



Universiteit Utrecht

Master Thesis

Is minimalism minimalist enough?

A look at agreement phenomena in a feature-free syntax

by

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Abstract

Whether minimalism is, in fact, minimalist or not is a debate held pretty often in conceptual literature of theoretical linguistics. Some say the goal of minimalism is to get closer to the biolinguistic questions of the field, while others insist it has missed the mark and should be renewed and revised, more radically than ever before. One of latter type of people is Boeckx (2015), who argues that minimalism is no longer true to its intended purpose, and that it is time for a new approach; one that is more minimalist than minimalism has ever been. In this thesis, I attempt to summarise Boeckx's views on minimalism, his own proposal, and its consequences. The bulk of the thesis will be dedicated to applying his ideas on the conceptual field of syntax to empirical research, thereby showing whether or not his ideas have consequences for theoretical syntacticians that should be deemed desirable, or perhaps undesirable. For current purposes, these empirical debates are restricted to the domain of agreement, including double agreement in Dutch, past participle agreement in French (and Italian), and the relation between verb movement and rich agreement (Rich Agreement Hypothesis).

Keywords: biolinguistics, minimalism, theoretical syntax, agreement.

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1. Introduction

The biolinguistic enterprise has come a long way since its unofficial incarnation halfway through the previous century. Fuelled by some of the first major publications by Chomsky, such as *Syntactic Structures* (1957), his review of Skinner's book about behavioural science (1959), as well as *Aspects of the Theory of Syntax* (1965), the conceptual groundwork for biolinguistics was only really determined by Eric Lenneberg's *Biological Foundations of Language* (1967), which highlighted the biological and psychological aspects in the approach to language. Even though Lenneberg's book is very old, its core questions still are relevant to this day, especially with respect to the relation between biology and language. If anything, the likes of Chomsky and Lenneberg have attempted to add linguistics to the category of natural sciences, like biology, chemistry, and physics, as opposed to the social sciences, to which it is often thought to belong to these days.

The term biolinguistics was first used in 1974 by Massimo Piattelli-Palmerini, as Chomsky (2007) recalls, on a conference where scientists of evolutionary biology, neuroscience, and linguistics, amongst others, came together in an attempt to debate whether language could be studied as its own unique subsystem of the organism that is the human being, or whether its properties were shared by other cognitive processes, such as the visual or motor systems; a discussion that is still relevant to this day. Some would even go as far as to ask, is language as we know it a cognitive process unique to the human brain, or could it be found in other animals, like birds (Reuland, 2013)? Reuland suggests that what sets humans apart from other animals is, in fact, the "ability to recursively combine minimal form-meaning combinations into interpretable expressions" (2013, p. 212). Ultimately, biolinguistics was taken to be a collaboration between biology and linguistics, to explore the issue from both sides.

According to Boeckx and Martin (2016), however, that collaboration has never taken off as promisingly as it had begun, with no real insights on the biology of language having been gained by linguistics. They claim biolinguistics these days is seen as a subsection of generative linguistics, but they point out that the biology department in papers that deal with the subject is rather short, and provides no new insights. So called biolinguists, as Boeckx and Martin (2016) refer to authors of these papers, will claim otherwise. However, according to Boeckx and Martin (2016), their written sections that deal with biology are interchangeable, have often nothing to do with the main topic of their respective papers, and contain absolutely no references to any authors from the field of biology.

Following major criticism from Cedric Boeckx towards the way the field of linguistics has evolved from those pure questions of language and biology, formulated in his *Elementary Syntactic Structures* (2015), it is not so hard to understand why linguistics quickly falls under social sciences. Without raising the suggestion of their unimportance, he observed how the work of linguists operating within the Minimalist Programme (Chomsky, 1995) has increasingly been focused on language-specific content and their comparison to that of other languages, rather than focusing on language as a biological property of human nature. At the core, the main question for a lot of linguists has not changed, but their focus on specific languages maybe leans a little too much towards the study of human culture and not human nature. Such criticism does feel as controversial, especially since most theoretical models within syntax these days are mere extensions of minimalism itself – something Boeckx still aligns himself with, in some form – however, since most theoretical proposals are based on primary linguistic data as a basis for achieving explanatory adequacy, such proposals are by definition language-specific, especially when the analyses themselves are comparative in nature.

The counter-argument for maintaining on the present course is not without merit as the only empirical data accessible for the theoretical linguist is, well, language itself and therefore language-specific analyses are unavoidable. Linguistics, which is at its core a ‘study of language’, is based on language in order to draw conclusions on the subject. Boeckx does not intend the contradiction, which is only apparent, since he does build a model of his own, pointing out how the absence of all language-specific content, including projections, categories, and features, removes the obstacle his fellow linguists will inevitably encounter in order to get to the biological properties of language.

Following previous publications on biolinguistics, such as Lenneberg (1967), Jenkins (2000), and most recently Chomsky (2017), the core questions concerning the biological aspect of language, as suggested by Lenneberg (1967), revolve around such topics as genetics, language evolution, language development, and those language universals that form the core of each language’s generative grammar, better known as Universal Grammar (UG). For theoretical linguists, UG is still most relevant and, according to Boeckx (2015), it is their task to find out the properties of UG if we are to come closer to an understanding with those scientists active in the field of biology, for which a stronger collaboration with psycholinguists and those active in language acquisition is a necessity. Over the years, UG has impoverished significantly, going from being extensively rich in the Principles and Parameters framework (Chomsky, 1981) to being minimalist. The question posed by Boeckx

(2015) is that maybe UG is still not poor enough, since the strong version of minimalism, the only version which is truly minimalist, is hardly ever adopted in empirical debates.

Linguistic concepts need to be tested by empirical data. No matter how fancy a theory can be on a conceptual level, its primary goal will always need to be explaining the data. As a rule of thumb, Ackema and Neeleman (2007) state that one theory can only be preferred over another on conceptual grounds if all else is equal, which is true from a scientific point of view; even Boeckx' model, however ambitious conceptually, must be tested empirically and account for the data before it can be accepted by anyone over the current range of available models. To test this, I will let loose his model as expounded in Boeckx (2015) on a number of different phenomena related to agreement to see if it can come up with an explanation minimally as complete as established models. Among the different phenomena in agreement to be tackled are double agreement in Dutch second person singular verbs, past participle agreement in French and Italian, and finally, the Rich Agreement Hypothesis.

Firstly, I will delve into the conceptual side of Boeckx' (2015) proposal, contrasting his approach to syntax to established proposals and explaining the role of syntax as he sees it, in chapter 2. Secondly, I shall attempt to explain or approach a number of currently debated phenomena in agreement through Boeckx' model in chapters 3, 4, and 5. In the final chapter, I will evaluate and assess the findings and propose a conclusion or continuation of the model.

2. Why minimalism is not minimalist enough

2.1 General background

At the heart of the current leading theoretical base for syntacticians lies Chomsky's *The Minimalist Program* (1995), which at the time 'replaced', or rather, evolved from the Principles and Parameters framework (Chomsky, 1981). It marked a change of course and pace, subtly urging linguists to re-approach UG from below and minimise its contents. Chomsky referred back to the original biology-oriented questions, acknowledging that there was "a problem for the biological sciences that is already far from trivial: how can a system such as human language arise in the mind/brain" (p. 2), which, he suggested, is a problem for biology and brain sciences, not linguistics. A phenomenon like the human Faculty of Language (FL) is unique in the natural world, but while biologists worry about its evolution and the question of its existence, it is the task of linguists to define FL. The questions, Chomsky suggested, to forego linguistic research should be as formulated in (1) and (2).

(1) "[W]hat are the general conditions that the human language faculty should be expected to satisfy?"

(2) "[T]o what extent is the language faculty determined by these conditions, without the special structure that lies beyond them?"

(Chomsky, 1995, p. 1)

The first question in (1) can then be split up in two aspects; conditions relative to other cognitive domains in the brain and conditions that are domain-internal, yet, crucially, must be natural elements of FL. Within theoretical syntax, the latter type of conditions are often explored, but as Chomsky indicates, it is not very precise, because there is no measure of relativity in this domain, nor the means to test most of its purely theoretical contents. Most telling are therefore those breakthrough moments when a theoretical notion receives support from outside of its domain, like with a syntactic operation such as Merge (Pulvermüller & Assadollahi, 2007; Petersson & Hagoort, 2012; Bemis & Pylkkänen, 2013; and others), which brings the field one step closer to the biological properties of FL.

These theoretical contents of the human language faculty form UG, and the implementation of minimalism redefined what UG actually is and how it should be approached. As formulated in Chomsky (2007), UG is seen in the biolinguistic perspective as "the theory of the initial state of FL" (p. 1), contrasting with language, which is then I-

language; merely one possible, personalised state of FL. Referring to UG as “genetic endowment”, one of the three factors that help shape the state of FL (Chomsky, 2001a; 2005; 2007), it was argued that “the less attributed to genetic information (...) for determining the development of an organism, the more feasible the study of its evolution” (2007, p. 4). It was that reasoning that moved UG from overspecification in the P&P framework to the minimalisation in the current programme. In the preface to the 2015 edition to *The Minimalist Program*, Chomsky states that minimalism in its strongest form proposes that FL is perfect and thus UG should be reduced to “the simplest possible computational operation” necessary to satisfy the conditions provided by the interfacing systems, the conceptual-intentional interface and the sensorimotor interface; for convenience’s sake shortened to LF and PF (although it is by no means intended to suggest that C-I and LF are equivalent terms). It is the philosophy of approaching UG from below that allows this view to rise. Investigating UG from below, as Chomsky (2007) describes it, indicates that one is trying to find a way to attribute as little as possible to UG, while still trying to account for the large variation in I-languages, perhaps by relying on non-language factors. It is in stark contrast with the approach of earlier programmes, which approach UG from above by attributing all properties to UG that can help to account for language acquisition.

In this impoverished version of UG, Chomsky argued, the “simplest possible computational operation” needed would be Merge. There are two main views of Merge (Ott, 2009), presented in (3). The first is most widely accepted in the literature, being closer to the Merge used in the P&P programme, while the second is suggested by Chomsky.

- (3) a. Merge is subject to Last Resort: each application of Merge(X,Y) must be licensed by a probe-goal relation between X and Y.
 b. Merge applies freely (or, alternatively: E[dge] F[eature] on L[exical] I[tem] deletes freely).

(Ott, 2009, p. 257)

In (3a), Merge is only initiated when it is required for a sentence to be well-formed. This is done through a probe-goal relation, in which X has a probe that searches for its goal and finds it in Y, after which Merge is initiated. In (3b), Merge is not restricted by well-formedness or any other attempt at regulation. Formulated in this strongest form of minimalism, the second definition of Merge (3b) is preferred for the following reason. While an argument for preferring a probe-goal relation for every application of Merge is that this makes syntax

“crash-proof” (Frampton & Gutmann, 2002), Ott poses the question whether that is at all desirable, siding with Chomsky. As Merge is applied freely, also generating structures that do not conform to the interface conditions, the fact that ungrammatical sentences such as “John slept Mary” or “John ran Mary blue” are still assigned some semantic interpretation is evidence in favour of that. Ott points out that there is therefore no reason to assume that syntax has a notion of well-formedness or grammaticality, since its main concern is structure, and therefore, Merge should apply freely, as in (3b).

Under the banner of minimalism, new approaches to old problems surfaced, relying on operations like feature-checking, Agree, Transfer, and Valuation. The rise of the feature as the main explanatory tool for syntacticians to tackle problems in I-languages is one of the most recognisable exports of the MP. Every phenomenon in language, from movement to morphology, is explained through features of some kind. Another suggestion by Chomsky (1995) was to eliminate the possibility of optionality within derivational structures, and to assign all examples of the application of optional rules to some other component of the language system. This has generally been taken to be the lexicon. Lexical items (LI) are assumed to come with a bundle of feature settings relevant to the item in question. If there is apparent optionality within a specific context, it can simply be assumed that it is a separate entry in the lexicon with only those feature settings that the entire structure follows from it. This model holds that the lexicon feeds syntax. In this model, the LIs provide the syntax with only one way to proceed, depending on the feature bundle accompanying it, specifying either a positive or negative value for a certain aspect.

This type of minimalism is commonplace in modern theoretical syntax, supplementing operations like case assignment in Pesetsky and Torrego (2001), or being the main reason for verb movement to the highest node in the T-domain, like in Bobaljik and Thráinsson (1998). It is a matter Chomsky (2001; 2007) addressed in his desire to go beyond explanatory adequacy. Where descriptive adequacy consists of pure observations, and explanatory adequacy strives to find out the underlying principles of language, going beyond explanatory adequacy would entail striving to find out *why* the principles of language are the way they are, i.e. “why these mechanisms should exist, and whether they are real or just dispensable descriptive technology” (Chomsky 2007, p. 4). The right question to be asked is then whether features are real or merely descriptive. What does it truly explain if we determine the difference between two lexical items A and B to be that one is [+RANDOM] and the other is [-RANDOM], with [±RANDOM] being a random syntactic feature that is not a ϕ -feature and not a reference to a non-language-specific domain, such as [±PROXIMAL] or [±DISTAL];

two notions clearly distinguishable by the naked eye and by a distinct demonstrative pronoun in many languages? It is used to describe the difference between two separate LIs, but it has no meaning outside of the minimalist model of theoretical syntax.

Of course, when it comes to the notion of Agree, such a statement becomes harder to defend, since the entire operation is built around the valuation of (un)interpretable features in many minimalist models. The idea is that some head of a phrase serves as a Probe in search of something to agree with within its c-command domain, possibly to move it to its specifier position, which then serves as a Goal. In order to make the move successful, the Goal must move up to satisfy the Probe's EPP feature and Merge internally. Since interpretability is the domain of semantics, the syntactic variant is called valuation. Some features on a LI are in need of valuation before the tree can move on to the interfaces. It is one of the operations that allow syntax to be crash-proof in certain models of so called 'weak minimalism' (Chomsky, 2001a; Ott, 2009). It is not unimportant to note that the Probe in these situations must always be a phase head, while the Goal must always be a phase complement, but I will touch upon phase theory later in this chapter. The larger point is that the full process of agreement is achieved through featural relations, but it is easier to understand why the theory has arisen to this structure for this particular phenomenon. Agree is easily observable and therefore more real, although its effects are mostly visible in, e.g. word order, morphological suffixes, or in semantic definitions, and therefore are mostly relevant for post-syntax. Syntax itself merely sets up the proper configurations through these valued features in order to meet the interface conditions at the point of Transfer.

In an attempt to paint the optimal generative framework, Berwick, Pietroski, Yankama and Chomsky (2011) have provided their view, emphasising that theoretical linguists need to know "*what* knowledge speakers acquire" (p. 1217), before they focus on *how* it is acquired. This ties in to the strong minimalist view as well, since the *what* in this case ties in with approaching UG from below, whereas the *how* is a focus on empirical data. They argue that in such an optimal framework, Merge is nothing but set formation; unrestrictive and not crash-proof. Word order is not relevant for syntax or semantics; only for speech production. Therefore, it is undesirable to impose a word order in either of these components and they propose that linear order "reflects an externalization of linguistic structures by the sensory-motor system" (p. 1218), rather the generative component of syntax or semantics. Embarking on a series of consequences of these assumption, one main theme returning in Berwick et al.'s (2011) optimal framework is that the difference of external versus internal merge requires some more explanation. When unrelated entities X and Y are merged into [X, Y], this type of

Merge is said to be external, whereas internal merge would be an example where X and Y are merged into [X, Y], while before, the situation was [Y, [Z, X]], thus indicating that X was already a part of Y, but on a different level. Allowing unrestrictive Merge always allows for the possibilities of both external and internal merge to occur. Assuming Merge is a biological process, what reasons would there be to allow only one of the two Merge variants to occur? It is, as they argue, senseless to assume that external merge is a simpler version of Merge, or to assume that internal merge is a more complex version. Ergo, they are both simply instances of Merge and thus part of our biology.

2.2 A more radical take on minimalism

2.2.1 ‘Minimalism’

In his book *Elemental Syntactic Structures*, Boeckx (2015) has provided a few arguments against minimalism, both in its weaker and stronger incarnations. Minimalism is still not minimalist enough, he concludes, even though the stronger version of minimalism is definitely a step in the right direction. His criticism is, however, aimed mostly towards the weaker incarnation. According to Boeckx, minimalism is suffering from a lack of focus on the right questions. He claims it is necessary to achieve biological adequacy in addition to explanatory adequacy, which means he wants linguistics to start building a bridge into the domain of biology for a cross-field collaboration. The focus has shifted from studying language as a biological phenomenon of human nature to the study of different languages. In order to achieve this biological adequacy, Boeckx thinks that the field of linguistics must strive to connect with other cognitive sciences, like biology, in order to unveil the secrets of the human brain. He argues, quite convincingly, that the elements preventing biological compatibility are those that cannot be taken as primitives, yet they are in many models. These elements include interface relations, the overabundance of features, and the position of the lexicon, all of which will be looked at and explained in this chapter.

Boeckx agrees with the basic principles of Chomsky’s motivations, often referring and citing his work, most notably this passage from Chomsky (1957).

[L]inguists must be concerned with the problem of determining the fundamental underlying properties of successful grammars. The ultimate outcome of these investigations should be a theory of linguistic structure in which the descriptive devices utilized in particular grammars are presented and studied abstractly, with no specific reference to particular languages.

(Chomsky, 1957, p. 11)

With these words in mind, Boeckx attempts to decrease the language-specific content from general models of FL. If UG is to represent the initial state of FL, free of language-specific settings, for lack of a better terminology, then it must be containing only universals of language. In order to do so, Boeckx argues that UG must necessarily be approached from below, as Chomsky also argued. He feels it is understandable that features and the lexicon have dominated the linguistic landscape for so long, given that linguists have to start somewhere and that, over time, a lot of important grammatical patterns and facts have been discovered, which can “formulate interesting generalizations that (...) are ‘too good to be false’” (p. 3). However, ultimately, Boeckx (2015) feels these models have been used to study languages, rather than FL, and that their relevance to the biolinguistic enterprise is considerably too small if we are interested in the primitives of language.

The lexicon in many minimalist frameworks occupies a position from where it feeds the syntax, and, in essence, determines the structure that syntax can form. Every lexical item in these models carries a feature bundle that determines what elements it can merge with, if at all. This is what Boeckx calls ‘lexicocentrism’; a system where all syntactic variation somehow depends on lexical properties of functional heads. A prime example is wh-movement in English, as in *Who did Mary kiss?*. In this situation, it is commonly accepted that *who* has moved up to SpecCP, because the functional head C has a lexical requirement in English that can only be met by the presence of a lexical item with the required feature in the specifier position. In this case, the feature in question is [+wh]. As Boeckx notes, this phenomenon is then generalised to something like this: “displacement takes place only to satisfy the lexical demands of the host” (p. 2). In addition, the displacement of the wh-element leads researchers to postulate the existence of EPP-features on functional heads, just because something has moved to their specifier position. To top off his argument, Boeckx cites George Lakoff, quoted in Boeckx (2015, p. 2):

So linguists fudge, just as has been done in the reflexive rule, by sticking on the arbitrary feature +REFL. Such a feature is a fudge. It might as well be called +CHOCOLATE, which would in fact be a better name, since it would clearly reveal the nature of the fudge.

Lakoff is not wrong, of course, in mocking this culture of features, or, as Boeckx calls it, “featuritis” (p. 6), since these days there are features in syntax for everything. There are edge-features, which allow a lexical item to be mergeable; move-features, like the EPP; agreement-related features, or unvalued/uninterpretable features; and then there are examples from the cartographic literature that show even finer-grained distinctions, the most notable of which are Cinque’s (1999) cartography of adverbial structure and the very detailed approach of Nanosyntax (Starke, 2010). Projections postulated by such approaches like ‘ProcessP’ or ‘EvaluativeMoodP’ are unlikely to be primitives or properties of UG, as pointed out by Boeckx (2015), even though they may serve a descriptive purpose. Instead, he says, they are more likely to be “the output of syntax-dependent interpretive processes” (p. 20).

Some infrequently asked questions were posed as early as Muysken and Van Riemsdijk (1986), as quoted in Boeckx (2015), and repeated many times since, saying “Even basic questions [concerning features] such as ‘how many are there?’, ‘what are they?’, ‘how do they distribute over syntactic structures?’ were hardly addressed, let alone answered” (p. 3). It is quite shocking indeed that these questions are still largely unanswered within minimalist literature, even though features have played a big role in theoretical linguistics for more than twenty years. This follows from the way minimalist analyses take place, as Boeckx explains in his book. A lot of these analyses begin only when the data is already there: pre-merged lexical entries that are simply tied together (merged, if you will) to form linguistic expressions. The power of features is then only descriptive, since the moment they are ascribed to the lexicon is when they are taken for granted and unargued for. “It’s not derived, it’s not constructed.” In such a framework, UG is not approached from below, but “from the lexicon, *in medias res*” (Boeckx, 2015, p. 5).

2.2.2 True minimalism

Spotting the flaws inside a programme is not a merit by itself, since anyone can stand by the side-line spouting criticisms, which may or may not be justified, but Boeckx plays by the book and presents the reader with a suggested alternative; one which may well be the start of a new programme, attempting to fix everything that the minimalist programme ultimately failed to do. What the criticism of Boeckx and many other minimalist sceptics boils down to, is that minimalism is not as minimalist as it could be. Throughout the literature, two ‘flavours’ of minimalism have been identified: the weak and strong versions of the thesis. In conceptual literature, the strong version seems a starting point when looking at Chomsky’s own writings, but in empirical literature, the weak version seems the more tangible of the two. Weak

minimalism is most equal to the Principles and Parameters programme in spirit, though not in form, whereas strong minimalism is the most radical suggestion from Chomsky's original proposal of the Minimalist Programme. It is therefore not surprising that the empirical theoretical syntactic tradition originating from the Principles and Parameters programme has chosen to ignore most of the strong minimalist aspects when it comes to analyses. Boeckx (2015) attempts to reargue for a lot of the merits of strong minimalism when it comes to the biolinguistic perspective, but goes even a lot further in proposing radical minimalisations.

The first and biggest problem Boeckx identified is the lexicocentrism found in many minimalist models, and he attempts to remedy the situation by diverting power away from the lexicon and back into the syntax. The lexicon has become too powerful in dictating what the syntax should do, i.e. feeding the syntax. Boeckx (2015) proposes that the lexicon goes back to its basic properties: the hosting of empty lexical items. The lexicon should be "primitive" and "structureless", and absolutely not "generative" (p.26), unlike in other models of a similar nature, such as Marantz (1997). No more feature valuations, no more instructions for the syntactic module. As an immediate result, Merge can be basic and structureless too. It no longer has to obey commands from the lexicon as to what can be merged together and what cannot. Thus, unrestricted Merge becomes possible. Only at the interfaces, demands are posed on what merged structure can make it through to LF and PF. Every lexical item, or, what Boeckx calls, "lexical precursor cells" (p. 27), is completely void of semantic or phonological information, thus giving the syntax free play. The lexical precursor cells contain minimally some feature that indicates that it is mergeable, however, since all entries in Boeckx' version of the lexicon have this feature, it is likely not a feature in the way minimalist features work, even though he uses the term 'edge-feature' to name them. Along with this edge-feature, these precursor cells contain indices to semantic and phonological information, only accessible once beyond the point of Transfer. This semantic information that the lexical item then carries is only unlocked when the merged piece of structure that passed the interface conditions has arrived safely 'on the other side', so to speak. No properties like case or person are involved during operations in the syntax. This does not mean that lexicalisation does not take place in our linguistic systems, according to Boeckx (2015), since he agrees with Pietroski (2008), who argued for a biolinguistic semantics, and quotes him in saying the following: "lexicalization is a large part of what makes humans linguistically [and, I would add, cognitively] special" (p. 32), thus acknowledging the importance of lexical information. However, lexicalisation is then a process that takes place within semantics, after the interface

with syntax.¹ The point of this, according to Boeckx (2015), is to create a sterile, non-language-specific environment that can be taken to represent UG.

With Merge operating unrestrictedly, one could wonder what happened to the other processes that were postulated to occur during syntax, such as Valuation, Project, and general Agreement, and Boeckx spares no expense: they must all go. One point where his proposed programme is more radical than Chomsky's is the way he handles the Projection Principle. Boeckx (2015), in his quest to rid syntax of anything language-specific, suggests that projection has no place in syntax either. It would completely remove the purpose of free Merge. When two equal lexical items X and Y are merged, they form [X, Y], where there is no order and no head. The set is symmetrical. Under projection, one of the two lexical items assumes the head position, and the other the complement position, transforming the set into a phrase, which causes an asymmetry. As Boeckx indicates, it can be inferred from structure to which category a set could belong in a language like English, following Kayne's (2011) line of thought where a lexical item merged with itself can form a singleton set, with no values to be valued at Valuation, in which case the lexical item in question can be analysed to be a noun, given the properties of that particular projection category. However, even though such interpretation may be imposed, Boeckx has chosen to refrain from doing so in order to be consistent in trying to make his syntax free of language-specific content.

Even though Chomsky (2013) admits that projection has no place in the simplest of Merge systems, such as the one in this model, it was never properly realised in that way in his models, since his take on phase theory relied on categories like little *v* and C. These projection categories act as phase boundaries in his models. Boeckx (2015) attempts to explain the asymmetries found in overt syntax, such as c-command relations or projection, through the notion of phases, or cyclic transfer. His hypothesis is given below (p. 38):

(4) Phases are the sources of all asymmetries found in Universal Grammar

As he points out, such a hypothesis gives rise to the following surmise (p. 38):

(5) Because all asymmetries in Universal Grammar are rooted in phases (cyclic transfer), all of them will be 'interpretive'

¹ For a detailed account on lexicalisation within Pietroski's (2008) biolinguistic semantics, please read his paper first hand.

According to Boeckx (2015), this means that the effects of these asymmetries will be felt only after Transfer, in post-syntax. Following that, syntax is always symmetric. Phases are, Boeckx argues, a self-organising means to regulate the Merge process. Unlike in Chomsky's lexicocentric model, in Boeckx' model, he assumes that phases are a natural means to cause the asymmetries his framework. He comes to two conclusions; (1) Transfer must occur cyclically, in order to avoid crashes in post-syntax, and (2) Transfer cycles must be small, so that the external systems can regulate the incoming structures step by step. When it comes to the final one, Boeckx' phase theory assumes that after each instance of Merge, a phase is formed. Instantly, the phase complement is sent to Transfer, whereas the phase-head, the phase edge, stays behind. Note that this head-complement relation is established purely on the notion that one is transferred and the other is not; there is no lexical property to separate these empty lexical precursor cells. Thus, to iterate, Merge does not cause asymmetry, but since a phase is formed after each instance of Merge, asymmetry arises when a phase is merged again, thus establishing a head-complement relation.

There are a few notable differences between Chomsky's (2001b) phases and Boeckx' phases, most of which stem from the differences between a lexicocentric model and a syntactocentric model. The first is that Chomsky proposed the Phase Impenetrability Condition (PIC), entailing that, at some point, the elements within the domain of the phase-complement are no longer available for further computation, unless they move to the edge of the phase. This phenomenon would account for long-distance dependencies via successive cyclic movement (from phase edge to phase edge). Simultaneously, the PIC offers a perspective on island effects as situations where the 'escape hatch', or the phase edge, becomes inaccessible to an element. Boeckx notes that the PIC does not hold for two reasons. First, the escape hatch explanation of island effects is hard to argue for. There have been accounts in the past, mostly relying on features, but as Boeckx (2015) says, "it should be obvious that imposing conditions on edge accessibility is simply a way of getting the facts, not a way of going beyond explanatory adequacy" (p. 84). Second, successive cyclic movement is untenable. This movement is taken to take place through some intermediate landing sites, either phase-edges, or edges of any projection, depending on the linguist, but the nature of these movements are argued for in publications like Abels and Bentzen (2009), as cited in Boeckx (2015). However, discussions in Takahashi (2010) and others, as cited in Boeckx (2015), have shown that not all of these intermediate landing sites can be reconstructed. It is also unclear why certain category restrictions exist when it comes to these movements. Thus, Chomsky's phases are born from lexicocentrism, since any movement is

caused by either morpho-syntax, semantics, or to avoid being trapped inside the phase. If movement is considered internal merge, and merge is free, the link between Chomsky's phases and islands is gone; if Merge is regulated by non-syntactic processes, it cannot be free. The solution Boeckx presents in this case is to assume not cyclic movement, but cyclic Spell-Out. Word order is irrelevant for the narrow syntax in his model, but in the interface with phonology, this becomes another story. The advantage of cyclic Spell-Out over cyclic movement is that Spell-Out does not try to explain non-syntactically motivated movement-related operations like chain formation or recursion, but still manages "to capture the interface reflexes of successive cyclic movement" (p. 85).

Another difference between Chomsky's phases and Boeckx' phases is that Boeckx allows for more phase boundaries in a clause, not just at the level of little v and C, and somehow not T. There is one main argument against Boeckx' view, which is that more phase levels result in more complexity. His proposal indeed suggests that the amount of phase boundaries grows as the syntactic derivation does. However, such a self-organisation should follow from good design in FL. He notes that this is actually an example of Chomsky's (2005) third factor, something called the Menzerath-Altmann law, given in (6), which was first formulated for human language, but was later extended to other domains.

(6) *Menzerath-Altmann law*

[T]he increase of a linguistic construct results in a decrease of its constituents, and vice versa.

(Boeckx 2015, p. 87)

For the current issue, this translates to "the longer the sequence to memorize, the more one chunks it" in Boeckx (2015, p. 87). From that perspective, it becomes only more tempting to adopt Boeckx' phases, given that it is his goal to impoverish narrow syntax and UG to the point where it can be taken as biologically adequate.

2.3 Agreement, Valuation, and Feature Inheritance

Since the bulk of this thesis will focus on debates concerning phenomena in agreement, it is time to look at how Boeckx (2015) generally envisions that this process takes place, given the model outlined in previous sections. As a consequence of being anti-lexicocentric in nature, operations like Agree and Valuation cannot exist in this feature-free, syntactocentric model. The very basis of Valuation consists of a difference between valued and unvalued features

attached to lexical items in the lexicon (Chomsky, 1995). The relation between Probe and Goal in the operation of Agree then occurs because the Goal has certain valued features that the Probe requires in order to value its unvalued features. Since the entire operation is built around the existence of lexical features, they have no place in Boeckx' model. However, he does want Agreement in his model to take place within the syntax, like Chomsky's model (1995), rather than, say, in morphology or phonology (as in Bobaljik, 2008). In order to maintain some notion of unvalued features in his syntax, Boeckx points out that unvalued equals the absence of content. When Merge is set formation, as in Boeckx' model, only the empty set matches that description. Syntactically, it behaves just like any other set and, as such, is something that Merge can work with. Apparently, "any model taking Merge to be set-formation anticipates the existence of elements without value/content" (Boeckx, 2015, p. 77).

Where Chomsky (1995) assumes that unvalued information is added to a lexical item in an additional operation taking place during Selection in the lexicon, Boeckx (2015) suggests that this "bundle" is fabricated via Merge, in the syntax. He provides an example considering syntactic adjunction, given in (7).

(7) { {Phase-head, {} }, Phase head, {Non-Phase head ...} }

Echoing Chomsky's claim that unvalued features originate on phase-heads, Boeckx proposes that a representation such as the one in (7) must be forwarded to Spell-Out immediately, which is in line with the forming of any new phase, where the phase-complement gets ready for Transfer. What happens next is best explained in Boeckx (2015)'s own words:

One possibility (...) is to let the adjoined material {Phase-head, {} } remerge with the non-phase-head inside the phase complement labelled by the phase-head to which μF originally was added. This is just another way of 'transferring' the adjunct, since the option under discussion amounts to making the adjunct part of the domain being transferred.

(...)

Once added to the non-Phase head and transferred, μF will pick its 'value' from the closest c-commanding intransitive phase (a copying operation that is the morphological equivalent of Chomsky's AGREE), which is the morphological component's way of handling empty sets (the semantic component appears to ignore

it).

[footnotes omitted] (p. 77-78)

The Chomskyan operation Boeckx is trying to mimic in this citation is Feature Inheritance (Chomsky, 2007), where “the unvalued feature on the phase-head (probe) is inherited by the non-phase-head sitting next to the goal in the complement domain of the phase (the domain that is being transferred)” (Boeckx, 2015, p. 75). He also notes that the only rightful domain for agreement is a transitive phase, which also explains why intransitive phases, nouns, presumably, are not in need of Valuation are always interpretable.

Considering Boeckx’ considerable effort to remove all things restricting Merge and syntax from the system, it is remarkable that he chose to rescue an insignificant operation like Feature Inheritance and use it to shoehorn Agreement into his narrow syntax. Granted, the example given above does serve as a demonstration of internal Merge at its finest – unrestricted, not necessarily complex – but its motivation eludes me. What it basically does is insert an empty precursor cell into the derivation, allowing c-command to determine what it agrees with. Given that there is no lexical content in the derivation at this point – at least not of the type that is common in lexicocentric models – this begs the question what it agrees with in the first place. The only difference between the empty set and the regular precursor cell seems to be that the empty set lacks the indices that link to information that will be unlocked at the interfaces, or at least, that is the closest one can get, based on the information Boeckx provides. It is unclear why Boeckx has chosen this approach and not, for instance, an explanation that would move the whole Agreement operation to take place during the semantic or morphological component, since the whole point of his cleaning up the syntax module was to bar “undesirable look-ahead” (p. 73). Operations removed from syntax, such as Valuation or Project, were removed specifically because they were worrying about things that occur post-syntax, so it seems like a strange choice to have Merge prepare empty sets for that same purpose. Not only that, but an approach derived from Bobaljik (2008), who believes that phi-agreement takes place post-syntactically during the morphology/phonology component, seems like it would integrate seamlessly in Boeckx’ model.

Having said that, the thought that Merge prepares empty sets without indices for the interfacing components to fill with content is not unbearable or unpractical by design, if maybe a little convenient, and it will be the operation most tested in the following chapters.

2.4 Brief summary

As this chapter serves as the foundation for the analyses of different phenomena under discussion in the following chapters, it is important that several important issues we discussed are to be remembered when reading on. At first, we established some of the basic tenets of Chomsky's (1995) Minimalist Programme. The new theoretical programme was founded with the intention of going back to the field's biolinguistic roots, with a renewed focus on the definition of Faculty of Language (FL) as a product of the human brain. UG had to be impoverished and the question of its contents had to be approached from a different angle.

Boeckx (2015) completely agrees with the need to focus on the definition of FL, but disagrees with Chomsky and many others on how to apply this philosophy to theoretical linguistics. He highlights the problems he sees in modern minimalism. The main issue, as identified by him, is lexicocentrism; the belief that the lexicon dictates what syntax can or cannot do, and the implication that syntactic operations can be motivated by demands from post-syntactic systems. Lexical content will no longer play any role whatsoever in narrow syntax. Only free, unrestricted Merge operates in syntax, with Merge being set formation. This also removes the need for lexical features, Valuation, and Projection; all of which, according to Boeckx, only exist to serve post-syntactic needs. Even phase theory gets an overhaul in order to fit his own view on successive cyclic spell-out.

The operation of Agree, however, survives the 'purge', and is allowed to remain in syntax, albeit in a wholly revised form. No longer will features need to be valued, since they have no influence on the syntax, but instead, an empty set will inherit properties from phase heads they merge with to indicate agreement.

3. Case I: Double agreement in Dutch

Having established what it is that Boeckx (2015) finds lacking in modern theoretical syntax, let us have a look at a select number of phenomena concerning agreement, and determine where the analyses of these phenomena grow incompatible with Boeckx' model, and, if necessary, remedy the situation by either converting an existing analysis to be compatible with Boeckx' definition of narrow syntax, or propose an entirely new analysis. For most of these phenomena, it is necessary to look at the interfacing systems, especially since it is postulated that syntax worries less and less about anything that comes after it. Since Boeckx (2015) already explores the LF-interface a little in his own work, these three chapters will focus on the PF-interface only.

3.1 General background

There is a phenomenon in Dutch called double agreement, which influences the spell-out of verbal morphemes for the second person singular form. The morpheme receives its regular spell-out in any sentence with the order subject-verb, but it receives a null spell-out in every case of verb-subject order. See example (8).

- | | |
|---------------------------|------------------------|
| (8) a. Jij spreek-t. | c. *Jij spreek |
| you speak-2sg | you speak- \emptyset |
| b. Spreek jij? | d. *Spreekt jij? |
| speak- \emptyset you | speak-2sg you |
| 'You speak/Do you speak?' | |

The subject-verb inversion does not only occur in interrogative sentences, but also in many other cases of adverbial or object fronting, as in (9). Dutch has been analysed as a language with V2, which means that constructions as in (9) are possible. They all mean the same thing, with a possible, optional emphasis on the fronted elements.

It is also important to note that the phenomenon only occurs in present tense. Consider (10) for examples in the past tense or with a third person pronoun. In a regular past tense paradigm, as in (10cd), there are no changes in morphology.

- (9) a. Jij spreek-t die jongen morgen.
 you speak-2sg that boy tomorrow
 ‘You’ll speak to that boy tomorrow.’
- b. Morgen spreek jij die jongen.
 tomorrow speak- \emptyset you that boy
 ‘Tomorrow you’ll speak to that boy.’
- c. Die jongen spreek jij morgen.
 that boy speak- \emptyset you tomorrow
 ‘You’ll speak to that boy tomorrow.’

- (10) a. Hij spreek-t.
 he speak-3sg
- b. Spreek-t hij?
 speak-3sg he
- c. Jij bak-te.
 you bake-past
- d. Bak-te jij?
 bake-past you

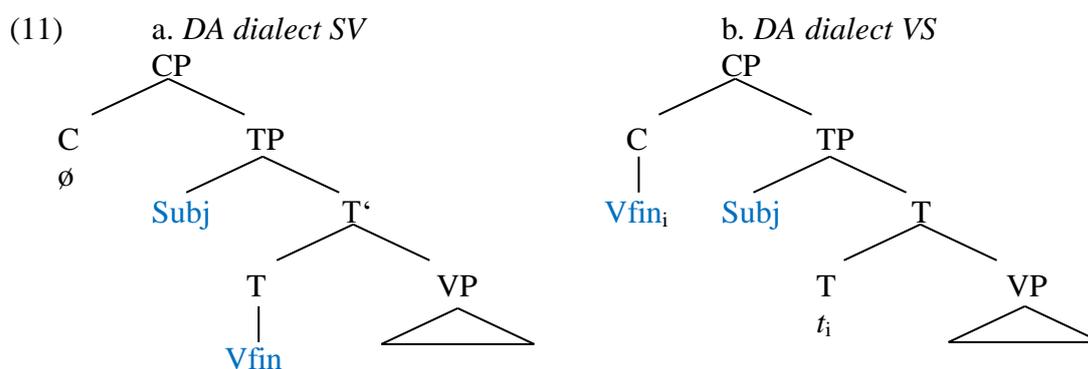
As becomes apparent through the data, the phenomenon in question only takes place with the second person singular. Two main accounts of this phenomenon come forward from the literature, both of which have their fair share of problems, even in a lexicocentric framework, but it is important to see what they say and why they must be rejected.

3.2 The syntax-based account

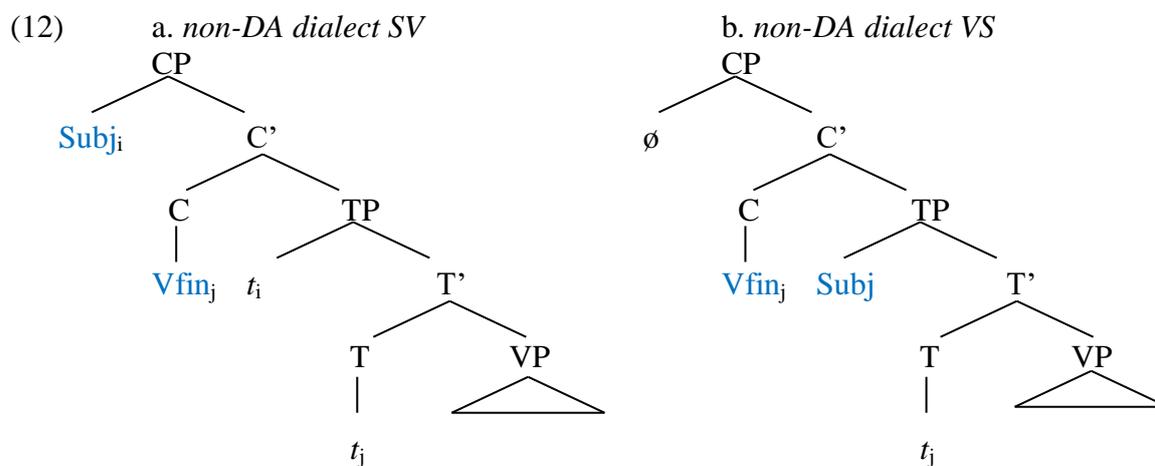
3.2.1 The proposal

The first account under discussion on the phenomenon in (8) is the feature-heavy, Distributed Morphology-based proposal by Van Alem (2017), expanding on a general idea by Postma (2011). The latter paper has established that there is a subdivision to be made, regarding double agreement in dialects of Dutch. There are those dialects that do not feature the phenomenon in (8), which we will refer to as non-DA dialects, and dialects that do feature it, from here on DA dialects. For non-DA dialects, the sole difference with DA dialects is that verbal inflection does not disappear when the subject and the verb undergo inversion, e.g.

instead of (8b), these dialects will have (8d) as the only grammatical option. (8a) is grammatical to all. Postma hypothesises that the difference between (8a) and (8b) in DA dialects is the position of the verb in the syntactic tree. As he explains, the verbal morpheme for the second person singular, as in (8a), is only spelled out when the verb is in the T-position (11a), whereas it is otherwise in the C-position, as in (11b). Note that in the description, the terms of SV or VS surface order are used to indicate whether or not subject-verb inversion has taken place in that specific instance; they bear no consequences for any typological word order analyses that may exist on the Dutch language.



Drawing up similar trees for the non-DA dialects would provide a very insightful difference, since in those languages, the verb always sits in the C-position, according to Postma (2011), with anything coming before it taking place in SpecCP, as is common in V2-languages, thus entailing optional re-raising of the subject from SpecTP to SpecCP. Consider the trees in (12) for support.



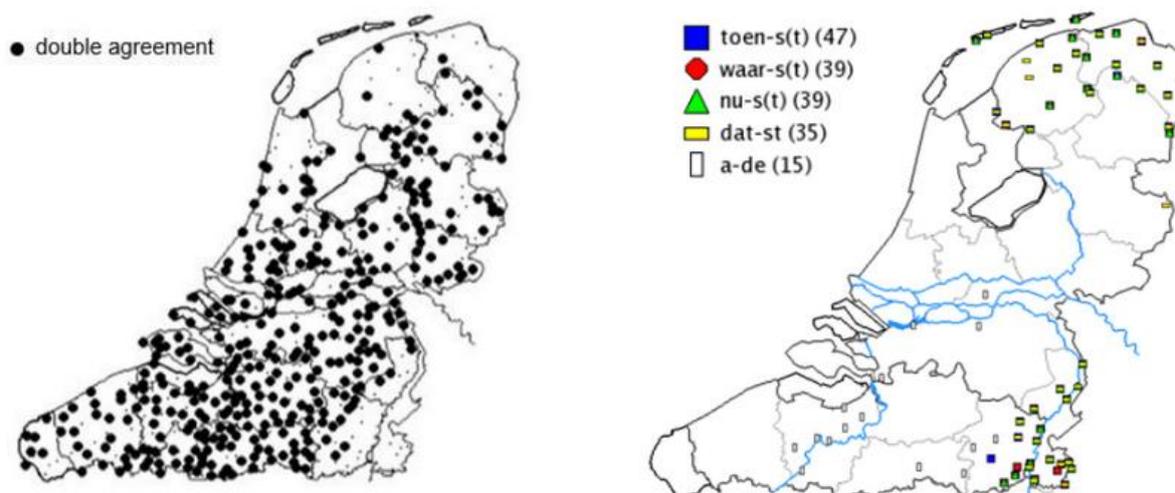


Fig. 1. *Double agreement and complimentiser agreement dialect distributions for second person singular pronouns.* (Van Alem, 2017, p. 8)

As can be observed in Fig. 1, which was built up from data from the DynaSAND library (Barbiers et al., 2006), the geographical distribution of CA-dialects seems to coincide with the distribution of non-DA dialects, a conclusion reached and explored in Van Alem (2017). This observed anti-correlation, as Van Alem calls it, would give rise to her eventual proposal of differences in the feature bundles these dialects store in their respective C and T nodes. She points out how three features are relevant for consideration here, namely [\pm SPEAKER], [\pm PARTICIPANT], and [\pm GROUP], the last of which I like to rename as [\pm PLURAL], purely for reasons of continuity with other proposals to be discussed later in this chapter. Van Alem suggests that non-DA dialects have a C node that requires valuation of all three of these ϕ -features, which results in the spell-out of the correct verbal morpheme at all times, inversion or not. This is, Van Alem argues, the reason why (8d) can be grammatical in a non-DA dialect, and it follows from the trees in (12), where it is suggested that the finite verb is always in the C-position in main clauses, and thus is always inflected. The observation that non-DA dialects often go hand in hand with CA dialects follows from the same proposal. Complementisers occupy the C-position in many subordinate clauses, like in (13c), and are inflected because of those same feature bundles that inflect the finite verb.

In DA dialects then, only the T node requires valuation of all these three features. As described by Van Alem (2017), when it comes to the C node, a setting of [\pm PARTICIPANT] will automatically result in a setting of [\pm SPEAKER], thus preventing the C node from assigning the proper second person singular morpheme to whatever resides in that particular

node. From her proposal, however, it is unclear as to how that works and why this is the case, but, at the very least, the assumption she makes seems observationally adequate.

3.2.2 Problems with this proposal

Despite the observational adequacy put forward in Van Alem's (2017) analysis, there are definitely a few ideas and conclusions in this proposal that serve further scrutiny. Firstly, Postma (2011) assumes for the DA dialects that V2 simply means raising up to T-head, rather than C-head, which is the regular V2 destination. Despite an example in (13) that made the surface order seem in place, one might question whether moving to T is at all a desirable reanalysis of V2; a question that I will try to explain. Despite there being little structural difference when it comes to V2 in DA dialects and V2 in non-DA dialects, movement from V to T is forced to obey completely different laws in the very same framework. The most recent version of the Rich Agreement Hypothesis (Koenenman & Zeijlstra, 2014), on which I shall expand in chapter 5, states that languages that feature verb movement from V to T necessarily have to be 'morphologically rich' in their definition. Koenenman and Zeijlstra (2014) define richness as follows. A minimum of three ϕ -features must be necessary to define all forms of the verbal paradigm before a language can be counted as rich². For the Standard Dutch verbal paradigm, given in (14), their definition renders this as poor, since all necessary forms can be defined with two features ([\pm PLURAL] and [\pm SPEAKER], as demonstrated in (15)). Standard Dutch can either be assumed to be an exception that falsifies their hypothesis, or it can be concluded that Standard Dutch features a proper V2, with verb movement going all the way up to C, thus being spared all this extra stress.

(14) *Verbal conjugation in Standard Dutch – spreken 'to speak'*

Person	Singular	Plural
1	spreek	spreken
2	spreekt	spreken
3	spreekt	spreken

(15) *Feature definitions in Standard Dutch*

spreek	→	[-PLURAL], [+SPEAKER]
spreekt	→	[-PLURAL], [-SPEAKER]
spreken	→	[+PLURAL]

² A more detailed discussion of their theory will follow in chapter 5.

Of course, it must also be considered what the motivation is for the verb to eventually move further up to C in simple declarative sentences that happen to have a fronted adverbial in SpecCP. As a reminder of the trees in (11) and (12), Postma (2011) argued for a structure for DA dialects where the verb moves from T to C in every case of the order verb-subject. This entails that adverbial clauses moving up to SpecCP must somehow trigger movement from the verb to C as well, whereas normally, the verb was already there. Neither Postma (2011) nor Van Alem (2017) provide any reason for the verb to move up. Previous analyses, like in (12), provide a much more satisfying explanation in that regard.

A second point of criticism is aimed towards Van Alem's (2017) analysis, which postulates that both C and T impose different requirements on verbs landing on their respective head positions. The problem is twofold. Firstly, it is assumed in Postma (2011) that verbs moving up to C also pass by T, since there is supposed evidence that the verb landed on that position, if we are to go along with the V2-in-T theory. According to Van Alem (2017), this T-head requires valuation of all three features at once, while the C-head only requires valuation of [\pm PLURAL] and [\pm PARTICIPANT], not of [\pm SPEAKER], which is automatically set to [+SPEAKER] whenever [+PARTICIPANT] is detected. So, if the verb lands on T-head on its way to C-head, all its features are checked at T-head. If the verb then moves up to C-head, there is no reason to assume any setting of the [\pm SPEAKER] feature to have any effect on the morphological properties of the verb, since those have already been acquired in T-head. Secondly, it is undesirable to postulate different featural requirements on C- and T-heads at all, since, following a discussion between Marc Richards and Noam Chomsky, the results of which were included in Chomsky (2007), there is no reason to assume that TP is a phase in this particular framework (for the moment ignoring Boeckx' (2015) take on phase theory). As Chomsky (2007) concludes after the discussion, the only phasal boundaries in the clausal skeleton must be v P and CP, the last of which selects TP. It is then by the process of Feature Inheritance that any unvalued features on T are passed down from C. To assume then that C and T carry different bundles of unvalued features is undesirable.

A final note, which is purely conceptual in nature and does not hold any bearing on the analysis itself, but which does tie in with the direction Boeckx (2015) wants syntactic theories to go in, is that this analysis is a prime example of the arbitrary nature of feature bundles. The observed phenomenon in (8) and (13) is indeed intriguing from a linguistic point of view, but the attempt at explanatory adequacy in this particular theory relies a bit too much on the notions of descriptive features. It is well-documented in the literature that ϕ -features

[±SPEAKER], [±PARTICIPANT], and [±PLURAL] are used during processes like Valuation or Agree, but the edits Van Alem (2017) imposes on them to account for the missing morphemes for second person singular verbs in the C-position in DA dialects are not argued for or explained. They are simply used to illustrate what we could already see, but now on a level too abstract for its own good. Abstractness in theoretical linguistics is a given and a necessity, but only when it can be used to pave the road beyond descriptive adequacy. As such, the analysis is observationally adequate, but not satisfying on an explanatory level. The lack of rules imposed on the feature system, as discussed in chapter 2, is the direct culprit of the decision to postulate an automatic setting. Van Alem (2017) assumes that the C-head in DA dialects automatically sets its value for [uSPEAKER] to [+SPEAKER] whenever [±PARTICIPANT] is valued as positive. How does this work exactly? What is it with this particular node that allows these features to behave this way? Why is it acceptable for a syntactician to assume such an unargued for hierarchy in these features, when so many other languages do not have this system in their ϕ -features? An analysis is only as strong as its empirical reach, and for that, Van Alem (2017) cannot be faulted, but the question that these types of feature-heavy analyses beg is what it is exactly in this approach that brings us closer to explanatory adequacy.

3.3 The phonology-based account

And so we turn to another account, as far removed from Postma (2011) and Van Alem (2017) as possible, rooted heavily in modern strong lexicalism, devised by Ackema and Neeleman (2007). Their proposal, scattered across multiple papers, and eventually also a book (to appear), single-handedly revived strong lexicalism in a somewhat renewed format that was less dependent on a generative lexicon or separated syntax and morphology modules, and thus revitalised the movement for modern linguistic theory. One of the major criticisms from the anti-lexicalist camp has always been that there were some seeming interactions between syntactic and morphological phenomena that would make a split into two separate modules undesirable. Double agreement in Dutch is one of those phenomena. Ackema and Neeleman have elegantly circumvented this argument by referring to the syntax-phonology interface as the answer to most of these problems.

In phonology, Ackema and Neeleman (2007) turn to the prosodic structure of the phonological string, more accurately prosodic phrases (ϕ s). The right edge of a syntactic

projection in Dutch is said to correspond to the right edge of a prosodic phrase. Consider then (16)³, a case of adverbial adjunct fronting, thus resulting in subject-verb inversion.

- (16) a. {Jij} {loopt naar school}.
 you walk-2sg to school
 b. {Naar school} {loop jij}.
 to school walk- \emptyset you

What is interesting to note is that the regular subject-verb order (16a) has the subject and the finite verb in different prosodic domains, whereas the inverted order (16b) features both in the same prosodic domain. This is, according to Ackema and Neeleman (2007), what causes the null spell-out for the second person singular morpheme in varieties of Dutch that have this particular phenomenon.⁴ To formalise, they postulate a spell-out rule that takes care of this phenomenon in the relevant varieties of Dutch, given in (17).

- (17) {[V Prt Add] [D Prt Add]} \rightarrow {[V Prt] [D Prt Add]}

In this rule, [Prt] stands for [Participant], signifying any ϕ -configuration that features [+PARTICIPANT], including first person morphemes – supposedly singular or plural, which does not seem specified, but let us assume this only counts for the singular forms, for argument's sake – and [Add] denotes the adding of spelled-out morphological content. To show that their rule applies consistently, they have constructed a few examples, which I have repeated in (18).⁵

- (18) a. {Volgens mij} {gaat op de heetste dag van 't jaar} {zelfs hij} {naar het park}
 according-to me go-3sg on the hottest day of the year even he to the park
 b. {Volgens mij} {ging op de heetste dag van 't jaar} {zelfs jij} {naar het park}
 according-to me went on the hottest day of the year even you to the park
 c. *{Volgens mij} {ga op de heetste dag van 't jaar} {zelfs jij} {naar het park}

³ All examples with glosses in this section are taken from Ackema and Neeleman (2007).

⁴ For a less simplified version of their proposal, I would refer interested readers to their paper.

⁵ Grammaticality judgements for these examples are copied from the source.

according-to me go- \emptyset on the hottest day of the year even you to the park
 d. ?{Volgens mij} {gaat op de heetste dag van 't jaar} {zelfs jij} {naar het
 park}
according-to me go-2sg on the hottest dag of the year even you to the park
 'I think even he(a)/you(b-d) will go to the park on the hottest day of the
 year'

The idea of these examples is that they added an adverbial phrase in between the subject and the finite verb while they are in inverted order. According to them, in Colloquial Dutch, the sentences (18a) and (18b) are perfectly fine. Note that (18a) features a present tense verb with a third person singular subject, whereas (18b) features a second person singular subject, but with a past tense verb. In Dutch past tense, there is only morphological distinction between singular and plural, and not between the three options of person. Now, this is where it gets interesting. (18c) and (18d) both feature a second person singular subject, but (18c) does not feature the spell-out of the morpheme, whereas (18d) does. According to Ackema and Neeleman, (18c) is strictly ungrammatical, whereas (18d) sounds less ungrammatical to them, which they take to be the ultimate argument for their theory to work.

The problem with their argument here is that not every native speaker of Dutch agrees with their grammaticality judgements. As I explained already in Van der Veen (2016), a little survey⁶ was conducted concerning the type of sentences used in example set (18), and while there seemed to be a clear difference in grammaticality judgements when it came to the second person singular in present tense, which people tended to see as less grammatical in sentences like these, there are enough reasons for me, as a fellow native speaker of Standard Dutch, to doubt this theory. Firstly, the difference in grammaticality between (18c) and (18d), as interpreted by Ackema and Neeleman (2007), is not something I can agree with. A sentence like (18c) seems perfectly fine to me, whereas (18d) seems strictly ungrammatical. What would cause this difference in acceptability of (18c)? Of course, since there are so many varieties of Dutch, some of which do not even have double agreement in this sense, it could be posited that these varieties do not feature rule (17), and a similar explanation may suffice for the difference in individual judgement on (18c). For instance, it can be suggested that

⁶ Admittedly, this survey was quite small-scaled, and it could and should be held in a more extensive form, including a geographical distribution of participants' location, which could help chart the difference in grammaticality judgement.

speakers may vary with respect to the setting of their prosodic phrase boundaries⁷, but given the hard-edged analysis on Dutch prosodic domains, it is more likely that this theory should be rejected.

3.4 A Boeckxian interpretation

Having looked at two theories for double agreement in varieties of Dutch, and given the conclusion that neither theory provided a satisfying analysis at both the conceptual and empirical level, attempting to translate any of these theories to the model by Boeckx (2015) could be considered somewhat pointless. However, a brief study of the mechanisms they use will indicate which one of the theories will be closer to what a Boeckxian theory on double agreement in Dutch might entail. We will take that as a starting point for an attempt at a new analysis for the current phenomenon.

In the case of the first account, the difference between (8a) and (8b) was said to be caused by the verb's sitting in a different syntactic node, that caused a different spell-out of the morphological aspect of the verb in question. I believe we can be brief about this. In Boeckx' model, word order is irrelevant to syntax and the syntax does not project, which makes it hard to distinguish two nodes in that domain. Projection instead takes place at the interfaces. Since this model tries to be syntactocentric, the absence of projections and feature bundles makes it very hard for this to work within narrow syntax, but perhaps it can work at the interfaces. Since ϕ -features are generally uninterpretable and have no consequences for the semantic interpretation of a clause (Bobaljik, 2008), the logical choice would be to see if we can make an analysis based on differences between syntactic nodes – a node-based analysis – work in the syntax-phonology interface. Of course, the main difference between the Ackema and Neeleman (2007) approach and this attempt at a node-based analysis, despite the fact that they both look towards the interface with phonology, is that Ackema and Neeleman look only at the surface word order and not at the syntactic structure. A node-based account necessarily needs syntactic nodes to work, which means any agreement issues relying on syntactic nodes would need to take place before the derivation is flattened.

Before we decide whether we can go with surface word order or with a node-based analysis, let us recap what Boeckx (2015) said about agreement and then see which approach we need to take, if not come up with a completely new one. In his model, agreement would take place via a c-command relation between an intransitive phase (generally a noun) and an

⁷ In fact, this has been suggested by Peter Ackema when asked about this on a conference.

internally merged empty set. He takes this to take place before Transfer, because it happens during Merge. We are propelled back into narrow syntax if we want to investigate differences in abstract agreement based on context-sensitivity. Again, I think we can be quite brief. There is no way this can be possible. Of course, it can be assumed that the agreement relation is established before Transfer and that the node containing the previously empty set then re-merges higher up in the derivation after agreement, but there is no purely syntactic reason as to why an SV order in Dutch would receive the full spell-out, whereas a VS order would not, since narrow syntax does not allow us to place conditions on individual nodes, movement, or simply word order; this can only be done at the interfaces.

3.4.1 A conceptual interlude

Language models can be elegant in concept, but ultimately they must be evaluated with respect to the way they explain the data. In a system that operates without syntactic projections, it seems more natural to assume more control towards the interfacing systems. The syntax-phonology interface is a much more suitable working space for a context-sensitive deviation in morpheme spell-out. Ackema and Neeleman (2007) and Bobaljik (2008) saw that too. Whether or not the syntactic structure is still accessible in phonology is something where theories differ. While Ackema and Neeleman (2007) refrain from making a statement about this, Bobaljik (2008) still assumes that the hierarchy can be accessed at the point of ϕ -agreement. The question is, which approach would best connect with Boeckx' (2015) view on syntax. Following this train of thought, it makes sense to assume that phonology still has access to the syntactic structure. Since syntax in this model only deals with empty items, the lexical precursor cells Boeckx (2015) mentioned, there has to be a point where the derivation is filled with semantic and phonological content. For this filling event to take place, there needs to be structure. This is the point where word order starts to matter. All clauses arriving beyond the point of Transfer necessarily must have met the interface conditions, which means that the derivation will have to obey some, necessarily language-specific, rules of phonology at one point. Since it is only possible for these rules to be checked if the structure is still accessible *and* at the same time filled with phonological content, it must be assumed that the structure is indeed accessible at the level of phonological coding.

An account that lies closest to Boeckx (2015) is Samuels (2009), who set out to propose a minimalist theory of phonology. At the time, Boeckx had not yet proposed the model we are discussing now, but it contains an extensive description of phonological processes and the interface that allows easy translating to the present. Samuels argues how an

ideal minimalist model should work “with synchronous cycles across the various modules” (p. 242), indicating, citing Chomsky to support her argument. What she means by that is that cyclicity in the computational model is greatly efficient, if it is assumed that syntax, semantics, and phonology all work in parallel cycles. Indeed, as also reflected in the Parsimony Principle, if there could be a single system ‘to rule them all’, so to say, such would be infinitely more minimalist than a different system for syntax, another one for semantics, etc. Following up on this philosophy, Samuels proposes a combination of Lexical Phonology (Kiparsky, 1982), Distributed Morphology (Halle & Marantz, 1993), and Chomsky’s (2001b) derivation by phase, culminating in what she calls Phonological Derivation by Phase. The idea is that every time a cycle is completed in narrow syntax, and the derivation is sent to Transfer, it allows phonological coding to be applied to the piece of the structure that arrived at the interface. As such, phonology would also work cyclically, much like syntax, and is therefore, by definition, derivational.

Coming back to the issue at hand, assuming cyclicity in phonology does provide a possible explanation for the phenomenon described in (8a) and (8b). If we assume that the model of Boeckx (2015) does operate in the way he described, then the agree-relation is established within narrow syntax. As hypothesised above, a possible difference between SV and VS orders, on narrow syntax’ end, could simply be caused by the receiving end of the c-command relation being re-merged higher up in the derivation, but only *after* agreement has taken place, meaning that the empty set is no longer empty. Let us assume that this is the verb, for ease of reference. Then, suppose, that everything below it can no longer escape the phase before cyclic transfer, thus entailing that the verb will be sent to spell-out as part of a different phase. At the interface, in a VS order then, the verb and the subject will arrive in different cycles.

3.4.2 Lexical insertion

When it comes to lexical insertion, Boeckx (2015) adopts the model of Bye and Svenonius (2012), which attempts to identify the processes that take place between narrow syntax and phonology. Spell-Out, in his view, has two sides, one of which is L-match, and the other is Insert. L-match has to do with matching the structure and the correct lexical item and is, for our purposes, not worth discussing in too much detail at this point. Insert is the moment where the structure is filled with phonological content, and thus, any instance of morpheme spell-out falls under this category. As cited by Boeckx (2015), the definitions and global overview of these processes are given in (19).

- (19) L-match and Insert
- a. L-match matches lexical structure entries to tree structures, making no reference to phonology
 - (i) Targets categories
 - (ii) Sees syntactic features, including both the projecting, semantically interpretable category features and the non-projecting, uninterpretable agreement features.
 - (iii) Sees syntactic structure, including dominance relations among features, syntactic words (the output of head-movement), and phase boundaries
 - (iv) May not uniquely determine a specific allomorph for insertion: the output of Match may include alternatives
 - b. Insert selects exponents for linearization, from the output of L-match, making no reference to syntax
 - (i) Operates on exponents associated by L-match
 - (ii) Sees phonological features, including segmental and autosegmental features
 - (iii) Sees class features in lexical entries (declension class, conjugation class, which have no syntactic content)
 - (iv) Sees phonological structure, including prosodic structure, which has been constructed in previous cycles
 - (v) Sees place attributes in lexical entries (for infixation and special clitics)
 - (vi) The output of Insert may be phonologically underspecified and is the input to Phonology

(as cited in Boeckx, 2015, p. 112)

It must be noted that this particular definition of L-match (19a) is not yet altered to be a part of Boeckx's model, speaking of features and projections as if they are part of the syntax. Boeckx (2015) remedies and re-defines L-match in his book, but it will have no bearing on the current issue, so it will not be repeated here. For now it should suffice to say that L-match has access to all of the syntactic structure, along with the semantic information unlocked at the conceptual-intentional interface, such as projections, categories, and interpretable features, as well as access to lexical content, which is matched with the precursor cells during this process. In a way, this process retains a lot of the functions that Boeckx (2015) has taken

away from syntax. For now, let us assume this process is as it is described in Bye and Svenonius (2012) and Boeckx (2015).

Insert is the more interesting process for our purposes, since this is where the bridge to phonology is being built. Syntactic structure is no longer accessible during this process, but phonological structure, and thus prosodic structure, is unlocked at this stage. Also, as indicated in the sub-processes, given in (20), this is where the derivation is linearized and where different morphemes are selected.

- (20) Insert: Realize, Linearize, and Phonologize
- a. Bottom-up
Insertion applies from the bottom of the cycle domain upward
 - b. Realize Context-Sensitive Allomorph
Where a lexical item has disjunctive exponence, then properties of the alternative allomorphs themselves determine which is realized (subject to the well-known Elsewhere Condition)
 - c. Maximize Links
All else being equal, an exponent with more links to the structure is preferred over one with fewer (intended to derive Maximum Exponence)
 - d. Linearize
 - (i) A non-affix is realized to the left of previously realized material of the same phase cycle (along the lines of Kayne (1994))
 - (ii) An affix is linearized to the right of previously realized material in the same phase cycle (following Brody (2000))
 - (iii) Linearization is subject to morpheme-specific position specifications of Vocabulary Items
 - e. Phonologize a preliminary phonological representation (the input to the phonological derivation)

(Bye and Svenonius, 2012, as cited in Boeckx, 2015, p. 114)

As can be read in (20), Insert knows five steps, indicating both the direction and order in which these steps operate. The output of L-match, which is an abstract tree filled with lexical information, is undergoing phonological insertion here, i.e. lexical information is now encoded with phonological information. Taking into account the steps taken during Insert, the picture becomes much clearer. Before we continue, returning to the issue of double agreement

in Dutch, it should be noted that the steps in (20) are morpho-phonological in nature; the syntactic structure is no longer accessible at this point.

A separate account by Idsardi and Raimy (2013), related to the current model, notes that when the lexicon is as impoverished as in Boeckx' model, resulting in the relocating of processes usually assigned to the lexicon to different components further down the computational line, it becomes impossible to postulate a direct link between syntactic structure and linear order as argued for by Kayne (1994). This follows from the model of Bye and Svenonius (2012), which also separates the hierarchical structure from the insertion and linearization processes. If indeed each process takes the output of the previous process as its input, the input to syntax would be the lexicon, and the output of syntax would be only structure. Whatever follows next will have this structure as an input and produce an output of its own. Insert works only with the output of L-match, which only leaves morphological and lexical cues for Insert to work with. As such, syntactic structure as it is in narrow syntax is lost in the computation. Boeckx (2015) states explicitly that he intends to follow these models with regard to these processes. It is therefore wise to include them in our current analysis.

3.4.3 Returning to the issue

Coming back to the empirical issue at hand, I believe we now have a sufficient toolkit to properly assess the two existing accounts that attempt to explain the difference between (8a) and (8b), here repeated as (21a) and (21b) respectively.

- (21) a. Jij spreek-t.
 you speak-2sg
 b. Spreek jij?
 speak- \emptyset you?

Let us not kid ourselves; the debate is far from over. It has gone on for a long time in the literature, and it will not be solved at this point. The only conclusion that can be drawn at the end of this chapter is which theory could be accepted given the new model by Boeckx (2015). As it stands, it seems like the node-based account by Postma (2011) and Van Alem (2017) needs to be rejected for this model, regardless of any problems that account may encounter in its analysis, as discussed in section 3.2. Since the issue of double agreement is the spell-out of a particular morpheme based on context – context-sensitive spell-out – that particular theory needs a model that allows the syntax to host morphological features as well. If one takes into

account that the hierarchical structure bears no influence on the morphophonology of a language, it is simply impossible for mere structure to influence the Spell-Out of morphemes; it has to be decided by linear order. Ackema and Neeleman's (2007) argumentation, as discussed in section 3.3, is not without its problems, but seems to be tenable the Boeckxian framework model. The phonology-based account is also not perfect, and would definitely benefit from more extensive empirical research, but it is favoured by the model and is thus the most elegant of the two in this context.

Whether the answer lies in prosodic structure or in the possibility that the re-merged verb is cyclically transferred in a separate phase from the subject, will depend on your theoretical preference. It would be interesting to see a similar phenomenon in another language, so a more widespread account would be possible. Regardless of which theory one prefers, non-DA dialects will operate in the same way, but without the observed distinction between SV and VS orders. As we saw, the prosodic structure cannot account for the examples in (18), but neither will a Phonological Derivation by Phase approach. At this point, it is better to leave it be.

4. Case II: Past participle agreement in French

4.1 General background

The second agreement phenomenon to be looked at is what is widely known as past participle agreement, occurring in some Romance languages, such as French. In this language, as well as in some other languages, like Italian⁸, for instance, object clitics can move to a position higher than the finite verb. It can be observed that in such sentences, the past participle is shown to have an agreement suffix, agreeing with certain properties on the object clitic. Consider the examples in (22), (23), and (24).

(22) *No agreement*

Jean a oublié ses chaussettes.
 John has forget.part- \emptyset his socks
 ‘John has forgotten his socks.’

(23) *Object agreement*

Jean les a oubliées.
 John them has forget.part-**3pl**
 ‘John has forgotten them.’

(24) *Subject agreement*

Alice et sa soeur sont parties.
 Alice and her sister are leave.part-**3pl**
 ‘Alice and her sister have left.’

In the examples, three different states of the past participle can be observed. In (22), a sentence with an SVO surface order, the object follows the non-finite verb, and is assumed to be in its complement position. No additional agreement is seen on the participle. In (23), the object clitic has moved up to a position above the finite verb, but below the subject. The past participle shows inflection in this situation. It must be noted that only object clitics can cause this effect, since full-fledged object phrases, like the one in (22), are not allowed to move to the same position as the clitic in (23). The phenomenon observed in (23) can only occur with

⁸ Though for our purposes, we restrict ourselves to French, to which all of our generalisations taken from the literature will apply, unless specifically stated otherwise.

verbs that receive *avoir* ‘to have’ as an auxiliary. For verbs that have *être* ‘to be’ as an auxiliary, the phenomenon in (24) occurs. In this case, the past participle agrees with the subject, rather than the object, and full object phrases are allowed in these passive constructions. The verb in (24) *arriver* is actually an ergative verb. Thus, it is assumed that the subject starts out in the complement position of the verb in Deep Structure.

Lefebvre (1988) kindly reminds us of all the different contexts of objects moving out of its projection, given in (25) (p. 235)⁹. Note that (25b) is a variant of (23), and (25d) is a variant of (24).

(25) a. *Relativized object*

les maisons_i que nous avons construites *t_i*
 the house-PL that we have-1PL build.part-**3PL**
 ‘The houses which we have built’

b. *Cliticised object*

ils les_i ont construites *t_i*
 they-MASC them have-3PL build.part-**3PL**
 ‘They have built them’

c. *Passive*

elles_i ont été construites *t_i*
 they-FEM have-3PL be.part-∅ build.part-**3PL-FEM**
 ‘They have been built’

d. *Ergative verb*

trois filles_i sont arrivées *t_i*
 three girl-PL be-3PL arrive.part-**3PL-FEM**
 ‘Three girls have arrived’

⁹ Due to the absence of glosses and translations in the source, I have added them myself, including any cursive or bold markings.

e. *Pronominal verb*

elles_i se sont livrées *t_i* à la police
 they-FEM REFL be-3PL surrender.part-**3PL-FEM** to the police
 ‘They have surrendered themselves to the police.’

In each of these examples, the object has moved leftward, in some cases to assume some additional function in the clause, like a subject (25c-e).

4.2 Kayne (1975): A look at clitic behaviour in French

One of the earliest attempts at explaining the behaviour of clitics in French was the one by Kayne (1975), who argued that object clitics and verbs were both dominated by a V-node, showing more than a few contrasting rules with full object NPs. The first observation he made was that nothing could intervene between the clitic and the verb, shown in (26). In this example, the past participle agrees with the clitic *le*. The intervening subject clitic *me* prevents the past participle from agreeing with *le*. The second observation was that these clitics could not be modified or contrastively stressed, as with *la* in (27a), compared to the different treatment of the clitic *elle* in (27b), which is given a contrastive role due to syntactic position, not stress. All examples with glosses in this section are taken from Kayne (1975, p. 75-92).

- (26) a. Jean me **le** donnera.
 b. *Jean **le** me donnera.
 ‘John will give it to me.’

- (27) a. *Jean **la** préfère.
 ‘John prefers *her*.’ (emphasis)
 b. C’est elle que Jean préfère.
 ‘It’s her that Jean prefers.’

The examples in (26) and (27) can be seen to contrast with full NPs in (28) and (29), respectively. For (27a) and (29), it must be taken into account that only the contexts where the clitic *la* and the pronoun *lui* are contrastively stressed are judged here.

- (28) a. Jean, paraît-il, est amateur de boxe.
 ‘John, it appears, is a boxing fan.’
 b. Jean voit souvent Marie.
 ‘John sees Marie often.’

- (29) Lui n’aurait pas fait ça.
 ‘He wouldn’t have done that.’ (emphasis)

In (28), the first example (28a) shows a subject NP separated from the verb, whereas (28b) shows an object NP separated. This distinction is important, because it shows the difference with the behaviour of clitics in (26), in that full NPs can be separated from their object or subject. Similarly, (29) shows that full pronouns can be contrastively stressed, unlike the clitic in (27a). Another difference between the behaviour of object clitics and full NPs, as reported by Kayne (1975), is that clitics cannot be conjoined, as shown in (30).

- (30) *Jean la et le voit.
 ‘John sees her and it.’

Backed by these observations, Kayne suggests there is enough evidence to support the notion that the sequence clitic + verb does not have a similar status as subject NP + verb or verb + object NP. The fact that a clitic cannot occur when there is no verb around supports his theory that such clitic + verb sequences have “some special syntactic status” (p. 83).

Before Kayne goes on to draw any conclusions, he presents a comparison with subject clitics in French. In a similar fashion to object clitics, subject clitics also contrast with full NPs in all the aforementioned situations. Nothing can intervene between the clitic and the verb (31), it cannot be modified (32) or contrastively stressed (33), nor can it be conjoined (34). Note that (31) contrasts with (28a), and (33) contrasts with (27b).

- (31) *Il, paraît-il, est fou.
 ‘He, it appears, is crazy.’
- (32) a. *Ils tous partiront bientôt.
 ‘All of them will leave soon.’

b. Tous les garçons partiront bientôt.

‘All the boys will leave soon.’

(33) *Il partira le premier.

‘*He* will leave first.’ (emphasis)

(34) a. *Jean et il partiront bientôt.

b. *Il et elle partiront bientôt.

‘John and he/He and she will leave soon.’

c. Jean et lui partiront bientôt.

‘John and he will leave soon.’

In addition to the already established contrasts, Kayne presents a long list of more examples of contrasts between clitic and full NP behaviour, which, despite being very interesting data to look at, may occupy too big a space for our current purposes. However, interested readers will be recommended to read the source text for Kayne’s full reasoning. In short, there are several more contrasts that allow Kayne to assume that the special status of clitics is hard to reject, including that subject clitics cannot be truncated phonologically, whereas full NP subjects can; clitics can be used referentially, whereas full NPs cannot; clitics and full NPs are subject to different rules of inversion; among others.

All of these contrasts, as concluded by Kayne (1975), seem to point out that clitics are more closely bound to the verb than full NPs. Subject clitics behave more like object clitics than subject NPs. For this reason, Kayne concludes that the clitic is dominated by V. This explains why both brands of clitic behave so similarly, why nothing can intervene between verb and clitic, and why they are so different from NPs.

The focus in Kayne’s (1975) analysis lies mainly on clitics, and not overtly on past participle agreement, and thus it could be asked why his analysis has any bearing on the subject at all. Despite his failure to mention past participle agreement in this particular publication, whether or not deliberately so, his analysis has highlighted some important differences between object clitics and object NPs. As shown in example (23), the addition of past participle agreement for object clitics to the list of differences could be another.

4.3 Lefebvre (1988): Co-Case marking

A second account by Lefebvre (1988) takes a different approach. Rather than focussing on clitics, she assumes right off the bat that the agreement morphology as spelled out on the past participle in examples (23) and (24) is an expression of the trace of the NP that the past participle governs. As Lefebvre indicates, such morphology can be seen as a Case marker. Given that the morphology is spelled-out on the Case assigner, rather than the assignee, she suggests the morpheme reflects “the case assigning properties of the past participle” (p. 234).

In order to account for the mystery as to why this agreement only takes place when the object is moved to the left, Lefebvre adopts the notion of Co-Case marking, “a special case of co-indexation which Co-Case marks an element moved out of its projections with the projection it is moved out of” (p. 234). Introduced in earlier work by Lefebvre, Co-Case marking has in previous literature been shown to account for languages with a very free word order and similarly a very rich case morphology. Especially in Romance languages, the notion has been used to suggest accounts for quantifier float, extraction of nominal complements out of their projection, and extraposition. Given the fact that all three of those phenomena still occur in French today, Lefebvre (1988) suggests that past participle agreement may also be a case of Co-Case marking.

Finally, Lefebvre (1988) also proposes that the agreement morphology on the past participle is part of a Case Chain (as in Chomsky, 1981), linking the past participle, its maximal projection, and the object NP, which has moved out.

4.4 Belletti (2017): Spec-Head relation

Starting with Kayne (1989), theories of a very different kind arrived, influenced by Pollock’s (1989) work on clause structure and Chomsky’s early writings on the Minimalist Program. Kayne (1989) proposed that past participle agreement is no different from (preverbal) subject agreement, in that they are both reflexes of an already established relation between a specifier and a head in an agreement-minded projection along the functional skeleton (Belletti, 2017). One of the ideas on the rise back then was the idea that what was known as IP or TP could be split up into multiple projections. One of these was AgrSP, the highest projection, used for subject agreement, another that occurs in some literature, like Bobaljik and Thráinsson (1998), is AgrOP, used for object agreement, and yet another, as suggested by Kayne (1989), was used for past participle agreement, possibly called AgrPstPrtP.

It works as follows. Consider example (23), repeated here as (35).

- (35) *Object agreement*
 Jean les_i a oubliées *t_i*.
 John them has forget.part-3pl
 ‘John has forgotten them.’

As indicated by Belletti (2017)¹⁰, the clitic *les*, would start out somewhere in the object domain of the verb, and move upwards, passing through any Spec position on the way to its final destination, where it ceases to be a head of its own maximal projection and conjoins with the finite verb. Agreement between the past participle and said clitic can take place once the clitic has passed through the Spec position of the AgrPstPrtP, where the past participle would move to and pick up the relevant agreement morphemes born from their brief Spec-Head relation. When there is no movement (22), the object does not move to the specifier of AgrPstPrtP, and no agreement takes place. Thus, there is no added morphology to the past participle.

This theory of agreement has been replaced with the minimalist operation of Agree, where a Probe searches for a Goal to express its agreement features. As Belletti indicates, Spec-Head relations under this view is derived only through EPP-features, triggering the necessary movement. Such movement, however, is no longer necessary for an Agree operation to take place, since Agree can take place over distance, which opens up a different questions and possibilities. For instance, the difference between (22) and (35) is now harder to explain. Belletti (2017) seems to refrain from going there too much, but acknowledges that the problem already exists, albeit in a somewhat different form, in a theory of Spec-Head relation.

Interestingly, a contrast between French, which we’ve been considering all the time so far, and other Romance languages featuring past participle agreement, shows that each language has different rules when it comes to the application of such agreement morphology. For instance, when it comes to *wh*-movement-induced agreement (as in (34a)), Standard Italian never shows past participle agreement, whereas Standard French shows that this is optional (see (36) and (37) (from Belletti, 2017, p. 8-9)).

- (36) *Wh-movement and past participle agreement in Italian*
 a. Quanti libri hai letto?

¹⁰ Belletti (2017) actually focuses on an Italian example (p. 3), but the workings in French are similar in this respect.

	how-many	book.MASC.PL	have.2SG	read
b.	*Quanti	libri	hai	letti?
	how-many	book.MASC.PL	have.2SG	read.MASC.PL

(37) *Wh-movement and past participle agreement in French*

a.	Combien	de voitures	a-t-il	conduites?
	how-many	of car.FEM.PL	have.3SG-3SG.MASC	driven.FEM.PL
b.	*Combien	a-t-il	conduites	de voitures?
	how-many	have.3SG-3SG.MASC	driven.FEM.PL	of car.FEM.PL
c.	Combien	a-t-il	conduit	de voitures?
	how-many	have.3SG-3SG.MASC	driven	of car.FEM.PL

As you can see when comparing (36) and (37), Standard Italian does not allow past participle agreement in situations of *wh*-movement. Standard French does allow it, but it has another interesting feature to the language in that it can apparently split up the QP *combien de voitures*, as seen in (37b-c). However, when the QP is split up, leaving the nominal complement behind (37b), past participle agreement is not allowed. As reported by Belletti (2017, p. 10), an explanation was suggested by Rizzi (1990), who proposed that we look at the behaviour of the adverbial *beaucoup* in French. Invoking a principle called Relativized Minimality (RM), Rizzi suggested this principle dictates that movement of the *wh*-quantifier *combien* in (37) should be “to and from the same syntactic position in the VP area also available for the adverbial modifier *beaucoup*”. Consider (38).

(38) *Adverbial modifier beaucoup ‘much’*

a.	Il	a	consulté	beaucoup	de livres.
	3SG.MASC	have.3SG	consulted	many	of book.PL
b.	Il	a	beaucoup	consulté	de livres.
	3SG.MASC	have.3SG	much	consulted	of book.PL
c.	*Il	a	beaucoup	conduites	de voitures.
	3SG.MASC	have.3SG	much	driven.FEM.PL	of car.FEM.PL

Adverbial positions are in general A'-positions, and Belletti (2017) has no reason to assume *beaucoup* is any different, and neither is *combien*. The Spec position of AgrPstPrtP is considered an A-position, as is usual for specifiers of agreement positions. It is then assumed

that the adverbial position is located lower than the AgrPstPrtP. With all these assumptions, Rizzi (1990) can conclude that (37b) and (38c) are ungrammatical because they feature illegal movement from an A'-position to an A-position.

In addition to all this, there are also some varieties of (southern) Italian that feature past participle agreement with the direct object in situ (Belletti, 2017).

(39) *Direct object in situ (Italian dialect)*

Maria ha	conosciute	le	ragazze.
Maria have.3SG	known.FEM.PL	the	girl.FEM.PL

This entails that (22), repeated below in a more elaborate fashion as (40), is not a generalisation throughout the past-participle-agreement-languages.

(40) *Direct object in situ (French)*

a.	Jean a	oublié	ses	chaussettes.
	John has	forgotten- \emptyset	his	socks
b.	*Nous avons	oubliées	notre	chaussettes.
	We have	forgotten-PL	our	socks

Sentence (39) is problematic for the current analyses, because it shows a case of past participle agreement where no overt instance of the direct object has passed through the Spec position of AgrPstPrtP. As reported by Belletti (p. 11), two possible analyses have been suggested in the past, staying in the same framework. The first is a suggestion that the direct object is not in its regular situ position, as (39) would suggest. Rather, it is dislocated to the right, and a silent clitic triggers the agreement. The second suggests that there is more structure within AgrPstPrtP. It could be assumed that the past participle has moved to the head of a higher phrase, and the direct object would not have moved past the Spec position of the lower phrase. Belletti is right to reject both suggestions in that there is not enough argumentation to support them. What motivates systematic right-dislocation? How can we justify the presence of a silent clitic? What is the nature of the higher phrase to which the past participle moves to, and why does it move there?

Belletti (2017) suggests a third analysis, for the first time looking at a more minimalist solution as part of her theory. Sentence (39) could result from a direct Agree relation between the past participle and the direct object, after the past participle head starts probing for the

direct object in its internal argument position. As she justly notes, this would raise more questions, especially with regards to that same sentence in Standard Italian or French, both of which behave like (40). Interestingly, Belletti then turns to Italian past participle small clauses, for what seems like a comparison. Consider the examples below.

- (41) *Standard Italian small clauses*
- a. Arrivata Maria, ...
 arrived.FEM.SG Maria, ...
 ‘Maria having arrived, ...’
- b. Conosciuta Maria, ...
 known.FEM.SG Maria, ...
 ‘Having known Maria, ...’

Note that (41a) features an unaccusative clause with the internal argument in its situ position, whereas (41b) features a transitive clause with a direct object in situ. In unaccusative structures, the past participle always agrees with the subject, even if it occurs in postverbal position, as in (41a). According to Belletti (2017), (41a) can be considered a standard case. (41b), on the other hand, could, by that same logic, be analysed as a passive small clause, but a similar example with a first or second person personal pronoun reveals that the direct object is assigned accusative case. If this were a standard case, that would predict that past participle agreement would occur in all Standard Italian sentences with a direct object in situ, which is not the case, as we saw in (38). Rather, Belletti proposes that PRO fills the subject position in sentences like (41b), suggesting that the relation between the past participle and the direct object genuinely is one of agreement, the likes of which we have seen in (39). In addition, she suggests that the direct object has indeed moved to the Spec position of AgrPstPrt, which accounts for the agreement on the past participle itself. The word order is then obtained by the past participle head moving up to a position higher in the clause, perhaps even C. Belletti (2017) suggests that agreement in this case should be necessary “due to special properties characterizing A[greement]S[mall]C[lauses], possibly crucially related to Case requirements” (p.12).

Since it is unclear whether the sidestep towards small clauses is meant as a comparison to sentence (39)¹¹, I am hesitant to assume that Belletti (2017) suggests those dialects

¹¹ The comparison between the two is somewhat implied in the structure of the paper, but an absence of an explicit mention prevents me from drawing that conclusion (see Belletti 2017, p. 11-12).

construct their sentences like small clauses, which would be a possible explanation, but it would be identical to the second analysis on (39) mentioned above, which she rejected in her paper. Of course, (39) is not a small clause, and (41b) is, but it is unclear why the two should behave so differently in their structure. Belletti says this is “due to special properties”, but does not expand on what these special properties are. Why should an analysis be accepted for one case and rejected for another? Especially when, to the naked eye, both (39) and (41b) seem to reflect the same surface order, and the analysis would ‘save’ both examples with a single assumption, rather than imposing two different analyses for what seems like similar phenomena.

Having looked at the theories by Kayne (1989) and Belletti (2017), we now have a good overview of what it is they proposed for past participle agreement, and the problems they faced. To recap, Kayne (1989) suggested that the object clitic moved up to the specifier of a node called AgrPstPrt, whereas the past participle itself moved to the head of that phrase, thus establishing the agreement in a Spec-Head relation. The analysis was challenged by additional data from varieties of Italian, where direct objects in situ could even agree with the past participle without visibly moving anywhere (as in 39).

4.5 A Boeckxian interpretation

Unlike with double agreement in Dutch, the two different theories pertaining to past participle agreement (in one way or another) do not actively oppose one another in debate. On the contrary, Lefebvre (1988) and Kayne (1989) do not necessarily cancel the other out, as they both agree that it is the act of the direct object moving to the left of the past participle that causes the agreement morpheme to appear, and that it is a Spec-Head relation that triggers the agreement morpheme on the past participle. It is striking, though, that papers as recent as Belletti (2017), despite mentioning a possible minimalist version of the theory, still operate within the Principles & Parameters programme. It is not hard to see why that is the case. As Belletti already indicated, adding minimalist operations to the mix ponders the question of what triggers movement of the direct object, which takes away some of the elegant nature that the current analysis had. Of course, an Agree operation with movement can be accounted for by simply saying that the probing head has a feature [EPP], and everything is solved. In addition, the difference between examples (38) and (39) could then simply be ascribed to the lack of an EPP feature, and the problem would be solved; but that would be bad linguistics.

Boeckx (2015) has attempted to make minimalism even more minimalist in the syntax department, and that will have its relatively predictable consequence: stuff will have to be

moved to the PF-interface. As with the previous phenomenon, movement and the spell-out of morphemes will have to be separated in a system where syntactic nodes do not project until the interface with semantics, meaning movement cannot bear any consequences on anything after Transfer. Agreement, however, does take place within the syntax, as discussed in chapter 2. At this point, two nodes agreeing with each other are free from any semantic or phonological information. Since we have already established in the previous chapter that agreement morphemes are in general free of semantic information (Bobaljik, 2008), we need to turn towards the interface with PF.

In section 3.4, it was concluded that hierarchical structure bears no influence on the morphophonology of a language, following models that Boeckx (2015) had already adopted. Much like the node-based analyses of Postma (2011) and Van Alem (2017) on double agreement in Dutch, proposals like Lefebvre (1988), Kayne (1989), and Belletti (2017) also hold on to syntactic projections, nodes, and movements as their primary anchors for determining past participle agreement. The model we follow here, however, assumes that only linear order can have a direct influence on morphophonology. If we stick to linear order, we must go back to what we see or hear on the surface in these cases of past participle agreement, and that is an object clitic followed by a past participle¹². In the most basic sense, that is what can be observed about linear order.

Borrowing slightly from the Ackema and Neeleman (2007) paper, perhaps the matter should be seen in prosodic phrases¹³, much like with double agreement. For information on French prosodic phrases, Di Cristo (1998) noted that, as a rule of thumb, a prosodic phrase is closed after each maximal projection. There is one “major exception”, he notes, concerning subject clitics, which are “always integrated into the following [prosodic phrase]” (p. 214). Note that Di Cristo explicitly singles out subject clitics, and does not extend the exception to all clitics. This leads to the assumption that this does not count for object clitics, or full subject NPs, but only subject clitics. Consider (42).

¹² Let us stick to the base example of (18)/(35).

¹³ It has come to my attention that the following paper deals with this issue in the manner I hint at in this section. Unfortunately, I was not aware of it at the time of writing.

- (42) *Prosodic phrases*
- a. {Jean} {a oublié ses chaussettes}.
- b. {Jean} {les} {a oubliées}.

The original examples of sentences (22) and (23) (or (35) and (39a) respectively) are here wrapped in the same prosodic phrases as those used in the Ackema and Neeleman (2007) paper. As Di Cristo (1998) indicated, prosodic phrases in French operate the same way as they do in Dutch; the right edge of a prosodic phrase coincides with the right edge of a syntactic projection (for the moment ignoring the one exception, as discussed above). As you can see, in (42a), where no morpheme shows on the past participle, the direct object is in the same prosodic phrase as the past participle. In (42b), the clitic is separated from the past participle. Let us see if this idea can be generalised. Consider (37), repeated here and wrapped in prosodic phrases as (43).

- (43) *Wh-movement and past participle agreement in French*
- a. {Combien de voitures} {a-t-il} {conduites}?
 how-many of car.FEM.PL have.3SG-3SG.MASC driven.FEM.PL
- b. {Combien a-t-il} {conduit de voitures}?
 how-many have.3SG-3SG.MASC driven of car.FEM.PL

In these examples, it must be noted that the agreement takes place with *voitures*, not with *combien*, as can be deduced from the fact that the agreement does not take place when *voitures* is in the same prosodic phrase as the past participle *conduit* (as in 43b). The analysis seems to hold. Most of the examples discussed earlier in this chapter will abide by this analysis, but there are a few examples from Italian that need to be looked at, all of which are repeated here and wrapped in prosodic phrases, assuming the rules for Italian prosodic phrases are identical to that of French, as speculated by Rossi (1998).

- (44) *(Standard) Italian small clauses in prosodic phrases*
- {Arrivata Maria}, ...
 arrived.FEM.SG Maria, ...
 ‘Maria having arrived, ...’

- (45) *Direct object in situ (Italian dialect)*
 {Maria} {ha conosciute le ragazze}.
 Maria have.3SG known.FEM.PL the girl.FEM.PL

At first glance, this is immensely problematic for this prosody-based analysis of agreement morphemes. Much like Ackema and Neeleman (2007), I am inclined to suggest that Italian speakers may have a looser notion of prosodic phrases and might set their boundaries differently, and that may or may not be an acceptable thing to suggest. Ackema and Neeleman thought it was acceptable; I am not sure. For French, this prosody-based analysis seems to work, given the data discussed in this chapter, but for Italian, based primarily on the data on small clauses, it does not.

Until a better analysis operating in the same model will surface, the Boeckxian model seems to take a hit here. Depending on how the conversions go with the final case, a judgement can be formed.

5. Case III: V-to-T movement and rich agreement

The third and final phenomenon to be discussed is the cross-linguistic phenomenon of verb movement from the V-head to the T-head, allegedly correlating with the richness of a language's regular verb paradigm. When it comes to the verb movement itself, the difference is observable when looking at French and English, as shown in (46). There is a vast literature on the subject and the previously mentioned correlation with rich verbal morphology was observed as early as the eighties (Kosmeijer, 1986; Platzack & Holmberg, 1989; Holmberg & Platzack 1991; Pollock, 1989; Roberts, 1993; Rohrbacher, 1994; Bobaljik & Thráinsson, 1998; Koenenman, 2000; Bobaljik, 2003; Koenenman & Zeijlstra, 2014; and many others).

- (46) a. John <often> kisses <*>often> Mary
 b. Jean <*>souvent> embrasse <souvent> Marie
 'John often kisses Mary.

In the classic example in (46), taken from Pollock (1989), it is visible that the adverb always precedes the verb in English, whereas it necessarily follows the verb in French. The difference has been analysed to lie in the structural position of the verb. In English, the verb stays in situ; the V-position (or the *v*-position, depending on your theoretical preference), whereas the verb in French rises up to the T-position. Hence, this phenomenon is called V-to-T movement.

The theory that this verb movement goes hand in hand with a richness in verbal morphology is called the Rich Agreement Hypothesis. There have been many flavours and variants in the literature over the years, but the two that remain relevant to this day are the weak version of the hypothesis by Bobaljik and Thráinsson (1998), and the strong version by Koenenman and Zeijlstra (2014). The definition of richness generally differs per paper and per author. The third account under discussion rejects this correlation completely and it is dubbed the Rich Spell-Out Hypothesis.

5.1 The Rich Agreement Hypothesis

5.1.1 Weak RAH

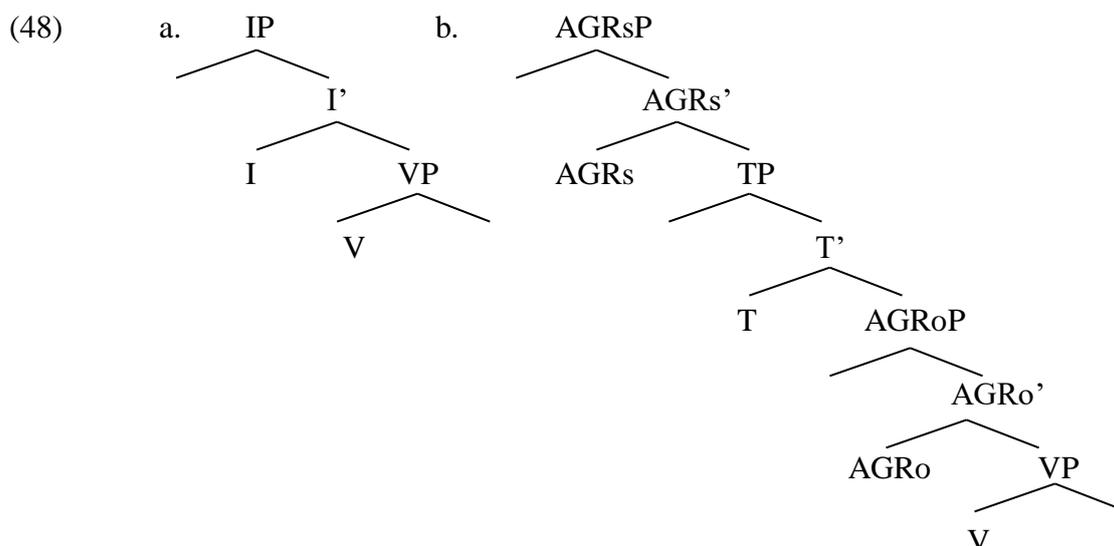
The weak version of the Rich Agreement Hypothesis, henceforth the RAH, assumes a one-way correlation between rich agreement and V-to-T movement (given in (47)) and is represented here by arguments presented in Bobaljik and Thráinsson (1998) and Bobaljik (2003).

(47) *Weak RAH*

If a language has rich inflection then it has verb movement to Infl.

(Bobaljik, 2003, p. 132)

The definition in (47) implies that poor languages can either move or remain in situ; only a non-moving rich language can falsify their hypothesis. Richness in their account is defined by the spell-out of both agreement and tense morphemes on the verbal stem at the same time. The reason for rich languages to move is then said to be reflected in the structure of IP. For English-type languages, there is no room in the structure to host two morphemes at the same time, as shown in (48a). In French-type languages, there is, because the IP is split up in three heads: AGRs, T, and AGRo (see (48b). This allows the language to express more morphemes on the verbal stem than a language with a single-head IP (as in (48a)).



An important reason for Bobaljik (2003) to assume a one-way correlation rather than a two-way one is the existence of various languages that have posed a problem for the RAH since its incarnation, such as poor languages that still seem to move the verb, like Faroese (Heycock et al., 2013), a dialect of Norwegian called Regional Northern Norwegian (Bentzen, 2004, 2005; Wiklund et al., 2007), and many others. Also data from language change seems compatible with the Weak RAH. Languages like Swedish and English used to have rich inflection at one point, which they lost over time, including subsequent loss of V-to-T movement, but there was a period of poor agreement with verb movement to be found (Platzack, 1988; Kroch, 1989).

5.1.2 Strong RAH

The strong version of the RAH was first defined elaborately by Rohrbacher (1994), and was reinstated much later by Koenenman and Zeijlstra (2014), which is the version focused on in this section. The correlation was now two-way, as in (49), making implications for both rich languages and poor languages.

(49) *Strong RAH*

Rich agreement and only rich agreement causes V-to-T movement.

(Bobaljik, 2003, p. 131)

Following this definition, only rich languages should have their verb raising up to T, whereas only poor languages should have their verb in situ. Richness, as defined by Koenenman & Zeijlstra (2014), then looks at the present tense paradigm. If the amount of different agreement morphemes used can be described with a minimum of three phi-features, the language counts as rich. The three phi-feature minimum is based on the smallest pronoun inventory found in the world, which is said to be in the language of Kuman, spoken in Papua New Guinea. The pronoun inventory of Kuman is given in (50), whereas the featural analysis is given in (51).

(50) *Kuman pronoun inventory*

	SG	PL
1	<i>na</i>	<i>no</i>
2	<i>ene</i>	
3	<i>ye</i>	

(51) *Kuman pronoun featural analysis*

<i>na</i>	→	[+SPEAKER], [-PLURAL]
<i>no</i>	→	[+SPEAKER], [+PLURAL]
<i>ene</i>	→	[-SPEAKER], [+PARTICIPANT]
<i>ye</i>	→	[-SPEAKER], [-PARTICIPANT]

As shown in (51), a minimum of three different features is necessary in order to distinguish each form in the inventory. The same would apply to any verb paradigm. Consider the paradigms of English and Icelandic, and their featural analyses, as given in (52) and (53), respectively.

(52)	a. English paradigm		b. Featural analysis
		SG	PL
	1st	<i>throw</i>	<i>throw</i>
	2nd	<i>throw</i>	<i>throw</i>
	3rd	<i>throw-s</i>	<i>throw</i>
			-s → [-PARTICIPANT, -PLURAL]
			-∅ elsewhere
(53)	a. Icelandic paradigm		b. Featural analysis
		SG	PL
	1st	<i>heyr-i</i>	<i>heyr-um</i>
	2nd	<i>heyr-ir</i>	<i>heyr-ið</i>
	3rd	<i>heyr-ir</i>	<i>heyr-a</i>
			-i → [+SPEAKER, -PLURAL]
			-ir → [-SPEAKER, -PLURAL]
			-um → [+SPEAKER, +PLURAL]
			-ið → [-SPEAKER, +PARTICIPANT, +PLURAL]
			-a → [-PARTICIPANT, +PLURAL]

As becomes clear from the paradigms, for English (52), only two features are necessary to describe all the different morphemes in the verbal paradigm, and thus the language qualifies as poor. For Icelandic (53), three features are necessary, and thus the language qualifies as rich. Needless to say, both of these languages obey the strong RAH according to this definition of richness.

5.1.3 Empirical problems for the RAH

Over the years, the Rich Agreement Hypothesis was always struggling to be accepted over the entirety of the field, due to the vast empirical database that seemed to violate it. There have been multiple languages that were deemed problematic, as well as data from language change and language acquisition that did not agree with the RAH either. Many of them were taken care of by Koenenman and Zeijlstra (2014), who have browsed through all the different accounts on many of the problematic languages, and managed to adopt, or re-analyse, until demands for their version of the RAH were met, and quite elegantly so, in most cases. As summarised in one place by Van der Veen (2016), however, there still is a lot of problematic data around for the strong RAH, some of which I shall repeat here as concisely as possible.

The first, and also the most intriguing case is that of Faroese. In both definitions of richness we have seen, the verbal paradigm of the language is classified as poor. What makes it interesting is that there are some speakers that never seem to raise the verb, whereas others can optionally do it. Koenenman and Zeijlstra (2014) adopt an analysis by Heycock et al. (2010), that perfectly suits their model. This approach essentially re-analysed all instances of

V-to-T movement to be V-to-C movement instead, with all constituents even higher in the tree moving up higher in the C-domain. This is convenient for Koenenman and Zeijlstra (2014), because this allows them to claim that V-to-T movement on its own is still triggered by rich agreement. However, Heycock et al. (2013) re-approached their initial analysis and rejected it on grounds of typology and learnability.¹⁴

In language acquisition, it is shown in multiple studies, concerning rich languages, that children go through a stage where they have acquired the movement, but not yet the inflectional paradigm (see Gathercole et al., 1999, on Spanish; Guasti, 1993, on Italian; Déprez & Pierce, 1993, on French; and Costa & Loureiro, 2006, on European Portuguese, amongst others). If rich verb morphology is indeed the trigger for verb movement, as Koenenman and Zeijlstra (2014) assume, then children should not move the verb up until they have acquired enough morphemes to be able to say they have a rich verbal paradigm. However, the fact that this is not structurally true, definitely poses an unaddressed problem for the strong RAH.

Also evidence from language change makes it difficult for the strong RAH. As mentioned in section 3.3.1.1, English and Swedish are two languages that used to be rich in verbal morphology and have V-to-T movement at the same time, but over time, the agreement impoverished, and V-to-T movement did not decline simultaneously (Platzack, 1988; Kroch, 1989; and most recently Haeberli & Ihsane, 2016). Bobaljik (2003) calls these interim-periods ‘time-gaps’ for which the strong version of the RAH cannot account. Especially Haeberli & Ihsane (2016) show, through extensive corpus research of Early Modern English, that the loss of verb movement in English, somewhere in the 16th century, occurs at a time where the paradigm still counts as rich according to Koenenman and Zeijlstra (2014) (note: not according to Bobaljik & Thráinsson, 1998), which can be interpreted as the nail in the coffin for the strong RAH.

The observant reader will have noticed that all these arguments only falsify the strong version of the RAH, but not the weak version, since that one does not make any predictions about poor languages. The truth of the matter is that there is no direct empirical evidence – to my knowledge – that falsifies Bobaljik and Thráinsson’s (1998) hypothesis. However, as stated in Van der Veen (2016, p. 7), “[t]he observation that children seem to learn V-to-I movement before they learn verbal inflection provides no reason to believe that the movement is triggered by morphology or that morphology is triggered by movement”. It must be

¹⁴ For the full analysis and argumentation, I would advise the reader to visit the paper itself.

questioned whether it is at all desirable to postulate such a correlation in light of that reasoning.

5.2 The Rich Spell-Out Hypothesis

Although it has been suggested before that the Rich Agreement Hypothesis be rejected in all its forms, as in Wiklund et al. (2007), here is yet another account that claims the same thing, and which offers an alternative in return. A short disclaimer: all of the following are things I have said before in Van der Veen (2016), but which I will repeat here, because it is relevant to the current topic. This account is based on the same strong lexicalist model initiated by Ackema and Neeleman (2007), which pushes all alleged interactions between morphology and syntax to the interfaces. Much like double agreement in Dutch, it is then assumed that all phi-agreement takes place within syntax, but that the spell-out of these morphemes takes place during Insert at the syntax-phonology interface. When the spell-out-part of phi-agreement is taken to be its separate mechanism, independent of the syntactic side of the story, a correlation with verb movement as posited by the Rich Agreement Hypothesis is by definition off the table. This approach is teasingly dubbed the Rich Spell-Out Hypothesis.

Central to this hypothesis is the assumption that all languages have rich agreement, no matter the phonology of the verbal paradigm. Rather than assuming a different morpheme for each different spell-out, as in Koenenman and Zeijlstra (2014), this allows us to assume a possibly similar spell-out for each different morpheme. Subsequently, what is often analysed as rich agreement morphology, which is only rich on the surface, we can now reanalyse it as merely rich spell-out. Considering the verb paradigm of English, which is very poor these days (see (52)), it seems undesirable to assume that there are no morphemes that separate first and second person singular, for instance, like in Italian or Icelandic. When one keeps in mind that the language used to have a very rich spell-out, the story becomes slightly more unsettling. Firstly, assuming the morphemes disappeared from the lexicon is undesirable, but assuming the spell-out impoverished over the years seems much more in tune with a biolinguistic perspective on language, since it places the audible (and visible) differences between the verb in *you work* and *I work* on the senses responsible for audible (and visible) input, rather than internal language systems. This audible input can then be traced back to phonology and, thus, spell-out. Subsequently, it assumes that notions of agreement like second person singular or third person plural are universal across languages. In other matters of phi-agreement, like the previously discussed cases of double agreement, it remains merely a matter of finding out what drives oddities in the phonological derivation.

In order to make this more tangible, consider this example from French. In French phonology, it is commonly known that the final consonants in a word are not pronounced if no vowel follows it. One could say the whole language is an example of where what is said deviates from what is written. Consider the verbal paradigm in (54a).

(54)	a. French paradigm (written)		b. French phonology	
	SG	PL	SG	PL
	1st <i>parle</i>	<i>parlont</i>	1st <i>parl-/ə/</i>	<i>parl-/ə/</i>
	2nd <i>parles</i>	<i>parlez</i>	2nd <i>parl-/ə/</i>	<i>parl-/e/</i>
	3rd <i>parle</i>	<i>parlent</i>	3rd <i>parl-/ə/</i>	<i>parl-/ə/</i>

From the verbal paradigm in (54a), it would seem that French has very rich agreement. Without having to try it, Koeneman and Zeijlstra (2014) would no doubt say that this language qualifies as rich. In Pollock's example in (46), French is also shown to have V-to-T movement, so this checks out. However, when looking at how these morphemes are produced in Colloquial French, the paradigm sounds very poor. As shown in (54b), only the second person plural retains its original pronunciation, but the rest has diminished to schwa. It must be noted that Rohrbacher (1994) has very elegantly analysed this phenomenon in Colloquial French, and has concluded that the language still counts as rich, because subject clitics, in addition to the actual subject, seem to attach to the verb in each of these cases. Thus, he says, the inflection might be poor, but the clitics have taken over its purpose. Consider example (55).

- (55) *Subject clitics in Colloquial French*
- a. *(Moi) je viens*
 I I come
 'I'm coming'
- b. *(Toi) tu viens*
 you you come
 'You're coming'
- c. *Hier, Jean (/) il est parti*
 Yesterday, John he is left
 'Yesterday, John/he left'

(Koeneman & Zeijlstra, 2014, p. 590)

As you can see, French allows these subject clitics to come up in sentences, despite there already being an overt subject. This analysis was quickly adopted by Koenenman and Zeijlstra (2014), as it should, but in the larger picture, it provides an argument for the Rich Spell-Out Hypothesis as well. As is well known, clitics are not replacements for morphemes, they are separate words that cliticise onto other words. The fact that the French paradigm is in reality poor does not negate the fact that it is written as rich, which is irrelevant to linguistic theory. Why should English be any different? The only reason why the phonology of English is not seen separately from the actual underlying morphemes is because it is pronounced as it is written, unlike French. However, we must treat every language similarly on the level of macro variation.

5.2.1 The actual proposal

The replacement theory proposed in Van der Veen (2016) relies on what is kind of a staple in phonological theory, namely Optimality Theory¹⁵ (henceforth: OT) (Prince & Smolensky, 1993). OT is a theoretical programme with a base assumption that an underlying form can have multiple surface forms, of which only one is optimal for a specific speaker. Which surface form is the most optimal is determined through a set of constraints that are ranked accordingly to favour the winner. These constraints usually consist of one faithfulness constraint, and several specified markedness constraints. The order of these constraints determines which surface form will be the most optimal form. The faithfulness constraint is violated, as it is called, for each instance where the possible surface form is deviating from the underlying form – or, for each instance where it is unfaithful to its source material, if you will. The markedness constraints pose several more conditions that may be violated, but they will have to be specified for each theory. Consider the following example. In phonology, one such constraint could be the deletion of the final nasal segment in a word. Now, if such a constraint would be preceding the faithfulness constraint in a contest between, say, the pronunciation of a word with or without the final nasal segment, the possible surface form without the final segment would win. However, if the faithfulness constraint precedes the markedness constraint, the result will be different. These contests are shown in so called ranking tables, depicting the ranking of constraints and the amount of violations of each possible form per constraint. Have a look at (56).

¹⁵ For those familiar with Optimality Theory, feel free to skip this paragraph.

(56) *Optimality Theory: A demonstration*

Underlying form: /kolen/				
	<i>input</i>	n-deletion	Faith	/e/ → /a/
	/kolen/	*		*
→	/kole/		*	*
	/kolan/	*	*	
	/kola/		**	

In the example in (56), the most optimal form, and thus the winner, is /kole/. The first form, /kolen/, encounters the n-deletion constraint, which, in this example, demands that the contestant removes the final nasal segment from the form. Any form that still has the final nasal segment violates this constraint, and, if there is a better candidate at this point, this violation is fatal for the current candidate. The third form, /kolan/, also violates n-deletion, so it also stays behind. The second and fourth forms both go on to the second constraint, which is Faith. Even though Faith is violated by both forms, /kole/ is the optimal candidate in this example, because the fourth form violates Faith twice, the second violation of which is fatal. Note that the third constraint, which demands some vowels to raise, is placed *after* Faith, which, in this case at least, plays no further role in the optimality contest, since a winner has already been picked. Needless to say, in any other situation, that constraint might have been able to break a tie.

The following analysis operates within the rules of OT, and the examples it contains should make the workings of OT clear. OT was never really a big hit in syntactic and morphological theory, but its usage in these areas has not been unheard of. Treading in the footsteps of Bresnan (1999), I proposed that for each language, there are six markedness constraints (*1sg, *2sg, *3sg, *1pl, *2pl, *3pl), along with one single faithfulness constraint, Faith. Any of the markedness constraints are violated when the form that is specified in its name does not receive its realisation at spell-out. Faith is violated for every morpheme that does not receive its distinctive spell-out. As an example, consider the rankings proposed for English; a paradigm that we all know well. In (57), the ranking is based on the knowledge that only the third person singular has an overtly spelled-out morpheme. The rest of the forms have been given a null spell-out, represented in the second column with a zero. It is assumed that the underlying form is always fully inflected, but the initial state is M >> F; that is to say *1sg, *2sg, etc. >> Faith.

(57) *Constraint ranking in English*

	<i>input</i>	*1sg	*2sg	*1pl	*2pl	*3pl	Faith	*3sg
	123456	*!	*	*	*	*		*
→	003000						*****	*
	003456			*!	*	*	**	*

Even though Faith is violated five times, considering this ranking, the second option is still the most optimal form of the three. Given the three possible surface forms - the upper of which is fully inflected, the lower of which is partially inflected, and the middle of which is poorly inflected, as in English – it is the position of Faith that determines the outcome mostly. In English, only the third person singular is visible, so only *3sg is ranked lower than Faith. The rankings of the other markedness constraints is inconsequential for the current example.

As a second example, consider the Spanish paradigm in (58) and the subsequent constraint ranking in (59).

(58) *Spanish verbal paradigm for 'to love'*

	SG	PL
1st	<i>amo</i>	<i>amamos</i>
2nd	<i>amas</i>	<i>amáis</i>
3rd	<i>ama</i>	<i>aman</i>

(59) *Constraint ranking in Spanish*

	<i>input</i>	Faith	*2sg	*1pl	*2pl	*3pl	*1sg	*3sg
→	123456		*	*	*	*	*	*
	003000	*!*****						*
	003456	*!*		*	*	*		*

As seen in (59), the fully inflected surface form is the only form not violating anything in the first constraint, which is Faith. Thus, there can only be one optimal form, which is the most faithful to the underlying form.

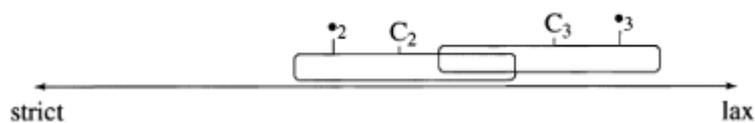
5.2.2 And the facts are for free

Now, so far this is very descriptive, and the analysis merely obeys the facts as we see them, before they are repackaged in an OT constraint ranking table, but there are two phenomena where this approach can truly shine. Firstly, there is the aforementioned language of Faroese, which poses a problem for the strong RAH, because it seems to have optional verb

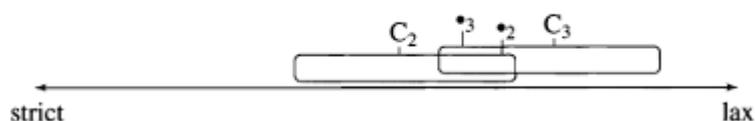
movement to T – within-speaker variation as well as between-speaker. In the model of Koenenman and Zeijlstra (2014), there is no room for optional verb movement. Their theory demands categorical results. The Rich Spell-Out Hypothesis (henceforth: RSH) does not necessarily demand similar categorical results, but an additional assumption has to be made. The verbal paradigm of Faroese is not relevant for this part; whatever morphemes follow the verbal stems will result in a ranking the present set of constraints is more than capable of coming up with. Let us instead focus on the optionality of verb movement that a single speaker of Faroese is apparently allowed to have. Let us assume two new constraints with respect to verb movement: VtoT and Stay. The initial state is assumed to be Stay \gg VtoT; an assumption that stems from what acquisition-based research has shown us.

In a model of stochastic OT by Boersma and Hayes (2000), named the Gradual Learning Algorithm (henceforth: GLA), variation between speakers (but also within speakers!) can be accounted for. Rather than assuming that the rankings of constraints are fixed, as in classical OT, the GLA assumes a continuous ranking scale with the language learner in mind. Naturally, categorical rankings are still necessary, especially with regards to the learner's input, but the gradient nature of the model allows for more flexibility when it comes to language acquisition and change; moreover, it allows room for mistakes, which can lead to an analysis of Faroese older models have not been able to consider. Consider example (60).

(60) a. *Common result: $C_2 \gg C_3$*



b. *Rare result: $C_3 \gg C_2$*



In this image, it is shown that each constraint is not a mere, flat entry, but it occupies a wider space along the ranking scale. In (60), C_2 will in most cases outrank C_3 , since it is higher on the ranking scale, but due to the overlap, it is possible for C_3 to outrank C_2 in some, albeit rare, cases. The same thing can be assumed for the constraints Stay and VtoT in Faroese, or for language learners that make mistakes, which is not necessarily relevant to current issue, but rather an additional benefit of the GLA model. Assuming input will help learners correct

their constraint ranking flows naturally with this model, where these rankings are not absolute, but gradual in nature.

The second advantage is then obviously the ability to deal with data from language acquisition and language change. In fact, Faroese is argued to be undergoing language change at this very moment (Heycock et al., 2013). Remember also all of the aforementioned problems for the RAH that come from children acquiring verb movement before they have acquired enough morphemes for Koenenman and Zeijlstra (2014) to conclude that the child has acquired rich inflection. They do not pose any problem for the RSH and its OT-based analysis. Using the GLA model by Boersma and Hayes (2000), not only can the inevitable mistakes be accounted for, but there is another problem that this OT-based analysis is causing that will immediately be solved.

The problem in question is that this model predicts patterns to occur that do not exist in natural languages, as far as we know: overgeneration. Naturally, in a model that uses constraint rankings to determine a morpheme's spell-out, it is essentially claimed that any thinkable verbal paradigms are possible, and any possible ordering of the constraints results in its own unique pattern, some of which may not occur in natural language. Thus, it is heavily overgenerating. There is, for instance, no known (SVO) language with a fully rich inflectional paradigm that does not have V-to-T movement. There is also no verbal paradigm where all morphemes are spelled-out, except for the second person plural. In a model of linguistic computation, this is undesirable. Stanton (2016), however, postulates a condition on the acceptability of overgenerated patterns. She suggests that an overgenerated pattern can be acceptable if learnability can account for its absence from natural language. For adult languages, this is impossible to attempt. I have yet to hear of a rich language with the verb structurally in situ. However, for child language, where this particular phenomenon does occur (see Gathercole et al., 1999; Guasti, 1993; Déprez & Pierce, 1993; Costa & Loureiro, 2006; and others), this can be accounted for. Assuming the constraints proposed earlier, verb movement is based on the ranking of merely two constraints. Spell-out of inflection depends on six different constraints, along with Faith. Any computer simulation will quickly confirm that it will take longer for a child to correctly organise the latter set of constraints, and thus it can be explained why children acquire verb movement before rich inflection. In fact, a simulation of exactly that nature has been run in a programme called OTSoft (Hayes et al., 2016), which has been designed to run any Optimality Theory-related simulations. The results confirmed the prediction that the spell-out of inflection takes longer to learn than a two-constraint rule like verb movement.

As for adult languages, at present, there is no explanation for the absence of rich languages leaving the verb in situ, which does leave an unpleasant mark on this theory at this time. It can be speculated that the need of inflectional information at the front of a sentence is the result of an interaction between processing cost and phonology, but for now, mere speculation is all it is.¹⁶

5.3 A Boeckxian interpretation

As is by now quite clear, the Boeckxian model and phonology-based approaches are a good match. This case is no different, and I shall tell you why.

Let us first approach the Rich Agreement Hypothesis. Despite its empirical problems and the differences between the two flavours, the question is whether Boeckx (2015) can allow this theory to exist within his new, syntactocentric model. What we need in order to establish this is some sort of way to allow a correlation between syntactic position and morphology/morphophonology to be postulated. I believe we can be quite brief, since this case is somewhat similar to the previous cases. Boeckx' narrow syntax is without lexical or morphological content, meaning that we have to turn towards the interfaces once again.

Starting with V-to-T movement, it has to be noted that, although the nodes do not project in narrow syntax, categories like V and T carry only conceptual weight at this point, and not structural weight; they are assigned to pre-existing structure after the derivation has been transferred to the conceptual-intentional interface. If a language is analysed to have its finite verb in the T-node, it was simply re-merged higher up in the tree than in situ-languages. Since we have established that narrow syntax does not care about word order, this is translated as a language-specific interface demand at either LF or, most likely, PF. This is, to remind ourselves, because narrow syntax does not feature any lexical content through which it can determine V-to-T movement deliberately. Ergo, the demand must come from somewhere else. It is likely that word order is a phonological demand, since that is where sentences are pronounced. Perhaps the terminology should be revised in such a case, since phonology does not care for projections either. A simple rule could formulate the demand, like in (61), but keep in mind it is just an example, not a proposal of the actual rule.

¹⁶ Literature on the processing cost of inflection exists and is vast, however, here is not the time nor the place. For now, it remains a matter of hypothesis and speculation only. Future research might look into this more specifically.

(61) *(Example) PF-demand*

The finite verb must precede the adverb in a declarative sentence.

Since such rules would necessarily be language-specific, and we are not discussing any specific languages, I shall not attempt to defend the demand in (61), but the idea behind it should be clear.

Likely, the difference in morpheme spell-out is also something occurring at the phonological side of computation, as we already determined in chapter 3. In this model, it is impossible to assign separate spell-out rules to different syntactical nodes, since the derivation is flattened by the time morpho-phonological processes have begun. Following that, it is likely to be assumed that the outcome of Agree is manifested purely in spell-out, not in the supposed absence or presence of morphemes, since those are required for the more abstract consequences of Agree. Naturally, such reasoning is direct support for the theory I have proposed in Van der Veen (2016), but a more theory-neutral view that must be considered as well involves morpho-syntax. For during morpho-syntax, which takes place after narrow syntax and before morpho-phonology, it can well be assumed, if it has your theoretical preference, that a specific language simply does not have the morpheme required by the Agree-relation. Subsequently, no morpheme results in no spell-out, so the result is the same. My own view on this is very clear: I prefer the spell-out based approach.

In all fairness, the question was whether a correlation between rich agreement and V-to-T movement can still be assumed, and even though some of the terminology is irrelevant in Boeckx' (2015) model, the concepts, after translation, still stand. If you wish, the Rich Agreement Hypothesis can still be postulated. Granted, V-to-T movement should now be reanalysed as a mere word order relation, but as a mere descriptive term, it will suffice. If it somehow becomes possible for this theory to circumvent all the empirical problems the RAH faces, it can still be defended. Admittedly, the theory is not as elegant anymore due to the separation of syntax and word order, which was the basis for it being proposed in the first place.

The Rich Spell-out Hypothesis does not suffer as much from the translation to the new model, since it already relied on the interfacing components quite a lot. V-to-T movement was taken as separate and independent from Agree, which does not need to change. The spell-out of morphemes also easily integrates into this new system, because it is manifested in phonology, where spell-out was applied already in previous models. Naturally, as indicated in

the theory-specific section, it still faces some unexplained overgenerated patterns, but future research might come up with an explanation for that.

To wrap this section up, despite any theoretical preferences one may have, the application of Boeckx' (2015) model is no reason alone to reject either of these theories.

6. Conclusion

Going back to the purpose of these analyses of different phenomena, it was important to see whether Boeckx's (2015) model could be applied to empirical issues within theoretical linguistics. After all, conjuring up a conceptual model is important, but in order for other linguists to join in on the enthusiasm of a so-called 'biologically adequate' version of minimalism, it has to be tenable for language-specific research as well.

Some of Boeckx' goals were to minimise the demands on narrow syntax and UG, even more than minimalism already did. He wanted the removal of the lexicon as a key component in the generative model. Lexical items came with features, subcategorization demands, and theta roles to give away, to the point where ultimately the lexicon could dictate exactly what the syntax should do. Boeckx (2015) called this lexicocentrism, and blamed its rise to fame in minimalist literature to a lack of focus on Language (note the capital L), as opposed to the study of languages. He thus vouched for a model that did not focus on language-specific elements of language, in order to get as close as possible to the Faculty of Language, as part of the biology of a human. In other words, he wanted a model that was biologically adequate, as well as attempting to go beyond explanatory adequacy. This could only be obtained if there was a skeletal version of narrow syntax so non-language-specific that all possible languages could be derived from it.

In the first of three cases, I looked at different theories on the phenomenon of double agreement in Dutch second person singular verbs. The issue at hand was a different agreement spell-out for subject-verb orders versus verb-subject orders. One of the theories, by Postma (2011) and Van Alem (2017), focused heavily on syntactic features in an attempt to explain why the difference could occur. The other, by Ackema and Neeleman (2007), instead focused on phonology to explain said difference, and they came up with an analysis based on prosodic phrases. After discussing both theories in detail and analysing them critically, I hypothesised that the model by Boeckx (2015) would likely not allow for the syntax-based analysis, due to its heavy dependence on features, syntactic projections, syntactic movement, and an interaction thereof. The phonology-based approach by Ackema and Neeleman was much easier to convert to the Boeckxian model, due to its non-reliance on the syntax, and its internal philosophy that word order and phonology are connected in a non-syntactic way.

The second case explored the phenomenon of past participle agreement in French, and, to a lesser extent, varieties of Italian. This variant of agreement manifested itself on past participles whenever object clitics or subjects moved higher up in the tree, passing the past participle in the process. First, we looked at Kayne's (1975) descriptive analysis of clitic

behaviour in French, and saw how clitics showed behaviour much different from full-fledged NPs. With Lefebvre (1988), the first analysis of past participle agreement is looked at, and she suggested the agreement on the past participle is a sign of co-case marking, suggesting the link with the case receiver is maintained through a case chain. Kayne (1989) then suggested that participle and object clitic establish their past participle agreement through a spec-head relation in a phrase called AgrPstPrtP. Finally, Belletti (2017) adds her two cents on the matter by comparing French past participle agreement with that of varieties of Italian, and finds interesting differences that require a new analysis. She attempts a minimalist view on the matter, using the operation of Agree. Ultimately, since all of these analyses fail to cover every example, and remain rooted in syntactic features, projections, and movement, I concluded a different analysis had to be made if the Boeckxian model would have an answer to this issue. I attempted to copy Ackema and Neeleman's (2007) prosodic phrases approach and apply it here. Much of the data seemed to fit prosodic patterns, but unfortunately not all, which means the Boeckxian model fell short in this case.

In the final case, I looked at the Rich Agreement Hypothesis and contrasted it with a recent proposal by myself, ironically titled the Rich Spell-Out Hypothesis. The Rich Agreement Hypothesis (RAH) in its strongest (and for now most relevant) form states that verb movement from V-head to T-head can only be licensed by 'rich' agreement in the verbal paradigm. The definition of rich differed per proposal, but Koenenman and Zeijlstra (2014) stated that a paradigm is rich if and only if all of its morphological distinctions can be captured with minimally three features. The RAH focused heavily on syntactic movement and morphological 'richness' interacting. It also needs mentioning that evidence from language acquisition and language change, amongst others, did not obey the RAH. The Rich Spell-out Hypothesis (RSH), about which I have previously written in Van der Veen (2016), stated that morphology is not rich, but the spell-out is, thus refuting all interactions between syntax and morphology on the matter. Using a model of Optimality Theory called the Gradual Learning Algorithm (Boersma & Hayes, 2000), it could be demonstrated that this approach towards the matter could, in fact, account for previously problematic data from language change and language acquisition, and even for within-speaker variation and mistakes. On the flipside, the approach heavily overgenerated, predicting patterns that do not occur in natural language. For the Boeckxian model, a focus on spell-out is preferred over an interaction between syntax and morphology. However, the RAH could easily be translated into an interaction between syntax and morpho-syntax, if it is assumed that certain demands for certain languages have to be met at the interfaces, as Boeckx (2015) does. The RSH merges seamlessly with the Boeckxian

model as well, which means that no ‘winner’ could be determined based solely on the adoption of the new model by Boeckx.

Looking back at all three cases, it is notable that the first pushes us into a certain direction, the second leaves us with no feasible theory, and the third gives us a choice. What does this tell us about the model by Boeckx (2015)? In a way, the results are in favour of the new model, if you agree with its conceptual tone and its more-minimalist-than-thou message. If you look at the third case once more, the very fact that the implication of this model merely shifts the tone of the debate, but does not render either approach we discussed as obsolete, is a good thing. This means that the suggested theories cannot be excluded on conceptual grounds, but must instead be judged on their empirical reach, as they would in any other model. For the second case, no suitable theory was found, which seems undesirable, but it must not be neglected that the existing theories fell flat even in their own, native frameworks, let alone in basic minimalist models. Again, the implementation of the Boeckxian model has no consequences to this debate, other than shifting its tone. Only when looking at the first case should we ask ourselves whether the effect of the model is at all desirable. Here, one theory was rejected because it didn’t fit the model, whereas the other was accepted on similar grounds, even though neither theory was stronger than the other on empirical grounds. It could and should be asked whether the tone of the debate should be allowed to shift when it has the consequences of rejecting a school of thought. Then again, that is exactly what Boeckx (2015) attempted to do. The theory of Postma (2011) and Van Alem (2017) had to be rejected purely in light of the new model, and, as such, on conceptual grounds. As Ackema and Neeleman (2007) said, ironically, also their competitors in this particular debate: one theory can only be preferred over another on conceptual grounds if all else is equal. Given that line of reasoning, Boeckx’s (2015) model can, upon implementation, indeed be used to reject this particular theory.

All in all, how much would this model really change if it were to be implemented on a larger scale? The consequences for conceptual literature and, if Boeckx (2015) is to be believed, collaborations with biological research into language are clear; with a state of the Faculty of Language as described by Boeckx, UG is poor enough to be of significance to non-linguists and theoretical linguists alike. For purely theoretical linguists, not a lot will change, but merely the reasoning will. Many concepts from weak minimalism can be translated as post-syntactic operations or interface demands, as we have seen in the case chapters, but in truth, not many theories will need to be changed or thwarted because of it. Some will, as we have seen in chapter 3, but only when all else is equal. Of course, given the widespread

influence that Chomsky's minimalism still has, the chances of a new model overtaking it completely is not something I would consider to be likely. Instead, it seems more probable that Boeckx's model remains confined to the circles of conceptual linguistics. If anything, it has been shown in this thesis that the model is more than capable of handling empirical research and contributing to their respective debates. In that light, perhaps it is about time someone put the reset button on the linguistic landscape again, some odd twenty years after Chomsky did the same with the Principles and Parameters programme. Minimalism has had a good run, perhaps Boeckxianism is where it is at now.

7. References

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