

# Morphology is in the eye of the beholder

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## 1 Introduction

The distinction between syntax (phrasal structure) and morphology (word structure) is well established in both descriptive and theoretical linguistic thinking. Even though there are various proposals in the linguistic literature that argue for eliminating the distinction (Selkirk 1982; Lieber 1992; Ackema 1995; Siebert 1999; Halle and Marantz 1993, etc.), none of these can really escape the distinction between word structure and phrasal structure and each must make special arrangements in its syntactic model to accommodate word structure and the properties that set it apart from phrasal structure.

In this paper, I argue that the idea behind proposals to unify syntax and morphology is essentially correct: there is indeed only a single computational system combining abstract symbols into larger structures. The empirical differences between word structure and phrasal structure that we observe in linguistic forms, and which are the reason why the authors mentioned in the previous paragraph all need to assume special syntactic operations that cater specifically to word structures, should, I believe, be explained through the interaction of syntax with phonology.

Put differently, I claim that the distinction between morphology and syntax is to some extent an optical –or rather, acoustic– illusion: it is not the structure-building mechanism that determines whether a given structure is morphological or syntactic. Rather, it is the phonological structure of the form onto which the output of the structure-building computation is mapped that lets us *perceive* a given structure as morphological or syntactic.

The model does not leave any room for rules or operations that only apply at the word level by definition. It does leave room for specifying idiosyncratic properties of lexical items in terms of their phonological structure or in terms of their morphosyntactic feature makeup. Standard phonological and syntactic mechanisms, combined with principles governing the mapping between syntax and phonology, then ensure that the structures come out the way we observe.

One consequence of the proposal is that certain generalisations and constraints that we observe in language are imposed by external systems. These include restrictions placed on the grammar by the modality in which it is expressed (e.g., the requirement of linearity), restrictions imposed by the parser (e.g., recoverability of the base position of a displaced element),

and generalisations and constraints resulting from the correspondence between semantic and syntactic structure and between syntactic and phonological structure.

While the latter type of restriction can still be considered grammatical, since the mapping between semantics, syntax and phonology is part of grammar in the broad sense, the former type of restriction is not grammatical. Rather, restrictions of this kind follow from what Chomsky has called *interface effects* or *third-factor effects* (i.e., from “principles of structural architecture and developmental constraints that are not specific to the organ under investigation, and may be organism-independent” Chomsky (2008, p. 133)) or the Faculty of Language in the Broad sense (FLB, Hauser, Chomsky, and Fitch (2002)). They do not constrain the grammar of an I-language directly, rather they constrain the development of an I-language.<sup>1</sup>

## 2 Syntax-morphology synthesis

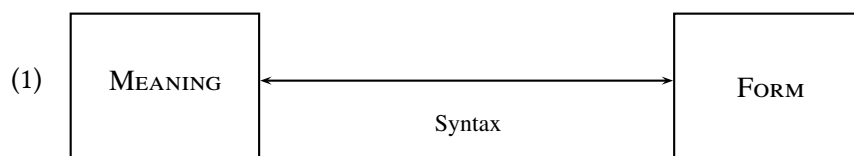
Lieber (1992, p. 21) states that:

[t]he conceptually simplest possible theory would (...) be one in which all morphology is done as a part of a theory of syntax (...) A truly simple theory of morphology would be one in which nothing at all needed to be added to the theory of syntax in order to account for the construction of words.

It is certainly true that the most elegant theory possible would not distinguish between a syntax module and a morphology module. There are, in fact, several other conceptual reasons for preferring a theory with a unified syntax-morphology module. Even though none of these arguments is really fatal to a two-module theory, they do show that if we are able to formulate a unified theory that is not less elegant than a two-module theory, we should prefer the unified theory.

### 2.1 Form-meaning pairing

The idea that language is form with a meaning goes back to Aristotle. The individual signs (the forms) have a particular meaning, but when signs are combined, it is the syntactic structure that links the form of such a combination to its meaning. Syntax, in this view of language, is the glue that mediates the pairing of form (phonology) and meaning (semantics). We can visualise this situation as in (1):

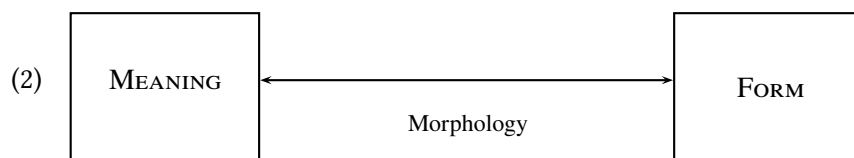


Interestingly, we can repeat the previous paragraph, replacing the word “syntax” with “morphology”: The idea that language is form with a meaning goes back to Aristotle. The individual

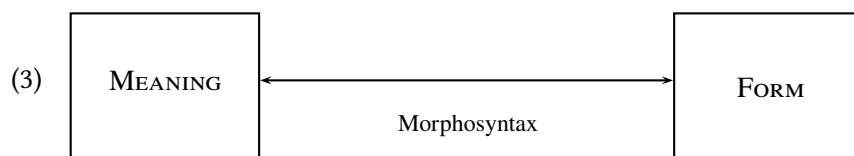
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<sup>1</sup>And in evolution they may have constrained UG, although that is mere speculation.

signs (the forms) have a particular meaning, but when signs are combined, it is the morphological structure that links the form of such a combination to its meaning. Morphology, in this view of language, is the glue that mediates the pairing of form (phonology) and meaning (semantics). We can visualise this situation as in (2):



What this means is that from a bird's eye view, syntax and morphology perform the same function: they link form to meaning. From such a perspective, it seems strange to even assume there should be two different modules. It would make much more sense to have just one module responsible for both word structure and phrasal structure:



I will refer to the single morphology/syntax module as “morphosyntax” or simply “syntax”. The obvious bias toward syntax in this nomenclature should not be taken as an indication that syntax is more important than morphology. It is simply the case that given the conventional connotations of these words, “syntax” more easily generalises to a hyperonym than “morphology”.

## 2.2 Distinguishing structures

In traditional terms, the difference between morphology and syntax lies in the kinds of structures that they deal with: morphology operates below the word level, syntax operates from the word level upwards. The problem with such a definition of the distinction is that the term “word” is not defined. Syntax deals with heads, not with words, but we cannot say that morphology “operates below the head”, because the standard view is that morphology operates on heads as well. Many of the heads that syntax deals with are realised overtly as affixes or merely as features of some word form (e.g. Infl or T/Asp in Germanic, Romance, Slavic, etc., C in languages with clitic complementisers, D in languages with clitic determiners, K (or Case) in languages with overt case morphology, etc.)

That is not to say that there have not been attempts to treat morphology as ‘sub-head’ syntax. Ackema (1995) argues that morphological structures should be analysed within X-bar theory as projections of negative X-bar levels, i.e., projections from  $X^{-2}$  to  $X^0$ . Similar proposals are found in Selkirk (1982), who proposes a distinction between  $X^\circ$  and  $X_{Af}$  elements, and Lieber (1992), who argues that X-bar theory should be extended with a recursive projection rule at the  $X^\circ$ -level of the form  $X^\circ \rightarrow X^\circ$ .

The general problem with such proposals is that even though they claim to do away with the distinction between syntax and morphology, they reintroduce the same distinction by adopting special constraints and principles in the syntactic component that only apply to word-level structures. Negative X-bar levels, or a recursive  $X^\circ$  level are nothing more than morphological structures in a “syntactic” jacket.<sup>2</sup>

The very influential theory of Distributed Morphology (DM; Halle and Marantz (1993) and much subsequent work) also assumes “syntactic structure all the way down”, i.e., that morphological structures are essentially syntactic. However, even though DM does not assume a separate morphology module, there are still a number of so-called “morphological” operations such as fission, fusion, impoverishment, etc. In other words, it suffers from the same problem: there is a special set of operations applying to word-level structures (complex heads, cf. the discussion below), that is located in a specific part of the grammar.

Note that the point is not that syntactic operations that only apply at lower levels in the structure (i.e., at the “word”-level) are impossible. Such operations may well exist.<sup>3</sup> There is no reason to assume that they are localised in a single place in the grammar, however. In DM, these operations apply at the end of the syntactic derivation, right before Vocabulary Insertion (cf. Harley and Noyer 1999). If they are truly syntactic, however, they should be able to apply throughout the syntactic derivation, not just at the end of it.

For Ackema & Neeleman (Ackema and Neeleman 2007), the fact that no theory can really do without separate word-level rules is an important reason for assuming that there is a separate morphology module. However, such an approach has to answer the question which structures are generated by the morphology module and which by the syntax module, or in other words, it has to define the term ‘word’. Intuitively, it is not difficult to distinguish between words and phrases in most cases, but as Haspelmath (2011) shows, there is no general, cross-linguistic property that distinguishes words from phrases.

This is not to say that there have been no attempts at defining the notion “word” in the literature. One attempt is made by Embick and Noyer (2001), working within DM. Embick & Noyer’s (2001) proposal captures what most would probably consider the most straightforward and intuitive way of defining the word. Specifically, Embick and Noyer (2001) define two notions, the *Morphological Word* and the *Subword*:

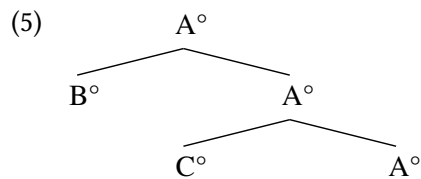
- (4) a. *Morphological Word*: At the input to Morphology, a node  $X^\circ$  is (by definition) a morphosyntactic word (MWd) iff  $X^\circ$  is the highest segment of an  $X^\circ$  not contained in another  $X^\circ$ .
- b. *Subword*: A node  $X^\circ$  is a subword (SWd) if  $X^\circ$  is a terminal node and not an MWd.

The problem with this definition is that it presupposes that we are able to tell when a specific node in the tree is “the highest segment of an  $X^\circ$  not contained in another  $X^\circ$ ”. This, however, is only possible by stipulation. The kind of structures that Embick and Noyer (2001) have in mind is represented in (5):

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<sup>2</sup>Cf. Siebert (1999) for a similar argument.

<sup>3</sup>Although I would expect that the restriction to lower-level structures is due to external factors, not because of a prohibition inherent in the rule itself.

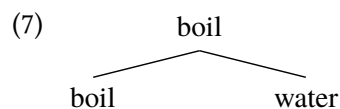
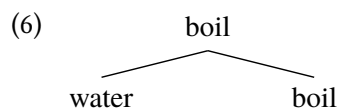


Here, the idea is that the top  $A^\circ$  node corresponds to a word. However, in a framework that adopts Beard's (1988) *Separation Hypothesis*, which DM does in the form of *Late Insertion*, there is no way to determine on the basis of the syntactic tree which nodes are  $X^\circ$  nodes and at which point an  $X^\circ$  projects into an  $X'$ . That is, there is one way to do this: Chomsky's (1995) *bare phrase structure* proposal. The point is that with structures such as in (5), bare phrase structure yields the wrong results for Embick & Noyer's definition.

In bare phrase structure, an  $X^\circ$  category is a minimal projection, i.e., a piece of structure that is not projected from another element (in other words, not composed by Merge). Similarly, a maximal projection is the highest node in a projection line. For the structure in (5), this means that B and C are indeed  $X^\circ$  categories, and so is the *lowest* A node. But the other two A nodes are non-minimal, i.e., they are *not*  $X^\circ$  categories. The highest A node could in fact be a maximal projection, if it does not project any further.

In other words, according to Embick & Noyer's definition, only the bottom A node is a morphological word, which is of course not what they want to say. Furthermore, the notion of "subword" becomes a contradiction, because every  $X^\circ$  is a MWd.<sup>4</sup>

To make the problem a bit more explicit, consider the following two structures:



(6) is the base for the word form *water boiler*, while (7) is the start of a derivation that leads to a verb phrase (*to*) *boil water*. The only difference seems to be the order in which both words appear, but this is not a property we would like to rely on, obviously: it will hardly do to argue that morphology branches to the left while syntax branches to the right. Besides, it is by now a common assumption that syntactic structures do not define linear order.

The point here is that when two heads such as *boil* and *water* are merged, the structure is still indeterminate: it could be the start of a derivation yielding *water boiler*, but it could also yield *to boil water*.<sup>5</sup> A possible response to this problem might be to argue that the trees should

<sup>4</sup>Note that this technical argument applies only to systems that assume no separation between syntax and morphology, such as DM. However, the point that it is not possible to reliably distinguish between words and phrases is independent of the theoretical framework one chooses, as Haspelmath (2011) shows. That is, lexicalist theories and theories such as that of Ackema and Neeleman (2004) face the same problem, but it turns up as the problem of deciding which structures are built by which module.

<sup>5</sup>A reviewer asks why the merger of *water* and *boil* cannot be the basis for a compound verb *to water-boil*. I assume that this is simply a peculiarity of English. A single morphosyntax module does not entail that there can be no idiosyncrasies in the morphosyntax of a language.

properly be represented as in (8) and (9), although it should by now be obvious that this does not bring us anything:



Such a representation simply begs the question. The zero-level projection marker and the “VP”-label are really just mnemonic devices reminding us of the kind of structure that we are dealing with: a “morphological” and a “syntactic” structure, respectively. One can of course raise such mnemonics to the status of theoretical devices, but that does not change the fact that they are there to make a distinction that does not follow directly from the properties of the elements involved: the fact that we combine an N and a V head does not tell us whether we are dealing with morphology or with syntax.

This is a point that is well worth dwelling on for a second: our model of grammar is based on the intuition that there is a distinction between the word-level and the phrasal level. This notion pervades our thinking about language, it guides our analyses every step of the way. It is reflected in our descriptive machinery, which we use without realising that something like “V<sup>o</sup>” or “VP” is nothing more than a mere description of what we observe, it is not an explanation.

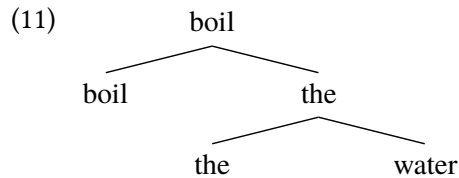
The existence of languages with elaborate and highly regular morphology emphasises this point, as Jackendoff (2002) also points out. Take the Classical Nahuatl word form *ahōniccuah*, which contains the following morphs:

- (10) ah-   ō-   ni-   c-   cua- h *Nahuatl*  
       NOT ANT 1sg.S 3sg.O eat PERF  
       ‘I have not eaten it.’

The single Nahuatl ‘word’ *ahōniccuah* corresponds to a five-‘word’ sentence in English. There is no sensible way for us to claim that the Nahuatl word is constructed through mechanisms fundamentally different from the English sentence. Semantically, the two utterances must express the same structure, and it would be unlikely if the language faculty were to contain two fundamentally different ways to do the same thing: relate semantic structures to phonological structures and vice versa.

Ackema’s (1995) proposal and the others mentioned above are compatible with this point, in that they argue that there is no distinction between syntactic and morphological operations. The problem remains, however, that they cannot explain why the Nahuatl ‘word’, while built by the same types of operations, still looks different from the English sentence. Their explanation is that there are simply different kinds of elements to which the operations apply:  $X^{<0}$  and  $X^{\geq 0}$ . But this is merely begging the question, a notational device introduced to describe the difference. If we drop the notational device, we have no way of deriving the empirical differences.

The only difference that seems empirically true is that the syntactic tree but not the morphological tree can be expanded with functional elements. A functional element such as *the* may not appear in a morphological tree. That is, in syntax, but not in morphology, we may have the following:



That is to say, *in English*. There are, however, many languages with affixal determiners. Similarly, can we really say that the Nahuatl example in (10) does *not* contain functional structure? But if it does, how are we to make sense of this fact? The usual assumption, that the affixes are either a morphological reflex of syntactic features on the lexical head, or that the lexical head moves to the functional heads to “pick up” the affixes, is inspired by our preconception that morphology and syntax are different. It follows from the assumption, it does not prove it.

Bare phrase structure also forces us to define more precisely what the term *head* means. Traditionally, the term *head* refers to the ‘projecting element’ in a node. In Government & Binding theory, the term *head* has come to mean ‘an indivisible unit in syntax’, i.e., a terminal element in the syntactic tree. In most cases, the difference between the two is inconsequential, primarily due to X-bar theory. The ‘terminal element’ definition of *head* leads to distinctions such as that between XP-movement and head movement, and it leads to the idea that heads can be *complex*, i.e., consisting of multiple  $X^{\circ}$  elements. However, with bare phrase structure, we cannot afford to be this imprecise. The point is that the ‘terminal element’ meaning of *head* in syntax, which used to be a primitive of the theory (meaning that we can stack heads and have complex  $X^{\circ}$  elements), is not a primitive notion anymore, but a *derived* one. A head is a syntactic structure that is simplex, i.e., not created by the operation Merge.

One (far-reaching) consequence of this is that we can no longer rely on the notion of *head* in our syntactic analyses: we cannot exclude a certain syntactic operation from applying to a phrase merely by stating that said operation can apply to only heads or, vice versa, we cannot exclude a syntactic operation from applying to heads by stating that it can only apply to phrases. Syntactic operations are only sensitive to the feature makeup of the elements involved. Without performing an *additional* check, the syntactic component does not know if a certain structural object is a head or not.<sup>6</sup>

What this means is that heads may be complements (and specifiers). In a bare phrase structure approach, this is essential, because every derivation starts out with the merger of two heads, one of which must be the complement of the other. But it has a further consequence: we can no longer rule out a phrase moving into or adjoining to a head position. This consequence is rarely (if ever) acknowledged in generative syntax, but it *is* a consequence. We will therefore need another way of ruling out unobserved operations such as whole DPs moving into  $V^{\circ}$  positions.

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<sup>6</sup>Again, this is a technical argument that does not hold for lexicalist theories, but see footnote 4.

Note that this is not just a problem of some far-out theory of syntax. It is a real problem that is just obscured by our preconceptions. We intuitively feel that there should be a difference between syntax and morphology, that is, between operations applying to phrases and operations applying to words. Because of this intuition, we have constructed our models in such a way that they reflect this distinction. But that does not mean that the distinction is there, at least not in the way that our models suggest.

### 2.3 Making sense of the intuition

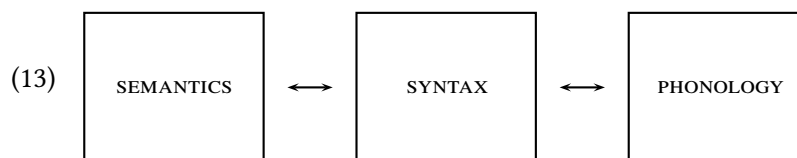
I do not wish to claim that the entire distinction between syntax and morphology is an illusion. There *is* a distinction, it is just not there where we suspect it. Our intuition that there is a difference between the level of the “word” (however vague that notion is) and the level of the clause is in some way correct, and our model of grammar should reflect it. It should just reflect it in a different manner than we are used to.

As mentioned above, a linguistic sign is a pairing of form and meaning. In modern linguistic thinking, this means that a linguistic sign consists of three sets of features: semantic, syntactic and phonological.<sup>7</sup> I assume that the semantic, syntactic and phonological structures are built up in parallel, each in its own module, with each module dealing only with the features relevant to it.<sup>8</sup>

In line with this view of the grammar, a lexical item can be represented as a three-way association of features:

$$(12) \quad \lambda x(\mathbf{car}(x)) \leftrightarrow \begin{bmatrix} \text{N, sg} \\ \text{count} \end{bmatrix} \leftrightarrow /kaɪ/$$

Important in this representation is that the three groups of features are separate, which is why they are visually separated here. Analogously, we may represent the grammar model as in (13):



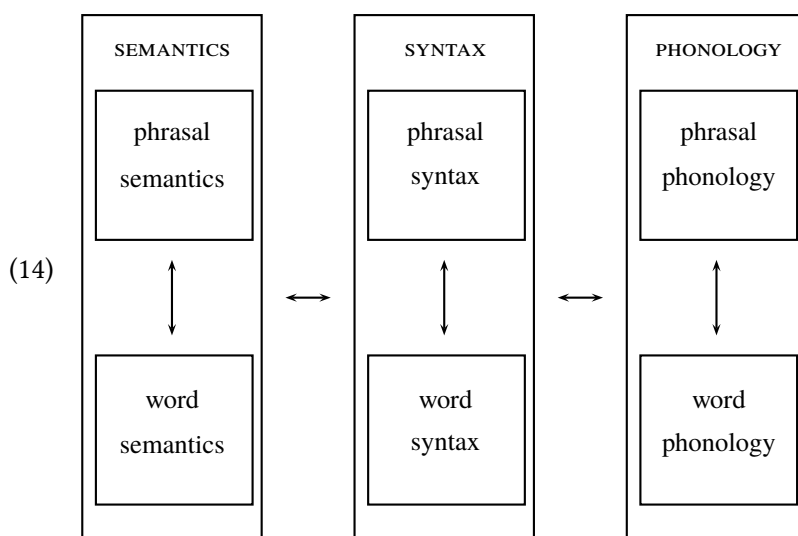
The representation of the grammar and of lexical items employed here is inspired by the grammar model developed by Jackendoff (1997, 2002).<sup>9</sup> Ackema and Neeleman (2004) adopt this model and explicitly discuss where morphology is located. They assume that each module has two submodules, one for phrasal structures and one for word structures:

<sup>7</sup>Cf. Chomsky (1965) ((1965), p. 214, fn. 15, cited in Freidin (2003)): “We might, then, take a lexical entry to be simply a set of features, some syntactic, some phonological, some semantic.”

<sup>8</sup>This seems in contradiction with common assumptions in minimalist syntax, which hold that syntactic structure is built up in phases, with each phase ending in the transfer of the phase complement to the interfaces. Although I do not believe there is a real contradiction, the issue is beyond the scope of the current paper. See Kremers (2014a) for discussion.

<sup>9</sup>Although I do not subscribe to all the details of Jackendoff’s (2002) model.





However, if we step back and think about this for a moment, one question immediately presents itself: why do we need to distinguish a word level box and a phrasal level box in each of the macromodules? It is not at all clear why the semantics box would need it. Whether we use a word such as *ahōniccuah* or a phrase such as *I have not eaten it*, or whether we say in English *unbearable* or in Arabic *lā yuḥmal* (lit. ‘it is not borne’), the semantics is the same. That is, *if* there were two semantic submodules, both modules would have to be able to express the same meanings.

Looking at it from a different perspective, the point becomes even clearer: how could we decide whether a semantic formula such as  $\lambda(x)(\neg\text{bear}(x))$  (for *unbearable*) is a word-level or a phrase-level structure? There is, in fact, no way to tell, because the distinction does not make sense in semantics. In other words, the semantics box can and *must* do without the two submodules.

Furthermore, given that we do not want to assume that the structure-building operations behind *I have not eaten it* are fundamentally different from the structure-building operations behind *ahōniccuah*, we should ask ourselves why we would want to assume two submodules in the SYNTAX box. Or, to phrase that question a bit more generally: what is the *actual* basis for making such a distinction? Why do we say that *boil water* is fundamentally different from *water boiler*?

The directly observable differences between syntactic and morphological structures are phonological in nature. That a *hót-dōg* is different from a *hót dóg* is reflected in the stress pattern. That *boil water* differs from *water boiler* is obvious from the word order,<sup>10</sup> the phonological element /əɪ/ and perhaps also the stress pattern. We say that *-er* is a suffix and thus belongs to morphology because of its requirement to attach to a stem. But this requirement is really a *phonological* requirement: it is the syllable /əɪ/ that must attach to a prosodic word in order to be phonologically licit.

<sup>10</sup>Which, in current minimalist thinking, is a PF phenomenon, cf. Chomsky (1995), Nunes (2004), Richards (2004), Fox and Pesetsky (2005), Kremers (2009), etc.

We have no direct access to the SYNTAX box in (14). Our assumption that it is divided into a word level and a phrase level box is based solely on observations made through the phonological system of language, i.e. on our observations of the phonology box. What is more, the distinction is very old and dates from a time when the structure of the phonology box was not very well understood.

Nowadays, however, we know that the prosodic structure of an utterance is essential for shaping its phonology. It is generally recognised that there is a *prosodic hierarchy* in phonology (cf. Selkirk (1981, 1984, 1995), Nespor and Vogel (1986), Truckenbrodt (1995)). This hierarchy has the Utterance at its top, and the syllable at its bottom:<sup>11</sup>

- (15) Utterance (U)  
 Intonational phrase (IntP)  
 Phonological phrase ( $\varphi$ )  
 Prosodic word ( $\omega$ )  
 Foot (Ft)  
 Syllable ( $\sigma$ )

The prosodic hierarchy is subject to the *Strict Layer Hypothesis*, which essentially states that the hierarchy is exhaustive and non-recursive. Each element at level  $n$  is completely contained in an element at level  $n+1$ , while containing one or more elements of level  $n-1$ . That is, an utterance contains one or more intonational phrases, and each IntP is contained in exactly one U (which it may share with other IntPs). In turn, each IntP contains one or more phonological phrases, etc. Furthermore, an element at level  $n$  cannot contain another element of level  $n$ . A prosodic word may not contain another prosodic word, a  $\varphi$  cannot contain another  $\varphi$ , etc.<sup>12</sup>

In the phonological literature, it is assumed that there are rules governing the mapping of syntactic structure onto prosodic structure. One of the most fundamental rules is that a (lexical) XP in syntax corresponds to a  $\varphi$ . These correspondence rules are not absolute, that is, there are cases where the syntactic structure and the phonological structure do not line up. This is in part due to conflicting correspondence rules, in part to the fact that syntactic structure is recursive, while prosodic structure is not, and in part to the fact that after the correspondence rules establish an initial prosodic structure, readjustment rules can rearrange the boundaries of prosodic constituents.<sup>13</sup>

Phrase-level syntax maps primarily onto the  $\varphi$ -level in prosodic structure, to a lesser extent also onto the level of IntP. Morphology maps onto the levels of the prosodic word, the foot and the syllable. *Well, obviously*, one is tempted to say, because morphology operates below the

<sup>11</sup>Although the prosodic hierarchy is a staple of phonological theory, it has been argued to be a mere notational device. Scheer (2008), for example, argues that the only notion that is theoretically real is the *boundary*. I do not believe Scheer (2008)'s idea would be problematic for the current proposal. Some of the details will need to be updated, but the basic idea is not affected.

<sup>12</sup>Here, I have factored out the Strict Layer Hypothesis into two constraints. Sometimes it is argued that it actually comprises more than two constraints. For example, Selkirk (1995) assumes four. Note that in Optimality Theory approaches, the principles are assumed to be violable. These issues are not relevant to the point at hand, so I ignore them here.

<sup>13</sup>Interestingly, Scheer (2008) argues that such mismatches are artefacts of the prosodic hierarchy and that they disappear when one assumes boundaries only. Again, the basic idea of the argument is not affected by this.

word level. But is this so obvious? I believe not. We should really be looking at it the other way around: it is not the structure-building component (either syntax or morphology) that determines which prosodic levels its output is mapped onto. Rather, it is the prosodic level on which the effects of a certain operation are visible that directs –or rather *squints*– our view of this operation as syntactic or morphological.

Consider this for a moment. *Ahōniccuah* is a single prosodic word and is therefore considered a morphological structure. *I have not eaten it* contains (at least) two prosodic words and is therefore considered syntactic. That is, there is nothing inherent to the structure-building operations behind *ahōniccuah* that force it to come out as a “morphological” structure. It is the phonological form that the structure-building operations map onto that prompts us to view it as a “morphological” structure.

Adopting this view leads us to an interesting conclusion. One crucial property of the prosodic hierarchy in (15) is that it is continuous: there is no break between any two levels. That is, if it is indeed the phonology that determines our view of the structure building component, i.e., of the SYNTAX box in (14), then we have no basis for distinguishing between word-level syntax and phrasal syntax (“morphology” and “syntax” in traditional terminology, respectively). There should be only *one* structure building component, and its operations should be oblivious of the kinds of phonological structures associated with the symbols to which they apply.

### 3 Data

Let us turn to some data and see how the model sketched in the previous section works in practice. I discuss four cases in this section: verbal nouns, specifically in Arabic, negation in German Sign Language (DGS), the Latin second-place affix *-que* ‘and’, and the Tagalog affix *-um-*, which behaves as an infix in certain circumstances. The data presented here is intended to show how morphological facts can be analysed without resorting to a separate morphology module or specific word-building rules in the syntax. Furthermore, the first two cases are facts that would be difficult to handle in a morphology module in any case, because they show interaction between syntax and morphology.

#### 3.1 Arabic verbal nouns

Arabic verbal nouns, or *masdars*, are in many ways your typical garden variety verbal noun. They allow two types of constructions, equivalent to Abney’s (1987) *Poss-ing* and *Ing-of* constructions:

- (16) a. ?aqlaqa-nī    -ntiqād-u    -l-rajul-i    -l-mašrūf-a    *Arabic*  
 annoy-1sg.Obj    criticising-NOM    DEF-man-GEN    DEF-project-ACC  
 ‘The man’s criticising the project annoyed me.’
- b. ?aqlaqa-nī    -ntiqād-u    -l-rajul-i    li -l-mašrūf-i  
 annoy-1sg.Obj    criticising-NOM    DEF-man-GEN    to DEF-project-GEN  
 ‘The man’s criticising of the project annoyed me.’

In these examples, the verbal noun is (*i*)*ntiqād* ‘criticising’, from the verb *intaqada*. In (a), it assigns accusative to its direct object *al-mašrūf* ‘the project’, making the construction the equivalent of the Poss-ing construction. In (b), the object is realised as a PP with the preposition *li-* ‘to, for’, similar to the Ing-of construction.

Morphologically, masdars are quite complex. As McCarthy and Prince (1990) argue, they contain four morphemes, which are partially prosodic:<sup>14</sup>

- (17) Root:            «nqd»           Nominaliser:   «i.a»  
           Stem VIII:  (σ)σ<sub>μ</sub>           Non-finite:   -σ<sub>μμ</sub>  
                           |  
                           t

The root of the lexical item is the triconsonantal sequence «nqd». Since this root derives both verbs and nouns (and also adjectives, but those are morphologically indistinguishable from nouns), it must be considered non-categorical. Each root in Arabic can in theory derive up to fifteen different verb stems, which are numbered I–xv in the Western philological tradition of Arabic.<sup>15</sup> The stem VIII marker is partially prosodic, consisting of an extrametrical syllable (indicated with parentheses) and a short (full) syllable, which is linked to a segment «t». The masdar also contains a non-finiteness marker, which is also prosodic, consisting of a long syllable. Finally, the masdar contains a nominaliser, consisting of the vowels «i.a».

These four morphemes are combined into a pronounceable form by the phonological component of the grammar. Specifically, the form can be described as in (18), where each × stands for a time slot, which are grouped into syllables and linked to segments. The segment /a/ is linked to two slots, indicating that it is long:

- (18) (σ)   σ           σ           (σ)  
       |    / \        / \        |  
       ×  ×  ×   ×  ×  ×   ×   ×  
       |  |  |   |  |  |   |   |  
       n  t  i   q  a  d   d

Arabic has a rule that every stem must end in an extrametrical syllable (which is syllabified with the following word, if possible), which holds the final root consonant, here «d». Apart from this language-specific rule, the form (*i*)*ntiqād* is composed through general phonological principles, the most important one being Left-to-Right Association: the root consonants are associated left to right with the positions in the grid that are compatible with consonants (essentially the onset and coda positions). The same is true for the vowels: they are associated from left to right with the nucleus positions. The second vowel spreads to the adjoining grid position, which would otherwise remain empty.<sup>16</sup>

It is clear how the phonology puts the morphemes together in order to create a word form. What is not clear, however, is how the four morphemes actually come together. Assuming that

<sup>14</sup>The symbol σ stands for a syllable, the symbol μ for a mora, the weight unit of the syllable. A syllable of one mora, σ<sub>μ</sub>, is short, a syllable with two moras, σ<sub>μμ</sub>, is long.

<sup>15</sup>Though no root exists for which all fifteen are instantiated.

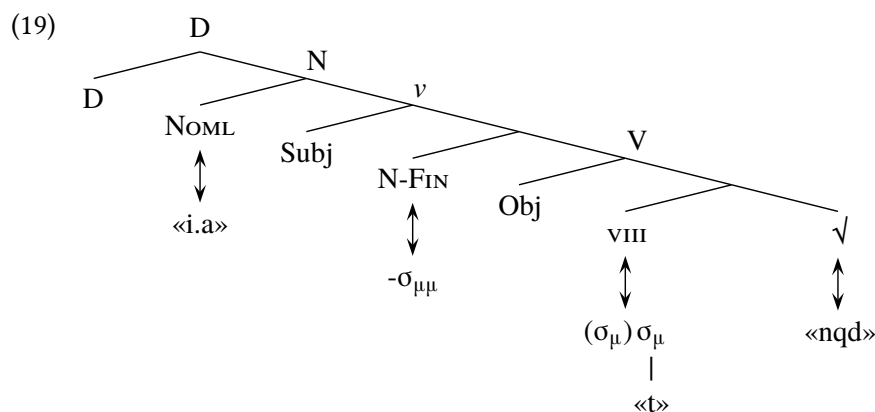
<sup>16</sup>In principle, this position can also be filled with a consonant, but since the «d» is associated with the final syllable, no consonant is available.

the word form is built up in the morphology, i.e., in a grammar module that is separate from syntax, after which it is inserted into the syntactic structure as a single head, does not explain why the masdar can assign accusative case. One might argue that morphology has the option to add an accusative feature to the word form it produces, so that it is able to assign accusative in syntax, but this would be a mere stipulation.

The syntactic account of verbal nouns, proposed by Abney (1987), is able to account for this fact and for the fact that verbal nouns can appear not only in the Poss-ing construction, but also in the Ing-of construction. Abney's account is rather straightforward: the attachment site determines the type of construction that is built. If the *-ing* suffix is attached to  $V^\circ$ , it creates a head  $N^\circ$ , so that the object must be licensed with a preposition. If *-ing* attaches to the VP, the verb can license its object inside the VP through accusative case, but the subject must be realised as a possessor, because *-ing* turns the VP into an NP. The functional projection embedding the NP is obviously a D projection, not an Infl projection, so that nominative case is not available.

Fehri (1993) provides an account of Arabic masdars along these lines. The problem with this analysis is that it is not clear how the actual word form is created. Abney's analysis includes a proposal that accounts for the creation of the gerund form, but even though morphologically, this is a much simpler form, the account is not entirely convincing. Abney (1987) extends the X-bar schema below the word level, which, as discussed above, means that he complicates the syntax in order to accommodate the special properties that morphological structures have.

In Kremers (2007, 2012a), I develop an analysis of masdar structures in which the four morphemes posited by McCarthy and Prince (1990) are distributed over the syntactic tree. The structure that I propose is the one in (19):<sup>17</sup>



It should be kept in mind that although the phonological forms are represented in the tree here, they are not involved in the syntactic computation. They only play a role in the phonological computation. For this reason, the associations to phonological structure are indicated with double arrows. This is to emphasise the fact that we need to think of the syntax as a computation with abstract morphosyntactic features, not as a computation involving “words”.

<sup>17</sup>In fact, in Kremers (2007) I labelled the root <nqd> as V, which is not correct, as the root is non-categorical: it can also derive nouns.

In (19), I have labelled the combination of the stem marker *vIII* and the root *V*, because the combination of the two morphemes establishes the verbal meaning *to criticise*. I take the non-finiteness marker to be the little *v* head, although nothing in the current analysis hinges on this. The nominaliser is added high in the structure here, which means that the resulting structure is a Poss-ing structure. For an Ing-of construction, the nominaliser would attach lower in the structure.<sup>18</sup>

A syntactic theory of word formation now needs to explain how the four morphemes end up together so that the phonology “knows” they must be realised as a single word form. The standard mechanism for this is head movement: the root moves to *vIII*, which then moves to *N-FIN* and further to *NOML*. Apart from the fact that these movements are not syntactically motivated (they are merely assumed to take place in order to get the phonological form to come out right), they complicate the syntax. If a head *X* moves, why would it be that it moves to the next higher head *Y*, and why would it not be possible for *X* to move further, stranding *Y* again? In fact, Ackema and Neeleman (2004, 2007) argue that excorporation of a head is possible in what they call syntactic *X°* complexes. If all complex heads are formed in syntax, why would excorporation be possible in some, but not in others?

In other words, a syntactic theory of word formation complicates the syntax to the point that we need to assume the existence of different kinds of complex heads, to which different constraints apply. This of course essentially amounts to sneaking the syntax/morphology distinction into the syntax. On the other hand, assuming morphological word formation in this case raises the question why the masdar can assign accusative case at all.

The solution to this problem is that word formation here is in fact neither morphological nor syntactic. Rather, it is phonological. We have already seen how the phonology constructs the word form, all we need to do is specify how the phonology “knows” that it needs to combine exactly these four morphemes into a single word form. The mechanism that is needed here can actually be derived rather straightforwardly from a proposal made by Ackema and Neeleman (2004). What we need is a principle governing the mapping from syntax to phonology that ensures that if a head *X* is combined with a structure *Y*, *X* and *Y* are phonologically realised together. This is what Ackema & Neeleman principle of *Input Correspondence* does:<sup>19</sup>

(20) *Input Correspondence*:

If *X* c-selects (a projection of) *Y*,  $\Phi(X)$  is associated with  $\Phi(Y)$ .

The notation  $\Phi(X)$  here and below in this paper is to be read as “the phonological material associated with the syntactic structure *X*”. *Input Correspondence* says that if a head *X* c-selects a projection of category *Y*, their phonological forms are realised together. In syntax, the relevant type of selection is categorial selection. In phonology,  $\Phi(X)$  is an autosegment that is associated with  $\Phi(Y)$  by standard means of autosegmental phonology.

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<sup>18</sup>Note that in that case, we would also expect the non-finiteness marker to be attached lower, which seems incompatible with the assumption that *N-FIN* corresponds to *v*. Since this issue is not relevant to the present discussion, I will leave it aside here.

<sup>19</sup>But see also Sadock (1992) for a very similar principle, holding between syntax and morphology. Note that the formulation here is slightly modified from Ackema & Neeleman’s.

Input Correspondence ensures that when the tree in (19) is mapped onto phonology, the four morphemes are realised together: the stem marker VIII selects the root, so that  $\Phi(\text{VIII})$  must combine with  $\Phi(\sqrt{\quad})$ . N-FIN selects the category V, NOML in turn selects little *v* (i.e., N-FIN). The result is that all four morphemes must be combined together, without syntactic movement having to take place.<sup>20</sup>

The crucial point here is that the word form that results from the structure is not just a function of the syntax or the morphology. It very much depends on the phonological form of the relevant morphemes. That seems like a platitude: if we combine a verb in English with a gerund affix, then obviously the phonological form /ɪŋ/ of the gerund marker determines that the word form comes out as /drɑ:vɪŋ/ or /swɪmɪŋ/, and not /drɑ:vʌm/ or /swɪmʌm/, or something.

Yet, on the other hand, it is not a platitude: what I am proposing is that the phonological form onto which an element is mapped has a larger influence on the final outcome of the derivational process than is usually assumed. For example, I assume that the fact that /ɪŋ/ is a suffix is simply part of the phonological matrix of the lexical entry *-ing*. That is, the fact that *drive<sub>v</sub>+ -ing* comes out phonologically as /drɑ:vɪŋ/ and not as /ɪŋdrɑ:v/ or /driŋɑ:v/, is a phonological fact. It is not something that needs to be derived in morphology or syntax, it follows from the lexical specification of *-ing* as a suffix.<sup>21</sup>

If we adopt the stance that the fact that *-ing* is a suffix is a phonological matter, we actually bring *-ing* in line with the prosodic morphemes that make up the Arabic masdar (and in fact much of Arabic morphology). With the masdar, it is quite clear that we do not wish to incorporate the exact placement of each morpheme in the word into our (morpho)syntax. It is much easier to let the phonology do this job, that is where all the relevant information is available anyway. The status of *-ing* as a suffix is essentially the same thing, just less extreme.

This is in fact a crucial point in the analysis. What I propose is that suffixes such as *-ing* are in fact prosodic morphemes, meaning that phonologically, they contain an *autosegment*. Specifically, they contain a prosodic alignment requirement. Because of this requirement, phonology needs to associate the element with another element on the segmental tier. Input Correspondence expresses this requirement.

This also answers another question that the c-selection requirement in Input Correspondence raises: there are many instances of c-selection in a syntactic derivation, but most of them do not yield a phonological structure in which the two elements involved are combined. For example, even though a verb c-selects its object,<sup>22</sup> the two are not necessarily combined into a single phonological unit. The reason is that in most instances of c-selection, the selecting head is not mapped onto a prosodic morpheme in phonology. If a selecting head and its selectee are both segmental, they obey the Non-Tangling Condition (Partee, Meulen, and Wall 1993),

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<sup>20</sup>Note that this does not account for the word order in (16) yet. This is beyond the scope of the current paper, but see Kremers (2012a) for further discussion.

<sup>21</sup>One could argue that this leaves unexplained the fact that many languages have a preference for suffixes over prefixes. However, this fact is most likely not something that should be expressed as a rule of grammar. Hawkins and Gilligan (1988) argue that the reason for this and related preferences should be sought in performance properties, both in production and in parsing. If they are right, we would not want to encode a preference for suffixes in the grammar, because this preference follows from third-factor effects.

<sup>22</sup>Obviously, thematic selection is involved as well, but that is applied in the semantic module.

or rather, in the current framework, Ackema & Neeleman's (2004) *Linear Correspondence*:

(21) *Linear Correspondence*

If a node X is structurally external to a node Y, then  $\Phi(X)$  is linearly external to  $\Phi(Y)$ .

Linear Correspondence ensures that two sister nodes are adjacent in the linear string. It only applies to elements that map onto the segmental tier, however. Elements that map onto autosegmental tiers are subject to Input Correspondence: they need to be associated with segmental material.

Summarising, the two principles Linear and Input Correspondence together describe the mapping of syntactic structures onto phonological structures. Linear Correspondence ensures that sister nodes are adjacent in the linear string, but only if they are both segmental. Autosegments are handled by Input Correspondence.<sup>23</sup>

### 3.2 Intermezzo: head movement

A few remarks about head movement are in order here. It has been pointed out often enough that head movement is problematic in a minimalist (bare phrase structure) approach for at least two reasons: it violates the extension condition and the moved head does not c-command its trace, at least under the simplest definition of c-command (cf. Chomsky 2001; Matushansky 2006). To solve this issue, Chomsky (2001) suggests that head movement may actually be a PF operation (see also proposals by Boeckx and Stjepanović 2001; Schoorlemmer and Temmerman 2012). The analysis sketched here for Arabic masdars suggests that something like head movement is taking place in the phonological component, since the end result of the derivation is that all heads (the root, the stem VIII marker, the non-finiteness marker and the nominaliser) end up in the position of the nominaliser.

There are two differences between the current analysis and PF movement proposals in the literature, however. First, note that PF movement is explicitly called *PF* movement: the existing proposals do not claim that movement is phonological. Rather, it takes place in a post-syntactic component (PF) that prepares the structure for phonology. I do not believe there is a need for such a component. Syntactic structures map onto phonological structures directly, not via some intermediate level of representation.

It should also be noted that I do not actually assume that there is *movement* in phonology. Movement is a syntactic process, describing the situation in which one element is associated with two positions in the syntactic tree. What happens in phonology is not movement in the literal or even metaphoric sense. In phonology, a head such as the root of an Arabic masdar is only linked to *one* position in the structure. It is just that this position is different from the one we would expect based on its position in the *syntactic* structure.

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<sup>23</sup>The formulation in the text suggests that the phonological form of the element in question determines which mapping principle is applied, which appears to be a look-ahead problem. We can probably solve this issue by assuming that Input Correspondence is really the sole mapping principle. When the phonological form of the element being mapped is not an autosegment, however, association is not possible and juxtaposition of the two elements is chosen instead. In a sense, then, linearisation is applied as a last resort. See Kremers (2013) for some discussion of this idea.



In other words, the relevant heads do not move, neither in syntax nor in phonology. It is the discrepancy between the syntactic and the phonological position that suggests movement, but this discrepancy is the result of an autosegmental, multi-tier phonological structure. Note that I do not wish to claim that all instances of head movement can be analysed in this manner. It is at least conceivable that some instances of head movement are truly syntactic movement operations involving just a head, be it as reprojection (Georgi and G. Müller 2010; Surányi 2005), morphological merger (Matushansky 2006) or some other mechanism. The analysis does offer a viable alternative for describing some head movement effects, however.<sup>24</sup>

### 3.3 Negation in DGS

Another domain in which a synthesis of syntax and morphology is helpful in adequately accounting for the data is negation in sign language.<sup>25</sup> The problem we face here is best illustrated with two basic negation facts from German Sign Language (*Deutsche Gebärdensprache*, DGS; examples from Pfau and Quer (2002)):

- (22) a. **head:** neg  
**hands:** MOTHER FLOWER BUY  
 ‘Mother does not buy a flower’
- b. **head:** neg  
**hands:** MOTHER FLOWER BUY  
 ‘Mother does not buy a flower’

The most common way to realise negation in DGS is (a): a headshake accompanies the manual verb. No other negation is necessary.<sup>26</sup> At first sight, the most straightforward analysis of negation would appear to be that it is a morphological negation marker, which is exactly what Pfau and Quer (2002) do.<sup>27</sup> But this analysis is called into question by the fact that the non-manual negation can in fact spread over the entire VP, as seen in (b): the headshake not only accompanies the manual verb, it also accompanies the object FLOWER.

If the non-manual negation were a morphological marker, this should be impossible. Note that both clauses in (22) express sentential negation, (a) is not constituent negation. Again, the apparent contradiction is resolved if we assume that the negation is simply a head in the syntactic structure, and that the phonology takes care of its realisation. The syntactic structure does not need to instruct the phonology where or how to realise it, it follows automatically from  $\Phi(\text{Neg})$  and from general phonological principles.

<sup>24</sup>Primarily, I believe, instances of (apparent) head movement that do not involve any semantic effect and appear to exist primarily as descriptive tools to get morphemes in their right places.

<sup>25</sup>In fact, the problem is not limited to negation in sign language, it is much more pervasive. See Kremers (2012b) for details.

<sup>26</sup>As Pfau (2008) notes, it is in principle possible to add a manual NOT to the sentence, but this is hardly ever done.

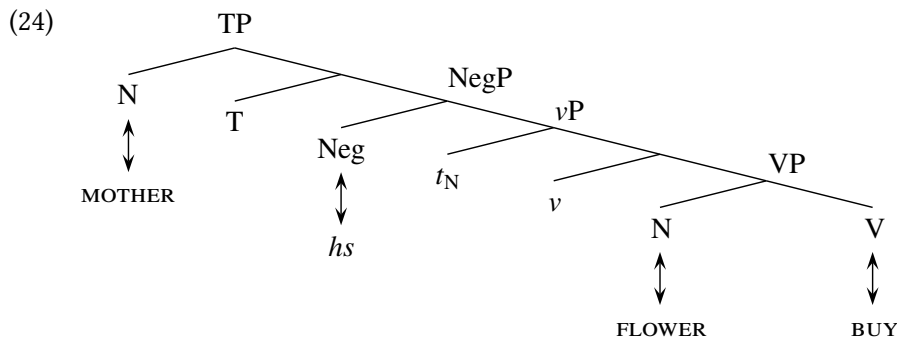
<sup>27</sup>See also Pfau (2002, 2008) for this view. Note, however, that such an analysis still faces the same questions that were raised in relation to Arabic verbal nouns: if word formation is syntactic, we must assume head movement without syntactic motivation, and if we assume word formation is morphological, we have to explain why negation is sentential.

Let us see, then, how this works. I assume that the lexical entry for the negation is something along the lines of (23):

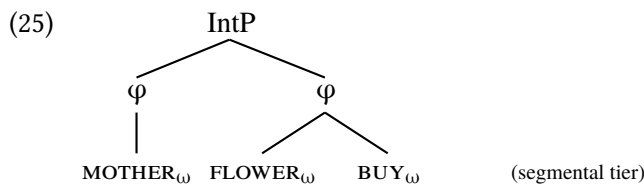
$$(23) \text{ NOT} \leftrightarrow [\text{Neg}, uV] \leftrightarrow \begin{matrix} hs \\ | \\ \omega \end{matrix}$$

Since semantics is not relevant to the issue at hand, I have simplified the semantic representation here. Syntactically, negation consists of a Neg head that selects a verb. Phonologically, the negation consists of a headshake,<sup>28</sup> which I have represented as *hs*, for lack of a true phonetic transcription system for sign languages. More importantly, I have added an additional specification to the phonological form: (23) states that the headshake is associated with a prosodic word.

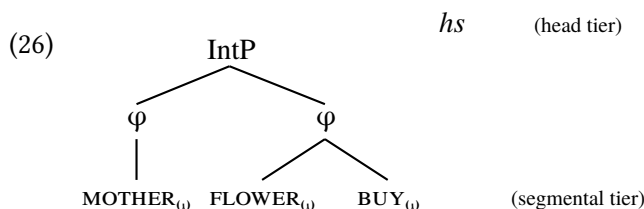
The syntactic structure of the clause in (22)a is roughly (24):



When this structure is mapped onto phonology, the phonological system must linearise MOTHER, FLOWER and BUY. Because the object FLOWER and the verb comprise the VP, the two signs are joined in a phonological phrase. The resulting structure is (25):

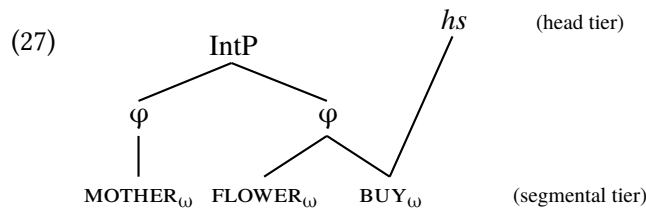


The negation is a prosodic morpheme, realised on an autosegmental tier:

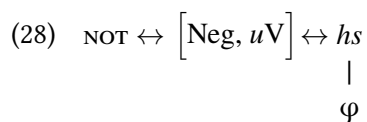


<sup>28</sup>Note that Pfau (2001) assumes that the manual NEG sign that is optional in DGS (and in fact seldom used) is in Spec,NegP. Neg° corresponds to the headshake.

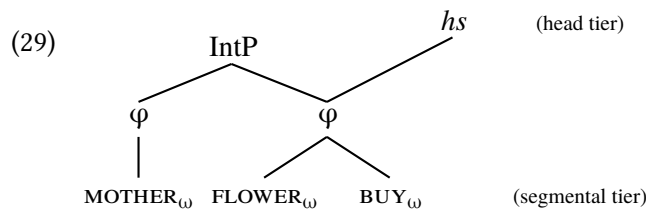
The phonological form of the negation specifies that it must be associated with a prosodic word. The segmental tier, however, contains three prosodic words. At this point, Input Correspondence becomes relevant:  $\text{Neg}^\circ$  selects a verbal projection, which means that  $\Phi(\text{Neg})$  must be associated with  $\Phi(V)$ . That is, Input Correspondence dictates that the headshake must be associated with the verb BUY:



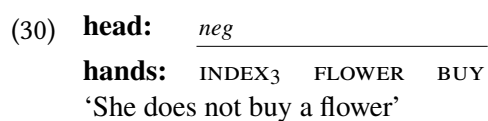
The possibility of spreading exemplified in (22)b can be accounted for by assuming that an alternative phonological form exists for the negation:



When this phonological form is selected, the headshake does not associate with just the verb. Crucially, however, it still obeys Input Correspondence, meaning that it associates with  $\Phi(V)$ , but now not with the prosodic word containing  $\Phi(V)$ , but with the phonological phrase containing  $\Phi(V)$ :



The assumption that the association between the headshake and the segmental tier is phonological is supported by the fact that when the subject does not constitute a phonological phrase on its own, negation can spread over it as well (Pfau 2008, p. 26):



In (30), the subject of the clause is a third person index, i.e., a pronoun, which does not constitute a separate phonological phrase. Instead, it is incorporated into the prosodic word that corresponds to the object, and hence into the phonological phrase that corresponds to the VP. In this case, if negation spreads over the phonological phrase, it spreads over the subject as well.

The analysis of DGS negation presented here has several advantages over a syntactic or morphosyntactic analysis. Most importantly for the present issue, it is not necessary to distinguish between a syntactic and a morphological structure. We do not need to answer the question whether the non-spreading negation is morphological or syntactic, we just need to assume the tree in (24). Furthermore, we also do not need to assume that the verb moves to Neg° in order to account for the association of the negation with the verb. As such, the current analysis presents the simplest analysis possible.

### 3.4 Latin *-que*

Another area where giving up the distinction between syntax and morphology allows for simpler analyses is in second place clitics. I will illustrate this point with only one clitic, Latin *-que* ‘and’, which Embick and Noyer (2001) have analysed within the framework of Distributed Morphology. In Embick & Noyer’s analysis, Latin *-que* attaches to the first MWd of the second conjunct.

- (31) bon+ī                  puer+ī                  bon+ae-que                  puell+ae                  *Latin*  
good+NOM.PL.M   boy+NOM.PL.M   good+NOM.PL.F-and   girl+NOM.PL.F  
‘good boys and good girls’

The analysis that Embick and Noyer (2001) propose is in essence rather simple: a morphological operation places *-que* after the first MWd of its second conjunct. However, as Embick and Noyer (2001) note, when a preposition is involved, the picture becomes more complicated:

- (32) a. circum-que ea    loca                                  (33) a. in rēbus-que  
          around-and those places                                  in things-and  
          ‘and around those places’                                  ‘and in things’  
          b. contrā-que lēgem                                  b. dē    prōvinciā-que  
          against-and law    from province-and  
          ‘and against the law’    ‘and from the province’

If *-que* conjoins two prepositional phrases, its position depends on the preposition of the second conjunct: if the preposition has only one syllable, *-que* attaches to the (first) word of the complement of the preposition. If the preposition is longer, however, *-que* attaches to it instead of the complement. For Embick and Noyer (2001), these facts are difficult to explain: from a morphological point of view, it should not matter whether a preposition has one or more syllables. This is especially true in a DM approach, where it is assumed that the phonological form of heads does not enter into the syntactic computation at all.

The solution to the problem is of course rather straightforward: *-que* attaches to the first *prosodic* word of the second conjunct, as argued for by Agbayani and Golston (2010). Although we can only guess as to what the phonological structure of Latin was, it is a safe bet that a monosyllabic preposition did not form its own prosodic word, but was incorporated into the

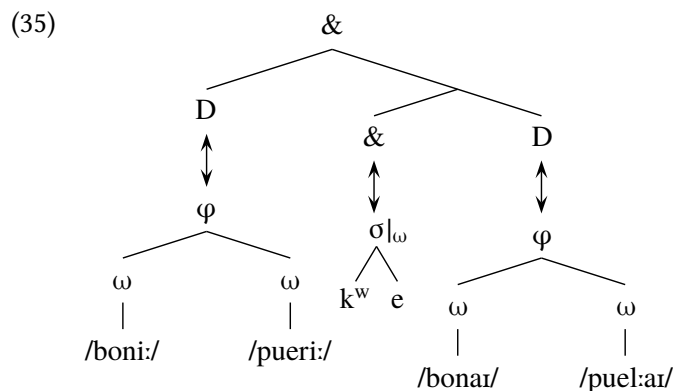
prosodic word of its complement, while bisyllabic prepositions behaved differently. They are phonologically heavier and presumably did form a separate prosodic word.

Embick & Noyer’s Embick and Noyer (2001) approach, which tries to account for the placement of *-que* in morphosyntactic terms, is fundamentally flawed: the positioning of the clitic is a phonological matter. We need to analyze *-que* as a prosodic morpheme:

$$(34) \quad \&^\circ \leftrightarrow \begin{array}{c} \sigma|_\omega \\ \swarrow \searrow \\ k^w \quad e \end{array}$$

I am assuming here that the syntactic head representing the conjunction is  $\&^\circ$ , even though the proper syntactic representation of conjunction is a complex matter that has not yet been resolved satisfactorily. For the present discussion, we can ignore these questions, focussing instead on the phonological structure of *-que*. The phonological specification in (34) says that *-que* is a syllable ( $\sigma$ ), which is at the right edge of a prosodic word, indicated as  $\omega$ .<sup>29</sup>

Assuming an  $\&^\circ$  head, the syntactic structure of the conjunction is (35):



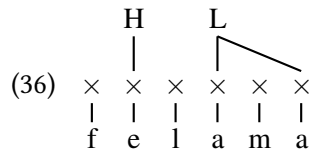
At this point, there is one assumption regarding the mapping of such structures onto phonology that we need to make:  $\Phi(\&)$ , which does not constitute a separate prosodic word, is incorporated in the prosodic structure of its *second* conjunct. If this was indeed true for Latin,<sup>30</sup> the phonological form of *-que* given in (34) suffices to explain the placement facts of *-que*.

In the case of the Arabic verbal noun and the DGS negation, it is Input Correspondence that determines which autosegments are associated with each other. However, the actual phonological mechanism that establishes the association is independent from Input Correspondence. In autosegmental phonology, elements on different tiers are associated with each other from

<sup>29</sup>Note that I will use this notation with a vertical bar throughout to indicate prosodic domain boundaries. A subscript indicates the type of boundary (here a prosodic word boundary) and the position of the subscript indicates whether it is a left or right boundary (here a right boundary).

<sup>30</sup>There are some indications that this is quite generally true: for example, it is usually easier to insert a pause between the first conjunct and the conjunction than between the conjunction and the second conjunct, cf. *a man – and a woman* with *a man and – a woman*: the latter seems more marked. In some languages the conjunction actually cliticises onto the second conjunct, making separation almost impossible, cf. Arabic *al-nahār wa-l-layl*, lit. ‘the-day and-the-night’, i.e., ‘day and night’.

left to right, a process aptly called Left-to-Right Association. The following example from Mende (Sierra Leone), provided by Leben (1973), illustrates this:

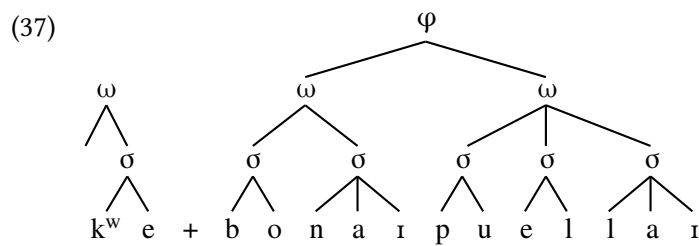


The word *félàmà* ‘junction’ is lexically associated with the tone pattern HL (high-low). These tones are distributed over the segmental tier from left to right. Therefore, the high tone is associated with the /e/, the low tone with the first /a/. Since (in Mende) every vowel needs to be associated with a tone, the low tone spreads to the second /a/. Note that tones can only be associated with vowels, therefore the consonants are skipped: vowels are so-called *tone-bearing units*. Autosegments are usually restricted in such a way: they can generally only associate with particular types of elements.

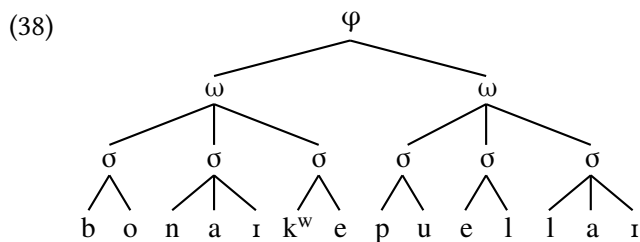
One way to look at Input Correspondence is to view it as a way to restrict possible associations. One might say that if a syntactic head X selects a head Y and  $\Phi(X)$  is an autosegment, Input Correspondence turns  $\Phi(Y)$  into the only  $\Phi(X)$ -bearing unit. As a result, Left-to-Right Association has no other option but to associate  $\Phi(X)$  with  $\Phi(Y)$ .

In the case of *-que*, the situation is different. I assume that the  $\&^\circ$  head does not select its two conjuncts, since it can conjoin elements of any category. This means that Input Correspondence does not play a role. As a result, Left-to-Right Association picks the first available element to establish an association with. This is why it is important that *-que* is contained in the prosodic domain of its second conjunct. Inside that prosodic domain, *-que* is associated with the first prosodic word. The right-alignment requirement then causes *-que* to be attached to the end of this prosodic word.

We can represent this as follows. The phonological system needs to combine the following two elements:



As discussed, since /k<sup>w</sup>e/ needs to align with the right edge of a prosodic word, the only way to incorporate it into the larger structure is to attach it after /bonar/:



As with the previous examples, we see that by placing the process that builds the word form in the phonology, we are able to describe the structure and formation of conjunctions with without having to refer to a morphology module. We only need to assume a syntactic tree along the lines of (35) and a lexical entry for the conjunction along the lines of (34), and then the phonology does the rest.

My claim is in fact stronger than this, though: not only is it not necessary to refer to a morphology module, it is in fact a mistake to do so. One should not try to account for the form *bonaerique* in (31) by bringing the  $N^\circ$  and the  $\&^\circ$  heads together in the hierarchical structure, after which the phonology is a relatively simple and “dumb” process that takes the tree and just plugs in the segments. After the hierarchical structure is built, but before phonology processes the structure, it is not clear at all in what order the elements in the tree are going to end up. The phonology takes an active part in determining the final linear order.

This is not to say that the process of phonological composition is nondeterministic. Given the same input structure, we expect the same output. It is just that the syntactic tree does not contain enough information to determine the output of phonology. The phonological forms of the elements in the syntactic tree influence the output of phonology as well and this information is (by definition) not part of syntax.

The area of grammar where this effect is felt most is no doubt word structure (i.e., *morphology* in its pretheoretic, descriptive sense), because constituent parts of words, i.e., affixes, are much more likely to determine their own alignment properties than constituent parts of phrases, i.e., words (although such cases do exist, e.g., *warm enough* vs. *how/too/so warm*, cf. Jackendoff (2002)). This is one of the factors that creates the impression of a separate module for word structure, but the impression is an acoustic illusion, in essence an epiphenomenon created by the interaction of several factors.

### 3.5 Tagalog *-um-*

The final example to be discussed here is one of the classics of Optimality Theory: Tagalog *-um-* affixation (Prince and Smolensky 2004, pp. 40–43). As is well-known, the Tagalog prefix *-um-* (often called the *agent trigger*, i.e., it signals that the noun marked with so-called “direct” case is the agent of the verb) is realised as a prefix on stems that begin with a vowel but as an infix on stems that begin with one or more consonants:

- (39) *aral* – *um·aral*  
*sulat* – *s·um·ulat*  
*gradwet* – *gr·um·adwet*

In Optimality Theory, these data are explained by the interaction of two constraints: NoCODA, which states that syllables should not have codas, and ALIGNL, which aligns the affix with the left edge of the stem. The following tableau shows the derivation of *um-aral*:

(40)

	NoCODA	ALIGNL
☞ u.ma.ral	*	
a.um.ral	**!	a
a.ru.mal	*	a!r
a.ra.uml	*	a!ra
a.ra.lum	*	a!ral

The constraint NoCODA ensures that the affix precedes the stem. If the stem starts with a consonant, however, this same constraint has the effect of moving the affix into the stem:

(41)

	NoCODA	ALIGNL
um.su.lat	**!	
☞ su.mu.lat	*	s
su.um.lat	**!	su
su.lu.mat	*	su!l
su.la.umt	*	su!la
su.la.tum	*	su!lat

Since *-um-* changes the argument licensing properties of the verb, I assume it is the instantiation of the *v* head, even though nothing really hinges on this assumption. The model described by Prince and Smolensky (2004) uses two constraints. Of these, ALIGNL does not really say anything more than that *-um-* is a prefix of sorts. The question we need to answer here is which is better: to formulate a general but violable principle that prefers left alignment, or to simply incorporate this information in the lexical entry of *-um-*. Since Tagalog also has suffixes, ALIGNL cannot apply to all affixes in Tagalog, or, more precisely, it must be ranked differently depending on the affix. That means that we must specify in the lexicon that ALIGNL is ranked high for *-um-*. Put differently, we need to specify in the lexicon that *-um-* is a prefix (cf. also the remarks above regarding the English gerund suffix *-ing*).

The NoCODA constraint expresses a more general property of language: there is a general tendency to avoid codas, which expresses itself in many ways, perhaps most prominently in the fact that CV is the most basic syllable structure of human language. What we should ask ourselves, however, is whether we should incorporate such a constraint into the grammar or whether we are dealing with a third-factor effect. For example, there are strong indications that this general preference for onsets vs. codas has to do with the way the brain times phonetic events which results from architectural constraints of the brain (cf. Goldstein, Byrd, and Saltzman 2006; Nam 2007).

I therefore assume that the coda dispreference is not a rule of grammar but the result of more general properties of the brain. In other words, NoCODA is not part of the I-language of a speaker. Rather, it is a factor that influences the development of an I-language in an



individual. As a result, a language may incorporate rules into its grammar that in some form or other disprefer codas, such as phonotactic restrictions, lexical or postlexical phonological rules, etc. However, the extent to which a language incorporates such rules will differ from language to language, because there may be other factors in the development of I-languages or in the historic development of a particular language that push the language toward having codas. For example, if in a language that has CV syllables word-final vowels are reduced to schwa and then dropped, CVC syllables result.

In OT-style analyses, NoCODA is of course not just a third factor effect influencing the development of an I-language. Rather, it is a violable rule of grammar. This of course raises the question why a root such as *sulat* has a coda in its final syllable at all. Why does it not come out as *sula*? The standard OT answer is of course that there is a faithfulness constraint FAITH that requires that the output is as faithful as possible to the input. If this constraint is ranked higher than NoCODA, it explains why *sulat* can have a coda.

But then what exactly is *faithfulness*? After all, a form such as *sumulat* violates at least one imaginable form of FAITH: if FAITH is interpreted as “do not break up sequences of segments”, the form *sumulat* violates it. If FAITH is then ranked higher than NoCODA, which it needs to be in order to account for the ungrammaticality of the form *\*sula*, we would expect *sumulat* to be excluded in favour of *umsulat*, despite the (double) violation of NoCODA of the latter form. Obviously, another interpretation of FAITH avoids this problem: if we interpret FAITH as “represent each input segment in the output” (more closely to how faith is usually understood), *sumulat* does not violate FAITH. But neither would *sumutal*.

We are therefore faced with the problem of finding a formulation of FAITH that does justice to the kinds of faithfulness that language shows. Note that both interpretations of FAITH are reasonable and valid for human language. Language does indeed tend not to break up sequences of segments in morphological processes: infixes are comparatively rare compared to prefixes and suffixes (Greenberg 1966). Similarly, morphological processes usually leave the base intact to a large extent. Few processes are known whereby something is removed from the base; usually something is added.

In other words, FAITH is not a straightforward constraint. Like NoCODA, it is undeniably true that some form of faithfulness must be operative in human language, since deviating too much from the root of a word would cause incomprehensibility. However, explicitly encoding it in the grammar is problematic. Faithfulness can mean different things in different contexts. This indicates that faithfulness is not a grammatical rule. Rather, we are dealing with a third-factor effect of sorts. It seems reasonable to assume that language underlies some recoverability requirement, in the sense that the form of an utterance must enable the parser to reconstruct the original hierarchical structure.

Like the preference for codaless syllables, I believe recoverability in this sense is also a principle that shapes language (and I-language) as it develops. It is not, however, a rule of the grammar and cannot be referred to in an account of specific derivations. The effect it has is that the grammar is a system that does not change or create forms arbitrarily but on the basis of rules.<sup>31</sup>

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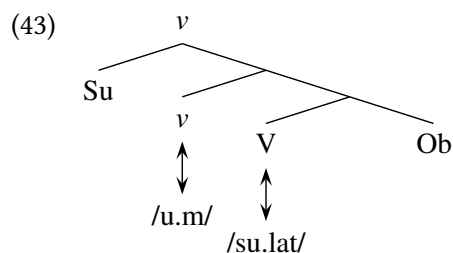
<sup>31</sup>Or constraints, or a combination of both. The considerations discussed here apply regardless of whether the grammar is rule-based or constraint-based.

If we interpret both FAITH and NOCODA as meta-principles rather than as grammatical principles, then we need a different way of explaining the placement behavior of *-um-*. The way to do this is rather straightforward and essentially amounts to encoding the coda-avoiding property of *-um-* directly into the phonological form: despite appearances, *-um-* is not a single syllable, it is bisyllabic. There is a syllable boundary between the two segments, that is, /m/ occupies an onset position.<sup>32</sup> We can express this by representing the morpheme as /u.m/, or more elaborately, as in (42):

$$(42) \quad [v, +ag] \leftrightarrow \begin{array}{c} \omega | \sigma \quad \sigma \\ \wedge \quad \wedge \\ u \quad m \end{array}$$

Here, the feature [+ag] is simply meant to represent the fact that *-um-* is an agent trigger. For convenience' sake, I have left out the semantic part of the lexical entry. The important part in (42) is the phonological part: a bisyllabic structure with two segments occupying slots in different syllables. In other words, *-um-* is a prosodic morpheme. Additionally, there is a prosodic word boundary at the left edge, meaning that /u.m/ is left-aligned (i.e., a prefix).

Syntactically, I assume that *-um-* is a *v*-head, as I mentioned above. This means that a clause containing *-um-* has a syntactic structure along the lines of (43):



By now it should be obvious that we do not need to assume much more than this structure: Input Correspondence ensures that  $\Phi(v)$  selects  $\Phi(V)$  and the phonological requirement that /u.m/ is left-aligned and bisyllabic can only be accommodated by creating the form *sumulat*. We do not need a morphological structure, Input Correspondence and the phonological structure of the affix are all we need to account for the data.

## 4 Syntax above the word

The data discussed so far all involve morphological structures or structures that are at least partially morphological, with some interactions with syntax. But since the essential idea of the current proposal is that there is no distinction between syntax and morphology, the natural question to ask is how the proposal deals with syntactic structures and whether there are benefits in this area. In this section, I discuss three issues that may benefit from an analysis

<sup>32</sup>In fact, McCarthy and Prince (1993) even say so themselves: “[*-um-*] falls as near as possible to the left edge of the stem, so long as it obeys the phonological requirement that its final consonant *m* not be syllabified as a coda” (p. 79).

as outlined here: linearisation of head and complement, focus and intonational contours. It should be noted that the discussions will be sketchy and tentative. The purpose here is mainly to indicate possible fruitful directions for future research, not to provide complete and detailed analyses of the relevant topics.

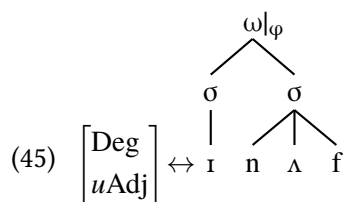
#### 4.1 Linearisation: Head/comp

One area where an analysis in terms of phonological structure may be possible is linearisation. Even though this issue is often thought of in syntactic terms (inspired mostly by Kayne’s (1994) influential work), there are in fact good reasons to consider treating it in phonological terms. First, linear order is primarily a modality requirement, i.e., an “external factor”, something that should not be part of the language faculty in the narrow sense (cf. Hauser, Chomsky, and Fitch 2002). Furthermore, in the model proposed here, syntactic structure is actually underspecified for linear order. The order of the morphemes in the Arabic masdar, or the position of negation in German Sign Language cannot be determined on the basis of the syntactic tree alone. In order to establish the linear structure, it is necessary to consider the phonological forms of the elements to be ordered.<sup>33</sup>

This point is emphasised by the fact that although linearisation is in many cases quite regular and predictable, there are cases of idiosyncratic linearisations. One such fact is pointed out by Jackendoff (2002):

- (44) a. *so/how/very/too*  
 b. *good good enough*

Adjectival degree heads in English such as *so/how/very/too* are normally linearised before the adjective, which is to be expected given that English is generally a head-complement language. However, in the case of *enough*, the only possible linearisation is Adj-Deg.<sup>34</sup> What is most likely happening here is that there is an idiosyncratic linearisation instruction that places *enough* after its complement. In the current proposal, we can simply incorporate this information in the lexical entry for the Deg head *enough*:



Here, the addition of a  $\varphi$ -boundary ensures that /ɪnʌf/ is aligned with the right edge of a phonological phrase, resulting in the correct linearisation. Other Deg heads lack such a  $\varphi$ -boundary in their phonological representations, so that a default linearisation rule applies.<sup>35</sup>

<sup>33</sup>See Kremers (2013) for a discussion of this point.

<sup>34</sup>Note that the point still holds if one were to assume that *enough* is not a Deg head but, e.g., an adverb.

<sup>35</sup>Formulating this default rule is beyond the scope of this paper, however.

Within the VP domain, there are strong indications that (default) word order correlates with stress assignment. For example, Truckenbrodt (1995) notes that a head-initial VP corresponds to a  $\varphi$ -final stress, and vice versa:

- (46) a. head-initial VP  $\leftrightarrow$   $\varphi$ -final stress (English: *read **the book***)  
 b. head-final VP  $\leftrightarrow$   $\varphi$ -initial stress (German: ***das Buch** lesen*)

The examples that Truckenbrodt (1995) provides are not from English and German, but they will do to illustrate the point. Nespors, Mohinisch, et al. (2008) also find that stress-initial phonological phrases are head-final, and vice versa.<sup>36</sup> In order to use these facts for a theory of linearisation, it is necessary to adopt two fairly standard assumptions: (a) the assignment of stress within a phonological phrase depends on the hierarchical structure of the VP; and (b) in a structure [<sub>VP</sub> V Obj] phrasal stress is assigned to the object.<sup>37</sup> We can now assume two stress alignment rules:

- (47) a. head-initial:  $\times|_{\varphi}$   
 b. head-final:  $_{\varphi}|\times$

In a language like German, which has different linearisation rules for different domains, it is necessary to associate the stress alignment rules to syntactic structure. For the VP, this can be done in the following way:

- (48) VP  $\leftrightarrow$   $_{\varphi}|\times$

This rule states that a VP in syntax is associated with a phonological structure in which the stressed prosodic word is right-aligned in the phonological phrase. Essentially, (48) states that a verbal category is associated with a phonological phrase in which stress is left-aligned. Crucially, it does not state that the V head is associated with the stress mentioned in the rule.

Obviously, the approach sketched here raises many questions. For example, it is not clear what happens when stress does fall on the verb, e.g., when the verb is (e.g., contrastively) focussed. This would require being able to distinguish between nuclear stress and other kinds of stress.<sup>38</sup> Furthermore, the idea that word order follows from stress alignment and not the other way around would require external confirmation, since the dominant view seems to be that syntactic structures are linearised before stress is assigned (cf. Truckenbrodt 1995).

However, in spite of these questions, the approach sketched here seems worth pursuing further, especially because it would be compatible with theories of *prosodic bootstrapping* (cf. Christophe et al. 2008), which argue that children pay attention to prosodic and stress cues and

<sup>36</sup>The data Nespors, Mohinisch, et al. (2008) discuss even shows that the phonetic realisation of stress differs in both cases:  $\varphi$ -initial stress is mainly realised through pitch and intensity, while  $\varphi$ -final stress is realised differently.

<sup>37</sup>Stress assignment is of course a much more complicated topic, but the facts suggest that these two basic assumptions are essentially correct (cf. e.g., Chomsky and Halle (1968), Gussenhoven1992b; see also Truckenbrodt (2006) for an overview).

<sup>38</sup>I suspect it is possible to do this by adding an appropriate semantic component to the rule in (48), though I'm not sure what this semantic component should look like.

are in that way able to establish the branching direction of the language in which they grow up, before they are able to parse words or phrases (cf. also Nespor, Mohinish, et al. 2008).

Note, however, that I explicitly do not want to make the claim that all linearisation requirements can be reduced to stress alignment. The stress alignment rule in (48) only handles VP linearisation and it seems unlikely that linearisation of other heads can be handled by a similar rule. Although such an approach loses the notion that all linearisation statements are derived in the same way, I do not see this as a problem. If linearisation is indeed a requirement of the modality (i.e., of the sensorimotor system), then any method for deriving linear order from a tree structure will do and different methods may be used in different contexts.

In fact, this idea is not new, even though it is not always expressed as explicitly as I do here. For example, Abels and Neeleman (2009) argue that moved elements are typically linearised to the left. This is a linearisation principle that applies only to moved elements, which means that it has nothing to say about the linearisation of elements that remain in their base-generated positions. Therefore, if one accepts Abel & Neeleman's (2009) conclusions,<sup>39</sup> one must also accept the premise that not all linearisation statements are derived in the same way.

## 4.2 Focus

Another area where a phonological/prosodic approach may yield advantages is in the interaction of linear order and prosody. For example, Samek-Lodovici (2005) discusses the following pair of Italian sentences:<sup>40</sup>

- (49) a. [Gianni ha RISO]<sub>F</sub> *Italian*  
 Gianni has laughed  
 'Gianni laughed' (context: *What happened?*)
- b. Ha riso GIANNI<sub>F</sub>  
 has laughed Gianni  
 'Gianni has laughed' (context: *Who laughed?*)

Samek-Lodovici (2005) notes that in Italian, the stressed prosodic word in the focussed constituent is right-aligned with an intonational phrase. In (a), the clause is in focus, answering the question *What happened?*, causing the stressed verb *riso* 'laughed' to be placed at the right edge of the intonational phrase, yielding subject-verb order. In (b), the order is verb-subject, with the focussed constituent *Gianni* right-aligned with the intonational phrase.

An admittedly simplistic approach, as the data are obviously more complex than this example shows, would be to say that there is an alignment requirement on focus, something along the lines of (50):

$$(50) \text{ focus} \leftrightarrow X_F \leftrightarrow *|_I$$

This rule says that the semantic (information-structural) feature of *focus* is associated, through a syntactic indiscriminate head *X* carrying a focus feature, with an instance of stress that must

<sup>39</sup>Which in fact I do; see also Kremers (2009), where I reach the same conclusion on different grounds.

<sup>40</sup>See also Féry (2013) for additional evidence that focus is expressed as prosodic alignment in many languages.

be right-aligned in an intonational phrase (I). When the derivation reaches the point where the subject is merged in the example (49)b, the focus semantics associated with it will cause focus stress to be assigned to the subject, which then activates the rule in (50). This rule then triggers linearisation of the subject in clause-final position. In principle, there are two ways in which this last step can be obtained. Either the effect is purely phonological, with just a reordering of the phonological string, or the effect may be mediated by syntax, with the rule in (50) triggering a syntactic operation that moves the *vP* or *TP* across the subject. The end effect is the same, with the linear order appearing as in (49)b.

### 4.3 Intonational contours

A final area I will briefly mention here is intonation. A typical property of questions in many (if not all) languages is that they show a specific intonational contour. Often it is even possible to form a question through intonation alone:

- (51) a. John left for Rome. (L%)  
 b. John left for Rome? (H%)

In English, a normal yes/no question uses inversion. A sentence such as (b) can be used as something like a yes/no echo question, expressing surprise and requesting confirmation.<sup>41</sup> Assuming that such a clause is headed by a *C* head with a [+Q] feature, we can represent this head in the following way:

- (52) INT ↔ [C,+Q] ↔ H|<sub>U</sub>

The rule in (52) states that a [+Q] *C* head is associated semantically with an interrogative operator and phonologically with a high tone at the right edge of an Utterance. Obviously this representation is simplified in several ways, but the idea should be clear. Intonation is represented on a separate autosegmental tier and the merger of an appropriate syntactic head triggers its inclusion in the phonological representation. The standard phonological principles then apply in order to associate the boundary tone with the rest of the phonological representation.

### 4.4 Summary

In morphology, the idea that a morphosyntactic head can be associated with a non-segmental, syllabic phonological structure is well-established since Goldsmith's (1976) and McCarthy & Prince's (1996) seminal works. The proposal made here is that the same idea should be adopted in syntax. There is no obvious break in the prosodic hierarchy: prosodic words are grouped into phonological phrases just as syllables are grouped into prosodic words. Therefore, any claim that syntactic heads cannot be associated with autosegmental phonological structure

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<sup>41</sup>As pointed out by a reviewer, the facts regarding declaratives with rising intonation in English is rather more complex than suggested here. See Gunlogson (2001) for details. The crucial point here is that the rising intonation makes a specific meaning contribution; how this contribution can be represented semantically is not relevant to the issue at hand.

would be a mere stipulation. Given that this is a possibility in morphology, we would in fact expect the same to be possible in syntax.

The examples discussed in this section suggest that this expectation is borne out. Obviously, more work needs to be done in each case, but I believe the idea to be viable and potentially very fruitful. Representing the intonational rise in questions as the phonological reflex of an interrogative C head seems a very natural way to deal with this phenomenon. Similarly, the idea that a Neg head is associated with an autosegmental feature such as headshake, which is represented autosegmentally in phonology and gets associated with the verb, seems the most straightforward way of dealing with the data.

Once one accepts this conclusion, the next step is also a natural one: by analysing the relevant data in the manner outlined here, it becomes impossible to draw a clear line between morphology and syntax. The DGS negation is a syntactic head, but its realisation on the verb appears to be a morphological effect. Verbal noun formation in Arabic would traditionally be considered a morphological process, but we can derive the correct form directly from the syntactic tree.

## 5 Counterarguments

As mentioned in the introduction, there have been many attempts to unify syntax and morphology into a single system. There are equally many criticisms of such proposals, which usually point out that there are differences between syntax and morphology that cannot be explained in a unified theory. In this section, I discuss several points that have been brought up in this discussion and show why I do not believe they pose a problem for the assumption that morphology is not a separate grammar module.

### 5.1 Lack of morphological movement

One typical argument that is often put forward in defence of a separate morphology module is the fact that there is no movement in morphology. This is hardly a convincing argument, however. In order to evaluate it properly, we first need to establish what kind of phenomenon would count as morphological movement. The kind of movement that is relevant for the argument must be theory-independent. In minimalism, for example, movement is a fundamental analytic tool and is therefore employed for theory-internal reasons in many instances. As a contrast, consider non-transformational frameworks such as HPSG, which explicitly do not employ movement as an analytic tool. In such frameworks, there are certain phenomena, generally called *long-distance dependencies*, such as *wh*-fronting, topicalisation, V2, etc., that are analysed by means of gaps, symbolised with so-called *slash* features.

This is the type of phenomenon that we should be looking for in order to find morphological movement: an element or category of elements that can appear in two different positions with respect to the other elements in the word form. There are two reasons, however, why such a phenomenon is unlikely to occur inside words. First, long-distance dependencies generally involve content words, not functional elements.<sup>42</sup> Word-internally, the equivalent would be

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<sup>42</sup>Of course, in minimalism, movement of functional elements is possible and in fact quite ubiquitous, in the form

movement of the lexical root of a word. Since most words contain only one lexical root, it is difficult to determine whether movement takes place: movement in syntax pied-pipes the morphemes attached to the moved root. If the same thing happens in morphology, movement cannot be detected.

This problem does not apply to compounds. Since compounds contain more than one lexical root, movement of one could theoretically be visible. Here, however, the problem arises that the relative order of the lexical roots in a compound is meaningful: a *car radio* is not the same as a *radio car*. A movement operation must obviously be detectable for the hearer. If the relative order of lexical items is relevant for establishing thematic relations, movement must be marked in some way. This is easier in syntax, because there are usually more lexical items available, which in addition have different categories. In English, for example, topicalisation of the object is easy to detect, because the default N-V-N order is disrupted and replaced by an N-N-V order. In contrast, topicalisation of the subject would be much harder to detect.

Obviously, if it were necessary to mark movement inside a word, language would find a way to do so, by adding a special functional marker, for example. The question is, however, why movement would take place at all. In syntax, movement is often related to discourse or information structure effects: marking of topic or focus, interrogatives, etc. Such effects are arguably irrelevant or even impossible at the word-level: since words do not convey full propositions, it is not clear what function topicalisation or interrogative marking inside a word would serve, for example.

The point here is not to show that movement inside words is fundamentally impossible. For the purpose at hand it suffices to show that the fact that we do not observe movement in morphological structures does not necessarily entail the existence of a separate morphology module. We can account for this observation in a way that is compatible with the idea that there is only a single morphosyntax module.

We can go one step further, however. It is not at all clear whether the observation is correct. The problem is that movement is often a defining characteristic of syntax. Notions such as “internal fixedness” and “uninterruptability” are typically part of the definition of the term “word” (cf. Haspelmath 2011, p. 38), which means that if we find movement, we automatically assume that the relevant phenomenon is syntactic. This tendency makes it impossible to ever prove or disprove morphological movement, obviously.

Romance object clitics illustrate this point. These clitics appear before the verb if the verb is finite and after the verb if it is non-finite:

- (53) a. lo      veo *Spanish*  
           3sg.m sehe.1sg.pr  
           ‘ich sehe ihn’
- b. ver-lo  
           sehen.inf-3sg  
           ‘ihn zu sehen’

---

of head movement, but this is exactly the kind of theory-internal movement that is irrelevant for the discussion.



Crucially, Romance object clitics must appear directly adjacent to the verb and adhere to a fixed order if there is more than one. Furthermore, there are co-occurrence restrictions (cf., e.g., Desouvrey 2005). These are properties that are more typically associated with morphological constructions. There are good reasons, therefore, to treat Romance object clitics as morphological.

Nonetheless, the fact that placement is variable and depends on the finiteness of the verb means that Romance object clitics are generally analysed syntactically (as suggested by the term *clitic*). Rather than considering the option that either the clitic or the verb stem moves word-internally, we assume that clitic placement is a syntactic process.

Summarising, the argument that there is no morphological movement is very weak. The kinds of movement (long-distance dependencies) we see in syntax make no sense in morphology, and it is not even clear how to identify morphological movement if the sheer fact that an element can appear in different positions is taken as an indication of its syntactic nature.

## 5.2 Morphology by itself

The final argument in favour of a separate morphology module that I discuss here is what might be called *autonomous morphology*: the idea, put forward by Aronoff (1994), that there are generalisations that can only be accounted for in terms of morphology. A typical example is the so-called N-pattern, discussed by Maiden (2004). The N-pattern is a pattern occurring in Romance verbal morphology, demonstrated here with the Spanish verb *jugar* ‘to play’:

	sg	pl
1st	<i>jueg-o</i>	<i>jug-amos</i>
2nd	<i>jueg-as</i>	<i>jug-áis</i>
3rd	<i>jueg-a</i>	<i>jueg-an</i>
inf	<i>jug-ar</i>	

Table 1: N-pattern: *Spanish*

The N-pattern is a stem alternation that contrasts the singular and 3rd person plural forms with the 1st and 2nd person plural forms. It occurs in many Romance languages and applies to different categories of verbs. Furthermore, the exact stem alternation also differs from verb to verb. In Spanish, for example, two frequently occurring alternations are *o/ue* and *e/ie*, but there are a few others, which occur in limited sets of verb, such as *u/ue*, which only occurs in *jugar*.

The alternation can also be suppletive, as in the French example *aller* ‘to go’, which uses the stem *v(a)-* in the singular and 3rd person plural forms, and the stem *all-* in the 1st and 2nd person plural forms:

	sg	pl
1st	<i>v-ais</i>	<i>all-ons</i>

	sg	pl
2nd	<i>va</i> -s	all-ez
3rd	<i>va</i> -	<i>v</i> -ont
inf	all-er	

Table 2: N-pattern: *French*

In short, whether a verb is subject to an N-pattern alternation or not is not predictable from the phonological form of the verb. In Spanish, for example, not all verbs with *e* or *o* show the stem alternations above (e.g., *tensar* ‘to tighten’, *poner* ‘to put’). This means that a purely phonological explanation is impossible.

Likewise, there is no syntactic or semantic property that distinguishes the two sets of forms. Therefore, so the argument goes, we need a separate morphology module to account for such facts. If morphology were just syntax, stem alternations such as this one would require a syntactic explanation, i.e., in terms of some syntactic feature or a combination of several features. There is no single feature or set of features that describes 1st, 2nd and 3rd person singular and the 3rd person plural. Only a disjunctive feature set could describe these forms, but disjunctive feature sets do not correspond to natural classes.

However, this argument only holds on a specific view of syntax: it presupposes that there is only a limited (presumably innate) set of syntactic features. This view automatically relegates any features not part of this set to morphology and/or the lexicon. If, however, syntax can also deal with language-specific features, the problem disappears. We simply need to assume that the syntactic system can, on the basis of the input forms, generalise a feature [Npat], which is assigned to the relevant verb forms (cf. Kremers 2014b, for details).<sup>43</sup>

Alternatively, one may assume that verbs that are subject to an N-pattern alternation have a special phonological form. Harris (1985), for example, assumes that an alternating verb such as *jugar* has a phonological representation with two slots, only one of which is associated with a vowel. Under specific circumstances (specifically, when the syllable is stressed) the second slot is also associated with a segment, resulting in a diphthong. Obviously, this analysis is language-specific: it could not account for suppletive N-pattern alternations such as for French *aller*.<sup>44</sup>

Again, for present purposes, it suffices to show that a separate morphology module is not the only way to account for autonomous morphology. How exactly individual cases can be analysed is a different question and is obviously beyond the scope of this paper. The example suggests that relevant phenomena can be analysed syntactically or phonologically. Note that I am not claiming that all relevant phenomena can be analysed uniformly. Some phenom-

<sup>43</sup>Note that the idea that features can be generalised on the basis of input forms does not exclude the possibility of a Universal Grammar. We must inevitably assume that the N-pattern is learnt on the basis of input forms alone, but this fact in itself is not sufficient to argue that the N-pattern is not syntactic, unless one adopts this as an axiom.

<sup>44</sup>It does have the advantage that it links the alternation to stress. This appears to be correct for Spanish, since the same alternation also shows up in other contexts, e.g., *viejo* ‘old’ vs. *vejéz* ‘age’.

ena may require a syntactic analysis, while others can be handled in phonology. This lack of uniformity does not mean that positing a separate morphology module is a better analysis, however. There is no guarantee that all relevant phenomena can be handled uniformly in a morphology module: we may need different morphological mechanisms to deal with different kinds of phenomena.

The only point of assuming a separate morphology module would be that it enables us to isolate language-dependent idiosyncrasies from syntax. This is a *conceptual* issue, however, one that is not relevant in a framework such as minimalism, which aims to relegate all idiosyncrasies to the lexicon. Note that assuming an [sc{Npat}] feature is compatible with this aim: [sc{Npat}] is a feature of individual lexical items. There is no need to adopt a special syntactic rule to accommodate it (cf. Kremers 2014b, for details).

### 5.3 Lexical Integrity

It is often claimed that words are subject to some principle that makes them opaque in syntax. Usually called the *Lexical Integrity Principle* (cf. Lapointe 1980; Anderson 1992), it states that the structure of words is not accessible to syntax and that constituent parts of words cannot be separated in syntax. This is taken as evidence for a separate morphology module. It is true that this would indeed be a possible explanation for lexical integrity effects: if words are generated in a separate module, it makes sense that the syntactic module treats them as atomic.

As a first problem, however, note that the lexical integrity requires a proper definition of the term “word”, which, as I have argued, is not unproblematic. If we cannot define “word”, then the Lexical Integrity Principle becomes meaningless. A definition of words as lexico-semantic units (i.e., lexemes) does not suffice, because particle verbs would certainly fall under that definition, even though they are separable in syntax. Nor can we define words as indivisible units in syntax, because that would make Lexical Integrity circular.

Secondly, even proponents of lexical integrity admit that it is not absolute. For example, Booij (2009) concludes:

In conclusion, this paper has shown that the principle of Lexical Integrity should be formulated in such a way as not to exclude the different modules of the grammar from ever having access to word-internal structure. Moreover, Lexical Integrity as the prohibition on syntactic manipulation of word-internal constituents is not an absolute universal, but rather the default situation. (Booij 2009, p. 98)

Similarly, Lieber and Scalise (2006) state that “[...] we know that morphology and syntax interact, and that this interaction is not a one way affair: morphology sees syntax and syntax sees morphology” (p. 10). Neither Booij (2009) nor Lieber and Scalise (2006) wish to argue that the Lexical Integrity Principle should be abandoned altogether, however. Rather, they claim that only a limited interaction between syntax and morphology should be allowed, because, as Lieber and Scalise state, “[...] this possibility predicts far more interaction than we find” (p. 30). They do not, however, explain what sort of interactions we would expect.

Moreover, I believe we would not in fact expect much more interaction than what we see. The discussion around lexical integrity usually revolves around words and their opacity to

syntactic operations. Words, however, are not the only structural units that are opaque to operations initiated at higher levels of structure. Syntactic phrases are often opaque in similar ways. Take, for example, the claim that constituent parts of words cannot be moved:

(54) \*Tea<sub>i</sub>, I have bought a t<sub>i</sub> pot.

It is true that English does not allow the fronting of a single member of a compound. However, a similar form of subextraction is disallowed for noun *phrases* as well:

(55) \*Blue<sub>i</sub>, I have bought a t<sub>i</sub> tea pot.

Why would we ascribe the impossibility of (54) to Lexical Integrity, when the apparently very similar fact in (55) cannot be ascribed to it? This question becomes even more compelling when we consider the following colloquial Russian data from Pereltsvaig (2008, p. 8, 10):

- (56) a. Čěrnogo<sub>i</sub> ja rešila ne pokupat' [NP t<sub>i</sub> xleba]! Russian  
 black I decided not to.buy bread  
 'I decided not to buy black bread.'
- b. V vagon ona xodila restoran obedat'.  
 to carriage she went restaurant to.dine  
 'She used to go dine in a carriage restaurant.'

These Russian examples show exactly the kind of displacement that English does not allow in (54) and (55). In (56)a an attributive adjective, *čěrnogo* 'black' is moved from its base position inside the noun phrase to sentence-initial position. In (56)b, part of a lexical compound is displaced.<sup>45</sup> Pereltsvaig (2008) argues that the displaced element can be either a topic or a focus, the crucial aspect that enables displacement being contrastivity. For our purposes, it suffices to observe that displacement is possible. These data suggest that "word status" is not the decisive property that determines whether subextraction is possible or not.<sup>46</sup>

Furthermore, the contrast between English and Russian shows that the factors that determine whether a structure is opaque or not are at least in part language-dependent. Another contrast, here between English and German, shows this as well:

- (57) a. Which book<sub>i</sub> were you looking for t<sub>i</sub>?  
 b. \*Welch-em Buch<sub>i</sub> hast du nach t<sub>i</sub> gesucht? German  
 which-DAT book have.2sg you to looked  
 'Which book were you looking for?'

English allows preposition stranding in *wh*-questions, German does not: in English, the preposition *for* remains in its base position while its complement, *which book*, is *wh*-fronted.

<sup>45</sup>Interestingly, it has pied-piped the preposition *v* 'in(to)', which suggests the displacement may be phonological in nature, as it targets a phonological unit, not a syntactic unit. I will not pursue this matter here, though.

<sup>46</sup>In fact, it is not clear how it could be, given the lack of a sound definition of the notion "word".

In the equivalent German clause, the preposition must be fronted along with the noun phrase. Stranding *nach* ‘to’ is not possible.

Lexical Integrity is also associated with the fact that parts of words are not available for processes such as (adjectival) modification, coordination, pronominal reference. This is illustrated by the following Hebrew examples (from Borer 2009):

- (58) a. *beyt morá* *Hebrew*  
house.CNSTR teacher  
‘the teacher’s house’
- b. *beyt sefer*  
house.CNSTR book  
‘school’

Both phrases in (58) are examples of so-called construct state constructions: a particular nominal construction in which a head noun is modified by a dependent noun. The head noun is a bare noun and cannot take a definite article. The modifying noun is either definite or indefinite and its definiteness determines the definiteness of the entire construction. The construct state construction is historically a possessive construction and can still be used in this way, as (58)a shows.<sup>47</sup> There are many lexicalised construct state constructions, however, which often have a non-transparent meaning. (58)b is an example of this. Literally, *beyt sefer* means ‘house of book’, but the lexicalised meaning of the construction is ‘school’.

As Borer shows, the two types of construct state constructions have different properties. The lexicalised construction is opaque to a number of operations: the constituent elements are not referential, cannot be modified, coordinated or referred to pronominally. The construction in (58)a is transparent to all of these processes. These examples show that integrity effects can even cut across a syntactic construction: although both examples in (58) are construct states, (58)b shows stronger integrity effects.

One might argue that (58)b is a typical example of a *lexical* integrity effect, because *beyt sefer* ‘school’ is obviously stored in the lexicon, while *beyt morá* ‘a teacher’s house’ is not. The point is, however, that there is no *structural* difference between the two examples. It is not the case that *beyt sefer* is a “word” just because it is subject to a strong structural integrity effect. As far as its structure is concerned, it is still a phrase.

One factor that appears to play an important role here is referentiality. The construct state *beyt sefer* as a whole is referential, but its component parts are not: *beyt sefer* refers to the concept SCHOOL, it does not refer to the concepts HOUSE and BOOK. In contrast, the referent of *beyt morá* consists of two entities, a teacher and a house, and both these concepts are referred to by the structure *beyt morá*. The constituent parts of the structure are therefore themselves referential, which makes the structure less opaque.

Similar effects can be observed in Dutch and German particle verbs. In these languages, it is possible to front or scramble the particle of a particle verb under certain circumstances (Zeller

<sup>47</sup>In Modern Hebrew, possession is often realised with the element *šel*, which can be translated with ‘of’. In Standard Arabic, the construct state is still the normal way to express possession.

2001; Lüdeling 2001; S. Müller 2002):<sup>48</sup>

- (59) a. An der Haltestelle stiegen hübsche Frauen ein. *Aus* stiegen  
at the bus.stop climbed pretty women in out climbed  
nur Männer.  
only men  
'At the bus stop, pretty women got on. Only men got off.'  
(Zeller 2001, p. 89)
- b. Ich weiß, dass die Sonne *auf* im Osten und *unter* im  
I know that the sun up in.the East and down in.the  
Westen geht.  
West goes  
'I know the sun goes up in the East and down in the West.'  
(Lüdeling 2001, p. 50)

However, fronting or scrambling is not always possible:

- (60) a. \*?*Ab* ist Nixon 1974 getreten. German  
off is Nixon 1974 stepped  
intended: 'Nixon resigned in 1974'  
(Zeller 1999, p. 64)
- b. \*... dat Jan het meisje *op* vaak merkte. Dutch  
that Jan the girl up often noticed  
(Neeleman and Weerman 1993, p. 436)

S. Müller (2002, p. 136) looks at these structures in detail and reaches the following conclusions:

It is clear that in many instances fronting, separation, and modification are impossible, but this is not due to general properties of particle verbs. In the case of particle fronting and intraposition, other factors interfere, like information structure and the possibility of establishing a contrast. Whether a particle can be modified or not depends on semantic factors. In my opinion, it is the right approach to allow fronting, intraposition, and modification and to account for the appropriate syntactic structures. Additional constraints like those discussed for fronting then rule out or specify the markedness of certain constructions.

Müller's conclusions are clear: the structural integrity effects that we observe with particle verbs are not the result of their structure. They result from external factors: information structure, contrast, semantics. The examples in (58) suggest that the same is true for Hebrew

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<sup>48</sup>In these examples, the relevant particle is in italics.

construct states as well: it is not the structure itself that yields integrity effects. Rather, they result from external factors.<sup>49</sup>

As stated above, it is not the purpose of this paper to explain integrity effects. The factors involved are obviously diverse and possibly language-dependent. The important point here is that integrity effects do not distinguish morphological structures from syntactic ones and that a strict distinction between syntax and morphology is therefore not a plausible explanation for them.

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#### 5.4 Structural Integrity: Outlook

Speculating on the cause of integrity effects, one thing stands out: in general, structures that are transparent are semantically complex, while semantically simplex structures are syntactically opaque. This is best illustrated by the Hebrew examples in (58). The construct state *beyt sefer* ‘school’ is opaque to a number of operations; semantically, it is simplex. This contrasts with *beyt morá*, which is transparent to the same operations and is semantically complex: it references two concepts rather than one. Similarly, those particle verbs that allow fronting of the particle are to a certain extent semantically transparent.

There thus seems to be a general integrity principle, which we may formulate as follows:

(61) *Structural Correspondence*

Semantic units correspond to syntactic units, and vice versa.

Obviously, we would need to specify what exactly constitutes semantic and syntactic units. Relevant syntactic units could be all levels of structure, all maximal projections, or possibly only those maximal projections that are also phases (at least CP, vP, DP). On the semantic side, the term *unit* is even more difficult to define, since not only concepts are intended, but also larger structures. Possibly, relevant structures are all structures that do not contain free variables.<sup>50</sup>

Whatever the definitions, however, the intuitive idea is that syntactic structures can only be transparent to outside operations if they constitute a semantic unit of some kind. Movement, for example, only applies to elements that form a meaning unit. Obviously, they may be part of a larger meaning unit (ultimately, they are part of a sentence / proposition), but they are semantically “complete” in some sense.

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<sup>49</sup>This does not mean that all integrity effects result from external factors. Hebrew construct states do not allow extraction, which is presumably a structural restriction. Similarly, the impossibility of preposition stranding in German may be a result of the fact that the preposition assigns (overt) case to the noun phrase.

<sup>50</sup>The name *Structural Correspondence* is deliberately reminiscent of Linear and Input Correspondence. In a fundamental way, the three principles all do the same thing: keeping together what belongs together across module boundaries.

If these speculations are on the right track, they have an immediate consequence for the morphology / syntax discussion. The smallest semantic units, those that are simplex, will tend to correspond to indivisible syntactic units, i.e., to heads. In turn, syntactic heads tend to correspond to prosodic words. These two tendencies strengthen the syntax/morphology illusion, but this is not a deep property of the language faculty. Rather, it is the result of the combination of two facts: (a) semantic units tend to be realised as syntactic units; and (b) syntactic heads tend to be realised as prosodic words. Both these facts are interface effects: if semantic units would not correspond to syntactic units, it would be impossible to encode any complex meaning in a reliable, reconstructable manner. Similarly, the prosodic word is the lowest constituent of the prosodic hierarchy that can provide enough forms to accommodate the wide variety of concepts that the semantic system provides, therefore, a syntactic head needs to correspond to (at least) a prosodic word.<sup>51</sup>

All of this is speculation, of course, and delving into these questions further would go well beyond the scope of this paper. There is, however, one more interesting consequence of a principle such as Structural Correspondence. Since Ross (1967), one of the questions that syntactic research has been trying to answer is why certain structures are “islands”, i.e., do not allow extraction. Given Structural Correspondence, however, we would expect opacity to be the norm rather than the exception. That is, the question that we should ask is not why certain structures are opaque, but rather why certain structures are transparent. I will leave these questions open for future research, however.

## 6 Summary and conclusions

The observation that syntax and morphology perform the same function, linking form (phonology) and meaning (semantics) suggests that there should be one unified module handling both syntactic and morphological structures. Pursuing this idea, the empirical differences between what are traditionally called syntactic and morphological structures can be explained if we take into account the contribution that the phonological module makes to the construction of linguistic forms. In essence, the syntax/morphology distinction is an acoustic illusion: a syntactic structure that is mapped onto a prosodic word is generally considered to be “morphological”, while a syntactic structure that is mapped onto phonological phrases is considered “syntactic”. In reality, however, the structure-building mechanism is the same.

For example, we can analyse a suffix such as the English gerund suffix *-ing* as a syntactic head if we assume that its placement, including the fact that it is a suffix, is handled in phonology. The lexical entry for the gerund head specifies that its phonological form is a syllable that must right-align with a prosodic word. Through the principle of Input Correspondence, categorial selection ensures that the prosodic word to which  $\Phi(-ing)$  attaches is the prosodic word corresponding to the head of the verbal projection that *-ing* c-selects. We can achieve this result by classifying suffixes such as *-ing* as prosodic morphemes, due to their prosodic alignment requirement. This means that they are not subject to the normal linearisation rules, allowing them to be associated with the correct prosodic word.

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<sup>51</sup>In fact, some languages, such as Chinese, seem to have opted for the syllable rather than the prosodic word. The lexical use of tone of course greatly expands the number of possible phonological forms.



In this way, we can deal with morphological structures in syntax without adopting special syntactic rules that apply only to word-level structures. Idiosyncrasies, which are often (though not always, cf. e.g., Turkish, Finnish, etc.) a hallmark of a language's morphology, can be handled in the lexicon. There is no need, for example, to mark *-ing* as a suffix. Its suffix properties follow from its phonological form as a prosodic morpheme.

Not all phenomena can be handled by assuming a specific phonological form. There are phenomena that must be handled by assuming idiosyncratic morphosyntactic features. An example of this is the [sc{Npat}] feature that can be used to describe the so-called N-pattern in Romance verbal morphology. Although it is obviously not possible in a single paper to show that every possible morphological phenomenon can be treated satisfactorily in the model outlined here, the examples discussed here should offer a sufficient starting point for analysing other phenomena.

Three often cited arguments in favour of a separate morphology module, lack of morphological (word-internal) movement, autonomous morphological processes and lexical integrity, do not provide enough evidence to support the idea of a separate morphology. Most importantly, although a separate module would be one way to account for the observations, it is not the *only* conceivable way. Furthermore, in the case of word-internal movement and lexical integrity, a closer look at the relevant phenomena shows that the evidence they present is not as strong as often suggested. Since there is no criterion for what would constitute word-internal movement, it is not at all clear how it can be shown that it does not exist. Moreover, since intuitively, movement is tied to syntax, possible cases of word-internal movement are by definition analysed as syntactic. This fact, however, makes the argument circular.

As for lexical integrity, it can be shown that integrity effect occur essentially at all levels of syntactic structure. Extraction of a member of a compound is not possible in English and many other languages, but the same is true for extraction of an attributive adjective out of a noun phrase. Rather than asking why words are different, we should ask why certain structures are *not* opaque to outside operations.

The view of the architecture of grammar sketched in this paper has interesting consequences for the theory of grammar. First, it suggests that the ideas behind prosodic morphology can and must be extended to syntax as well. This opens the way to new analyses of phenomena such as linearisation, focus alignment, intonation, and probably others. Furthermore, a number of properties that are often understood as rules or principles of grammar may in fact be third-factor effects: properties that are not formalised as part of UG or of the grammar of a language (i.e., an I-language) and which therefore have no direct influence on derivations. They do, however, have an influence on the development of an I-language and can thus shape the grammar of a language, albeit indirectly.

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