the PR model is an inconsequential shift for syntacticians' everyday sort of analysis – this does not change the nature of our grammatical architecture much, retaining derivation by phase, Merge, Agree, and the kinds of functional structure we are familiar with at present. In another sense, however, the PR model is a dramatic shift, as we suddenly have incorporated reference—a central semantic notion—into the syntax itself. Let us look at some specific examples of how syntax and semantics are intertwined by looking at Sheehan and Hinzen's (2011) (henceforth S&H) discussion of the referential possibilities of DPs and CPs, before exploring the consequences for *v*P structure that Diercks et al. (2020) proposed as an extension.

3.2 Reference of DP phases

As for the DP-level, S&H point to Longobardi's (1994; 2005) proposals regarding the range of interpretations available for DPs, particularly the proposal that proper names raise to D. Modifying and building on Longobardi's approach, they propose that there is a three-fold ontology of DPs in terms of their referential capabilities:

(121)   Referential capabilities of DPs (S&H p.415)
      a. Indefinite existential nominal reference
      b. Definite descriptions (contextually bound free variables)
      c. Proper names (maximally specifically referential, with rigid reference)

One illustration that they rely on here draws on data from Elbourne (2008):

(122)   a. The Pope is usually Italian.
      b. (Pointing at the Pope) He is usually Italian.
      c. #Joseph Aloisius Ratzinger is usually Italian.

Both definite descriptions and pronouns can refer to different individuals (as specified by context), whereas proper names have much more rigid reference to a specific individual. S&H claim that these three sorts of DP reference are syntactically derived, that is to say, there are syntactic correlates of all three interpretive possibilities.

> When the D-position is empty (there is no determiner and there is no movement to D), a default existential interpretation is derived, where reference is to an arbitrary instance of the predicate. In short, reference is restricted merely in virtue of the predicate's content, or by the interior of the nominal phase. (S&H p.421)

> Definite reference, in contrast, involves both the D° position and the base predicate position, such as an instance of a definite determiner in D° and the noun occurring in N°. In this case, both the phase interior and phase edge determine reference. (S&H p.421)

Proper names, in contrast, consist of movement from N° to D° with N° substituting for D°, such that

> [R]eference is unmediated by descriptive content and only the phase edge determines reference, resulting in the rigid referential properties of proper names. (S&H p.421) [5]

> Broadly speaking, then, the three referential possibilities nicely correlate with the three logically possible ways in which the phase edge and interior can contribute to the determination of reference: only the phase interior mediates reference, or both the interior and edge do, or only the edge is involved. (S&H p.421)

3.3   Referential properties of CP phases

S&H then extend this threefold ontology of phases, correlating the three referential possibilities of DPs for reference to individuals to a threefold ontology of reference by CPs to facts. Specifically, they claim that CPs may be indefinite, representing propositions, definite, yielding facts, or rigid in their reference, denoting truth.

(123)   Referential capabilities of CPs (S&H p.424)

    a. **Reference to Propositions**: C° is empty or underspecified, through a quantificational operator (optionally null in English), yielding an indefinite interpretation;

    b. **Reference to Facts**: C° is pro-form (obligatorily overt in English) with a TP-restriction, yielding a referential interpretation;

    c. **Reference to Truth**: C° is substituted by V°/T° overtly or covertly (covertly in English, overtly in V2 languages), yielding a rigid interpretation unmediated by a descriptive condition.

S&H correlate these referential possibilities with the various interpretations of clauses in embedded contexts in particular, discussing non-factive clauses as indefinite reference, factive clauses as definite reference, and root clauses and embedded clauses with root clause properties

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[5] The proposal of movement of proper names to D is adopted directly from Longobardi (1994, 2005).

as those with the rigid interpretations that come from a truth-conditional (i.e. truth-referring) clause. Therefore, there are particular interpretive (referential) properties of phases, and the syntactic realization of a phase (specifically, the relationship between the phase-internal material and the phase edge) has specific referential consequences depending on the phase in question.

Hinzen (2012), Sheehan and Hinzen (2011), and Hinzen and Sheehan (2013) therefore make the distinctive claim that phases are in fact "units of semantic significance," namely, grammar makes linguistic reference possible, with the referent varying based on the kind of phase (Hinzen 2012:323). These proposals are set forth as relevant to all phases (DP nominal reference, *v*P event reference, and CP fact reference).

3.4   Toward an ontology of *v*P phase reference

Here we repeat the claims that were first set forward by Diercks et al. (2020). Sheehan and Hinzen (2011) and Hinzen and Sheehan (2013) do not extend a detailed analysis of the reference of phases to *v*Ps. Their comments are mainly restricted to the notion that *v*Ps refer to events, though Sheehan and Hinzen (2011) do comment that more specific reference with respect to *v*Ps may well have to do with the boundedness of events (i.e. the aspectual properties of predicates). We develop this idea here in more depth; specifically, we propose that there is also generally a threefold ontology of *v*P phases based on the aspectual properties of predicates, as shown in (124):

(124)   Referential capabilities of *v*Ps (to be expanded on below)

    a.   **Existential event reference** (e.g. existential/presentational clauses)

    b.   **Atelic events** (boundedness of event is addressed but is not rigid)

    c.   **Telic events** (maximally specifically reference, with rigid reference to bounded event)

Here telic events are those where the predicate dictates a specific culmination point; atelic predicates do not (Beavers 2012 offers a good overview of the relevant issues). Existential clauses, on the other hand, are the most unspecified sort of event that does not refer to a bounded event at all, but rather a state of existence. For this ontology to hold in the PR model it should be demonstrable that telic events show maximal involvement of the edge of the phase in the syntactic derivation, with atelic events showing less, and existential reference to events showing the least involvement of the edge. As we will see, the involvement of both verbs and objects in *v*P-based event reference complicates this threefold ontology, though notably in exactly the ways predicted by the PR model.

Perhaps the classic English diagnostic for telicity of predicates is the distinction in application of in/for modifying PPs, for example *in an hour* (compatible with telic predicates) and *for an hour* (compatible with atelic predicates) (Vendler 1967; Dowty 1979; Thompson 2006; Beavers 2012, among many others).

(125)   **English** (Thompson, 2006): 213

    a.   Mary ate an apple in an hour/??for an hour.

  b. Mary walked ??in an hour / for an hour.

As noted by a variety of work, verbs alone do not determine the aspectual properties of a predicate, which are instead determined by the combined verb phrase material (Verkuyl, 1972, 1989, 1993; Verkuy, 1999; Pustejovsky, 1991; Zagona, 1993; Garey, 1957; Tenny, 1987, 1992; Krifka, 1989, 1998, 1992; Dowty, 1991; Jackendoff, 1991, 1996; Travis, 2010). For example, bare plurals in English yield atelic readings of predicates (126b), and objects with quantized reference yield telic predicates (126d), whereas objects with non-quantized reference yield atelic predicates (126c).[6,7]

(126)   English (Thompson 2006, 212, Beavers 2012, 24)

  a. Mary ate an apple in an hour/??for an hour.
  b. Mary ate apples ??in an hour / for an hour.
  c. John drank wine ??in an hour / for an hour
  d. John drank a glass of wine in an hour/??for an hour

What we see, then, is that the properties of multiple components of a *v*P can influence the aspectual properties of a predicate. Thompson (2006) shows a variety of evidence (including word order of manner adverbs, among others) that there is movement of DP objects to the edge of *v*P in telic contexts, proposing that telicity is produced by checking [bounded] features at an aspect projection.

Thompson's proposal, therefore, is precisely that movement to the edge of *v*P correlates with telicity. Rather than adopt the proposal that this is the result of checking a [bounded] feature, we propose that this is a direct result of the fundamentals of the PR model: 1) phases are referential, 2) *v*P phases refer to events, 3) most specific reference to an event corresponds to telicity, and 4) the general strategy for achieving more specific reference within a phase is moving to the edge of the phase. Given this general PR approach, and following on Sheehan and Hinzen's (2011) suggestion that boundedness is the correlate of "referential specificity" with respect to events, a finding like Thompson's (that telicity corresponds with enrichment of the phase edge) is exactly what we would predict. The one new component here that is not directly suggested in Sheehan and Hinzen's work is that raising of the DP object (rather than just the verb) can correlate with higher specificity of reference.

3.5   Intermediate Summary: Phase Reference model

To recap: the approach developed here by Sheehan and Hinzen (2011), Arsenijević and Hinzen (2012), Hinzen and Sheehan (2013) makes phases the foundational component of Universal Gram-

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[6]Aspectual inflections (e.g. progressive vs. perfective) also influence the aspectual interpretation of predicates (Mary has written the book vs. Mary is writing the book).

[7]Likewise, in English paths/goals represented in PPs can influence the interpretation of an event with respect to telicity, where specific goals of directed motion generate telicity whereas paths of motion alone do not, showing that it is not only objects that play a role in telicity of events, though we focus on object properties here (Thompson 2006: 214).

mar. Specifically, phases are both grammatical, and referential, such that the grammatical structures that result show the syntactic properties of phases entangled with the properties of reference to individuals, events, and facts. We will claim that the syntactic properties of phases are the result of children's meaning-seeking in acquisition. That is to say, the *v*P phase is the result of children grammaticalizing events, the CP phase is the result of them grammaticalizing situations, i.e. an event anchored in time and space, and the nominal phase the result of grammaticalizing reference to individual entities.

4 Applying the Phase Reference model to Acquisition

As we have noted above, previous generative proposals (depending on their level of commitment to the Continuity Hypothesis) have proposed different levels of the core functional hierarchy as part of children's UG knowledge. Some proposals take an approach of strong or weak continuity, suggesting that the underlying structures of syntax are present innately, but don't appear as such in child language for reasons (that vary based on the implementation being considered). We discussed Rizzi's (1994) well-known truncation hypothesis in §2.3, where children's utterances may utilize a subpart of the underlying grammatical structure of a sentence (truncated the clause). Importantly, children's utterances are taken to be potentially variable: at times they may simply be taken to use a VP structure, other times a TP structure, or a CP structure. This is meant to capture the fact that despite the fact that children at the root default stage are only dependably utilizing the eventive core of a sentence (see §3 of Chapter 2), they will nonetheless sometimes communicate using utterances that (at least appear to utilize) higher syntactic structures. Meant to capture similar fact, the Agreement and Tense Omission Model (ATOM) of Schütze and Wexler (1996) argues (as it is named) that while CP-level structures may be present, agreement and tense are missing. Alternatively, Clahsen (1990/1991) proposes that an underspecified functional head may appear at an early stage which later is re-analyzed by a child as a CP head, with agreement projections inserted underneath it. (See also Hyams 1996 and Jordens 2012.) What we see, then, is that it has long been recognized that lower structures are acquired before later structures, but that there are persistent reasons for positing some degree of higher structure that is consistently available.

The approach we will suggest here hews most closely to that of Clahsen (1990/1991) and Clahsen et al. (1993/1994), who propose a level of functional structure above the verb phrase that is underspecified at early stages of acquisition. But what is that functional structure? Why does it exist? We will suggest that it follows quite directly from the Phase Reference model discussed above, when considering in an acquisition context per DMS.

In Acquisition, children's main job is to identify what things mean (the so-called "Principle of Reference," ud Deen and Becker 2020). This means, for any particular target of acquisition at any point, children's goal is to establish a referent for some unit of language. At the word level (especially the kinds of nominals acquired early, which tend to be concrete and discrete entities in the world), the word can equal the referent. But once reference for higher-level structures become a target of acquisition (e.g. an event composed of a verb and arguments), there is necessarily investigation of some level of abstraction. Recall Hinzen and Sheehan's (2013) phase schematic:

(127)   [EDGE [INTERIOR]]

Our proposal in adapting the Phase Reference proposals to acquisition, then, is that the 'edge' in this schematic is a hypothesized structure the child brings as part of UG.[8] The idea is not that children acquire referential meaning and grammar as two separate entities, but instead that grammar is itself the cognitive tool for reference (this of course being the central proposal of the Phase Reference model, albeit for adult grammars). If, again, the Grammaticalization Mechanisms children are bringing are phase reference, Merge and Agree, we would expect any potential child language structure that is either under investigation by a child or grammaticized by a child to take this form.

(128)   Children's Grammaticalization Working Hypothesis:
          [F_{Refer} [descriptive content]]

Therefore as children acquire units of linguistic content, they associate that linguistic content with a head (this is essentially a variant of κP, Wiltschko's 2014 categorization mechanism), and Merge that head it to the existing grammatical structure. In fact, we could model it something more like (129), where different kinds of reference are being investigated in different structures, either because the target referent is different (individuals or events) or because the timeline is different (events or propositions).

(129)   Intermediate Realizations Children's Grammaticalization Mechanisms:
          a. [$F_{Refer:individual}$ [nominal content]]
          b. [$F_{Refer:event}$ [verbal content]]
          c. [$F_{Refer:proposition}$ [$_{vP}$ event]]
          d. [$F_{Refer:context\text{-}links}$ [$_{CP}$ proposition]]

(129) is not meant to imply that the different phase heads are innate: only the abstract referential mechanism in (128) is innate. The different versions of F_{Refer} in (129) are simply annotated as such for expositional clarity. As discussed by Rakhlin and Progovac (2020), the different domains of grammatical functions (i.e. thematic, anchoring, linking) are correlated with non-linguistics cognitive development. In this case, each of the subtypes of F_{Refer} in (129) are simply the result of applying F_{Refer} to different kinds of descriptive content. So F_{Refer} atop nominal content refers to an individual/entity, F_{Refer} atop verbal content refers to an event, etc. Phases in adult grammar, therefore, are the grammatical realization of an acquisition strategy: the child uses grammaticalization mechanisms (namely, the foundational assumption that building functional structure associates with reference, associating linguistic content with a categorial identity per Wiltschko (2014), and Merging that content into existing structure, per Chomsky (2001)) in order to identify the referent of a structure. The key here for our concerns is the difference between what unit of reference is being 'solved' by a child at any given point of time. If we accept a DMS-style approach to using adult grammar to understand child development, we do see some clear stages emerge, namely, thematic-identification, anchoring, and linking.

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[8]Alternatively, it could be phrased that it is the linguistic-grammatical realization of their reference-seeking task: while not substantively different for our concerns, that could be comforting for those with non-nativist predispositions).

Recall from our discussion in §5 that the thematic domain is the eventive core: the predicate and its arguments. The anchoring domain is essentially the acquisition of displacement - that an event can be non-local in time,space,and participants, and therefore grammatical events must be anchored in time/space/participants in order to to create a a fact/proposition in the sense of Hinzen, or a situation in the sense of Ramchand and Svenonius (2014): a situation that can ultimately be described as true or false. Linking, then, is associating that proposition in real world context: discourse context, pragmatic context, etc. Depending on the language, some linking properties would be lexical, some grammaticalized into the morpho-syntax of the language. There is well-known variation in adult grammars on that point.

The core structural/developmental idea here is not new at all: what we are proposing is an underspecified functional projection in order to explain intermediate stages of knowledge. This idea has been explored on many, many occasions, including Clahsen (1990/1991), Clahsen et al. (1996), Hyams (1996), and Jordens (2012), just to name a few. The connection with phases (and the particular model of phases adopted here) is certainly novel, to the extent of our knowledge. But hopefully the fact that its implementation is via a well-established generative approach to child language acquisition (underspecified functional projections) is encouraging to the skeptic.

We propose that the intersection of cyclic movement through the edge of phases, and phases corresponding with grammatical-functional domains (Wiltschko, 2014; Ritter and Wiltschko, 2014) is good reason to think that there are correlations between these two. It therefore suggests to us that a 'phase' is essentially the stage during which a child is grammaticalizing one of these functional domains, which necessarily proceed in sequence: first the thematic domain, then the anchoring domain, then the linking domain. These generally correspond to phase structures in a clause: the *v*P, the CP, and the extended projection of matrix CP.[9]

The suggestion, then, is that phases in adult grammar, like all other structures in DMS, are ontogenetic fossils, the structures that remain as a result of passing through acquisition stages. As discussed by Rakhlin and Progovac (2020), there are good reasons to think that these developmental sequences are in fact connected with other aspects of cognition outside of language as well. As we have maintained throughout this monograph, we are concerned with drawing correlations between adult syntactic structures and extra-syntactic facts (namely, stages of acquisition), but we are not making a specific claim about causation. We think it is reasonable and perhaps defensible to assume that the linguistic sequences follow general cognitive developments, but it's quite difficult to disentangle to what degree the linguistic developments also enable the cognitive developments (see, for example, de Villier's 2007 discussion of the interface of language development and Theory of Mind). "[T]]here is likely a reciprocity between cognitive and linguistic development. Therefore, fully disentangling causality in the language-cognition relationship is dauntingly complex " (Rakhlin and Progovac, 2020, 14). Therefore we carefully avoid claiming which development precedes the other (grammatical knowledge vs. non-linguistic cognition). Instead, we continue to focus on the task of demonstrating the core correlation (Minimalist derivations of adult grammar, and acquisition timelines).

Phases as Workspaces (PAW) has some interesting consequences. First, this has the consequence that there is in fact no specific inventory of particular phase heads. Rather, a phase is

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[9]The extended projection of matrix CP, while not considered a phase in much theorizing, is still subject to its own instance of transfer/spellout

defined (in the idealized sense) as when a child has reached complete grammaticalization of one functional domain, and has begun acquisition of the next functional domain. As we show in §6, this is exactly what we find in how phases operate in adult grammars (quite unexpected from the standard implementation of phases as originated by Chomsky 2001). Furthermore, this suggests that while there should be very clear bottom-up structure building from functional domain to functional domain, it rather leaves open the question of the specific sequence of acquisition of structure *within* a functional domain. We think this can capture the apparent simultaneous processes of acquisition of some kinds of structures (but not others), and can also capture the kinds of proposals that have been advanced for adult language grammars, that operations have been proposed to be simultaneous within the phase: we take this up in §7.

5 'Early' acquisition of V2 in German

As raised by a reviewer at an earlier stage of this work, children acquiring German begin to place (finite) verbs at a V2-like position quite early in acquisition. On the standard analysis of German V2, the finite verb is in a C-level head and the element preceding it (usually, but not always, the subject) is in Spec,CP; VP and *v*P are head-final. Nonetheless, German children at early stages will place verbs in a V2-like position: (130) shows the verb preceding negation.

(130) da   paß nicht   German child (M 2;4)
       there fits not
       'These pieces do not fit together'. (Clahsen, 1990/1991, 376)

If children are acquiring that word order early, does that mean they are acquiring CP early? If so, what does that say for the approach to bottom-up acquisition and phases that we have sketched above? While the connection to phases and the underlying motivation are different, in fact the structural account that we propose is essentially identical to that presented by Clahsen (1990/1991) to account for acquisition of V2 (among other things) in German. We outline this here, both to consider the puzzle raised by German V2, but also to discuss the analysis of Clahsen (1990/1991), which is an important precursor to our proposals here.

Clahsen identifies five stages for the acquisition of core German phrase structure. Clahsen (1990/1991) looks at four syntactic features across those stages: verb placement, negation, subject agreement, and (exceptional, for German) null subjects. In the investigation in Clahsen (1990/1991), most of the relevant acquisition occurred between stages II and IV, with the transition between stages II and III only being minimal and gradual for these structures. So the main distinction to be looked at here is between stage II/III (conflated for the purposes of this discussion), and stage IV. The various patterns documented by Clahsen (1990/1991) are summarized in (131).

		Stage II/III	**Stage IV**
(131)	**Verb position**	V never precedes subject; variable V positions; finite V placed in V2; non-finite verb placed V-final	largely adultlike, including complex contexts like phrasal verbs and auxiliaries/modals
	Subject agreement	properties of subject don't determine verb agreement (II); high proportion of errors (III)	largely adultlike
	Negation	postverbal NEG with inflected V preverbal neg with uninflected V	NEG placement largely adultlike; preverbal NEG pattern stops
	null subjects?	grammatical subjects often absent	grammatical subjects usually present

We can see the variable verb and negation placement at stage II/III in the example in (132). The uninflected verb form appears utterance-final in (132a), following negation. In contrast, the verb bearing the *-t* inflection appears initial, preceding negation (Clahsen assumes with a null subject, here; though clearly there is no overt subject).
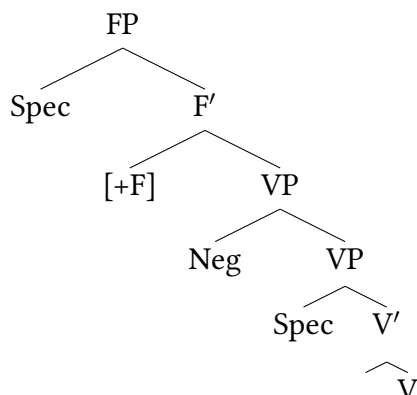
(132) a. nich aua   mache   German child (S 1;10)
          not   ouch make

       b. macht nich aua   German child (S 1;10)
          makes not   ouch
          (Clahsen, 1990/1991, 378)

The preverbal negation in (132a) is distinctly non-target like (as is the uninflected root verb form): this is clearly the root default / optional infinitive stage of acquisition for this child.

On the approach taken in DMS, a major question is raised about the position of the verb in German at Stage II/III. If, at this point, the only structures that exist for the child are VP/*v*P, how is it that the children have two verb positions available? It is not shocking that they distinguish early between the position of inflected verbs (initial/V2) vs. uninflected verbs (final), as this directly matches the word order in the target. But what is their internal grammar at that point? Following standard assumptions of German V2, the VP/*v*P is head-final; the uninflected verb remains structurally low, explaining its final position (in both child and adult grammars). So what position are the non-final verbs in?

Clahsen (1990/1991) proposes that there is a functional projection (labeled FP here, +F being +finite) that children posit above the verb phrase at Stage II/III. This is the position where finite verbs are positioned at this stage.
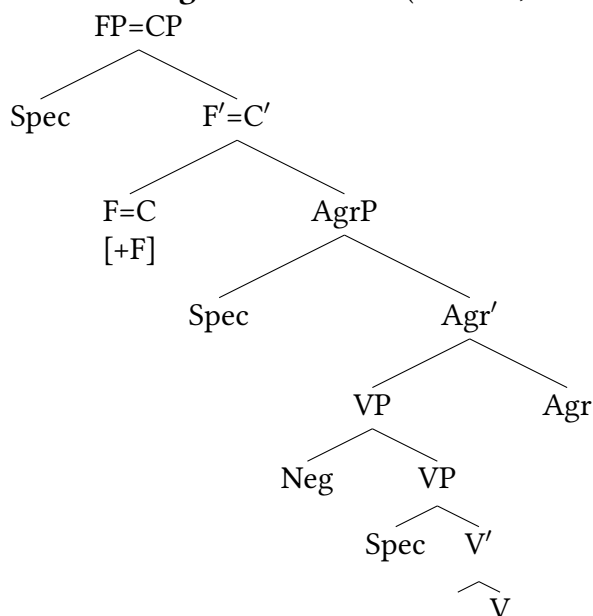
(133) **Clahsen Stage II/III Structure** (Clahsen, 1990/1991, 382):

```
          FP
        /    \
    Spec      F′
            /    \
        [+F]      VP
                /    \
             Neg      VP
                    /    \
                 Spec     V′
                           |
                           V
```

A key for Clahsen is that, even at stage IV, it is implausible that children have acquired the full clause structure: embedded clauses have not arisen at this point. Once embedded clause do arise, they are essentially target-like immediately, suggesting that children have already acquired the requisite structures by the time they are embedding clauses. This does show, however, that syntactic development is not complete at stage IV, despite V2 operating robustly. But even moreso, it shows that at stage II/III when there is a non-final position of verbs, that this is clearly not an adultlike CP at work, so this is not an instance of early CP acquisition (i.e. it doesn't contradict the "bottom-up" claim of DMS, or Organic Grammar, or Rakhlin and Progovac (2020)). It does, of course, pose a challenge

At Stage IV, subject agreement is target-like, subjects are largely overt, and root infinitives disappear, resolving the placement of negation. Here, Clahsen suggests that an agreement projection (we might assume it to be TP) is now present, and FP is still present, but the agreement projection is between FP and VP.

(134)   **Clahsen Stage IV Structure** (Clahsen, 1990/1991, 384):

```
             FP=CP
           /      \
       Spec        F′=C′
                 /      \
             F=C         AgrP
             [+F]       /    \
                    Spec      Agr′
                            /     \
                         VP        Agr
                       /    \
                    Neg      VP
                           /    \
                        Spec     V′
                                  |
                                  V
```

Centrally, as we represent in (134), what was previously FP (and the placement of the finite verb) is what the child will eventually grammaticalize as CP. So here we don't see a strict-

bottom up structure building as a Merge-based system (with nothing else considered) might imply. Rather, we see a period of time where structures may be posited and adjusted, before they are finalized. So while children are clearly using a position for a verb (specifically, a finite verb) that precedes the rest of their verb phrase structures at a very early stage, this position appears to be able to be adjusted as necessary, based on the transparent input they are receiving.

On the approach we've set forward here, we essentially follow Clahsen's proposals. We have suggested that on the phase-reference approach to phases, children enter with a working hypothesis that essentially posits a functional head to grammaticalize whatever morphosyntax pattern they are encountering, while their 'acquisition task' is mainly focused on particular underlying functional domains: first, thematic properties events, next anchoring events in space/time, then linking the resulting situation to the discourse structure. Presumably, with each new stage of grammatical knowledge that a German-learning child adds, a clear and necessary hypothesis will include a head-initial functional projection at the root of their structure. The predominance of V2 in the input requires this in order for a child to capture basic word order facts. Like with the shift between (133) and (134) above, additional material may be added within a phase without a violation of Merge-based structure building, as these structures have not reached a target-like state, yet.

What proceeds in a strict Merge-based cycle is the finalization of grammatical structures, not the initial uses of those forms. Initial uses of forms is, following the usage-based model, most likely largely driven by frequency of forms in the input. Given that non-V-final structures are predominant in German, there is no surprise to find children replicating that in their own utterances, despite not yet grammaticalizing the CP structures where verbs sit in adult German. In a case like German V2 (and, in fact, any verb movement in any language), it is completely transparent when there must be a new structural position for a verb as additional structures are added in a child's grammar: the position of verbs is evidenced overtly in essentially every sentence they hear. It is therefore not mysterious that children can learn verb positions early. What is necessary is a mechanism for them to posit a grammar for that, as Clahsen suggests and which is provided as well by the phase reference model of phases as acquisition workspaces.

The idea of PAW here is that, for a period of acquisition, a child will be positing structures that provide an intermediate grammar, but need not be target-like, and need not be their final structures, either. It also means that those structures may be adjusted as the child continues to acquire morphosyntax within that phase. But there is, of course, when the posited structure never needs to be revisited, as new data provide no challenges, because they have in fact reached a target-like structure: this is when a phase is considered complete. We predict a specific sequence of structural hierarchy regarding this point of grammaticalization, not the first initial uses of any particular morphosyntactic construction.

This shows why, despite the broad bottom-up pattern of acquisition, we cannot simply say that acquisition is simply 'bottom-up, based on Merge.' However, the core claims of DMS should still hold: we should see the effects of these acquisition domains in adult syntax. And this is what we claim occurs: by the time a child is finalizing their target grammar for the inflectional (anchoring) domain, and by the time they are grammaticalizing embedded CPs and other CP-level material, the VP/vP domain has long been target-like. It is this pattern, we suggest, that results in the grammatical effects that surface in phase-theoretic grammar across languages. In this way,

phases are ontogenetic fossils. The next several sections show some additional benefits of this kind of approach.

6   Variable size of phases

One phenomenon that this provides a natural explanation for is that it is quite difficult to simplistically identify a given functional projection as a phase. We overview evidence in this section showing that phases in adult grammar can be of different sizes: sometimes a vP is a phase, for example, but at other times the lower phase is an Aspect Phrase that contains vP. This kind of outcome is deeply curious in isolation and has provided pressing theoretical puzzles even for those who have argued for this conclusion. But in the model presented here where phases are acquisition workspaces driven by a small number of core referential tasks (individual-identification, event-identification, anchoring, etc), it falls out quite naturally: different constructions of different sizes can accomplish the referential goal. Transfer/spellout of one phase occurs when a child transitions to a new referential task (e.g. from event-identification to anchoring of events) We would expect, therefore, there to be a "maximal phase," i.e. the biggest structure that belongs to an event (approximately vP), or a situation (approximately CP), but that if some levels of that phase are truncated in any given sentence (i.e. not all functional structure is present in all sentences), then whatever structure is the highest one in the relevant domain shows the properties of the phase in adult grammar. This follows from our DMS proposals here, but this also precisely matches how phases appear to be behaving.

There is a broad range of research showing that rather than the rigid approach to phases assumed by Chomsky (2001) wherein there is a specific inventory of functional heads that are phase heads, instead a more dynamic approach is required, which allows different projections to be phases depending on the syntactic context (Bobaljik and Wurmbrand, 2005; Wurmbrand, 2013; Bošković, 2013, 2014; Harwood, 2015; den Dikken, 2007; Gallego, 2010; Bošković, 2005; Gallego and Uriagereka, 2007a,b; Despić, 2013; Takahashi, 2010, 2011). Two prominent, relatively recent approaches to this are Bošković's (2014) "highest phrase is a phase" approach, and Harwood's (2015) numeration sub-array approach. Bošković (2014, 28) claims that "the highest projection in the extended projection of a major (i.e., lexical) category functions as a phase," such that the phasal status of a given projection depends on its immediate syntactic context, namely, whether that projection is the final functional projection in the extended projection of a lexical category in that particular construction, or whether there are additional functional projections above it. Harwood (2015), in contrast, proposes that the Numeration is divided into sub-arrays that correspond to phases, such that once a sub-array is exhausted, the final projection merged projects a phase. Both accounts have shortcomings, which we outline below, but first we examine some of the motivating empirical facts.

Bošković (2014) builds his argument based on extraction (inside DPs) and ellipsis of VPs and aspectual projections; we focus our discussion on the former, mainly because it is easier to summarize succinctly. Bošković (2014) shows that there are extraction asymmetries within noun phrases. It is well-established that some languages don't have articles in the structure that is traditionally referred to as a noun phrase (TNP), not only on the surface, but that those

TNPs are in fact syntactically NPs, not DPs: Serbo-Croation is one of these languages.[10] In Serbo-Croation possessors consistently behave as adjectives, and are treated as NP adjuncts. As a result, possessors can c-command out of the TNP, whereas they cannot in languages like English where the TNP is a DP. Therefore, the possessors in the subject TNPs in (135) cannot c-command out of the TNP, with the result that coreference between a possessor and a R-expression/pronoun is possible in English:

(135)   a. His$_i$ father considers John$_i$ highly intelligent.
        b. John$_i$'s father considers him$_i$ highly intelligent.

In contrast, because possessors are adjuncts to NP (without a DP present) in Serbo-Croatian, they can c-command out of the TNP. This results in Condition B (136a) and Condition C (136b) violations when the possessor is coreferent with a pronoun or R-expression that it c-commands.

(136)   a. *[$_{NP}$ Kusturicin$_i$ [$_{NP}$ najnoviji film]] ga$_i$ je zaista razočarao.
            Kusturica's   latest   movie him is really disappointed
            'Kusturica$_i$'s latest movie really disappointed him$_i$.' b.
        b. *[$_{NP}$ Njegov$_i$ [$_{NP}$ najnoviji film]] je zaista razočarao   Kusturicu$_i$.
            his   latest   movie is really disappointed Kusturica
            'His$_i$ latest movie really disappointed Kusturica$_i$.'
            (Bošković, 2014, 31)

Bošković's central claim is that the highest phrase in the extended projection of a lexical category is a phase: the result of this approach is that specific functional projections are not phases, but any can serve as a phase if it is the final phrase in the extended projection of the lexical head. This suggests that bare NPs themselves can behave as phases in languages like SC, whereas the presence of a DP level in other languages (like English) ensures that NPs are not phases. Bošković explores this specifically in reference to extraction patterns.

As Bošković notes, "it is standardly assumed that complement-to-Spec movement is impossible, an assumption that has received a more general treatment in terms of antilocality, a ban on movement that is too short" (Bošković, 2014, 32). If movement must proceed by phase edge, however, this results in a ban in movement of the complement of phase heads, as it will necessarily violate anti-locality (Abels, 2003). Bošković notes that this in fact the case for TNPs in SC:

(137)   a. Pronašla sam   sliku   ovog   studenta.
            am   found picture.ACC this.GEN student.GEN
            'I found the picture of this student.'
        b. ?*Ovog   student$_i$ sam pronašla [$_{NP}$ sliku   t$_i$].
            this.GEN student.GEN am   found   picture.ACC
            'Of this student I found the picture.'
            (Bošković, 2014, 33)

Bošković attributes the unacceptability of (137b) to the ban on extracted a phase complement: here, N° is a phase head, as the last head within its own (here, minimal) extended projection.

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[10]We refer the reader to Bošković 2014, 31 for a range of references on these background conclusions.

This contrasts with English, where of-complements are well-established to be able to move out of a TNP. Bošković claims this distinction is because the English TNP is a DP, not a NP, and as a result the DP is a phase, not the NP. There is therefore no ban on moving the complement of N°, because it will raise through the edge of DP, not through the edge of NP. Bošković (2014, 34) provides the examples in (138), drawing on a range of previous work.

(138)   a. Of which city did you witness the destruction?
            (Huang 1982, 542, Chomsky 1986, 80)
        b. Of whom do government employees see pictures every day?
            (Bach and Cooper, 1978, 281)
        c. of which cars were the hoods damaged by the explosion?
            (Ross, 1967, 242)

The key to Bošković's claim about phases draws on constructions where additional structure is added to the NP in SC: some numerals and quantifiers project additional structure in SC, such that the NP is not always bare. In instances like this, notably, possessors no longer c-command out of the TNP, suggesting that there is now a higher phase on the TNP. So (139b) is largely acceptable, whereas its parallel in (136a) was not.

(139)   a. [$_{QP}$ Pet/Mnogo [$_{NP}$ njegovih$_i$ [$_{NP}$ filmova]]] je proslavilo   Kusturicu$_i$.
            five/many   he.GEN   movies.GEN is made.famous Kusturica
            'Five/Many of his$_i$ movies made Kusturica$_i$ famous.'
        b. ?[$_{QP}$ Pet [$_{NP}$ Kusturicinih$_i$ [$_{NP}$ filmova]]] ga$_i$ je obogatilo
            five   Kusturica.GEN   movies.GEN him is enriched
            'Five of Kusturica$_i$'s movies made him$_i$ rich.'
            (Bošković, 2014, 36)

If adding a quantifier extends the phase (to QP, instead of NP), as (139b) suggests, then we would expect that extraction of N° complements becomes acceptable just in these instances, as extracted the N° complement would no longer require movement from complement to specifier of the same immediate projection. And in fact, Bošković reports that this is just what we see. (140) shows sub-extraction of an N° complement to be acceptable when a QP is present (140a), but unacceptable in the absence of the quantifier (140b).

(140)   a. Ovog   studenta$_i$ sam pronašla mnogo/deset slika t_i.
            this.GEN student.GEN am found   many/ten   pictures.GEN
            'Of this student I found many/ten pictures.'
        b. ?*Ovog studenta$_i$ sam pronašla sliku t_i.
            this.GEN student.GEN am found   picture.ACC
            'Of this student I found a picture. (Bošković, 2014, 36)

(140b) therefore exhibits the unacceptable extraction from NP that we saw previously, which becomes acceptable in the event that a quantifier is added to the structure (140a).

Bošković (2014) makes the same point from P-stranding: prepositions should be impossible to strand via movement of their complement without evidence of additional functional structure

in the PP. Bošković notes that in Turkish there are two classes of prepositions: those that can be stranded by movement of their complement noun phrase, and those that cannot. Notably, the prepositions that can be stranded are those that bear agreement morphology (141b) that is absent in the non-stranding prepositions (141a).

(141) a. *Biz [NP Pelin-in arkadaş-ı]ᵢ dün   [PP tᵢ için] para   topla-dı-k
           we.NOM   Pelin-GEN friend-POSS yesterday   for   money collect-PST-1PL
           'Yesterday, we collected money for Pelin's friend.'

       b. Ben   araba-nınᵢ dün   [PP tᵢ önün-de]   dur-du-m.
          I.NOM car-GEN   yesterday   in.front.of-3SG.POSS.LOC stand-PST-1SG
          'Yesterday, I stood in front of the car (not behind it).'
          (Bošković 2014, 39, credited to (Şener, 2006))

Bošković (2014) interprets this as additional evidence supporting his conclusion that phase heads are the final head projected by a lexical category. When a preposition bears no additional functional structure it itself is the phase head, resulting in the inability for its complement noun phrase to extract (i.e. P-stranding is unacceptable) in (141a). In contrast, the agreement morphology in (141b) is taken as evidence of additional functional structure above the strict PP, which means that the PP itself is no longer the phase. As a result, extraction of the complement of P is acceptable, as the extraction does not require movement to the edge of PP, but rather to the edge of whatever functional projection is hosting the inflection. In the end, Bošković (2014) finds consistent evidence for

> … a contextual approach to phases on which the highest projection in the extended domain of a lexical category counts as a phase. Since lexical categories do not always project the same structure, what counts as a phase within a particular domain varies. Thus, in the traditional noun phrase, DP is the phase in English. NP is typically the phase in SC, a language that lacks DP, except when a numeral/quantifier, which projects QP above NP, is present; in such cases QP functions as a phase and NP ceases to be a phase. (Bošković, 2014, 73)

Another argument for contextual approach to phases comes from Harwood (2015), who argues that the lowest phase in a clause may variably be *v*P or ProgP (the functional projection introducing progressive aspect). To address this Harwood uses a range of evidence from ellipsis, but for our purposes we focus on the more transparent fronting evidence. It has been observed by various researchers (e.g. Johnson 2001) that there is a correlation between what can be elided in verb phrase ellipsis and what may be fronted in VP fronting (VPF). Here, the progressive-marked *being* auxiliary is obligatorily fronted in VPF (if present):

(142) Darth Vader says that Han Solo was being frozen in carbonite, then. . .
       a. [**being** frozen in carbonite] he was.
       b. *[frozen in carbonite] he was **being**.
          (Harwood, 2015, (63))

(143) Darth Vader says that Han Solo was being stubborn, then. . .
       a. [**being** stubborn] he was.

b. *[stubborn] he was **being**.
    (Harwood, 2015, (64))

In contrast, the non-finite perfective auxiliary *have* cannot be fronted in the same way:

(144)   If Luke says he would have fought hard, then. . .
    a. *[**have** fought hard] he would.
    b. [fought hard] he would **have**.
        (Harwood, 2015, (65))

Harwood (2015) notes that Sailor (2012) observes that pseudo-clefting appears to target the same material as VPF. Hence, in the presence of a progressive, pseudoclefting a verbal projection must include the nonfinite progressive auxiliary *being*, whereas it cannot include *have*.

(145)   Elmer Fudd should be being criticised.
    a. No, [**being** praised] is what Elmer Fudd should be.
    b. *No, [praised] is what Elmer Fudd should be **being**.
        (Harwood, 2015, (66))

(146)   Elmer Fudd should have been criticised.
    a. *No, [**have** been praised] is what Elmer Fudd should.
    b. No, [praised] is what Elmer Fudd should **have** been.
        (Harwood, 2015, (67))

Notably, however, on the assumption that what you see is what you get (WYSIWYG), a progressive functional projection is not present in clauses that don't bear progressive semantics or morphology. Operating on the assumption that the only phrases that can undergo movement are phases (Holmberg, 2001; Chomsky, 2008; Roberts, 2010; Fowlie, 2010), Harwood (2015) is led to the conclusion (along with ellipsis evidence and intermediate positions of low subjects, among other things) that progressive aspect is a phase.

Notice, however, that a progressive auxiliary is not necessary in order for pseudoclefting or VPF to occur, as (144b) and (146) show. What is being fronted in those instances? The straightforward answer is that it is the *v*P phase. This of course necessitates, though, that sometimes the lower phase in a clause is *v*P, and sometimes it is ProgP. While some of the details differ from the proposals of Bošković (2014), in broad strokes we see a similar conclusion, that the specific size/structure of a phase can vary based on context.

Harwood proposes that the explanation for this variability is rooted in the numeration, i.e. the selection of lexical items that a derivation draws from in building a syntactic structure.

(147)   Explaining Phase Variability (Harwood, 2015)
    a. Phases are determined by sub-numerations.
    b. The last item from a sub-numeration to be merged into the workspace projects the phase, irrespective of what that item is.

This proposal rests on the assumption that Progressive aspect (and lower projections) are part of the first sub-numeration a clause is built from, and perfective is part of the second.

(148)   a.   [Prog *be*, ProgAsp, Passive/Copula *be*/v, Voice, V]
        b.   [C, T, Modal, Inf, Perf *have*, PerfAsp]

While this offers an explanation for how this is implemented syntactically: to a large degree it is simply stipulated. How are sub-numerations determined? This remains an open question for Harwood. "The question arises of course as to why the aspectual system should be divided in this way. That is, why should perfect aspect be contained in the second sub-numeration and therefore, the higher phase, along with tense and modality, whilst progressive aspect is contained within the first sub-numeration and therefore, the lower phase, along with voice and the lexical verb? Although I have no definite solution, I tentatively propose that the first sub-numeration is made up of material that comprises the predicate layer of the clause, and that progressive aspect, yet not higher material, forms part of the predicate" (Harwood, 2015, 559). This is reasonable, given the example below, where predicates of various sorts can be conjoined with progressive aspect:

(149)   Julia is tired and suffering from a cold and (thus) [a good candidate for a miracle cure]/[in a terrible mood].
        (Harwood 2015, 559, credited to Heycock (2011))

Nonetheless, there is still no principled reason presented as to *why* progressive aspect should be considered a part of the predicate, whereas higher aspect is not. Our proposal here is largely co-extensive with both Bošković's and Harwood's: we happily accept the empirical conclusions that phases correlate to different specific structures depending on the construction under consideration. That is to say, phases appear to apply to a specific domain, and the highest projection in that domain in any particular construction demarcates the edge of the phase. But what determines this domain? Harwood's sub-numeration proposal suffers as it is the result of stipulation. Bošković's has a more natural foundation, but runs into problems specifically in distinguishing the lower and upper phases in a clause, where it's not clear what the lexical category is that is projecting in the case of a full CP phase: why does the phase stop at Asp/vP and not project further?

We believe these questions are reasonably resolved on the approach to phases as the ontogenetic fossils of acquisition workspaces, workspaces that are defined by the core referential task that a child is working on at any given moment in development. So, the thematic functional domain is defined by the child's acquisition of the semantic components of an event. The anchoring domain is their acquisition of a situation/fact (an anchored event), and the linking domain is their acquisition of the discourse relations to place that situation in context, where the phases in question align with the functional domains of Ritter and Wiltschko (2014).

A question remains, however: why does progressive behave as part of the thematic domain, rather than as part of the anchoring domain? We will follow on a suggestion by Wiltschko (2014), that imperfective aspect on its own is compatible with an event-internal perspective, as opposed to perfective, which requires a perspective outside of the event (an event can only be viewed as complete if conceptualized from a perspective outside the event itself). More on this in §7.2.

So, as pointed out by many researchers, phases are not a single syntactic projection. This

is in fact what we would expect under the DMS approach. The "freezing" of syntactic operations that is observed for is also expected (i.e. the Phase Impenetrability Condition), in the sense that syntactic operations in adult grammars are in fact fossilized acquisition pathways. So phase-internal content is "frozen" in the *v*P (event-identification) phase specifically bc the child has completed grammaticalization of event-level structures, and continued with higher level structures.[11] This means that "transfer" or "spellout" in the adult grammar sense corresponds to a shift in referential task by a child. Successive cyclic movement proceeds by phase, because as a child completes the an acquisition workspace, the assumption is that their input has made clear when a particular phrase is linearized outside the thematic domain, as we discussed previously for wh-movement in §2.

7   Apparent 'Simultaneity' within phases

In addition to this approach providing some clarity about the structure of phases, as well as how the children's hypothesis spaces are constrained at different stages of acquisition, the phase-based approach provides some hope of addressing some other apparent counter-examples to directly linking every instance of structural hierarchy to the acquisition timeline.

So far in this chapter we have discussed stages of development correlating to domains in adult grammar, and the discussion of phases in adult grammar (correlating to those stages) being instances of ontogenetic fossils, i.e. the remnants of early acquisition workspaces. We have also discussed how this addresses an indeterminancy that otherwise might occur on a DMS approach: there is some degree of internal tension between 'strict' Merge-based structure building (which could potentially suggest a one-to-one correlation of functional projections in adult grammar to grammaticalization sequences in child acquisition) and the variability, fluidity, and gradualness of children's development. In this subsection we offer some empirical observations regarding phases in adult grammars that in fact suggest that some of the overlapping that occurs in child acquisition may in fact persist in adult phases. In short, we need to distinguish childrens initial working hypotheses (and/or non-final grammatical patterns) from their final grammaticalizations. In fact, evidence of both persists into adulthood: sometimes we see evidence of strict structural hierarchy, but at other times we see evidence of operations occurring simultaneously (interacting with each other) within phases.

A well-known fact about human language syntax is that there is a close connection between the properties of complementizers and the properties of inflection, documented from early generative work (for discussion of a range of relevant work, see Fortuny 2008, 31ff and references therein). This is evident, for example, when choices of complementizer in embedded clauses vary based on the finiteness of the embedded clause (e.g. *Maya wants **for** Alex to come back* vs. *Maya hopes **that** Alex will come back*). The C-T dependency plays out in other domains as well. This

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[11]A major question that requires investigation but which requires more time/space than this monograph can allow is to look at complex argument structures inside verb phrases: causative constructions, applicative constructions, lexical ditransitive constructions, etc. The structural analysis of these constructions (both cross-linguistic generalizations and language-particular variation) carries important implications for the proposals here, making a host of language-specific predictions for acquisition, depending on the properties of the relevant constructions in the respective languages.

can be seen most obviously in some languages by the presence of the same phi-features on T and on C.

(150)   Partial Paradigm of West Flemish Complementizer Agreement (Fortuny 2008, 32, attributed to Haegeman 1992)

   a. da-n-k   ik komen
      that-1SG-I I   come-1SG

   b. da-∅-j   gie komt
      that-2SG-you you come-2SG

   c. da-∅-se   sij komt
      that-3SG-she she come-3SG

   d. da-∅-me   wunder komen
      that-1PL-we we   come-1PL

We also see this in questions across a broad range of languages: interrogatives (and at times other clause types) involve movement of inflection (T°) to the complementizer head (and in many Germanic languages, this movement occurs in almost all contexts, yielding V2 word order).

A much more detailed account of the relationship between C and T is available in Fortuny (2008, Ch. 2). For our purposes here, it is sufficient to point out this close connection. A prominent way of handling this in recent approaches to Minimalist syntax is to assume that the relevant features for triggering various kinds of operations in the T domain in fact originate on C: "[p]hase heads (e.g. C and v) are the locus of important features driving derivations, and non-phase heads (e.g. T and V) are necessary for their operation" (Ramchand and Svenonius, 2014, 153). On this feature inheritance model, as outlined by Chomsky (2007, 2013), this approach necessitates some degree of countercyclicity.[12] On this approach, all T heads are defective, in the sense of lacking the features we normally associate with T. The relevant tense/agreement features were instead considered properties of C, which is merged atop T, and then T inherits the relevant features from C, at which point relevant syntactic operations like agreement and movement of subjects to Spec,TP proceeds. Many of the details of the proposed operations were designed to solve theory-internal concerns about feature valuation and spellout; the interested reader can consult Chomsky (2007, 2013), Richards (2007), Epstein et al. (2015), Ouali (2008), Obata (2010), Obata and Epstein (2011), Miyagawa (2010), Miyagawa (2017) for details and for expanded applications of the model.

What we see, then, is an apparent interdependence between components of the C-T complex in adult syntactic operations. We don't intend to focus on the details of the feature inheritance account too closely, as from what we can see, it is not nearly as widely adopted as the other kinds of Minimalist assumptions that we adopt in this work. Instead, we use it to illustrate the core observation, that operations *within* a phase tend to be model-able as simultaneous and/or interrelated, whereas operations across phases tend to be much more restricted (how restricted depends on the model of spell-out that is adopted, i.e. PICI or PICII: details below).

So what are the developmental facts about acquisition of this domain? What is interesting is that in some way they appear to match the expected Asp < T < C hierarchy. But in other ways,

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[12]Chomsky (2019) later deemed this foray into countercyclic operations a mistake.

	Dynamic	**Durative**	**Telic**	**Examples**
State	-	+	-	*be asleep, believe, trust, love*
Activity	+	+	-	*sleep, run, sing*
Accomplishment	+	+	+	*run a mile, make a chair, write a book*
Achievement	+	-	+	*wake up, reach the top, recognize, die*

Table 4.1: Aspectual features of classes of events: table from (van Hout, 2016, 589)

we see a lot of overlap in acquisition of tense and aspect. The next section gives an extremely abbreviated summary of concepts about tense and aspect, followed by a discussion of how children acquire tense/aspect.

7.1   Relevant background on tense/aspect

van Hout (2016) offers a succinct description of the temporal properties of utterances:

> Lexical aspect characterizes the temporal profile of event descriptions; a situation with a sleeping child can be referred to as a state of affairs (be asleep) or as a happening (sleep, wake up). Grammatical aspect imposes a perspective by focusing a particular time slice of a situation, such as the ongoing process (the baby was sleeping, mom was waking up the baby), the event as a whole (the baby slept, mom woke up the baby), or the resulting state (the baby has slept, mom has woken the baby up). Tense locates a situation at a certain time (was sleeping, is sleeping, will sleep). (van Hout, 2016, 587)

There is a long history of work in lexical semantics distinguishing the temporal properties of predicates (Vendler 1957, Smith 1991, Dowty (1979, 1991), among a host of subsequent work). As van Hout (2016) notes, Smith (1991) identifies these as related to dynamism (temporal activity/change), durativity (whether an event is instantaneous or sustained over time), and telicity (containing a culmination or natural end point). We therefore fine classes of predicates behaving in predictable ways with respect to syntax and lexical semantics, based on these kinds of temporal properties, as Table 4.1 shows.

What we find, then, is that predicates have natural tendencies to be interpreted with particular aspectual properties. Grammatical aspect, of course, allows us to coerce different interpretations: so a telic achievement (like *wake up*) can readily be placed into an aspectual form to produce an imperfective interpretation (e.g. *I am waking up*). Furthermore, additional properties of predicates (e.g. their transitivity, and the specificity/quantization of their objects) plays a role in the telicity of the predicate. So while these classes are certainly identifiable, there are many layers of complexity.

7.2   Acquisition of Tense/Aspect

A prominent theory of tense/aspect acquisition is what is called the "Aspect First" hypothesis.

> Without exception, all studies find a remarkably consistent trend in the first stages of tense–aspect marking: Perfective aspect or past tense appears mostly on telic verbs, whereas imperfective aspect or present tense appears mostly on atelic verbs. Children reserve particular tenses or aspects for verbs of certain aspectual classes, undergeneralizing tense–aspect forms according to the lexical–aspectual feature of telicity. And so most verbs appear in just one tense or one aspect. This phenomenon of skewed production is referred to as the Aspect-First pattern, because the first markings seem to reflect (lexical) aspect. (van Hout, 2016, 596)

The observation here is that inherent temporal properties of predicates—(a)telicity, i.e. lexical aspect—are the primary determinant in what tense/aspect forms get marked in early child utterances across languages. For details, see (among others) Shirai and Andersen (1995), Andersen and Yasuhiro (1996), and Li and Shirai (2000).

What emerges here is a pattern, cross-linguistically, of "children underus[ing] their tense–aspect forms in very specific, non-target-like ways" van Hout (2016, 596). In general, children tend to us tense/aspect markings mainly to reflect the (a)telicity of the predicate, nothing close to the full range of tense/aspect readings that target-like tense/aspect forms encode. To give specific examples, Brown (1973) notes that while initially using bare verb forms, English-learning children typically acquire the *-ing* progressive morpheme quite early (the first in the children Brown studied). These forms, from early on, tend to be used on a restricted set of verbs, namely, verbs that tend to be classified as activity verbs (*play, ride, write*) (Li and Shirai, 2000, 58). Likewise, past tense forms tended to only be used on a small set of verbs (that were punctual and telic), e.g. *fell, dropped, slipped, crashed*, and *broke*: non-durative, completed events (Li and Shirai 2000, 58, Bloom et al. 1980). van Hout (2016, 596ff) overviews a large range of cross-linguistic examples that replicate this kind of pattern; the details differ as to which tense/aspect forms are utilized in each language, but the aspect-first pattern remains of some tense/aspect form initially being employed based on these core lexical properties of predicates.

A broad range of approaches have been proposed to explain this tendency.[13]

(151)   Theories of Aspect-First pattern (van Hout, 2016, 597ff)

   a. Immature cognitive development: Young children initially lack the cognitive category of time, therefore they cannot map tense inflection on the target temporal category (Bronckart and Sinclair, 1973; Antinucci and Miller, 1976)

   b. A predisposition/learning strategy: Children map the verb inflection system onto certain basic, aspectual notions, thus using aspect to learn tense (Bloom et al., 1980)

   c. Incomplete grammar: Children's very initial grammars do not yet have a proper tense category (Weist et al., 1984)

   d. Prototypes: The learner creates the best exemplars of the prototypical tense–aspect associations: [progressive, dynamic, atelic] and [past tense, result, telic]. (Shirai and Andersen, 1995; Andersen and Yasuhiro, 1996; Li and Shirai, 2000)

   e. Semantic Cost: children initially avoid aspect coercion (i.e. placing a lexical predicate of one aspect into a distinct grammatical aspect), either because it is semantically-

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[13]The list in (151) is largely quoted directly from van Hout (2016, 597), but the last entry is quoted from van Hout (2016, 599).
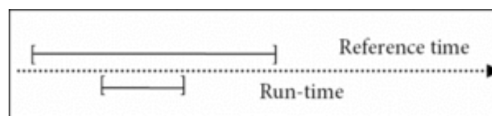
Figure 4.1: Visualization of perfective (van Hout, 2016, Fig 25.1). Screenshot in manuscript will be redrawn in final version.

pragmatically complex or because it involves costly processing resources (Slabakova 2002; van Hout 2007b,a see also Ramchand 2018).

As pointed out by van Hout (2016), many of the theories above assume an initial incorrect generalization on the part of children (e.g. using past tense to mark telicity, later corrected to be past tense). Alternatively, it has been proposed that children may simply avoid aspectual conflicts between grammatical aspect and lexical aspect (as that requires additional semantic processing), instead actively utilizing forms for perfective/imperfective that both match adult usage and the lexical properties of predicates, only later moving to full adult usages that allow for mismatches in grammatical and lexical aspect (van Hout and Veenstra, 2010).

Without making specific claims about intermediate stages of children's knowledge in this respect, what we want to simply point out is that this overlapping of tense and aspect acquisition is in fact expected from a DMS perspective that incorporates phases as workspaces. As suggested by van Hout (2016, 609), it may be that the temporal properties of predicates "can direct the learner in gathering more aspectual detail from prefixes and verb particles." Aspectual properties of predicates can help a child bootstrap their way into anchoring categories, but acquisition of tense/aspect morphological forms may be more mixed (structurally speaking). This appears in that children at early stages of anchoring acquisition tend to mark atelic predicates with progressive aspect (*-ing*), the structurally lowest tense/aspect form in English, but mark telic predicates with past tense (*-ed*), the structurally highest tense/aspect form in English.

One aspectual distinction that appears to adhere to a strict hierarchy is that (crosslinguistically) perfective aspect is structurally higher than perfective aspect. We discussed work arguing for these being in different phases (with progressive occurring in the lowest phase) in §6 above, and similar outcomes appear in a range of languages (for example, see Coon and Preminger 2017 on split ergativity varying along the perfective/progressive split, correlating with domain distinctions). As suggested by both Ramchand (2018) and Wiltschko (2014), a possible source of this distinction arises from the perspectival difference between imperfective and perfective. The imperfect point of view is one in which an event is atelic (ongoing), which is necessarily a point of view internal to the event. In contrast, telicity and perfective aspect (completed events) is necessarily a point of view from *outside* of an event, which requires perspective-taking on the event from some reference point outside the event. These distinctions are illustrated in Figures 4.1 and 4.2. It is not implausible, then, to consider the imperfective aspect a component of the event, whereas perfective aspect is not, instead being a component of the anchoring context. This naturally explains why we find progressive aspect occurring in the lowest phase, but perfective aspect in the anchoring phase (Ramchand, 2018; Harwood, 2015).

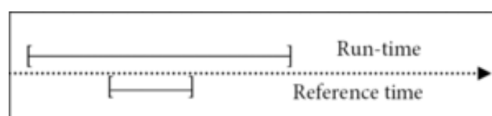What appears to be occurring in acquisition, then, is a gradual transition utterances that

Figure 4.2: Visualization of imperfective (van Hout, 2016, Fig 25.2). Screenshot in manuscript will be redrawn in final version.

contain a temporally underspecified event, to an event that is anchored in time and space. Notably, even before acquiring tense/aspect morphology children are sensitive to temporal properties of events. This is evident from the acquisition timelines discussed above in acquiring the morphology, but as Torrence and Hyams (2004) showed, children even use root infinitives in tense-like ways, tending to use telic verbs in past-time contexts and atelic verbs in present-time contexts (see also Hyams 2007). Notably, however, acquisition here doesn't proceed in a rigid sequence as might be expected on a simplistic interpretation of DMS. Instead, there is overlapping and simultaneous acquisition of some elements.

Notably, however, this overlapping occurs in particular syntactic domains, and only locally domains. Our current line of thinking is that the apparent simultaneity of operations within a phase (alternatively phrased as the apparent interdependence of structures and operations within a phase) may well be a consequence of the fact that the phase is an acquisition workspace, and that children are working on decoding and grammaticalizing multiple structures within that phase at the same time. And while one phase (the *v*P, thematic) is being finalized, the next phase (CP, anchoring) may begin to be under investigation. But by the time the extended projection of the CP is under investigation (linking), the *v*P is wholly grammaticalized. If this is in fact the developmental sequence (as does seem to be the case, empirically-speaking) this is most consistent with Chomsky's (2001) PIC2 as was given above in (118): 'spellout' (i.e. the finalization of a phase) does not occur until a second subsequent phase is underway.

At present, we consider it an empirical question whether each and every instance of structural hierarchy is subject to DMS timeline-based predictions. That is to say, it's not clear whether the bottom-up structure-building must correlate with bottom-up acquisition for every single functional projection in a language, or whether it largely holds only between the functional domains of Wiltschko (2014) and Ritter and Wiltschko (2014) (i.e. phases) and not within those domains (i.e. not within phases), or whether it also holds at each new instance of Merge within phases. Detailed investigations of the grammar of any particular language should bear that out. For the most part, we assume that the only universal derivation-acquisition timeline associations are between acquisition workspaces (i.e. functional domains, phases) and not for each and every functional projection. That is to say, we assume that it is possible that English past tense is grammaticalized by a child as T° before the perfective auxiliary *have* is grammaticalized as Perf°, despite T° appearing structurally higher than Perf°. This is the idea of the acquisition workspace: until it is left behind for another one, nothing is considered 'settled,' and the child is still entertaining alternative analyses for their conclusions. Given the broad existence of anti-locality constraints in adult syntax, it's reasonable to think that we won't see counter-cyclic ordering paradoxes in movement or other properties within phases due to these hyper-fine-grained reanalysis processes within phases.

PAW seems to us to be the much more cognitively plausible account, otherwise we are left attempting to explain what (cognitively) drives acquisition of specific functional heads in a specific sequence, sequences which seem to us to be inconsistent with how child cognition develops (there is much more variation in child development, linguistic or otherwise, than this would seem to predict). Likewise, per all the argumentation in Wiltschko (2014), it is also the most empirically plausible in the sense that there are not universal grammatical categories, and the core functional hierarchy of the clause (while showing persistent similarities across languages) is not universal in its finest-grained details. But it does raise a question: what determines the final structural height of functional projections *within* a phase, then, if it is not directly linked to acquisition? We assume that, for the most part, this is either determined semantically by logical precedence relations (Ramchand and Svenonius 2014), or is simply empirically accessible to the child, for example in the word order or morpheme order of the language(s) they are acquiring.

Our comments in this section are certainly less detailed and precise then they are in some other sections. The point, however, is that the approach to phases as acquisition workspaces fundamentally linked with the different referential tasks that children are taking up in development shows a lot of promise not only to explain distinct acquisition stages, but also to potentially explain the "messiness," the lack of obvious stages at some points. Instead, if the stages correlate to a few core shifts in acquisition "attention" (i.e. event structure, to anchoring, to linking) we would expect to see distinctions between major stages, some overlap between stages, and a lot of overlapping and simultaneous acquisition within stages. We would likewise expect this to be reflected in adult grammars, which we suggested here has been the root of proposals like Feature Inheritance, and phase-based Agree/Movement operations.

8   Conclusions: Phases as Acquisition Workspaces

In this chapter we have claimed that phases, as modeled in the Minimalist Program, are themselves also ontogenetic fossils. Specifically, we proposed that a possible extension of DMS is to conceive of phases as the by-product of acquisition proceeding in stages. Those stages, we proposed, can be delimited by the referential task that a child is taking up at each stage.

Importantly, these stages of acquisition overlap, and the earlier stages of acquisition only fully crystallize (grammaticalize) once additional subsequent stages are underway. This is precisely what is predicted under the Phase Impenetrability Condition (specifically, PICII).

There are many potential extensions of this line of thinking. First, by proposing distinct referential tasks that children are tackling at different stages of development (i.e. the lexical semantics of verbs, i.e. the structure of events, then anchoring that event in context, then anchoring the resulting sentence in the discourse context) we in effect are saying that children are only building hypotheses about particular parts of syntax at each of those stages. This restriction of the hypothesis space may potentially provide avenues to reducing the computational burden the child faces at any given point, as they would be solving only a portion of the language puzzle at each different stage.

Beyond that, we would perhaps expect some interplay between cognitive development at each stage, and the correlating syntactic structures that have developed, as (presumably) the

development of those syntactic structures would provide representational mechanisms for those cognitive developments. This is precisely the argument of Rakhlin and Progovac (2020):

> We … argue that, alongside experiential learning, which grows out of the basic inborn competence about mechanical properties of objects and intentionality of agents, gaining hierarchical syntax allows children to hone their causal inference and psychological reasoning skills leading to important cognitive advances that have been observed in children of preschool age. We … propose that 1) adding the transitivity layer serves as a basis for expanding children's intuitive understanding of causality, extending it from a purely mechanistic understanding requiring spatial and temporal contiguity between two objects to infer a causal relation to being able to conceive of non-obvious, discontinuous causes; 2) acquisition of non-agentive transitives serves as a representational foundation for understanding divergent desires; 3) adding the finiteness layer allows the emergence of temporal displacement; 4) acquisition of the CP layer bootstraps the attribution of false belief (as has been argued before), and provides a representation of counterfactual reasoning more generally. (Rakhlin and Progovac, 2020, 5)

We don't walk through all of the evidence that they cover, but Rakhlin and Progovac (2020) show clearly that 1) acquisition proceeds bottom-up in the general way that DMS suggests, and that 2) there are systematic corresponences between the acquisition of particular syntactic structures and the development of correlating linguistic and non-linguistic cognitive abilities. We do not mean to imply an overly-simplistic set of explanations for these facts. Our main purpose, rather, is to simply show that there are consistent correspondences between Minimalist derivations of adult grammatical constructions and other aspects of cognition. Our attention here is specifically on language development, but Rakhlin and Progovac (2020) argue that there are many non-linguistic correlates as well. We think there is a lot of promise correlating thematic/anchoring/linking domains to other aspects of cognition (as discussed above). This suggests a subsequent hope of connecting abstract theoretical mechanisms like phases (and the grammatical properties they interact with) with aspects of non-linguistic cognition.

Returning to the conception of phases as acquisition workspaces, a reasonable question at this point is whether the resulting hypothesis allows too much flexibility to be useful in the end. The simplest version of correlating bottom-up structure building and acquisition is very appealing in its simplicity: each level of structural hierarchy correlates with a related stage of language acquisition. In addition to being appealing, it is of course *wrong*. Children clearly don't add morphemes to their grammar in a strict one-by-one basis, and they often are adding acquiring various layers of knowledge at the same time. So does this doom DMS from the outset?

We obviously don't think so, in large part because despite the sometimes simultaneous acquisition of distinct morphosyntactic patterns, and despite the large degrees of variation in specific language patterns that children acquire (by hypothesis, largely driven by differences in input), there are nonetheless persistent kinds of bottom-up acquisition patterns, across languages, that simply appear to us to be way too common to be a coincidence. DMS deals with these paradoxes in two main ways. One, some acquisition processes will be *truly counter-cyclic*, meaning a pattern is acquired well before its surrounding grammatical context is acquired (e.g. English wh-phrases), or vice versa, where the grammatical context is acquired but the specific construction

in question lags behind (e.g. relative clauses). These issues are central in this volume, taken up in §2 and §3, among others.

But there are other kinds of challenges that don't clearly fall into those categories, as we've discussed here. For example, the simultaneous (and we might even say 'messy') acquisition of different kinds of tense and aspect within a single language could easily be viewed as a a repudiation of the DMS approach. But as we've discussed above, adult grammars in fact seem to reflect these facts of acquisition. What counts as a "phase" in adult grammar is in fact highly variable, based on context. And sequences of operations within a phase are well-known to be entangled, such that some have even proposed them to be simultaneous. If that's the case, though, why do we still find the kinds of strict hierarchy of functional heads that we do find? for example, in English, Progressive aspect is structurally lower than perfective aspect, which is structurally lower than modals/tense (e.g. *Alex might have been eating the tacos.*). Presumably, children still use the available grammaticalization mechanisms (namely, Merge) to grammaticalize, so adult structures are still hierarchical in the expected way. But the key here is that we would expect the details of the hierarchies that emerge (e.g. which heads bear inflection, word order, etc) to simply fall out from children's analysis of the data itself, rather than from anything more universal about language or cognition. And this predicts two main things: first, there should be a fair degree of cross-linguistic variation in the details of structures within phases/domains, much more than there is in the relative ordering of phases/domains themselves. This is precisely the argument of Wiltschko (2014), Ramchand (2018), and Ramchand and Svenonius (2014), that this is an accurate description of the state of adult grammars universally. Second, this predicts that, despite much apparent messiness, there should be clearly distinguishable (if not distinct) stages in acquisition, where children are first taking up thematic tasks, then anchoring tasks, then linking tasks. Again: this seems to be true. Despite the large amount of messiness within phases, and despite the fact that there is some overlap, children reach adult-like knowledge of core thematic structures of events long before they reach adult-like knowledge of tense/aspect constructions, and that also far outpaces their ability to use the detailed discourse/pragmatically-linked structures associated with *linking*. This of course does not imply that children won't utter grammatical forms with morphology referencing anchoring/linking at earlier stages, but rather that they don't take up the referential task of grammaticalizing those functions until later.

In the end, between countercyclic processes and acquisition-by-phase, we believe that a DMS-style account is not only viable, but directly captures quite a broad range of acquisition facts, and offers promise of an explanation for the existence of phases that has been elusive otherwise.

Chapter 5

Conclusions

This section offers our conclusions, both in the form of a summary of our main claims (§1), as well as discussion of some major theoretical implications in §2-§4. We include some additional speculative extensions (§5) that we find intriguing, but which either take us too far beyond this core discussion, or which fall in areas where we lack sufficient expertise. We conclude the paper with the short (but quite significant) point that this is not a holistic theory of acquisition of syntax, let alone of language acquisition more generally. Most of the properties of language have been untouched by our discussion here, despite the expansive nature of the predictions.

1 Summary of Developmental Minimalist Syntax

The proposal as developed here grew out of an attempt to find connections between Minimalist syntactic theory and other aspects of language and/or cognition: extra-syntactic correlations. The proposed interpretive principle attempts to do that. What emerges, of course, is a modernized version of theories of acquisition of syntax (though that was not the main goal originally). Again, recall from (4) above the interpretive principle defining DMS, repeated here as (152).

(152) **Developmental Minimalist Syntax (an interpretive principle)**
      The Minimalist derivation of adult language structures recapitulates the development of those same syntactic structures in ontogeny.

      This kind of approach leads to a specific conceptualization of Minimalist proposals about Universal Grammar (UG) as largely being proposals about the nature of the Grammaticalization Mechanisms that are employed on language. Put another way—in a slightly weaker claim— accurate Minimalist proposals about UG are in fact detailed articulations of the specific structures that result from grammaticalization. A number of conclusions fall out from this, which we outlined in this work as 9 principles of DMS.

(153) DMS Principle #1:
      The theory of Universal Grammar (composed of at least Merge and Agree) is effectively a description of the nature of grammaticalization in language acquisition.

(154)   DMS Principle #2:
        In acquisition, new syntactic structures typically incorporate existing structures (with
        some principled exceptions).

(155)   DMS Principle #3:
        Sequence of structure building in the Minimalist derivation of a sentence correlates with
        the timeline of acquisition.

(156)   DMS Principle #4:
        Syntactic movement is reanalysis.

Essentially, the principles above articulate the proposal and consequences of Minimalist syntactic operations being articulations of the precise way in which grammatical representations are formed. As we summarized in Chapter 2, there is a wide range of work showing that the empirical predictions of these principles are broadly attested: hierarchically lower structures are acquired before higher structures.

Many of the particular details, while important, are aside from the main claim here, which is to claim that as a result of this correlation, the entire derivation of adult grammatical structures recapitulates their acquisition pathways; adult grammatical knowledge of syntax encodes the pathways by which those structures were acquired. These conclusions may not necessarily lead to the conclusions below, but on the strictest interpretation of the Minimalist program (and, according with the proposals of both usage-based grammarians and typologists), we find these conclusions:

(157)   DMS Principle #5:
        Syntactic categories are emergent (Wiltschko, 2014)

(158)   DMS Principle #6:
        Parameters are emergent (Roberts 2019, among others)

As we noted previously, (157) and (158) are not new claims, even within the Minimalist framework. But they are the most naturally compatible approaches to categories and parameters for a DMS-style approach to Language. While there are certainly persistent irreconcileable differences, these principles as stated above do allow for a synthesis of the Minimalist Program with various aspects of Construction Grammar approaches to language (specifically, Tomasello 2003).

To our knowledge, the complete suite of claims above have not been proposed in the configuration articulated here, or with the degree of predictive force that (4)/(152) carries; that said, for the most part the preceding principles/claims have direct and/or indirect precedents in the literature. The most novel proposals we have to offer follow directly from the discussion above, but we think have the potential to clarify a number of outstanding issues both within syntactic theory itself, but also in theories of syntax-acquisition correspondences (and, likewise, theories of acquisition of syntax). If the canonical result of DMS as articulated in (4) is that syntax is acquired in a bottom-up fashion, counter-cyclic processes are the exceptions that prove the rule. It is well-established that nothing like rigid stepwise acquisition occurs among actual children learning actual natural languages, as we might envision on a brutalistic application of Minimalist

derivations to acquisition. Rather, acquisition is fluid and gradual, with some clear exceptions to the bottom-up principles. That said, it is precisely our claim that these clear exceptions should find corresponding clear exceptions in adult grammatical derivations: that these would make up the class of counter-cyclic operations that have been discovered in adult grammars.

(159)   DMS Principle #7:
        Counter-cyclic phenomena in adult language grammars correlate to counter-cyclic acquisition processes.

(160)   DMS Principle #8:
        Counter-cyclic syntactic operations exist.

It would be shortsighted to claim that we have demonstrated this to be true in this single piece of work: the predictions here are expansive. That said, we have shown that for a couple major classes of counter-cyclic operations, for well-known domains where they apply, that the predictions seem to be upheld. We discussed this for late merger of relative clauses in English, for look-ahead in wh-movement, and for counter-cyclic object marking constructions in Zulu. These case studies are only that—case studies—and much more work will be necessary if the ideas proposed here are to be widely accepted as explanatory for counter-cyclicity more generally. Nonetheless, we find the results promising, and perhaps a way to make sense of the problem that acquisition does, clearly, seem to be bottom-up from lexical categories to higher functional categories, but at the same time, clearly is not *only* in that fashion. The approach here assumes both are possible, but that whatever does happen in acquisition is encoded in adult grammars. Chapter 3 discussed the ways in which the acquisition data are consistent with this approach.

Finally, the last principle that we proposed has to do with phases.

(161)   DMS Principle #9:
        Phases are acquisition workspaces.

While the evidence is less direct for this proposal than for our others, we nonetheless devoted a fair degree of discussion to this idea, as it has potential not just to derive some puzzling properties of phases in adult grammars (variable sizes of phases, simultaneity of operations within phases) but it also has promise to provide some clarity to the enduring metaphor that phases promote "computational efficiency," despite our model being one of competence, not performance. We have suggested that many of these metaphors are in fact fairly accurate, but the efficiency that our model is achieving is in fact acquisition-related computation, and not online processing in adults.

These don't all get equal treatment in this volume: specifically, we don't discuss (157) and (158) much at all, but they are reasonable (and perhaps necessary) consequences of this approach (and, both already have been argued for on empirical and theoretical grounds).

2   Reflections on the SMT

Recall from our introduction some of the critiques of counter-cyclic analyses: quite recently both Sportiche (2019) and Chomsky (2019) offered fairly direct critiques of Late Merger (and other counter-cyclic operations). The critiques against Late Merger leveled by Chomsky (2019) and Sportiche (2019) are entirely reasonable, based on Chomsky's Strong Minimalist Thesis (SMT): allowing counter-cyclic operations *does* expand the the generative capacity of UG, and the model resulting from DMS is not the simplest possible model of syntax as could be deduced based on the interface conditions of language (i.e. the need for syntax to interface with both sensory-motor articulation and interpretation). On this account, however, we recall Chomsky's (2001) comments on this issue:

> The strongest minimalist thesis SMT would hold that language is an optimal solution to such conditions. The SMT, or a weaker version, becomes an empirical thesis insofar as we are able to determine interface conditions and to clarify notions of "good design." While the SMT cannot be seriously entertained, there is by now reason to believe that in nontrivial respects some such thesis holds, a surprising conclusion insofar as it is true, with broad implications for the study of language, and well beyond.

> Tenable or not, the SMT sets an appropriate standard for true explanation: anything that falls short is to that extent descriptive, introducing mechanisms that would not be found in a "more perfect" system satisfying only legibility conditions. If empirical evidence requires mechanisms that are "imperfections," they call for some independent account: perhaps path-dependent evolutionary history, properties of the brain, or some other source. It is worthwhile to keep this standard of explanation in mind whether or not some version of a minimalist thesis turns out to be valid. (Chomsky, 2001, 2)

It's important to note that the SMT is a theoretical heuristic, a philosophical stance, and not (as Chomsky points out) a necessary truth. And we agree: given the depth of achievements of Minimalist syntax in providing a framework to discover, describe, and analyze grammatical constructions across the world's languages, there is good reason to think that the framework developed (driven by the SMT) is a reasonable result. But what we have claimed in this work is that there is in fact a strong and reasonable independent account (as Chomsky insists upon) of the diversions from the SMT-inspired UG mechanisms of Merge and Agree (and perhaps phases) alone. These may well be "imperfections" from the perspective of an optimal mapping between the PF/SM and LF/CI interfaces, but by now it should be more than clear (on moral, biological, social, and cognitive bases) that humans are composed of little that is perfect. So we find it unsurprising that the SMT as articulated as a theoretical heuristic may not hold in its strictest sense in actual human cognition.

Now, we are not equipped to say whether these "imperfections" arose from the evolution of language or instead are simply a necessity of the maturational processes involved in cognitive development (we're inclined to say the latter, though without any real evidence). But wherever they are found, the strong developmental correlates of countercyclic processes in adult grammars are sufficient, in our minds, to consider the viability of DMS on a much broader scale.

3 On the nature of explanation in syntactic analysis

For the most part, in the field of generative syntax, what is considered relevant evidence is a set of grammatical and ungrammatical sentences, and the analytical mechanisms that the theoretical framework provides make up the explanatory mechanisms, i.e. what can be used to explain the pattern of syntactic judgments that has been observed. What mechanisms exist in the theory is of course a moving target, but there is certainly relative stability in a number of major theoretical constructs that Minimalist syntacticians have been using over the last 20 years. But what does it mean to explain a grammatical pattern with a feature set, an Agree relation, a movement operation, etc? Essentially, these are instances of attempting to explain a particular language pattern by recognizing it as an instance of a much more general (albeit abstract) pattern, that is represented by the theory, a theory that has developed to this point to explain a range of unrelated language patterns. This is inherently circular to some degree: not in a problematic way (as we are building systematic descriptions of the properties of natural language grammars) but to truly establish our findings within cognitive science, we need to translate these properties of grammar to other aspects of human cognition and to demonstrate the relevance of these findings to other researchers on cognition.

A large number of researchers are already working hard at this. There is a broad range of work on the correlations of generative syntactic structures with various kinds of processing effects in the psycholinguistics literature, there is a host of interesting work on semantic acquisition, and generative approaches to acquisition of morphology and syntax have a long, long history. And while these are productive and energetic fields, to our knowledge they have not fed direct theoretical conclusions back into the field of generative syntax (for the most part). On the other hand, by proposing systematic correlations between the derivational structures of adult grammars and the sequences of acquisition, DMS explicitly roots the nature of adult grammars in the psycholinguistic processes of child language acquisition. In this way, Developmental Minimalist Syntax opens the door to kinds of explanatory mechanisms that are not available in Traditional Minimalist Syntax, which restricts the data set to adult grammars for the most part. To some degree this should be quite frightening to the Minimalist syntactician: as if the range of grammatical constructions in the world's (roughly) 8,000 languages was not a large enough data set (not even considering the infinite range of dialectal and idiolectal variation that emerges), we also now include the highly individually and cross-linguistically variable acquisition pathways that children take to reaching those adult language grammars. The data set we are responsible for seems impossibly huge. On the other hand, by opening the door for explanatory links and direct empirical predictions outside of adult grammars, we may in fact grow confidence and broaden the influence of generative syntactic work, as we can show its relevance to empirical properties outside of acceptability judgments.

The DMS account as developed here offers additional kinds of explanatory mechanisms in syntactic analysis that have either not previously been used (to our knowledge), or which are not broadly used in the field yet. One comes from the work of Hinzen and colleagues, as noted in §4: if phase heads are inherently referential, and movement to the phase edge results in more specific reference, the semantics of specificity of reference ought to play a more direct role in

syntactic explanation.[1] Again, this complicates syntactic analysis in ways, but if Hinzen and Sheehan (2013) and Sheehan and Hinzen (2011) are on the right track, we may in fact find more general and more precise explanations than we are left with otherwise.

Likewise, we have proposed the existence of *input-driven movement*, syntactic movements that are simply grammaticalizations of a surface property of language (the position of a constituent) and not (necessarily) any deeper motivation (we have suggested that look-ahead movements and head movement of verbs are two instances of this). On the approach DMS takes, syntax is composed of a computational system but the computations are not performed in the absence of a target outcome, but explicitly in order to achieve a target outcome. So the computational system is not in fact blind, but in fact is a tool to grammaticalize patterns in the input. In this sense, then, referencing the outcome in the course of a computation is not just reasonable, it is to be expected. Some of these movements may eventually become part of a larger, more abstract generalization that a child forms (i.e. a feature-driven operation), but others may remain little more than the observation that a particular constituent moves to a particular position (we suggested that many of these may currently exist in analyses as EPP-driven movements).

To be clear, in most ways DMS does not change the everyday operating framework for the "ordinary working grammarian" (to borrow the title of Chris Collins' blog): we quite intentionally do not want to do so, we are not attempting to propose re-analyses of any portion of the existing body of syntactic work, although DMS may itself suggest reanalysis of particular cases. But it should loosen some of the strongest constraints on that ordinary working grammarian. Counter-cyclic analyses ought not be taboo, but they do come with predictions: operations that apply at 'earlier' or 'later' stages in a derivation than their structural height implies should necessarily correlate with 'earlier' or 'later' acquisition. And again, this ought not obligate a syntactician of adult grammars to also investigate acquisition (an unreasonable methodological requirement). But it does help grammarians of adult languages to more directly articulate the predictions of their analyses for child language.

And, alternatively, if DMS turns out to be on the right track, it also opens avenues for generative and non-generative acquisition researchers alike to make direct predictions for adult grammars. Constructions that are acquired early ought to be derivationally early in adult syntax, and constructions that are acquired late ought to be derivationally late in adult syntax.

While we expect some large amount of the properties of syntax will still fall out from the core grammaticalization mechanisms (i.e. 'UG,' Merge, Agree, phases, etc) this does open the door for aspects of development to serve as explanations for adult language grammatical properties. This will likely be wholly welcomed by Minimalist syntacticians, as the entire purpose of the research program is to identify the simplest possible mechanism to explain human language syntax. If a pattern can be explained by development (whether linguistic or non-linguistic), then all the better. But, notably, this does not replace the need for core grammaticalization mechanisms (modeled as 'UG'), which operate on input and provide precise representational structures for the resulting knowledge that children internalize. It allows for some amount of the patterns that we

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[1]To be clear, issues of specificity of reference are engaged widely in syntax on both empirical and analytical levels (see, for example, Lopez 2012 and Diesing 1992 as just two examples. However, to our knowledge the kind of claim in Hinzen and Sheehan (2013) and Sheehan and Hinzen (2011), where specificity of reference is a fundamental property of syntactic grammar itself, is novel to their proposals.

find in adult grammars to be explained by factors external to those mechanisms.

4 Does UG help with Language Acquisition?

In a recent entitled "Child language acquisition: Why universal grammar doesn't help," Ambridge et al. (2014) consider some major UG-based proposals for acquisition strategies, arguing that these UG mechanisms are not in fact necessary for the children to learn the structures under consideration. This is the typical history of nativist vs emergentist approaches to language, where an innate mechanism is proposed to cover an apparent gap in the accessible aspects of language, to which emergentists argue that innate mechanisms are unnecessary. A lot of knowledge has been generated by these debates and subsequent investigations.

As we mentioned above, DMS is not an argument for a language-specific UG: we are quite careful to make no claims in this regard, one way or another, as we don't have anything helpful to contribute on that point. Rather, we claim that what Minimalist syntacticians are modeling when they propose and refine notions of UG is the precise nature of how children form grammatical generalizations about input: acquisition of grammar is binary (merging two structures together), and it is additive (retaining earlier stages of grammatical knowledge), among other things. In this way, proposals about 'Universal Grammar' in the Minimalist generative syntax literature (at least, post-2001) are in fact proposals of how syntactic structures are grammaticalized by children in acquisition.

So in some sense, the question of whether UG "helps" or not may be the wrong one for our current concerns.[2] It is widely accepted that grammaticalization of structures exist: the claim of DMS is that what Minimalist syntacticians have to offer is a precise formulation of the nature of that grammaticalization process (or, at the very least, the representational qualities of the outcomes of grammaticalization). We may find ourselves in a minority of Minimalist syntacticians, as we are indifferent as to whether the 'UG' we are modeling is specific to language or not.[3] But, in our view at least, Minimalist syntacticians model the nature of grammatical (syntactic) structures: nothing in the day-to-day operations of a standard practitioner of Minimalist syntax is dependent on whether the model being used for analysis (or being modified and added to based on empirical/theoretical concerns) is a model of an innate, language-specific UG, or whether it is simply a precise description of the grammaticalization operations that generate syntactic structures, themselves potentially being language-related instantiations of more general cognitive abilities. It is our opinion that the contributions of Minimalist syntacticians can be evaluated separately from that question.

Despite the success of generative models in analyzing adult language, many—though certainly not all—researchers in the domain of children's language acquisition do not adopt Chomsky's MP, instead operating within a usage-based linguistic framework. One of the most prominent and vocal among these researchers is Michael Tomasello, whose work responds rather

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[2]Ambridge et al. 2014 were not incorrect to ask the question, of course, because this is how Universal Grammar has been articulated historically within generative linguistics.

[3]We're actually unsure of the general mood of Minimalist syntacticians on this point; we suspect that many would be indifferent to the issue, as we are.

directly—and rather critically—to generative approaches to language acquisition. According to Chomsky's (1980) poverty of the stimulus argument, children cannot develop an understanding of the principles of grammar of their native language based solely on the speech that they hear around them as children, since this input is deficient in certain respects. More specifically, even though certain constructions are either unattestested or incredibly rare in children's linguistic input, children nevertheless acquire these constructions (see Lidz and Gagliardi 2015 for a more detailed explanation of this line of reasoning). In this way, Chomsky holds that children—or humans more broadly—must have some innate linguistic ability, which he terms Universal Grammar (UG).

However, as Tomasello summarizes, the posited existence of UG raises two central problems: the linking problem and the continuity problem. First, by assuming the existence of an underlying UG possessed by every child speaking every language, researchers must then also address how children know to link their abstract UG to the particulars of the language that they are learning. This mandate—also known as the linking problem—is a logical consequence of the UG proposal and (per Tomasello) had not yet been satisfyingly answered by the generative syntax community at the time (though his perspective on the issue appears to be unchanged: see Ibbotson and Tomasello 2016). In a related vein, in what is known as the continuity assumption, it is usually thought that UG remains the same at all stages of a human's linguistic development. In this way, then, the continuity problem arises: how can linguists understand the changing nature of children's language if UG is always the same? Once again, per Tomasello, generative syntacticians have not yet fully answered this question (though many have addressed it in depth, of course).

As we have discussed, a number of more recent generative approaches propose sufficiently abstract UG mechanisms to allow syntactic patterns to be generated on an emergent basis, rather than proposing an information-rich Universal Grammar (see Wiltschko 2014, Roberts 2019, and Biberauer and Roberts 2017, along with a host of additional related work by Theresa Biberauer and Ian Roberts: consult the references in the cited work). This suggests solutions to the "linking" and "continuity" problems. Linking UG to input is, in effect, a moot point on this approach. "UG" as we've conceived of it is simply the precise formulation of either the mechanism for, or the outcome of, grammaticalizing a syntactic pattern. All of the findings of usage-based linguists about children adhering very closely to input before reaching those final stages of grammaticalization are completely compatible with DMS. As for continuity: in some senses, this is what is often termed a maturational account, or a "Gradual Emergence" theory, to borrow a term from Rakhlin and Progovac (2020): a child's knowledge of syntactic structures of language is not the same as that of adults. But there may in fact be strong continuity with respect to what is actually "UG:" the grammaticalization mechanisms may be wholly consistent from childhood through adulthood.[4]

Many generative researchers in language acquisition have argued for a nuanced view where certain specific patterns/constructions argue for innate knowledge, even if statistical learning plays a heavy factor as well (Lidz and Gagliardi 2015, among others). Given the extraordinary

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[4]It's also possible that they are not: "late" processes like Late Merger may be the kinds of processes that rely on later cognitive developments (whether they are innate or not) and are in fact not available to children at the earliest ages. This would potentially help explain both their late applications, as well as helping to address the potential overgeneralization issues with a syntactic theory that allows such operations (Sportiche, 2019).

breadth and depth of grammatical patterns in human languages, this discussion won't be ending anytime soon. But we suspect that one contribution of this work will be to show the (perhaps surprising) ways that Minimalist approaches to syntax and usage-based approaches to acquisition may in fact share a lot of common ground when Minimalist proposals are interpreted via DMS. Again, we are not proposing a full synthesis of Construction Grammar with Minimalism (and depending on the version of Construction Grammar, there may be more or less compatibility). But the point is that the two research traditions have tended to allow their core theorizing to be driven by different research questions, and DMS can potentially allow us to incorporate insights from both into our understanding of adult grammars, and how we arrive at them.

5   Speculative Extensions

In this section we offer discussion of potential extensions of the DMS approach to areas that we haven't discussed so far. The potential connections need much more investigation to consider their validity, but they seem to be promising enough to us to merit inclusion here, even if in the form of preliminary discussion.

5.1   DMS and the prosody-syntax interface

While a full exploration of the concerns of this section goes far beyond what we are able to tackle at this point, the syntax-phonology intersections implicated by DMS are fairly far-reaching and non-standard. As stated succinctly by Richards (2017, 23), "The current consensus about this relationship in Minimalist circles, as [we] understand it, is that a phonological derivation begins once the syntactic derivation of a spellout domain is completed. The details of the phonological derivation are not often a focus of interest for syntacticians … it is generally assumed that the derivation begins with a syntactic tree and performs a series of operations to convert that tree into a representation that can be used by the phonological interface." DMS, however, suggests that something entirely different is possible in the syntax-phonology interface. Rather than phonology necessarily coming *late* in the logical sequence of operations, by interpreting the UG mechanisms specifically as a Grammar Acquisition Device, phonological forms of morphemes/words/phrases are accessible (to some degree) by children, by necessity: phonological forms of one sort or another make up the input they are receiving. This predicts, however, that relatively robust interactions of (early-accessible) phonological material with aspects of syntactic structure are possible.

There are of, of course, some ways in which phonology *is* necessarily late (derivationally-speaking). Consider tone assignment in Bantu languages, to give just one of a multitude of obvious examples: underlying forms may have tone marked on a few moras/syllables only, but the surface form realizes tone across the entire morphophonology of the sentence. No sequence of the surface linear structure is somehow immune to receiving a tonal designation and pronunciation. So in the Tiriki example in (162), all syllables are underlyingly toneless except for the verb root in the embedded clause (162a); in pronunciation, however, a phonological process of high tone anticipation spreads the H tone in the embedded clause leftward to the first syllable of the main-clause infinitive.

(162) Tiriki High Tone Anticipation (Michael Marlo, pc)

    a. <u>Underlying Representation</u>
       xu[r̥eev-a] muundu xu[xól-a] ʃiindu
       INF[ask-FV] someone INF[do-FV] something

    b. <u>Surface Form</u>
       xú[r̥éév-á] múúndú xú[xól-a] ʃiindu
       INF[ask-FV] someone INF[do-FV] something
       'to ask someone to do something'

Clearly, phonological processes like these are unconstrained by details of syntactic structure, as even an embedded H tone can spread onto matrix clause material. This is of course just one example of an endless number of such purely-surface phonological processes: they illustrate what is largely considered to be the norm, that phonology operates on already-completed syntactic structures.

Despite this reality, Richards (2016, 2017) argues that it is in fact the case that there are syntax-phonology interactions in the course of a syntactic derivation, quite to the contrary of standard assumptions. The details of the puzzles and solutions proposed by Richards go beyond our current concerns, but we will attempt to overview them very briefly here, in order to demonstrate some of the substance behind Richards' reasoning and proposals about these syntax-phonology interactions. Richards develops a theory explaining the cross-linguistic distribution of overt movement (among other things). For example, (163) illustrates that some SVO languages require movement of subjects to preverbal position (English) whereas others do not (Italian).

(163)   a. A man has arrived [English]

       b. È arrivato un uomo [Italian]
          is arrived a   man
          (Richards, 2016, 1)

Likewise, some languages move wh-words to the left edge of a sentence (English), whereas others do not (Japanese):

(164)   a. What did John buy? [English]

       b. John-wa nani-o   kaimasita ka? [Japanese]
          John-TOP what-ACC bought   Q
          (Richards, 2016, 1)

Richards notes the long-standing observation that some languages exhibit the classical EPP (where Spec,TP *must* be filled, even if by an expletive) (165), whereas other languages apparently allow Spec,TP to be empty (166).[5] The sentences in (165) and (166) are all synonymous with the English example.

(165)   a. **<u>There</u>** arrived a man. [English]

       b. **<u>Il</u>** est arrive un homme.[French]

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[5]It is often assumed that the EPP holds for all languages, and languages like (166) must have a null expletive in Spec,TP; Richards' analysis does not require this.

(166) a. Apareció un hombre. [Italian]

  b. É arrivato un uomo. [Spanish]

  c. Va venir un home. [Catalan]

Richards argues that the effects in (163), (165) and (166) can in fact be derived from the metrical requirements of each language, respectively. Setting aside (a great many) details, the languages in (165) are those whose tense affixes in T° place a prosodic requirement to their left, that they must necessarily follow a prosodic boundary, which must be satisfied in those languages by merging something in an adjacent position (i.e. Spec,TP). Languages like those in (166) are largely similar, but the tense affixes' need for a prosodic boundary is satisfied by their verbs: Richards reviews a range of literature showing that Italian, Spanish, and Catalan all contain verb-internal metrical boundaries that precede their tense morphemes, whereas French (and English) lack the same. Therefore, the prosodic requirements of T° are satisfied by the verb in (166), but must be satisfied by a separate phrase in (165), an overt subject (the expletive in these instances). Richards abstracts these requirements into quite general statements (Contiguity Theory) accounting for many kinds of movement (e.g. wh-movement and head movement), but the details go far afield from our current concerns.

Richards (2017) removes some of the more stipulative aspects of the proposals in Richards (2016), instead suggesting that his proposals can be maintained without those stipulations, and instead simply adopting modest revisions of Match Theory (and a particular theory of Agree), paired with the assumption that prosodic structures are constructed alongside syntactic ones, i.e. along with the cyclic structure building of Merge. "In a sense, the proposal of Contiguity Theory is a very modest one; this kind of phonological operation, which applies to a syntactic tree, can apply, not after the syntactic derivation is completed, but while it is still under way. In fact, if there are any operations that the phonological derivation can perform before the syntactic derivation is complete, perhaps it makes sense for them to be performed as early as possible, if the goal is for the derivation to produce linguistic objects as quickly and efficiently as it can" (Richards, 2017, 24).

As noted by Ott (2017) in his review of Richards 2016,

perhaps the most interesting challenge posed by [Richards'] proposals concerns the place of morphophonology in the overall organization of the grammar. In his model, at least some syntactic operations apply in the service of constructing prosodic structure in tandem with the syntactic derivation. Phonological information such as the presence of metrical boundaries is directly accessed by the syntactic computation; the phonology does not merely impose output conditions on completed derivations" (Ott, 2017, 722). Yet, at the same time, "if [Richards] is right, the phonology is more than an ancillary mapping relating the internal computational system to articulation and perception: it is an 'active player' in syntactic computation (*contra* long-standing claims by Chomsky). (Ott, 2017, 723)

This might be somewhat irrelevant if it were not for the promise that Richards (2016, 2017) offers: "What weighs far more than any of the questions raised by his proposals is the fact that his book represents the first serious effort to rationalize a fundamental property of natural language in a way that goes beyond a mere restatement of surface observations in technical terms" (Ott,

2017, 722). For the most part, the Minimalist program offers little in terms of underlying explanations for the kinds of movements that are possible (or not) across languages, but Richards provides systematic arguments linking these with aspects of the prosodic systems in different languages. Yet, Contiguity Theory requires such a decidedly non-standard theory of the phonology-syntax interface that it poses a significant challenge to incorporate these observations into our current models.

Of course, this kind of outcome is precisely what is predicted by DMS: we expect phonological properties of language (at least, those acquired by children sufficiently early) to be not only be eligible to participate in syntactic operations, but perhaps also *likely* to participate/shape syntactic acquisition, since the syntactic observations children are making would be in the context of the phonology they have acquired (at each respective stage of acquisition).

But the question looms: is there any empirical substance to this? That is, as for the prosodic properties that Richards argues play a role cyclically within the syntactic derivation, are these properties in fact acquired early, in the way that DMS requires? We can't possibly address this in any depth, but the short answer is: yes, it appears that prosodic properties a child's first language are in fact central to quite-early stages of acquisition. For an excellent overview of the relevant facts, we refer the reader to de Carvalho et al. (2018); as de Carvalho et al. (2018) report, infants are sensitive to prosodic properties of language from birth; four-day-old infants have been shown to distinguish their native language from a foreign language (Mehler et al., 1988); Mehler et al. (1988) likewise show that infants use prosodic information (as opposed to segmental phonetic information) to draw such distinctions. By 7-10 months babies are sensitive to the coherence of intonational phrases (Hirsh-Pasek et al., 1987) and multiple studies have shown that children as young as 6 months to be exploiting prosodic boundaries to find word boundaries (Shukla et al. 2011, among others: see de Carvalho et al. 2018, 23). Far beyond simply parsing words, de Carvalho et al. (2018, 24ff) discuss a range of evidence showing that children as young as 18 months use prosodic boundaries to disambiguate syntactic structures, meaning that (at that point) they have already acquired knowledge of some degree of systematic correlation between syntactic structures and prosodic structure (de Carvalho et al., 2017; He and Lidz, 2017; de Carvalho et al., 2015; Christophe et al., 2008). Notably, all of this is happening before children are producing utterances of any syntactic complexity.

Of course, none of this is direct evidence substantiating a link between the prosodic properties proposed by Richards to be relevant in particular languages, and the acquisition of those particular properties in those particular languages, in the timelines predicted by DMS for the syntactic structures of those particular languages. This of course is a substantial research program and nothing we can even attempt to answer. Our point, of course, is that such a link is quite plausible based on what we *do* know about timelines of acquisition of prosody and syntax, and that taken in context of the rest of what we have proposed for DMS here, we find the potential confirmatory evidence from vastly different empirical domains to be tantalizing, albeit still beyond our reach.

As a final comment on Richards' proposals, Ott (2017, 722) offers the following critique:

> Does the book accomplish its declared goal of 'develop[ing] an explanatory theory
> of when movement takes place and when it does not' (5)? In view of the empirical
> and conceptual complexity of the material, answering this question is anything but

straightforward. The theoretical machinery established by [Richards] in the course of the discussion is not always obviously more principled than the features it is designed to replace (but see Richards 2017), not least because the system requires a number of rather awkward assumptions at various points. Irregularity is a case in point: verbs with irregular stress patterns in languages such as Spanish behave exactly like regular verbs with regard to EPP effects; this, [Richards] argues, indicates that the syntax simply does not 'see' the irregularity and treats all verbs as exhibiting regular stress (while being generally sensitive to metrical information). He is forced to adopt the same reasoning for lexical accent on wh-phrases in Basque, a perfectly systematic property of the language that is nevertheless inaccessible to syntax as a lexical idiosyncrasy. Similar assumptions are required for null subjects in pro-drop languages with EPP effects and null tense affixes: to explain how *pro* can provide, and null affixes require, metrical support, [Richards] is forced to assume that the syntax is oblivious to the fact that these elements ultimately remain unpronounced. (Ott, 2017, 722)

An example of syntax being oblivious to final phonological outputs (despite being centrally driven, per Richards, by 'initial' phonological outputs) is colloquial Finnish (Holmberg and Nikanne, 2002), which Richards analyzes along the same lines as English and French, requiring a preverbal phrase to support the prosodic requirements of T°. This is evidenced by the presence of overt expletives as in (167).

(167) **Sitä** leikkii lapsia   kadulla. [Finnish]
       EXPL play   children in.street
       'Children play in the street'

Nonetheless, Finnish displays a limited pro-drop property, with null subjects available in some instances, which might otherwise be unexpected given the analysis of the expletive in (167) emerging because of a prosodic requirement.

(168) Puhu-n   englantia
       speak-1SG English
       'I speak English'
       (Holmberg, 2005, 539)

"We can think of the behavior of pro-drop as another instance of syntax being blind to lexical idiosyncrasy; Finnish does have pronounced pronouns as well as the unpronounced ones that are involved in pro-drop, and syntax ignores this distinction, treating all DPs as pronounced" (Richards, 2016, 22).

Richards (2016, 17) is transparent about these requirements, stating in his introduction: "we will discover a number of differences between the structure that we want the syntax to make reference to and the actual phonological structure of the output. I will attribute these differences to postsyntactic phonological operations, which will in some cases obscure the reasons for the behavior of the syntactic computation. On the approach developed here, these mismatches shed light on the amount of access syntax has to phonological information, and thus ultimately on the architecture of the derivation; crucially, the syntax is not simply 'looking ahead' to the final

phonological representation, but is working from a 'rough draft' of the phonology which will later be revised." This in effect creates a number of opaque interactions, where later (phonological) processes can obscure the prosodic context that required a movement in the first place.

Without additional context, Ott's criticism is a fair one: what theory is there of "rough draft" phonology (addressing prosody/metrical structure) apart from final phonology, especially one in which the rough draft phonology is available to syntax but the final phonology is not? We are surely wearing our bias on our sleeves at this point: DMS provides just such a theory, on principled grounds that have been developed almost entirely separately from the empirical domains that Richards is concerned with. As we noted above, not only do children have access to metrical properties of language at an early acquisition stage, but it is largely thought that these properties of languages are critical bootstrapping devices that allow children to unlock the morphosyntactic properties of their language; as such, we ought not be surprised if the metrical properties of language play into the correct formulation of syntactic generalizations. In contrast, it is obvious that aspects (in production, at least) of final morphophonological forms only approach the target quite late in acquisition.[6] As such, lexically-specific and otherwise idiosyncratic properties of portions of a language ought not affect major syntax-prosody generalizations, on the assumption that the lexical exceptions are among the last patterns acquired.

On a DMS-style approach, we would not only expect children to have access to certain prosodic structures of language early, but we might well expect them to draw syntactic generalizations based on prosody (nothing in DMS as we've articulated it to this point requires this, but these kinds of analyses are readily available and quite reasonable, in our opinion). Richards' (2016; 2017) work on prosody-syntax interactions take us far outside our own expertise, and we are quite unqualified to comment on its viability on its own. But (as Ott suggests) if some of the largest issues standing in its way are 1) the interaction of prosody with core syntax, and 2) the split between syntax-visible phonology (e.g. metrical structure) and syntax-invisible final spell-outs of morphophonological forms (e.g. tone assignment in Tiriki, null pronouns in Finnish, and a host of other phenomena), then a DMS perspective has a lot to offer.

5.2   Late Insertion

Just how far does this "early" vs. "late" correlation go, between adult syntactic derivations and sequences of grammaticalization by children? If DMS is on the right track, it should be quite far. Consider, as a possible extension of DMS, "Late Insertion" approaches to morphology. The Distributed Morphology approach to morphological form argues that the generative processes con-

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[6]This strikes us as reminiscent of of the (U-shaped) pattern that is quite familiar from language acquisition in general: children's output is in fact more target-like *before* grammaticalizing structures/rules (since beforehand they are simply replicating/mimicking item-based patterns directly); after that point they become less target-like (due to overgeneralization of the abstractions they arrive at), after which they gradually settle onto the target as they acquire various lexically idiosyncratic aspects of the languages. (See Cournane 2019, as well as the discussion of such acquisition pathways in Chapter 2.) While we are firmly in the realm of speculation, here, it seems that as far as acquisition of metrical structure and syntactic structure goes, a DMS account of a Contiguity-Theory-style analyses would suppose that grammaticalization of things like movement occurs at a point when children have reached appropriate grammatical generalizations about their languages prosodic structure, but before they have acquired the various lexical exceptions: hence, Richards' "rough draft" metaphor.

tributing to final morphological forms are distributed across the derivation of a sentence (Halle and Marantz, 1993; McGinnis, 2017). On this approach, surface morphological forms are inserted derivationally *late*, based on features that arise from both lexical properties and syntactic derivations.

For example, consider the copula 'be' in English, which inflect in a highly irregular paradigm (*I am, I was, you are, you were*, etc). Such irregular verb forms depend not only on lexical specifications, but also on features like tense, and the φ-features of the grammatical subject. In the Minimalist model, however, tense and φ-features are not valued on the functional heads they are expressed on until after the derivation of (most of) the sentence. Therefore most of the sentence is constructed before the relevant form of the copula 'be' is inserted, presumably after spellout. Like bottom-up structure building, this kind of model is confusing for newcomers to generative morphosyntax who (understandably and naturally) try to draw a connection between the time-course of derivation and some real-world time-course, often production. We of course are modeling competence and not performance, but if so, what do 'late' vs. 'early' mean, if anything? They certainly can refer to logical dependencies, but does this need to be modeled in a derivation with a (metaphorical) time-course?

DMS suggests another potentially intersecting possibility: perhaps late-inserted elements are late-acquired. Evaluating this is tricky, as it is well established that children use target-like morphological forms (by mimicking input) long before grammaticalizing adult-like generalizations for either regular or irregular forms. But the acquisition of regular morphological generalizations is nonetheless informative for us. A common error children make is over-regularization: applying a regular morphological rule/form to an instance where an irregular one applies (e.g. *I go-ed* instead of *I went*). It is only after this point that children approach the adult-like target, gradually de-regularizing the relevant morphological forms (Cournane, 2019). Therefore, we might expect that the late-inserted irregular forms of the English copula are acquired late, as well.

This is a hypothetical example: actually testing this requires a close look at specific morphophonology in specific constructions in specific languages. The more surface-oriented the pattern, the later children should be finalizing those forms. More specifically, instances where we see opacity (an apparently-syntactically-active structure that is bled and does not appear as a surface forms) or irregularity (that is dependent on morpho-syntactic features that arise in the course of the derivation of a sentence), those are where the relevant timelines should become clear. Again, we consider this a potential extension of DMS. If derivational timing. generally, corresponds to sequences of grammaticalization in acquisition, we expect correlations between analysis of adult grammars and acquisition: adult grammars have their form in part because of the acquisition pathways by which children arrive at them.

5.3 On the Necessity of Wholesale Late Merger

One last speculative extension that we think is worth mentioning has to do with the relationship of our claims and Takahashi and Hulsey's (2009) "Wholesale Late Merger," as summarized in §2.1 of Chapter 1. Recall Lebeaux's (2000) General Congruence Principle (introduced in §2.3 of Chapter 2), which states the same kind of explicit correlation between adult grammar and

acquisition pathways as we have proposed in DMS.

(169) **General Congruence Principle**: Levels of grammatical representation correspond to (the output of) acquisitional stages.
(Lebeaux, 2000, 47)

As we mentioned in Chapter 2, this proposal seems to have been almost entirely forgotten/ignored in the syntax/acquisition literature: DMS essentially is an attempt to revive it in a modern framework. But a discussion of some of Lebeaux's ideas on the matter are highly relevant to our claims.

Despite the broad similarities between DMS and the General Congruence Principle, as well as between DMS and Lebeaux's general approach (including an emphasis on lexical acquisition preceding functional acquisition, with children's intermediate grammars reflecting that intermediate knowledge), Lebeaux specifically argues that a Minimalist approach to structure building cannot correlate with acquisition. Lebeaux (2000, xxi) argues that a verb phrase like *see the big man* ought to be derived by the sequence of Merge operations in (170):

(170)   man + big $\rightarrow$ [$_{NP}$ big man] + the $\rightarrow$ [$_{DP}$ the [$_{NP}$ big man]] $\rightarrow$ [$_{VP}$ see [$_{DP}$ the [$_{NP}$ big man]]]

As Lebaux points out, however, it is well documented that children start with lexical categories before adding functional categories. So a child will utter phrases like "see man" before they utter full noun phrases like "the big man." The General Congruence Principle therefore doesn't match the structure building Lebeaux assumes of Merge in (170). As such, Lebeaux claims that that a grammar consisting of Merge-based structure building is insufficient, and instead there must be a way to model lexical categories combining before each lexical category projects its functional categories. Lebeaux suggests that this can be accomplished via his operation "Project-α," which composes two syntactic structures: "one a pure representation of theta relations, and one a pure representation of Case" (Lebeaux, 2000, xx). Project-α combines the thematic structure in (171a) with the functional structure (which crucially lacks lexical information) in (171b) to produce the mroe familiar structure in (171c)

(171)   Project-α operation: (Lebeaux, 2000, xxii)
   a.   [man [see woman]]
   b.   [the ___ [see [a ___]]]
   c.   [the man [see [a woman]]]

This conception of adult grammar allows the adult grammar to more closely match the sequence of acquisition, making a proposal like the General Congruence Principle more palatable.

This discussion misses an observation, however: while the Minimalist derivation of a verb phrase could potentially proceed from the nominal, adding functional nominal structure to the nominal, and only then proceed to the verb, this is not possible for subjects: if subjects are first-merged in a specifier position, they must already be built before merging into the structure. This is why Minimalist syntacticians assume a metaphor of the 'workspace,' a derivational space where structures can be built independently before combining with each other (Nunes 2004). This is a

logical necessity of bottom-up structure building via Merge: any phrasal structure that is first merged above the verb-complement sequence necessitates having previously been constructed before entering the derivation of the root clause.

Children, of course, go through the root default / root infinitive stage, where children readily utter SVO sentences that are telegraphic, lacking functional projections: this Lebeaux's point, to argue that an initial grammatical stage contains only lexical categories, and only later combines (via his Project-alpha operation) with a Case-frame to produce a more target-like utterance. But the Minimalist program is faced with the "build structures separately before merging" problem for even the most basic sentences, before considering acquisition at all. So instead, the assumption is that there is are separate workspaces where arguments and adjuncts can be constructed, before being merged into the clausal structure under consideration (presumably located in its own workspace. So the subject and object noun phrases are presumably constructed in their own workspaces in (172a) and (172b), respectively; only after those phrases are constructed are they merged into the main structure in (172c)

(172)   a.   the + man → [$_{DP}$ the man]

   b.   a + woman → [$_{DP}$ a woman]

   c.   see + [$_{DP}$ a woman] → [$_{VP}$ see [$_{DP}$ a woman] + $v°$ → [$_{vP}$ $v°$ [$_{VP}$ see [$_{DP}$ a woman]
   + [$_{DP}$ the man] → [$_{vP}$ [$_{DP}$ the man] [$_{vP}$ $v°$ [$_{VP}$ see [$_{DP}$ a woman]

Once we assume separate workspaces, many of the concerns fall away that were driving Lebeux's (2000) proposal of Project-α in order to maintain the General Congruence Principle. In §5 of chapter 2 we suggested that these workspaces correspond to distinct master trees in the master tree inventory. That is to say, children are not acquiring each individual adult sentence in a bottom-up fashion: this would be a pure behavioralist approach to language, and fails to explain the creative aspect of language use. Instead, children are acquiring abstract generalizations. This includes the maximal structure of a sentence in their language, but it also the maximal structure of a noun phrase. What we suggested in §5 was that nominal and verbal projections are acquired in parallel as part of separate "master trees," the generalization a child is reaching about a maximal sentence structure, or noun phrase structure, in their language. Our claim was that this corresponds to the separate 'workspaces' that have always been necessary in Minimalist theorizing.

Of course, verbal/clausal projections (i.e. sentential utterances) are not acquired absent of nominals: virtually every sentence contains (minimally) a predicate and a nominal that it is predicated of. The master tree for a sentence in a language therefore necessarily contains 'slots' for those nominals. So at an early stage of development, we'd expect a child to have a generalization something like (173a), or once made more abstract in the master tree, to look something like (173b).

(173)   a.   ___ hit ___
   b.   ___ V ___

In a more Minimalist implementation, it is plausible to assume that the empty slots are marked with a placeholder of some kind identifying that position as nominal: the minimal nominal is

normally assumed to be a D feature (represented on D°). On the approach sketched here a category D° may not be acquired at the earliest level, so we might expect this placeholder for a while to simply be the phase head F*Refer*, the referential phase head that will eventually be grammaticalized as the maximal nominal projection (call it D°). (See Chapter 4 for details.) So we might expect the structural generalization for the master tree at the *v*P level of grammar to look something like (174), where we annotate the placeholder with both its original and final categorial designation:

(174) $[_{vP}$ F*Refer*=D° $[_{vP}$ *v*° $[_{VP}$ V° F*Refer*=D°$]$ $]$ $]$

Like we said above, the D° heads in (174) are essentially placeholders: the empty slots in (173), identifying positions in the master tree where nominal arguments will be merged into the structure.

It may not be lost on the reader that the formulations in (173) look strikingly similar to a Construction Grammar model of abstract constructions. Likewise, an astute reader may well notice that the Minimalist implementation of these abstractions in (174) looks strikingly similar to a Wholesale Late Merger model, where only D° heads are initially merged into a structure, and the restrictor (the NP complement) is Late Merged at a later point in the derivation (this was introduced in §2.1 of Chapter 1).

This discussion and our claims in the rest of this monograph collectively lead us to this conclusion: a DMS approach necessitates WholeSale Late Merger, as described by Takahashi and Hulsey (2009) (we don't resummarize the claims here, instead referring the reader to §2 of chapter 1). Let us walk through the line of reasoning that brings us here. The telegraphic nature of early child speech shows that they acquire lexical categories first, acquiring the extended functional domain of nominals and the extended functional domain of clauses in parallel (§5 of Chapter 2). The DMS approach in general requires a master tree (as suggested originally for Organic Grammar, Vainikka and Young-Scholten 2011), and specifically a master tree inventory (see Chapter 2). If this is the case, we know that children represent nominals in their early grammatical generalizations, but we also know that during that period they have not fully acquired the full complexity of nominal structures. Nonetheless, when they have acquired full complex DPs, they don't re-acquire new argument positions for those DPs, rather just slotting the full DPs in where nominal arguments had always been in their grammar. The result is essentially a proposal where a nominal placeholder (D°) occurs in the clause master tree (and, presumably, in other constructions in the master tree inventory that derive from the full clause master tree). And the derivation of particular sentence would therefore require slotting a full nominal into the position of that D°: Wholesale Late Merger. Notably, Wholesale Late Merger was proposed by Takahashi and Hulsey (2009) completely absent of acquisition consideration, rather existing to explain properties of anti-reconstruction of specific movements (among other effects) in adult grammars.

There is much more that could be said about this here, but for the purposes of keeping the monograph to a respectable length we will simply leave this as a potential extension of DMS to be explored in more depth in the future. But what we find is that a Minimalist model of adult grammatical knowledge is probably well-modeled as an inventory of constructions, the 'contructicon' (following the Construction Grammarians). We have referred to this as the master

tree inventory, where fully abstract maximal structures are stored. A fully detailed discussion of the structure of the master tree inventory is important: if distinct 'master trees' are essentially the equivalent of distinct 'workspaces,' what are the inter-relationships between them? We know that the actual sentences can reference multiple master trees to articulate their form: a clause master tree is used for every sentence, but that will include reference the nominal master tree for the structure of arguments, and may well recursively reference the clause master tree for subordinate clauses as well. But what about proposals like Sideward Movement, movement across structures in different workspaces (Nunes, 2004)? Likewise, Müller (2017) argues for the operation "Structure Removal," (similar to Pesetsky's 2019 Exfoliation operation), both of which remove structure as part of the derivation of a sentence. But what structure is being 'removed'? Perhaps a derivation simply builds the structure, then removes it, but this would appear to raise its own questions about look-ahead (and economy of operations). On an approach here, where a master tree exists that contains the maximal sentential structure within a language, it is easier to imagine where structure removal operations are removing structure from to arrive at a structure under consideration: from a representation of the master tree. The question of the properties of the master tree inventory goes far beyond what we're prepared to discuss with any coherence, but if DMS does in fact turn out to be on the right track, a restrictive theory of the master tree inventory will quickly become necessary.

6   What remains to be explained? Most of Language

We want to be clear: we have explored in a large degree of depth a very few core questions. Specifically, empirically, from a syntactic perspective we have mainly been looking at concerns of structural hierarchy and the degree to which it is consistent with a cyclic derivation based Merge, a structure-building operation. The core proposal is that this derivation systematically corresponds to timelines in language acquisition, suggesting a strong link between those two. As we've articulated it, the UG operations of Minimalist syntax are in fact the mechanisms by which grammaticalization occurs (or at least, precise descriptions of the outcomes of grammaticalization), which allows us to posit very little to innate knowledge and instead assume most properties of syntax are emergent, as the Minimalist Program generally attempts to do.

Despite this outcome, we have tried to steer well clear of polemics about what is innate, and language specific, and what is in fact learnable by general mechanisms. And we've also said nothing about actual learning mechanisms. These are important questions, even central questions, but ones which we have very little to say about. Our claims are focused on a conceptualization of syntactic acquisition that accounts for these systematic correspondences between acquisition and adult grammars.

Despite making more predictions than we could possibly test in our lifetimes, this also leaves most of Language untouched. From one perspective, the claims here are so sweeping that we can't even defend them thoroughly in an entire monograph, we can only provide case studies arguing that they are reasonable. From another perspective, however, we have simply made a claim correlating a few key outcomes of Minimalist syntactic research to some key properties of language acquisition. Apart from all the subfields of linguistics that we have nothing to say about, even within syntax we have completely ignored many central areas of generative syntactic

research (e.g. binding, ellipsis, agreement, case: to name just a few extraordinarily large areas of research.) If it turns out that DMS is on the right track, it will surely offer some important perspectives on these areas of research. But we want to be completely clear that even if DMS turns out to be the correct way to relate adult grammars to syntactic acquisition, this would still only be a small piece of understanding the never-ending puzzle of human language.

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