

# Interpreting embedded verb second

## Causal modifiers in German

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The paper discusses V2 order in German adverbial clauses expressing causation. As an embedded root phenomenon, V2 order in adverbial clauses is associated with an assertive force potential of the clause. Furthermore, V2 adverbials can express a wider range of semantic interpretations than their verb-final counterparts. They can be interpreted as central adverbials, modifying the event structure, or as peripheral adverbials, modifying the discourse structure. In this paper, I will derive the possible interpretations of V2 adverbials from three factors: an invariable semantics of the causal connective *weil* (roughly ‘because’), the syntactic attachment site of the adverbial clause and the contribution of pragmatic principles.

### 1. Introduction

German is an asymmetric verb second (henceforth V2) language. The finite verb appears in the second position in main clauses (1-a) and in clause final position in subordinate clauses (1-b).

- (1) a. Sam isst nur Salat.  
Sam eats only salad.  
‘Sam only eats salad.’  
b. Sam ist sehr mager, weil er nur Salat isst.  
Sam is very skinny because he only salad eats.  
‘Sam is very skinny because he only eats salad.’

Yet, in spoken German, adverbial clauses introduced by *weil* ‘because’ are frequently used with V2 order:

- (2) Sam ist sehr mager, weil er isst nur Salat.  
Sam is very skinny because he eats only salad.

In this paper, I will argue that V2 *weil*-clauses are not simply a colloquial synonymous variant of standard verb-final *weil*-clauses, but that they have specific syntactic, semantic, and prag-

matic properties. V2 *weil*-clauses can express a wider range of semantic relations than causal adjunct clauses with verb-final order. The main issue of this paper is to identify the possible semantic interpretations of V2 *weil*-clauses and to determine the factors that are relevant to the composition of these different interpretations. I will develop a compositional analysis to explain the possible interpretations of V2 *weil*-clauses based on three factors: the semantic contribution of the connective *weil*, the syntactic attachment site of the adverbial clause relative to its host clause, and general principles of utterance.

In the next section, I will show that V2 and verb-final *weil*-clauses do not occur in the same contexts. In section 3., we will see that these syntactic differences go hand in hand with interpretive differences. Based on these observations, I will develop a syntactic analysis of V2 adverbials in section 4. Since the different interpretations cannot be traced back to syntax, in section 5., I will develop a compositional derivation of these differences at the syntax-semantics-pragmatics interface. In section 6., I will show that V2 *weil*-clauses cannot occur in presupposed contexts due to the assertive force potential triggered by the movement of the finite verb to C.

## 2. The distribution of V2 and verb-final *weil*-clauses

The occurrence of V2 *weil*-clauses is more restricted than the occurrence of standard adjunct clauses with verb-final order. The V2 realizations are subject to numerous syntactic restrictions. Verb raising is blocked whenever the causal clause is within the scope of an element of the preceding main clause.<sup>1</sup> Whereas example (3-a) is well-formed, V2 order is illicit if the *weil*-clause is within the scope of the matrix negation (3-b):

- (3) a. Paul fährt nicht nach Berlin, weil er dort Familie **hat**, sondern weil er die Stadt besichtigen will.  
 Paul goes not to Berlin because he there family has but because he the town visit wants  
 ‘Paul doesn’t go to Berlin because he has family there but because he wants to visit the town.’
- b.\*Paul fährt nicht nach Berlin, weil er **hat** dort Familie, sondern weil er will die Stadt besichtigen.  
 Paul goes not to Berlin because he has there family but because he wants the town visit.

The same degradation can be observed if a V2 *weil*-clause is within the scope of a correlative or a presupposing focus particle. If the adjunct clause has verb-final order, the sentences are fine.<sup>2</sup>

- (4) a. Petra hat heute deshalb gefehlt, weil sie krank **ist**.  
 Petra has today COR missed because she ill is.  
 ‘Petra has missed today because she is ill.’
- b.\*Petra hat heute deshalb gefehlt, weil sie **ist** krank.

<sup>1</sup>I will avoid the term matrix clause since we will see that V2 *weil*-clauses are unembeddable.

<sup>2</sup>list of abbreviations used in this paper: COR = correlative, PART = particle, verb-IMP = imperative morphology, PRO = pronoun

- (5) a. Lukas hat auch gefehlt, weil er krank **war**.  
 Lukas has too missed because he ill was.  
 ‘Lukas has missed, too, because he was ill.’  
 b.\*Lukas hat auch gefehlt, weil er **war** krank.

Unlike *weil*-clauses with verb-final order, V2 *weil*-clauses may not be within the scope of the matrix interrogative operator.

- (6) a. Bist du böse, weil ich deinen Geburtstag vergessen **habe**?  
 Are you angry because I your birthday forgotten have?  
 ‘Are you angry because I have forgotten your birthday?’  
 b.\*Bist du böse, weil ich **habe** deinen Geburtstag vergessen?

Binding theory provides another difference between the two types of adjunct clauses. Unlike the *weil*-clause with verb-final order in (7-a), the V2 clause in (7-b) can contain R-expressions which are co-referential with a subject NP in the associated main clause. Thus, condition C is only violated if the adjunct clause has verb-final order.

- (7) a.\**Sie<sub>i</sub>* war verärgert, weil *Maria<sub>i</sub>* nicht eingeladen **war**.  
*she<sub>i</sub>* was angry because *Maria<sub>i</sub>* not invited was.  
 ‘*She<sub>i</sub>* was angry because *Maria<sub>i</sub>* wasn’t invited.’  
 b. *Sie<sub>i</sub>* war verärgert, weil *Maria<sub>i</sub>* **war** nicht eingeladen.

Similarly, variable-binding into an embedded V2 clause is degraded as shown in (8-b).

- (8) a. Niemand<sub>i</sub> war verärgert, weil er<sub>i</sub> nicht eingeladen **war**.  
 nobody<sub>i</sub> was angry because he<sub>i</sub> not invited was  
 ‘Nobody<sub>i</sub> was angry because he<sub>i</sub> **was** not invited.’  
 b.\*Niemand<sub>i</sub> war verärgert, weil er<sub>i</sub> war nicht eingeladen.

Another difference between *weil*-clauses with verb-final order and those with V2 order is linked to the V2 restriction found in German. Whereas a canonical verb-final *weil*-clause may occur as the first constituent of a V2 host clause (in traditional German terminology the *Vorfeld*), a V2 *weil*-clause cannot function as the first constituent in a V2 clause (see (9-a)). According to Haegeman et al. (2008), adverbials that occupy the *Vorfeld* must be integrated into the syntactic structure of the associated main clause. Therefore, the ungrammaticality of example (9-b) suggests that V2 *weil*-clauses are characterized by higher syntactic disintegration.

- (9) a. Weil Paula krank **ist**, war sie heute nicht in der Schule.  
 because Paula ill is was she today not in the school.  
 ‘Because Paula was ill she missed school today.’  
 b.\*Weil Paula **ist** krank, war sie heute nicht in der Schule.

Finally, V2 *weil*-clauses are often set off by comma intonation (\\) from the associated main clause, whereas *weil*-clauses with verb-final order are generally integrated into its prosodic contour (/).

- (10) a. Paula fährt morgen nach Hamburg, (/) weil ihre Mutter Geburtstag **hat**.  
 Paula goes tomorrow to Hamburg (/) because her mother birthday has.  
 ‘Tomorrow, Paula goes to Hamburg because it’s her mother’s birthday.’  
 b. Paula fährt morgen nach Hamburg, (\) weil ihre Mutter **hat** Geburtstag.

The distribution of V2 order in German *weil*-clauses is summarized in table 1.

	Verb-final <i>weil</i> -clause	V2 <i>weil</i> -clause
within scope of negation	+	-
within scope of a correlative	+	-
within scope of a focus particle	+	-
within scope of the matrix interrogative operator	+	-
within c-command domain	+	-
position in the <i>Vorfeld</i>	+	-
prosodic integration	+	-

Table 1: The distribution of V2 and verb-final order

Most of the properties in table 1 are subject to c-command configurations. On the basis of these structure diagnostics, I assume that, unlike adjunct clauses with verb-final order, V2 *weil*-clauses must be generated in a position outside the c-command domain of the associated main clause. In section 4, I will show that verb-final *weil*-clauses and V2 *weil*-clauses are distinguished in terms of their external and internal syntax. The latter are less integrated into the syntactic structure of the main clause than the former. Before returning to the syntactic structure of the sentences, I will first discuss the possible interpretations of V2 *weil*-clauses.

### 3. The interpretation of V2 *weil*-clauses

Embedded V2 order is also attested for other Germanic languages, mainly for Frisian and Mainland Scandinavian (see Heycock 2005 for an overview). In German, embedded V2 is possible in relative clauses that modify indefinite heads and in the complements of bridge verbs:

- (11) a. Berlin hat viele Häuser, die stehen leer.  
 Berlin has many houses that are empty.  
 b. Paul glaubt, Paula hat den Bus verpasst.  
 Paul believes Paula has the bus missed.  
 ‘Paul believes Paula has missed the bus.’

Interestingly, these embedded V2 clauses share the same distributional restrictions as V2 *weil*-clauses, namely, they cannot occur in the scope of negation, of a correlative or of a focus particle (see table 1). However, when it comes to the semantic interpretation of the embedded V2 clauses in (11), they receive the same interpretations as their verb-final counterparts. The V2 relative in

(11-a) is interpreted as restrictive relative and the V2 complement clause in (11-b) is interpreted as selected argument of the matrix verb (as argued in Frank 2000 and Gärtner 2001).

In contrast, there are important interpretive differences between *weil*-clauses with verb-final order and those with V2 order. Whereas *weil*-clauses with verb-final order can only express one possible reading, V2 *weil*-clauses can have three possible interpretations. In the following, I will provide an overview of the three semantic interpretations (see also Antomo & Steinbach to appear; Uhmann 1998; Scheffler 2005; Wegener 1993). My basic assumption is that the connective *weil* expresses in all three readings the same semantic contribution, which I will briefly present in the following subsection.

### 3.1. The semantics of *weil*

Connectives act as logical predicates taking propositions as their arguments. I will treat propositions as existential quantifications over events by assuming that predicates have in addition to their semantic arguments an event variable as one of their arguments. To form a proposition, this variable must be bound by a quantificational operator such as an existential quantifier (as argued in Davidson 1967 and Johnston 1994).

The connective *weil* introduces a two-place relation over events which it links causally. To represent the semantics of *weil*, I will use the two-place predicate CAUSE. I assume the semantic type of *weil* to be  $\langle t, \langle t, t \rangle \rangle$ . The truth conditions of CAUSE are given in (12) and the formal representation of a standard *weil*-clause (13-a) is given in (13-b).

(12) If  $q$  and  $p$  are propositions, then CAUSE ( $p$ ,  $q$ ) is true iff  $q$  is true as a result of  $p$  being true.

(13) a. Paula kauft eine Tulpe, weil sie Blumen mag.  
Paula buys a tulip because she flowers likes.  
'Paula buys a tulip because she likes flowers.'

b. CAUSE ( $\exists e_1$  [like (Paula, flowers,  $e_1$ )],  $\exists e_2$  [buy (Paula, tulip,  $e_2$ )])

Whereas the semantic contribution of *weil* is the same for all three interpretations, that is, it expresses a causal relation between two events, the selectional requirements that *weil* places on its  $q$ -argument are less determined (see also Pittner 1999:339 et seqq.). We will see that the first argument ( $p$ ) is always the proposition of the clause introduced by *weil*. In contrast, the second argument (the modifiee) can have different values.

### 3.2. Propositional modification

In the standard case, both arguments of *weil* are propositions. In example (14-a), the *weil*-clause expresses a reason for the propositional content of the main clause. The snow is the reason why the road is white.

- (14) a. Die Straße ist weiß, weil es geschneit **hat**.  
 the road is white because it snowed has.  
 ‘The road is white because it has snowed.’  
 b. CAUSE ( $\exists e_1$  [snow ( $e_1$ )],  $\exists e_2$  [white (street,  $e_2$ )])

If the adverbial clause is not within the scope of the matrix negation or another element of the matrix clause, it can have V2 order. This doesn't effect the causal relation between the two clauses. As example (14-a), example (15) is interpreted as causal relation between two propositional arguments.

- (15) Die Straße ist weiß, weil es **hat** geschneit.  
 the road is white because it has snowed.

Thus, this interpretation is available for both types of adverbial clauses introduced by *weil*. But, whereas it is the only possible interpretation available for integrated *weil*-clauses with verb-final order, V2 *weil*-clauses can express two additional readings.

### 3.3. Epistemic modification

A V2 *weil*-clause does not always modify the propositional content of its associated main clause. In example (16), the two-place predicate CAUSE takes an epistemic argument as its second argument and expresses a causal relation between a proposition and the speaker's attitude. The V2 *weil*-clause is interpreted on the illocutionary level and expresses an evidence for the claim expressed in the associated clause. The reason why the speaker believes that there must have been an accident is that the car's inflatable safety bag is deployed.

- (16) Es hat einen Unfall gegeben, weil der Airbag **ist** aufgegangen.  
 it has an accident given because the airbag has deployed.  
 ‘An accident has happened because the airbag has deployed.’

This semantic interpretation is represented in (17). The difference to an interpretation as propositional modification such as in (15) is that in (17), the predicate CAUSE takes an epistemic argument as argument. Thus, the causal relation is established between a proposition, the clause introduced by *weil*, and the speaker's attitude, represented according to Scheffler (2008:79) as an epistemic operator MUST.

- (17) CAUSE ( $\exists e_1$  [deploy (airbag,  $e_1$ )], MUST [ $\exists e_2$  [happen (accident,  $e_2$ )]])

Note also that the temporal order of the two arguments of the predicate CAUSE is inverse to that of a causal relation between two propositions as in example (15).

If the adverbial clause shows verb-final order, an interpretation of the *weil*-clause as epistemic modifier is not possible. This becomes clearer if the *weil*-clause is in the *Vorfeld* (and hence a constituent of the matrix clause).

- (18) a. Es hat einen Unfall gegeben, (/) weil der Airbag aufgegangen **ist**.  
 it has an accident given (/) because the airbag deployed has.

- b. Weil der Airbag aufgegangen ist, hat es einen Unfall gegeben.  
because the airbag deployed has has it an accident given.

Both examples in (18) can only be interpreted as causal relation between two propositions as represented in (19): A suddenly deploying safety bag causes an accident.

- (19) CAUSE ( $\exists e_1$  [deploy (airbag,  $e_1$ )],  $\exists e_2$  [happen (accident,  $e_2$ )])

### 3.4. Speech act modification

Another possible reading, which is only available if the *weil*-clause has V2 order, is an interpretation as speech act modifier. By uttering the V2 *weil*-clause in example (20), the speaker justifies the utterance of the preceding main clause. In this case, CAUSE takes an utterance as its cause argument as represented in (21).

- (20) Paula hat den Job übrigens bekommen. Weil das wolltest du doch unbedingt  
Paula has the job by-the-way got. because that wanted you PART absolutely  
wissen.  
know.

‘By the way, Paula got the job. Because you absolutely wanted to know it.’

- (21) CAUSE ( $\exists e_1$  [want to know (you, it,  $e_1$ )], ASSERT [ $\exists e_2$  [get (Paula, job,  $e_2$ )]])

Note, that in this use the V2 *weil*-clause can follow either an assertive speech act, an interrogative clause, or an imperative.

- (22) a. Hast du mal fünf Euro? Weil ich habe mein Portemonnaie vergessen.  
has you PART five euro? because I have my purse forgotten.  
‘Do you have five euro? Because I forgot my purse.’  
b. Vertrauen Sie diesem Mann nicht! Weil ich kenne ihn.  
trust-IMP you this man not! Because I know him.  
‘Don’t trust this man! Because I know him.’

Again, this interpretation is not available for *weil*-clauses with verb-final order. Whereas the V2 *weil*-clause in example (22-b) is interpreted as speech act modifier, the corresponding *weil*-clause with verb-final order in (23) can only express a reason for the propositional content of the main clause.

- (23) Vertrauen Sie diesem Mann nicht, (/) weil ich ihn kenne!  
trust-IMP you this man not (/) because I him know!  
‘Don’t trust this man (only) because I know him.’

In this section, I have shown that V2 *weil*-clauses can express three different semantic interpretations, whereas *weil*-clauses with verb-final order have only one possible reading<sup>3</sup>. I argued

<sup>3</sup>Note, however, that a *weil*-clause with verb-final order can express all three semantic interpretations if it is separated by comma intonation from its matrix clause (see example (34)).

that the semantics of the connective *weil*, represented by a two-place predicate CAUSE, is the same for all three interpretations. It expresses always a causal relation between its two arguments. Yet, the selectional requirements that it places on its q-argument allow for three different types of arguments. If the *weil*-clause has V2 order, the modifiee can be either a proposition, an epistemic state, or an utterance. In contrast, if the adverbial clause has verb-final order, the modifiee can only be a proposition.

It is an interesting question to know why V2 *weil*-clauses allow a wider range of semantic relations than causal clauses with verb-final order. In the next two sections, I will develop an account to derive the different interpretations from different degrees of syntactic attachment and the contribution of general principles of utterance.

#### 4. The syntax of V2 *weil*-clauses

##### 4.1. Internal syntax

In this subsection, I will adopt an analysis which has been proposed by Haegeman (2003) and Haegeman (2006) for the syntactic description of different types of conditional clauses. I will argue that the C-system of *weil*-clauses with verb-final order is truncated, whereas V2 *weil*-clauses contain a full-fledged CP similar to that of root clauses. The theoretical background is provided by a richly articulated left periphery in the sense of Rizzi (1997). In the following, I will provide empirical arguments for this proposal.

V2 *weil*-clauses have illocutionary force, whereas their verb-final counterparts are generally integrated into the illocution of their host clause. The adverbial clause with verb-final order in (24-a) is within the scope of the matrix interrogative operator. Both clauses form together one illocution. In contrast, the V2 adverbial in example (24-b) forms an independent illocution and is interpreted as speech act modification.

- (24) a. Kommt er, weil er es versprochen hat?  
 comes he because he it promised has?  
 ‘Does he come because he promised it?’  
 b. Kommt er? Weil er hat es versprochen.  
 comes he? because he has it promised.  
 ‘Does he come? Because he promised it.’

Along with Rizzi (1997), I assume that illocutionary force is encoded in a syntactic head Force. Then example (24-b) suggests that V2 *weil*-clauses have a Force projection, whereas verb-final *weil*-clauses lack Force. Further support for this assumption is provided by the fact that speech act markers can be inserted into a V2 *weil*-clause, whereas the insertion of an assertive particle into a verb-final *weil*-clause leads to degradation.

- (25) a. ?Das Kind ist traurig, weil es nämlich seinen Teddy verloren hat.  
 the child is sad because he PART his teddy lost has.  
 ‘The child is sad because it lost his teddy bear.’



- b. Paula ist krank, weil sie war heute nämlich nicht in der Schule.  
 Paula is ill because she was today PART not in the school.  
 ‘Paula is ill, because she wasn’t at school, today.’

Both examples show that V2 *weil*-clauses have an illocutionary force potential and hence the syntactic head Force, whereas adverbial clauses with verb-final order lack Force. According to Haegeman (2003), the syntactic head Force licenses the presence of TopP and FocP. If the projection Force is not present because the clause has no illocutionary force, TopP and FocP do not occur, either. That this conclusion holds for German adverbial clauses expressing causation can be observed in example (26). Here, argument fronting is not available in *weil*-clauses with verb-final order, which lack Force, but it is possible if the adverbial clause has V2 order and hence Force.

- (26) a. \*Paula ist zufrieden, weil jeden<sub>1</sub> sie t<sub>1</sub> begrüßt hat.  
 Paula is pleased because everybody<sub>1</sub> she t<sub>1</sub> greeted has.  
 ‘Paula is pleased because she greeted everybody.’  
 b. Paula ist zufrieden, weil jeden<sub>1</sub> hat sie t<sub>1</sub> begrüßt.  
 Paula is pleased because everybody<sub>1</sub> has she t<sub>1</sub> greeted.

Assuming that the target position of fronted arguments are FocP and TopP, I conclude that the verb-final *weil*-clause in (26-a) lacks these projections. In contrast, the grammaticality of example (26-b) suggests that V2 *weil*-clauses contain target positions for fronted arguments and hence a complete C-layer. In this aspect, German verb-final and V2 *weil*-clauses behave just like central and peripheral adverbial clauses in (amongst others) English (see Haegeman 2003 and Hooper & Thompson 1973).

According to Haegeman (2006) and Julien (2007), I will distinguish between the projection Force, which encodes illocutionary force in unembedded clauses, and Sub (= Subordination), the highest projection in embedded clauses. Whereas verb-final *weil*-clauses are headed by SubP and lack Force, V2 *weil*-clauses have the same internal structure as root clauses. In sum, I assume the three structures in (27) for the syntactic representation of the left periphery of (a) unembedded main clauses, (b) V2 *weil*-clauses and (c) *weil*-clauses with verb-final order.

- |                                 |     |     |     |       |     |
|---------------------------------|-----|-----|-----|-------|-----|
| (27) Unembedded clause          |     | Top | Foc | Force | Fin |
| V2 <i>weil</i> -clause:         |     | Top | Foc | Force | Fin |
| Verb-final <i>weil</i> -clause: | Sub |     |     |       | Fin |

#### 4.2. Syntactic attachment

Concerning different types of adverbial modifiers, it has often been observed that there is a connection between the syntactic position of the adverbial clause relative to its host clause and its interpretation (see Johnston 1994, Haegeman 1991 and Haegeman 2003). In this section, I will investigate the question to which extent the semantic differences between different types of *weil*-clauses in German can be traced back to syntactic differences.

Generally, a canonical *weil*-clause with verb-final order is analyzed as an adjunct to the matrix IP as shown in figure 1<sup>4</sup>.

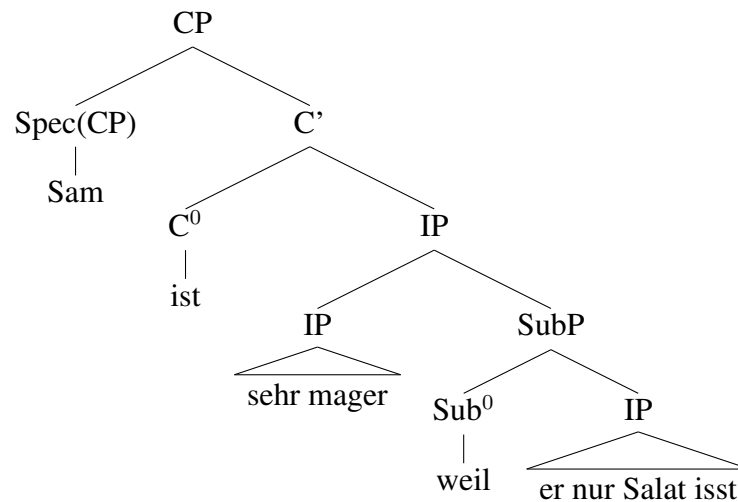


Figure 1: Syntactic attachment of verb-final *weil*-clauses

In this position, the adverbial clause cannot take scope over the matrix speech act operator. The modifiee, that is the value of the *q*-argument of the CAUSE relation, is limited by syntactic principles to the propositional content of IP. As a consequence, verb-final *weil*-clauses are always propositional modifiers and cannot be interpreted as epistemic or speech act modifications.

In section 3, we have seen that unlike verb-final *weil*-clauses, V2 *weil*-clauses can be interpreted as propositional, epistemic, or speech act modification. An exact mapping between semantic representation and syntactic structure would therefore result in three different syntactic positions for V2 *weil*-clauses for the three uses shown in section 3. The different interpretations would hence arise as a result of the adverbial clause being adjoined to three different projections. However, structure diagnostics do not suggest a syntactic distinction of three different attachment sites. Quite contrary to what one might expect, syntactically, the three uses of the V2 adverbials behave the same way. In table 1, we have already seen that V2 adverbials are outside the *c*-command domain of the associated clause. This is true for all three semantic uses of V2 *weil*-clauses, even for the event-modifying adverbials such as (15). Take, as an example, the interaction with negation and variable-binding. The examples (28) and (29) contain each a propositional modifier, an epistemic modifier and a speech-act modifier with V2 order. As can be seen in (28), all three types of V2 *weil*-clauses are outside the scope of negation. Similarly, variable-binding is degraded in all three uses as shown in (29).

- (28) a.\*Ich gehe nicht ins Kino, weil ich mag den Film, sondern weil ich mag  
 I go not in-PREP cinema because I like the film but because I like  
 den Schauspieler.  
 the actor.

<sup>4</sup>The use of the notion “CP” corresponds to the full-fledged structure in (27).

- b.\*Paula ist nicht krank, weil sie war nicht in der Schule, sondern weil ich habe  
 Paula is not ill because she was not in the school but because I have  
 ihre Mutter getroffen.  
 her mother met.
- c.\*Spiel nicht mit dem Hund, weil er hat ein Kind gebissen, sondern weil  
 play-IMP not with the dog because he has a child bitten but because  
 er hat Flöhe.  
 he has fleas.
- (29) a.\*Niemand<sub>i</sub> war verärgert, weil er<sub>i</sub> war nicht eingeladen.  
 nobody<sub>i</sub> was angry because he<sub>i</sub> was not invited.
- b.\*Jeder<sub>i</sub> schläft schon, weil in seinem<sub>i</sub> Zimmer brennt kein Licht mehr.  
 everybody<sub>i</sub> sleeps already because in his<sub>i</sub> room burns no light anymore.  
 ‘Everybody<sub>i</sub> is already sleeping because the light in his<sub>i</sub> room is turned out.’
- c.\*War denn niemand<sub>i</sub> verärgert? Weil er<sub>i</sub> hat so wütend geguckt.  
 was PART nobody<sub>i</sub> angry? because he<sub>i</sub> has so furious looked.  
 ‘Wasn’t anybody<sub>i</sub> angry? Because he<sub>i</sub> looked so furious.’

Thus, even if a V2 *weil*-clause modifies the propositional content of the matrix clause as in (28-a) and (29-a), it is outside the scope of negation or variable-binding. Similarly, none of the three types of V2 *weil*-clauses can function as the first constituent of a V2 clause as shown in (30).

- (30) a.\*Weil es hat geschneit, ist die Straße ganz weiß.  
 because it has snowed is the road totally white.
- b.\*Weil der Airbag ist aufgegangen, hat es einen Unfall gegeben.  
 because the airbag has deployed has it an accident given.
- c.\*Weil du wolltest das unbedingt wissen, hat Paula den Job bekommen.  
 because you wanted it absolutely know has Paula the job got.

Again, the examples shows that V2 *weil*-clauses are distinguished by a lower degree of syntactic integration than *weil*-clauses with verb-final order. Even event-modifying V2 *weil*-clauses turn out to be syntactically unembeddable. Furthermore, the examples show, that the semantic differences between the three uses of V2 *weil*-clauses cannot be traced back to three different syntactic attachment sites. Concerning negation, the possibility of variable-binding and the integration as the first constituent of a V2 clause, propositional, epistemic, and speech-act modifying V2 *weil*-clauses behave exactly the same way.<sup>5</sup> Thus, the connection existing between meaning and syntactic position does not function in a ratio of 1:1. Instead of establishing a syntactic distinction between the three semantic uses of V2 *weil*-clauses, the application of the structure diagnostics reveals rather a contrast between *weil*-clauses with verb-final order on the one side and V2 *weil*-clauses on the other (see also table 1 in section 2). Since all V2 *weil*-clauses show the same pattern with respect to the structure diagnostics, it seems reasonable

<sup>5</sup>The three semantic types of V2 *weil*-clauses behave also the same way with respect to the following structure diagnostics: They cannot be within the scope of a correlative or of the matrix interrogative operator and they are prosodically independent. Argument fronting and the insertion of speech act marker is possible in all three types.

to explain this pattern for reasons independent of the semantic interpretation of the adverbial clause. In section 6, I will argue that the movement of the finite verb to C is associated to an assertive force potential. As a consequence, a V2 *weil*-clause cannot be presupposed and must be outside the scope of the associated main clause. The syntactic similarities between the three uses of V2 adverbials arise therefore as a consequence of the position of the finite verb in these clauses, and they are independent of the semantic interpretation of the causal relation.

However, there are some differences between propositional and epistemic modifiers on the one hand, and speech-act modifiers on the other. Interpreted as a speech act modification, a V2 *weil*-clause can be used alone. In example (31), the adverbial has no overt linguistic antecedent. This means that the modifiee of the causal relation has not to be realized.

- (31) (context: A gives a wrapped box to B)  
 a. Weil du hattest doch Geburtstag.  
     because you had PART birthday.

In contrast, example (32) shows that *weil*-clauses which function as propositional or epistemic modifiers cannot be used without a sentential antecedent.

- (32) a. \*(Die Straße ist ganz weiß), weil es hat geschneit.  
       (the road is totally white) because it has snowed.  
     b. \*(Es hat geschneit), weil die Straße ist ganz weiß.  
       (it has snowed) because the road is totally white.

Furthermore, we have seen in section 3.4. that, unlike propositional and epistemic V2 *weil*-clauses, speech act modifiers can follow either an assertion, an interrogative, or an imperative clause. This means that the assertive force potential of the V2 adverbial cannot be absorbed by the associated main clause. These differences between speech act modifying V2 *weil*-clauses on the one hand and propositional and epistemic clauses on the other suggest two distinct syntactic treatments. Let's consider first the speech act modifying use of V2 *weil*-clauses. According to Haegeman (1991), the fact that a conditional clause as (33) can be used alone is an argument to abandon syntactic attachment of the adverbial.

- (33) If you will come this way.

Since in example (31) there is nothing to attach the adverbial clause to, this suggests that speech act modifying *weil*-clauses act as parenthetical constructions. Adopting Haegeman (1991), I propose that speech act modifying V2 *weil*-clauses are syntactic orphans which are completely outside the syntactic representation of the associated clause as shown in figure 2.

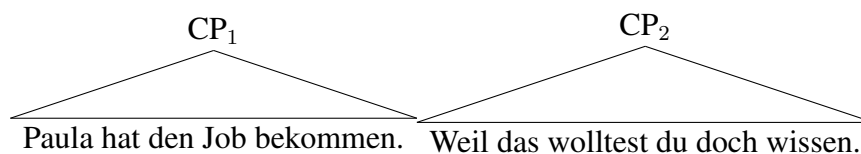
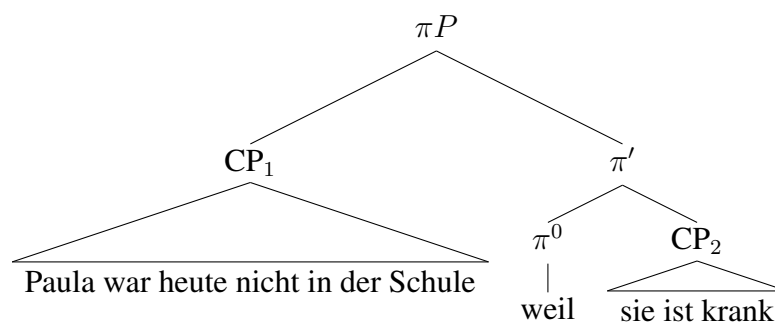


Figure 2: Orphan analysis

Syntactically, the V2 adverbial is not linked. As a consequence, the semantic relation between the two independent CPs is not determined by grammar but has to be interpreted at the level of discourse (see section 5). I assume the semantic type of *weil* to be  $\langle t, \langle t, t \rangle \rangle$ , i.e. *weil* takes two propositional arguments. In the proposed structure in figure 2, *weil* takes only one argument. Here, semantic restructuring has to take place, inserting  $CP_1$  into an argument slot of the CAUSE relation<sup>6</sup>.

If there is a structure diagnostic allowing to distinguish speech act modifying V2 *weil*-clauses from the other two types of V2 *weil*-clauses, no syntactic difference between propositional and epistemic adverbials can be found. Scope effects suggest that both types are generated outside the syntactic *c*-command domain of the matrix clause. This is surprising since propositional V2 *weil*-clauses have the same interpretation as adverbials with verb-final order (see (14-a) and (15)) and should therefore attach to the same projection, namely IP. Yet, even event modifying V2 *weil*-clauses such as (15) cannot be analyzed as IP adjuncts since they turned out to be syntactically unembeddable as we have seen in example (28) and (29). As we will see in section 6, the syntactic disintegration of V2 adverbials is due to independent reasons, namely the assertive force potential of German V2 clauses, which disallows the clause to be within the scope of another clause. I therefore propose one and the same syntactic analysis for epistemic and propositional V2 *weil*-clauses, assuming that both attach to the same projection. Since propositional and epistemic V2 *weil*-clauses are outside the *c*-command domain of the associated clause but cannot be used alone, I assume that they are coordinate structures. Both clauses are conjuncts of a supra-sentential paratactic phrase  $\pi P$  (see Gärtner 2001 for a similar analysis for V2 relatives). The conjunction *weil* is in the head of the paratactic phrase,  $\pi^0$ . In conformance to its semantic type  $\langle t, \langle t, t \rangle \rangle$ , *weil* takes two propositional arguments. Assuming a structure like figure 3 therefore reduces semantic computation to functional application. Furthermore, the insertion of an epistemic operator MUST into the semantic representation of  $CP_1$  is not affected by this structure. Another fact which can be derived from the structure in 3 is that V2 *weil*-clauses cannot appear alone under an epistemic or propositional interpretation as shown in ((32)). Since one of the argument positions of the two-place predicate *weil* is not saturated, such a structure would violate the  $\theta$ -criterion<sup>7</sup>. Thus, semantic restructuring seems to be impossible for propositional and epistemic V2 *weil*-clauses.



<sup>6</sup>Another problem for this analysis is the fact that it is not clear in which position *weil* is situated. One possibility is to assume a recursive structure. I will leave this open.

<sup>7</sup>Thanks to Stefan Keine for pointing this out to me.

Figure 3: Paratactic analysis

The V2 *weil*-clause in figure 3 can modify either the propositional content of CP<sub>1</sub> or it can take an epistemic argument. Hence, the syntactic structure in figure 3 is semantically undetermined since it can be interpreted as propositional or epistemic relation between the two conjuncts. This ambiguity is only resolved at the level of utterance processing by the contribution of pragmatic principles.

Thus, verb-final *weil*-clauses and V2 *weil*-clauses are not only distinguished in terms of their internal syntax. Whereas the former are structurally integrated in the domain of the matrix clause as IP-adjuncts (see figure 1), the latter are distinguished by a higher degree of syntactic disintegration. Speech-act modifying V2 *weil*-clauses behave like parentheticals and are analyzed as being outside the syntactic representation. In contrast, propositional and epistemic V2 adverbials are conjuncts of a paratactic phrase<sup>8</sup>. If the semantic interpretation of V2 *weil*-clauses allows to distinguish three different types of V2 *weil*-clauses, the syntactic behavior suggests the distinction of only two different syntactic attachment sites. The distinction between propositional, epistemic, and speech act modifying uses of V2 *weil*-clauses arises then on the one hand as the result of the adverbial clause being the conjunct of a coordinate structure or a syntactic orphan and, on the other hand, since this distinction is not sufficient to distinguish the three semantic uses, from the contribution of pragmatic principles (see section 5).

Another interesting question concerns the analysis of epistemic and speech-act modifying *weil*-clauses with verb-final order. As V2 *weil*-clauses, a verb-final *weil*-clause can take an epistemic argument as in (34-a) or modify a speech act as in (34-b) if it is separated by comma intonation from its matrix clause.

- (34) a. Es hat geschneit, (\) weil die Straße ganz weiß ist.  
 it has snowed, (\) because the road totally white is.  
 b. Kochst du etwas? Weil ich heute Besuch bekomme.  
 cook you something? because I today visit receive.  
 ‘Can you cook something? Because I’m receiving a visit, today.’

Interpreted as epistemic or speech-act modification, a *weil*-clause with verb-final order is outside the c-command domain of the associated clause as can be seen in example (35).

- (35) a.\*Jeder<sub>i</sub> schläft schon, (\) weil in seinem<sub>i</sub> Zimmer kein Licht mehr brennt.  
 everybody<sub>i</sub> sleeps already (\) because in his<sub>i</sub> room no light more burns.  
 b.\*War denn niemand<sub>i</sub> verärgert? Weil er<sub>i</sub> so wütend geguckt hat.  
 was PART nobody<sub>i</sub> angry? because he<sub>i</sub> so furious looked has.

The data in (35) raise the question of whether clauses like (34-a) and (34-b) should be analyzed as the corresponding V2 *weil*-clauses, namely as coordinate structures and syntactic orphans respectively. The scope effects in (35) seem to support this analysis. On the other hand, the fact that the finite verb is in the clause final position is a clear signal for subordination and therefore for syntactic integration. In this paper, I will not investigate this question but leave it for further

<sup>8</sup>A point that needs to be clarified by future research is that in my analysis, *weil* is not tied to an uniform structural position.

research.

### 5. A compositional derivation at the syntax-semantics-pragmatics interface

We have seen that explaining the three possible interpretations of V2 *weil*-clauses by three different attachment sites is not possible since scope effects suggest that all V2 *weil*-clauses must be generated in a position outside the c-command domain of the associated clause. Even propositional V2 *weil*-clauses turned out to be syntactically unembeddable. Hence, there isn't a one-to-one correspondence between syntactic structure and semantic meaning since the syntactic differences are not a sufficient explanation for the semantic differences. In this section, I will show that the semantic differences can be derived from the interaction of three factors: The semantic contribution of the connective *weil*, the degree of syntactic integration, and pragmatic principles. Therefore, V2 *weil*-clauses are a phenomenon located at the syntax-semantics-pragmatics interface.

I propose that the semantic contribution of the connective *weil* is the same for all three interpretations. In section 3.1., I have introduced a two-place predicate CAUSE which expresses a causal relation over its two arguments *p* and *q*, *p* expressing a cause for the modifiee *q*. Whereas the argument *p* is always the proposition introduced by *weil*, the modifiee of the causal relation, *q* can be of different types. As we have seen in section 3, the *weil*-clause can modify either the propositional content of the associated clause, the speaker's belief state or the utterance of the previous speech act:

- (36) a. CAUSE (*p*, *q*)  
 b. CAUSE (*p*, MUST *q*)  
 c. CAUSE (*p*, ASSERT *q*)

If a V2 *weil*-clause can express one of the three interpretations represented in (36), a *weil*-clause with verb-final order can only modify the propositional content of *q* (see (36-a)). This is due to the fact that the interpretation of the latter is determined by syntactic principles. Since a verb-final *weil*-clause is adjoined to IP, an analysis strongly suggested by scope effects (see table 1), it can only modify elements which are inside IP, hence, the propositional content of the matrix clause.

In contrast, V2 *weil*-clauses are syntactically less integrated. As coordinate structures or syntactic orphans (see figures 2 and 3), they are generated in a position in which they can take scope over the associated CP. As a consequence, they can modify not only the propositional content of the associated clause but they can also express an epistemic or a speech act modification. Speech act modifying V2 *weil*-clauses have been analyzed as completely unattached orphans. This means that syntax doesn't give any hint on how to interpret the causal relation between the two clauses. Similarly, the coordinate structure in figure 3 is semantically ambiguous since the causal clause can express either a propositional or an epistemic modification. Both representations generated by syntax are semantically not enough determined since syntax doesn't encode the way how the causal relation must be interpreted. Therefore, it is necessary to define the processes by which the object of the causal modification is determined. Since syntax generates an

undetermined representation and the semantic contribution of the connective *weil* is the same for all three interpretations, I will argue that pragmatic principles are needed to derive in each case the appropriate semantic interpretation.

It has often been observed that syntactic disintegration leads to semantic and pragmatic disintegration (as argued in Blakemore & Carston 2005; Carston 2002; Haegeman 1991; Posner 1979). Since the semantic interpretation of syntactically disintegrated structures is not determined, the processing of these constructions involves pragmatic strengthening. Due to the lack of syntactic informations to guide towards the appropriate semantic interpretation, the semantics of the clause linkage must be derived at the level of utterance processing on the basis of general principles of utterance such as the principle of relevance. According to Sperber & Wilson (1986) and Sperber & Wilson (2004), utterances create expectations of optimal relevance. An utterance is relevant if it connects with the hearer's knowledge yielding a conclusion deducible from the utterance and the context. The hearer has to look for the interpretation with maximal contextual relevance at minimal processing efforts by constructing a hypothesis about the intended meaning. To do so, the hearer must enrich the linguistically encoded meaning until the utterance satisfies his expectation of relevance. In a relevance-theoretic framework, it is a general assumption that linguistically encoded meaning is undetermined and must be pragmatically enriched: ambiguities must be resolved, references must be assigned and other indeterminacies must be dealt with. Therefore, Relevance theory treats the identification of the logical form (LF) as inferential. This is illustrated in an example from Kempson (1988:19) about the assignment of pronominal references.

(37) John came in. He felt tired.

Grammar doesn't define the reference of the pronoun *he*. It only imposes constraints on the value to be associated with. *He* must refer to a male, it is a singular form and, following binding-theory, cannot be bound. Only at the level of utterance interpretation, the pronoun *he* will be interpreted as co-referential with *John* since this is the interpretation with maximal contextual relevance. Thus, the utterance is enriched with extra-linguistical information in order to maximize relevance. Haegeman (1991) adopts this analysis to the interpretation of orphan constituents. She argues that a conditional clause like (38) refers like a pronominal to a referent which must be recovered from contextual, thus extra-linguistic, information.

(38) If you don't mind (said by a speaker moving past neighbours to take his seat in a crowded cinema).

Similarly, the value of the modifiee of a V2 *weil*-clause must be recovered at the level of utterance. A V2 *weil*-clause must be associated with another event, which can be a proposition, an epistemic state, or an utterance. At the level of utterance processing, the V2 *weil*-clause is inserted into a conceptual schema which has two open positions.

(39)  $\lambda p \lambda q$  CAUSE (p, q)

The complete derivation of the semantic interpretation of example (40) is shown in (41). The V2 clause which is introduced by *weil* (= SubP) is always inserted for p (see (41-b)). In contrast, the value of the variable q must be recovered from the context. For example (40), the interpretation



which satisfies most the expectation of relevance is an interpretation as speech act modification, thus,  $q$  has the value represented in (41-c): The causal clause expresses a reason for the utterance of  $CP_1$ .

(40) Die Bild ist in der Schublade. Weil alleine findest du es nie.  
 the picture is in the drawer. because alone find you it never.  
 ‘The picture is in the drawer, because you won’t find it by yourself.’

(41) a.  $\lambda p \lambda q$  CAUSE ( $p, q$ )  
 b.  $p = \text{SubP}$   
 c.  $q = \text{ASSERT}(CP_1)$   
 d. CAUSE ( $\text{SubP}, \text{ASSERT}(CP_1)$ )

Since the interpretation of a V2 *weil*-clause is characterized by a high degree of structural underspecification, the logical form of a construction like (40) must be contextually enriched. More precisely, this represents a narrowing of the selectional requirements which the two-place predicate *weil* places on its second argument. This pragmatic process is driven by the search for relevance. In contrast, for syntactically integrated *weil*-clauses with verb-final order, the value of  $q$  is constrained by syntax: Since the adverbial clause is adjoined to IP, it can only modify the proposition of  $CP_1$ .

In this section, I have argued that the interpretation of V2 *weil*-clauses must be compositionally derived from an unchanging semantics of the connective *weil*, the syntactic attachment site of the adverbial clause, and the contribution of pragmatic principles such as the principle of relevance. Grammar generates an incomplete logical form which must be enriched by non-grammatical principles at the level of discourse in order to satisfy the hearer’s expectation of relevance.

## 6. The semantics of V2 clauses

Propositional V2 *weil*-clauses modify the event structure and should therefore be generated in a low position in the syntactic structure. Nevertheless, structure diagnostics (see (28) to (29)) suggest that event-modifying V2 *weil*-clauses are attached very high in the syntactic representation, and that is why I argued in section 4.2. that propositional V2 *weil*-clauses are coordinate structures. Since even event-modifying V2 adverbials turned out to be syntactically unembeddable, there is no syntactic asymmetry between V2 clauses that modify the event structure and those which modify the discourse structure such as it is attested for central and peripheral adverbials in English. Now, the question which is raised by the German data is why propositional V2 *weil*-clauses behave syntactically like peripheral adverbials. Why are V2 adverbials which modify the event structure syntactically unembeddable?

Scheffler (2008:55 et seqq.) argues that causal clauses with V2 order (introduced by *weil* or *denn*) are unembeddable because the causal meaning of these clauses is contributed by a conventional implicature. She argues that the connective has the semantics of the logical  $\wedge$  and that the causal relationship is derived as a conventional implicature. As a consequence, the

causal modifier cannot be embedded in any other functor such as negation. Yet, her proposal does not account for the fact that embedded V2 clauses are generally outside the scope of matrix functors. As shown in example (42), even V2 relatives and V2 object clauses are illicit if they are in the scope of negation or of a negative predicate.

- (42) a.\*Ich kenne keinen Linguisten, der forscht über Dialekte.  
 I know no linguist who works about dialects.  
 b.\*Peter glaubt nicht, Paula hat das Buch gelesen.  
 Peter believes not Paula has the book read.  
 ‘Peter doesn’t believe that Paula read the book.’  
 c.\*Peter bezweifelt, Paula hat das Buch gelesen.  
 Peter doubts Paula has the book read.

The observation that Embedded Root Phenomena are not possible in negated clauses is also attested for embedded V2 in Frisian (see (43)) and argument-fronting in English (see (44)).

- (43) \*Pyt betwivelet dat hy hie my sjoen.  
 Pyt doubts that he had me seen.  
 ‘Pyt doubts that he had seen me.’
- (44) a. John believes that this book Mary read.  
 b.\*John doesn’t believe that this book Mary read.  
 c.\*John regrets that this book Mary read.

The examples show that explaining the syntactic unembeddability of causal V2 clauses by arguing that conventional implicatures do not embed, cannot account for the degradation of other Embedded Root Phenomena in negated contexts. That is why, in this section, I will argue that V2 order is only possible in asserted clauses which have a full-fledged CP and therefore illocutionary force. As a consequence, embedded V2 clauses cannot occur in presupposed contexts. Since negation triggers presupposition, the degradation of V2 order in negated *weil*-clauses, negated relative clauses and negated object clauses can therefore be explained by one single assumption: Embedded Root Phenomena such as V2 order occur only in asserted contexts.

The syntactic disintegration of event-modifying V2 *weil*-clauses such as (15) is due to reasons independent of the semantic interpretation of the causal relation. Unlike English, German is an asymmetric V2 language with the distinction of V2 and verb-final clauses. Generally, V2 order is limited to unembedded main clauses. But in specific contexts, subordinate clauses such as object clauses selected by bridge verbs, restrictive relative clauses which modify indefinite heads, and adverbial clauses introduced by *weil* can have V2 order, too (see also Antomo & Steinbach to appear; Gärtner 2001, Gärtner 2002; Meinunger 2004; Truckenbrodt 2006). Therefore, V2 order in German *weil*-clauses can be subsumed under the notion of Embedded Root Phenomena (as described in amongst others Hooper & Thompson 1973; Haegeman 2003; Heycock 2005; Julien 2007; Sawada & Larson 2004; Vikner 1995; Wechsler 1991). It has often been observed that, in German, the movement of the finite verb to C is associated with an assertive force potential. In the following, I will first briefly discuss the semantics of V2 in German according to Truckenbrodt (2006). Based on this approach, I will argue that due to the specific semantics of V2 order, V2 *weil*-clauses are always outside the scope domain of the

associated clause. Thus, the syntactic disintegration of propositional V2 *weil*-clauses is independent of the interpretation of the clause linkage and can be explained in terms of assertion and presupposition.

According to Truckenbrodt (2006), grammatical features in C interact with specific interpretations which are triggered by two context indices  $\langle \text{Deont}_S(x)_1 \rangle$  and  $\langle \text{Epist}_2 \rangle$ . All sentential speech acts are volitional on the part of the speaker S, i.e. they are deontic. By uttering a speech act, the speaker S wants something from the addressee A. In an imperative such as (45-a), S wants A to do something. In declarative and interrogative clauses, S wants to change the epistemic states of A or his own epistemic state. By uttering a question like (45-b), S wants to know something from A, by uttering an assertion like (45-c), S wants to convey knowledge to A.

- (45) a. Lies das Buch! ('Read the book!')  
*S wants from A that A reads the book.*
- b. Hat Tim das Buch gelesen? ('Has Tim read the book?')  
*S wants from A that it is Common Ground whether Tim has read the book.*
- c. Tim hat das Buch gelesen. ('Tim has read the book.')
- S wants from A that it is Common Ground that Tim has read the book.*

All three types of speech acts, imperatives, interrogatives, and declaratives, are deontic because the speaker S wants something from the addressee. In contrast, only interrogative and declarative clauses are epistemic speech acts which involve a call for updating the Common Ground. According to Stalnaker (1978), an asserted proposition like (45-c) is added to the Common Ground and becomes common knowledge of S and A. This means that the Common Ground is intersected with the proposition p and that all possible worlds which are not compatible with p are excluded. Truckenbrodt's (2006) central claim is that grammatical elements in C interact with the two context indices  $\langle \text{Deont}_S(x)_1 \rangle$  and  $\langle \text{Epist}_2 \rangle$  represented in (46):

- (46)  $\langle \text{Deont}_S(x)_1 \rangle$       '*S wants from A*'  
 $\langle \text{Epist}_2 \rangle$                 '*that it is Common Ground that/if p*'

Except in purely exclamative utterances, the context index  $\langle \text{Deont}_S(x) \rangle$  is present in all utterances. In contrast, the presence of  $\langle \text{Epist} \rangle$  depends on grammatical features in C. It is only active if C is marked [+WH] and/or if C contains a finite verb with indicative or Konjunktiv II<sup>9</sup> morphology. In declarative clauses, the finite verb in C is the only trigger of  $\langle \text{Epist} \rangle$ . In the following minimal pair from Reis (2006:371), only the sentence with V2 order can express an assertive speech act. By uttering (47-a), the speaker makes an assertion which triggers an update of the Common Ground. In contrast, the root *dass*-clause with verb-final order (see (47-b)) has no epistemic but only an exclamative or (depending on the modal particle *ja*) a directive reading. Thus, the example shows that only V2 clauses have an assertive force potential and can therefore modify the Common Ground.

- (47) a. Alle            sind rechtzeitig da.  
           everybody are punctual there.  
           'Everybody is there in time!'

<sup>9</sup>Roughly, Konjunktiv is the German subjunctive mood

- b. Dass (ja) alle rechtzeitig da sind!  
 that (PART) everybody punctual there is!  
 ‘(It’s imperative that) everybody is there in time.’

Furthermore, the context index  $\langle \text{Deont}_S \rangle$  is extended by the specification ‘from A’ if C contains a finite verb. As argued in Truckenbrodt (2006:270), only [V-in-C]-clauses presuppose a reaction from the addressee A. This is shown in the following example. By uttering (48-a), S expects from A an answer to his question. In contrast, the *ob*-question with verb-final order does not express the expectation of an answer from A. Instead, (48-b) can be paraphrased by the expression ‘I wonder if...’.

- (48) a. Kommt er?  
 comes he?  
 ‘Does he come?’  
 b. Ob er wohl kommt?  
 if he PART comes?  
 ‘I wonder if he comes.’

Thus, a clause with the finite verb in C expresses the expectation that A reacts. In interrogative clauses, this reaction should be an answer to the question, in declarative clauses, A should accept the information *p*. Thus, in both cases, A controls whether the desired update of the Common Ground will be felicitous or not. According to Truckenbrodt (2006:265), the interaction of the two context indices with grammatical features in C is summarized in (49) (his (17)).

- (49) In a context index  $\langle \text{Deont}_S(,x)(, \langle \text{Epist} \rangle) \rangle$  in C  
 a. Epist is present iff  
 (i) C contains a finite verb with indicative or Konjunktiv II or  
 (ii) C/CP is marked [+WH].  
 b.  $x = A(\text{addressee})$  iff C contains a finite verb with person inflection.

Therefore, a German V2 declarative has the semantics shown in (50):

- (50)  $\langle \langle \text{Deont}_S, A \rangle, \langle \text{Epist} \rangle \rangle$   
 ‘S wants from A that it is Common Ground that *p*’

In German, V2 declaratives have an assertive force potential. The movement of the finite verb to the left periphery is associated with an update of the Common Ground. In contrast, embedded clauses typically lack illocutionary force since the finite verb does not move to the C-system. Therefore, *weil*-clauses with verb-final order such as (1-b) lack illocutionary force. Instead, they are integrated into the speech act of their matrix clause. In contrast, a V2 *weil*-clause has assertive force independent of its associated main clause. The movement of the finite verb to the left periphery triggers an update of the Common Ground. As a consequence, a V2 clause cannot be presupposed, that is, it cannot already be part of the Common Ground.

Thus, only asserted *weil*-clauses allow V2 order. This is a correlation between the syntax and pragmatics of adverbial clauses which has often been observed. Amongst others, Hooper & Thompson (1973), Julien (2007), and Sawada & Larson (2004) note that presupposed adverbial

clauses resist root transformations. Root transformations or Main Clause Phenomena are syntactic transformations normally limited to root clauses which occur in some embedded contexts. For instance, in English adverbial clauses, left dislocation is blocked if the adverbial clause is presupposed. Adopting this idea to German *weil*-clauses, I propose to explain the syntactic distribution of V2 and verb-final *weil*-clauses (see table 1) in terms of assertion and presupposition. V2 order as a root transformation is illicit whenever the adverbial clause is presupposed.

Based on this assumption, the degradation of V2 order in presupposed *weil*-clauses is due to a semantic mismatch. Now I'm able to explain the degradation of V2 *weil*-clauses in environments in which a *weil*-clause with verb-final order is fine. We have seen in section 2 that V2 order is not possible if the causal clause is within the scope of negation, a focus particle, a correlative, or of the matrix interrogative operator. Strikingly, these are all elements which presuppose the existence of their complement. I will first consider the case of negation. In example (3-b), repeated here as (51-a), we have seen that a V2 *weil*-clause cannot be within the scope of matrix negation.

- (51) a. \*Paul fährt nicht nach Berlin, weil er hat dort Familie, sondern weil er will die Stadt besichtigen.  
       'Paul doesn't go to Berlin because he has family there but because he wants to visit the town.'  
       b.  $\neg$  CAUSE (p, q)

As shown in (51-b), the negative operator takes scope over the CAUSE predicate. As argued in Sawada & Larson (2004), negation forces presupposition. Thus, example (51-a) presupposes that Paul has family in Berlin. Since V2 order is not possible in presupposed clauses, example (51-a) with the formal representation in (51-b) is not well formed. V2 order is only possible if CAUSE takes scope over negation as in (52-a), represented in (52-b). In this case, the reason why Paul is not going to Berlin is that he has family in Berlin (and he doesn't want to see them). The *weil*-clause is not within the scope of negation and hence not presupposed.

- (52) a. Paul fährt nicht nach Berlin, weil er hat dort Familie.  
       Paul goes not to Berlin because he has there family.  
       b. CAUSE (p,  $\neg$  q)

Similarly, V2 order is not possible if the *weil*-clause is in the scope of another presupposition trigger. Just like negation presupposes that there IS an event p, a correlative such as *deshalb* (see example (4)) presupposes the existence of its complement event. In the same way, a V2 *weil*-clause cannot be within the scope of a focus-sensitive particle such as *sogar* 'even'. According to Horn (1969) and Krifka (1993), focus-sensitive particles are presupposition triggers.

- (53) \*Sogar Paul ist nach Berlin gekommen, weil Tim wohnt hier.  
       even Paul is to Berlin came because Tim lives here.  
       'Even Paul came to Berlin because Tim lives here.'

Other presupposition triggers are according to Beaver (2001:11) iterative adverbs, unstressed constituents, and questions. As expected, this material, which is thought to induce presupposition, is incompatible with V2 *weil*-clauses. In example (5), we have already seen that V2 order

is illicit if the adverbial clause is in the scope of *auch* ‘too’, example (10) shows that V2 *weil*-clauses are prosodically independent and example (6) demonstrates that a V2 adverbial cannot be integrated into a question.

Following Sawada & Larson (2004), I assume that presupposed adverbial clauses are in the scope of a covert existential quantifier as in (54) and that quantifier-restrictions are presupposed to be non-empty. If the adverbial clause expressing causation is in the scope of an existential quantifier, it is presupposed and cannot undergo root transformations. Therefore, it cannot have V2 order.

(54)  $\exists (\text{CAUSE}_{e_1} (e_1)) (e_2)$

In contrast, if the adverbial clause is not presupposed, CAUSE expresses according to Johnston (1994) a relation between two closed events as represented in (55), which is the representation of a *weil*-clause with V2 order.

(55) CAUSE ( $\exists e_1, \exists e_2$ )

Comparing the two representations in (54) and (55), it becomes apparent that V2 *weil*-clauses have a larger semantic domain than presupposed *weil*-clauses. Whereas in (54) the connective *weil* combines with open event descriptions, in (55), *weil* combines with a quantifier  $\exists$  and an event description. This semantic difference is reflected in syntax. As we have seen in section 4.1., V2 *weil*-clauses have a full fledged CP-layer, whereas adverbial clauses with verb-final order have a reduced left periphery. Since V2 *weil*-clauses have an extra layer of structure, they allow root transformations as, for example, Left Dislocation (see (56-a)). According to Grewendorf (2008:65), German Left Dislocation targets the specifier of TopP. Hence, Left Dislocation is only possible if TopP projects. As shown in (56-b), Left Dislocation is not possible in *weil*-clauses with verb-final order since they have a reduced left periphery.

- (56) a. Lass uns lieber Tim einladen, weil den Lukas, den will ich nicht sehen.  
 let us rather Tim invite because the Lukas, PRO want I not see.  
 ‘Let’s invite Tim, because I don’t want to see Lukas.’  
 b.\*Lass uns lieber Tim einladen, weil den Lukas, den ich nicht sehen will.  
 let us rather Tim invite because the Lukas PRO I not see want.

Based on cross-linguistic evidence, Sawada & Larson (2004) conjecture that, compared to temporal *when*-clauses, adverbial clauses expressing causation have in general an extra layer of structure. *Because*-clauses allow cross-linguistically root transformations, whereas temporal adverbials resist Main Clause Phenomena. Yet, in German, there is a distinction between *weil*-clauses with verb-final order and those with V2 order. Only the latter allow root transformations, whereas the former cannot undergo root transformations.

In sum, in this section, I have argued that the syntactic unembeddability of V2 *weil*-clauses is due to their assertive force potential. Since V2 clauses are always asserted, a V2 *weil*-clause cannot occur in presupposed environments. Thus, the fact that even event modifying V2 *weil*-clauses such as (15) are syntactically unembeddable can be explained by independent reasons. The availability of V2 order in German *weil*-clauses is a semantic phenomenon, explainable in terms of assertion and presupposition. The degradation of V2 *weil*-clauses in combination with

a presupposition trigger is a projection of a semantic phenomenon into syntax.

## 7. Conclusion

This paper investigates formal and functional differences between German V2 and verb-final *weil*-clauses. V2 *weil*-clauses are syntactically unembeddable, whereas causal adverbials with verb-final order are typically within the c-command domain of the associated main clause. I have argued that the syntactic unembeddability of V2 *weil*-clauses is due to the assertive force potential of German V2 clauses. As an embedded root phenomenon, V2 order is not possible in adverbial clauses in presupposed contexts. As a consequence, a V2 *weil*-clause cannot be in the scope of negation, a correlative, or another presupposition trigger.

A *weil*-clause with verb-final order structures the event by expressing a reason for the propositional content of the matrix clause. In contrast, V2 adverbials can express three readings. Like their verb-final counterparts, they can be interpreted as event modification. Furthermore, they can express a reason for the speaker's attitude or a speech act modification. I proposed that this semantic differences between V2 and verb-final *weil*-clauses can partially be traced back to syntactic differences. Verb-final *weil*-clauses have a truncated CP domain in that they lack ForceP, TopP and FocP and they are merged within the proposition of the matrix clause as IP-adjuncts. In contrast, V2 *weil*-clauses have a full fledged CP and they are outside the syntactic domain of the main clause. Yet, I have shown that the three different interpretations of V2 *weil*-clauses cannot be traced back to three different syntactic attachment sites since all three types of V2 adverbials are syntactically unembeddable and allow Main Clause Phenomena. That's why I have proposed a compositional analysis which derives the three semantic representations from an invariant semantics of the causal connective *weil* and the syntactic disintegration of V2 clauses which leads to a third factor, namely pragmatic strengthening guided by the search for relevance.

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## Adjectival resultatives cross-linguistically: a morphophonological account

Víctor Acedo-Matellán

Not all languages that feature complex resultative constructions permit them when the resultative predicate is an AP. Well-known data from Slavic and new data from Latin attest that observation, and the following one: in both languages resultatives always feature a prefix as the resultative predicate. I suggest, then, that in these languages there is a morphological requirement such that the resultative predicate must end up affixed onto the verb, and that this prefixation mechanism cannot target adjectives, since they are always inflected for agreement. The analysis is couched within a syntactic view of argument structure and morphology.

### 1. Introduction: resultative constructions cross-linguistically

The allowance for complex resultative constructions varies cross-linguistically according to the statement in (1):

- (1) Some languages which allow complex resultative constructions do not allow them when the resultative predicate is an AP.

For instance, English, Latin and Russian allow complex resultatives based on adposition-like elements, as the ones underlined in (2): the resultative predicate is expressed as a particle, which can be either prefixed –as in the Latin and Russian examples of (2b) and (2c), respectively– or not –as in the English example of (2a). By contrast, only Germanic allows for complex resultatives based on APs, as shown in (3):<sup>1</sup>

#### *English, Latin and Russian*

- (2) a. He slept the hours away (Hale & Keyser 2002)  
b. Serpentes putamina \*(ex-)tussiunt (Plin. *Nat.* 10, 197)  
snake.NOM.PL eggshell.ACC.PL out-cough.3.PL  
'Snakes cough out the eggshells.'

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<sup>1</sup> If not otherwise indicated, the Latin examples have been extracted from the Latin language corpus of the *Bibliotheca Teubneriana Latina*, in its second version of 2002 (BTL2). In particular, all the searches have been conducted in the subcorpora of Early and Classical Latin texts (spanning up to the II century AD), Post-classical Latin being dismissed. The references of the works excerpted are also the ones provided in the BTL2.

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- c. Ivan vy-kopal klad (Babko-Malaya 1999)  
 Ivan out-dug.PRF treasure.ACC  
 ‘Ivan dug out the treasure.’

*German, Latin and Russian*

- (3) a. Die Teekanne leer trinken (Kratzer 2005)  
 the teapot empty drink  
 ‘To drink the teapot empty.’  
 b. \*Poculum vacuum bibere  
 goblet.ACC.SG empty.ACC.SG drink.INF  
 ‘To drink the goblet empty.’  
 c. \*Ona mylila men’a skolzkim (Strigin 2004)  
 she soap.PSTme slippery  
 ‘She soaped me slippery.’

This puzzle, though noted by Svenonius (2004), Gehrke (2008) or Son & Svenonius (2008) has received little attention in the literature, and no attention at all in the case of Latin. In this paper I will try to provide an explanation for it in terms of the morphophonological features of the functional head encoding change or transition, the Path, within a syntactic theory of argument structure and morphology. Moreover, I will show that the analysis proposed generates a typology which, being finer-grained than that of Talmy’s (1991, 2000), Mateu’s (2002), Mateu & Rigau’s (2002) or Snyder’s (2001), is more suitable to account for the cross-linguistic differences in the expression of results. The discussion will set out, in the next section, from the limitations of Talmy’s satellite-/verb-framed distinction and its syntactic interpretation in Mateu (2002) and Mateu & Rigau (2002) as a way to explain the availability of complex resultative constructions cross-linguistically.

*2. The satellite-/verb-framed typology applied to resultatives, and its limitations*

Talmy (1991, 2000) has proposed two major typological groups with respect to the way events of change are realised. On the one hand, there are *s(atellite)-framed* languages, where the element encoding change or transition is expressed independently from the verb, in the element Talmy calls the *satellite*; on the other hand, there are *v(erb)-framed* languages, where the transition component and the verb are one and the same morpheme or root. An illustration of this distinction is to be found in the motion event of (4), as expressed in s-framed English (4a) and v-framed Catalan (4b). The different components of the motion event as proposed by Talmy are listed below:

*English and Catalan*

- (4) a. [The ball]<sub>Figure</sub> [rolled]<sub>Event + Co-event</sub> [in[to]<sub>Path</sub> [the pit]<sub>Ground</sub>]<sub>Core schema</sub>  
 b. [La pilota]<sub>Figure</sub> va [[entrar]<sub>Event + Core schema</sub> a[l clot]<sub>Ground</sub> [rodolant]<sub>Co-event</sub>  
 the ball PST.3.SG go\_in.INF at=the pit rolling

*Figure*: entity which moves or is stationary in relation to another entity taken as reference.

*Ground*: that referential entity.

*Path*: the dynamic or static relation between Figure and Ground.

*Core schema*: the ordered set of Path + Ground.

*Co-event*: an event accompanying the main motion event (usually expressing manner).

Talmy applies this distinction to semantically analogous, change-of-state constructions involving APs, what we traditionally know as (*complex*) *resultative constructions* (Simpson 1983). This is illustrated in (5) through a contrast between s-framed German (example from Talmy 2000) and v-framed Spanish: while German encodes the resulting state of the complex event as an adjective, the verb being left to express manner, Spanish encodes the resulting state as the verb, and the manner is expressed as an adjunct:<sup>2</sup>

#### *German and Spanish*

- (5) a. Der Hund hat [den Schuh]<sub>Figure</sub> [kaputt]<sub>Core schema</sub> -[gebissen]<sub>Event + Co-event</sub>  
 the dog has the shoe in\_pieces -bite.PST.PART  
 ‘The dog bit the shoe to pieces.’
- b. El perro [destrozó]<sub>Event + Core schema</sub> [el zapato]<sub>Figure</sub> [a mordiscos]<sub>Co-event</sub>  
 the dog destroy.PST.3.SG the shoe to bites

The main typological insight in Talmy’s work is, thus, the fact that languages differ as to the realisation of the so-called Core Schema, that is, the set formed by Path plus Ground, be this Core Schema an actual location or direction, as in (4), or a state or a change of state, as in (5).

Apparently problematic for this typology are some motion predicates in v-framed languages like Spanish where what looks like the Core Schema is expressed as a PP, while the verb is left free to encode the manner Co-event. In particular, manner-of-motion verbs in v-framed languages seem to allow a directional PP, but only in case it does not induce a telic reading of the event:

#### *Spanish*

- (6) a. Nadaron hacia la isla durante cinco minutos  
 swim.PST.3.PL towards the island during five minutes  
 ‘They swam towards the island for five minutes.’
- b. \*Nadaron a la isla (en cinco minutos)  
 swim.PST.3.PL to the island in five minutes  
 ‘They swam to the island in five minutes.’

Therefore, it seems that the typological difference Talmy (1985) was aiming at must include some reference to the boundedness of the Path and the co-varying inner-aspectual properties

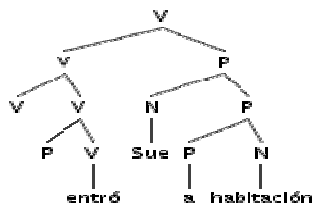
<sup>2</sup> The contrast in (5) helps us define what is meant by *complex* in the expression *complex resultative construction*. Indeed, the Spanish predicate in (5b) (crucially, without the adjunct) could trivially be called a *resultative construction* —along the lines, for instance, of Nedjalkov (1988)— since it implies a result (in this case a resulting state); on the other hand, the German construction of (5a) is a *complex resultative construction* in that it depicts a *complex event* (Levin & Rappaport Hovav 2005) involving the attainment of a resulting state but, also, a differentiated activity leading to that state (in the case of (5a), a biting activity). Other “trivial” resultative constructions are the ones involving a light causative verb and an adjectival predicate encoding the result, as in English *Make the bedroom clean* or its Catalan equivalent *Deixar l’habitació neta*. In this work I am concerned only with *complex* resultative constructions.

of the predicate, as first pointed out by Aske (1989) and incorporated in Talmy (1991, 2000). Consequently, in this work I consider telicity to be a hallmark of complex resultative constructions.<sup>3</sup>

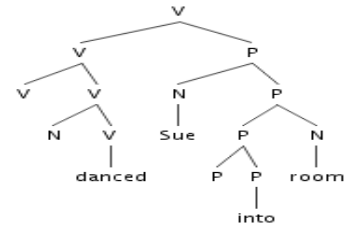
Within the Generative tradition, there have been attempts at interpreting this Talmy-Aske typology. Prominent ones are Mateu's (2002) and Mateu & Rigau's (2002), who propose that the cross-linguistic variation involved in the availability of secondary resultative predication is to be treated in morphosyntactic terms. In particular, and drawing on Hale & Keyser (1993, 1998), they put forth an *l-syntactic* account of that typology, whereby the *Path*, the element encoding transition, a birelational head of adpositional nature, is *fused* or *conflated* with the eventive head in v-framed languages, as represented in (7a). This conflation prevents any other element from merging with the eventive head in those languages. By contrast, in s-framed languages the Path is not fused with the verb: this enables other elements to get adjoined to and fuse with it, as the unergative [<sub>V</sub> V N] structure in (7b), which encodes a dancing event.

### Spanish and English

(7) a. Sue entró en la habitación (bailando)



b. Sue danced into the room



So the Talmy-Aske typology, adapted to a syntactically based model, proved an adequate first step to deal with the cross-linguistic variation in the expression of complex events, since it can explain why s-framed languages may generate both telic complex directed motion constructions and complex adjectival resultative constructions, while v-framed languages do not feature any of these constructions.<sup>4</sup> Much as Talmy himself never made any prediction as to the availability of both types of complex predicate in s-framed languages, it remains as yet unexplained, under the view presented so far, why it is the case that some languages qualifying as s-framed, such as Russian, permit adpositions but not APs to be resultative predicates:

### Russian

(8) \*Ona mylila men'a skolzkim (Strigin 2004)  
 she soap.PSTme slippery  
 'She soaped me slippery'

<sup>3</sup> But see Borer (2005) or Folli & Harley (2006) for the view that Aktionsart is orthogonal to the well-formedness of some predicates of this kind.

<sup>4</sup> Snyder (2001) proposes a unified account of complex events, including complex directed motion constructions and complex resultative constructions, in terms of complex predicate formation. In particular, he suggests that for a language to allow for complex events it must be able to productively generate endocentric N-N compounds. Although I will not go into the details of his theory here (see Son 2007 or Son & Svenonius 2008 for a critique), I just note that Latin and Slavic do permit the expression of complex events, such as double object constructions (Latin) or complex directed motion constructions (Latin and Slavic; see below) while being known for their lack of productive compounding.

Other researchers, like Svenonius (2004) or Gehrke (2008), have pointed out to a general lack of complex adjectival resultative constructions in Slavic. Thus, the analogy of PPs and APs as resultative predicates of satellite nature, while being maintainable in Germanic, cannot be maintained in other s-framed languages like Russian.

I report a similar situation in Latin, a language which Talmy himself qualifies as s-framed. Consider the contrast in (9):

*Latin*

- (9) a. Serpentes putamina \*(ex-)tussiunt (Plin. *Nat.* 10, 197)  
 snake.NOM.PL eggshell.ACC.PL out-cough.3.PL  
 ‘Snakes cough out the eggshells.’  
 b. \*Poculum vacuum bibere  
 goblet.ACC.SG empty.ACC.SG drink.INF  
 ‘To drink the goblet empty.’

Analogously to what has been shown for Russian, in (9a) a prefix, *ex-* ‘out’, encodes the resultative predicate conveying the main eventuality of the sentence, that of putting out or expelling, while the verb *tussio* ‘cough’ encodes the manner in which the expelling event is carried out. This type of predicates is abundant in Latin:

*Latin*

- (10) E-dormi crapulam (Cic. *Phil.* 2, 30)  
 out-sleep.IMPERAT.2.SG hangover.ACC.SG  
 ‘Sleep off that hangover.’  
 (11) Acta quae ille in aes in-cidit (Cic. *Phil.* 1, 16)<sup>5</sup>  
 act.ACC.PL which.ACC.N.PL that.M.NOM.SG in brass.ACC.SG in-cut.PRF.3.SG  
 ‘The acts which he engraved on brass.’  
 (12) Repente ad pabulatores ad-volaverunt (Caes. *Gall.* 5, 17, 2)  
 suddenly at forager.ACC.PL at-fly.PRF.3.PL  
 ‘They flew upon the foragers suddenly.’

In spite of the wide availability of complex resultative constructions based on adposition-like elements, as just illustrated, example (9b) is representative of the fact that APs, like *vacuum*, do not make appropriate secondary resultative predicates in Latin. (9b) is of course a made-up example, but the fact that Latin disallows AP-based resultative constructions is a solid conclusion, drawn from a corpus research inspired in the one Boas (2003) built for resultative constructions in English. The list of the combinations of adjectives and verbs I have searched for is in table (13):<sup>6</sup>

<sup>5</sup> The unprefixed correspondence of *in-cido* ‘engrave’ is *caedo*. The change (apophony) in the first vowel of the verb (/i:/ instead of the diphthong /ae/) is a very frequent phenomenon involved in Latin prefixation.

<sup>6</sup> Boas selected a set of adjectives recurrently used in the literature on resultatives as resultative predicates, such as *dead*, *clean*, *awake*, etc. Afterwards, he browsed a corpus to find out which verbs were most often used in resultative constructions with those adjectives. In applying this methodology to Latin, I have first established the correspondences of the English adjectives in Boas’s set and then the correspondences of (some of) the verbs he established as more collocative for each adjective, wherever possible. I have filtered Boas’s subcorpus somewhat: I have dismissed light verbs, for instance, which Boas takes into account, such as *get*, *render* or *make*, since these verbs do not form *complex* resultative constructions (see footnote 2) and hence they combine felicitously with adjectives in v-framed languages. In some cases I have added other verbs which I imagined

(13)

ADJECTIVES	VERBS
<i>aeger</i> ‘ill’	<i>bibo</i> ‘drink’
<i>cassus/inanis/vacuus</i> ‘empty’	<i>bibo</i> ‘drink’, <i>haurio</i> ‘scoop’, <i>poto</i> ‘drink’
<i>experrectus</i> ‘awake’	<i>calcitro</i> ‘an animal kick’, <i>clamo</i> ‘scream’, <i>figo</i> ‘prick’, <i>grunnio</i> ‘grunt’, <i>osculator</i> ‘kiss’, <i>plaudo</i> ‘clap’, <i>quatio</i> ‘shake’, ‘jerk’, <i>terreo</i> ‘induce terror’
<i>amens/demens/insanus</i> ‘insane, mad’	<i>clamo</i> ‘scream’, <i>loquor</i> ‘talk’, <i>strideo</i> ‘yell’
<i>mortuus</i> ‘dead’	<i>caedo</i> ‘cut, knock’, <i>calcitro</i> ‘an animal kick’, <i>cudo</i> ‘knock’, <i>occido</i> ‘kill’, <i>tundo</i> ‘strike, knock’, <i>verbero</i> ‘smite’
<i>mundus/nitidus</i> ‘clean’	<i>frico</i> ‘rub’, <i>lambo</i> ‘lick’, <i>lavo</i> , ‘wash’, <i>luo</i> ‘wash’, <i>polio</i> ‘scour, polish’, <i>rado</i> ‘scrape’, <i>sorbeo</i> ‘suck’, <i>sugo</i> ‘suck’, <i>tergeo</i> ‘wipe’, <i>verro</i> ‘sweep’
<i>niger</i> ‘black’	<i>amburo</i> ‘burn’, <i>aspergo</i> ‘spray’, <i>cremo</i> ‘burn’, <i>pingo</i> ‘paint’, <i>maculo</i> ‘stain’, <i>spargo</i> ‘spray’, <i>tingo</i> ‘dye’, <i>tundo</i> ‘beat’, <i>uro</i> ‘burn’, <i>verbero</i> ‘smite’
<i>opimus/pinguis</i> ‘fat’	<i>cibo</i> ‘feed’, <i>pasco</i> ‘pasture’
<i>planus</i> ‘flat’	<i>aro</i> ‘plough’, <i>caedo</i> ‘cut, knock’, <i>cudo</i> ‘knock’ <i>premo</i> ‘press’, <i>sicco</i> ‘dry’, <i>sorbeo</i> ‘suck’, <i>sugo</i> ‘suck’, <i>tero</i> ‘grind’, <i>tundo</i> ‘beat’, <i>volvo</i> ‘roll’
<i>plenus</i> ‘full’	<i>farcino</i> ‘stuff’, <i>farcio</i> ‘stuff’, <i>saturo</i> ‘stuff’, <i>sorbeo</i> ‘suck’, <i>stipo</i> ‘cram’, <i>sugo</i> ‘suck’
<i>quietus/tranquillus</i> ‘calm’	<i>cano</i> ‘sing’, <i>lallo</i> ‘lull’
<i>raucus</i> ‘hoarse’	<i>clamo</i> ‘scream’, <i>loquor</i> ‘talk’, <i>strideo</i> ‘yell’
<i>aridus/siccus</i> ‘dry’	<i>amplector</i> ‘hug’, <i>bibo</i> ‘drink’, <i>bullio</i> ‘boil’, <i>clamo</i> ‘scream’, <i>complector</i> ‘hug’, <i>ferveo</i> ‘boil’, <i>fleo</i> ‘weep’, <i>flo</i> ‘blow’, <i>frico</i> ‘rub’, <i>mulceo</i> ‘caress’, <i>mulgeo</i> ‘milk’, <i>ploro</i> ‘cry’, <i>premo</i> ‘squeeze’, <i>rado</i> ‘scrape’, <i>sanguino</i> ‘bleed’, <i>sorbeo</i> ‘suck’, <i>strideo</i> ‘yell’, <i>stringo</i> ‘squeeze’, <i>sugo</i> ‘suck’, <i>tergeo</i> ‘wipe’, <i>verro</i> ‘brush’
<i>surdus</i> ‘deaf’	<i>caedo</i> ‘cut, knock’, <i>clamo</i> ‘scream’, <i>cudo</i> ‘knock’, <i>strideo</i> ‘yell’, <i>tundo</i> ‘beat’
<i>tortus</i> ‘crooked’	<i>caedo</i> ‘cut, knock’, <i>cudo</i> ‘knock’, <i>tundo</i> ‘beat’

The subcorpus obtained was composed by all the sentences where each adjective combined with at least one of the verbs of the same row in the box. Despite the ample range of adjectives and verbs used and their high absolute frequency in Latin, none of the sentences of the mentioned subcorpus featured an adjectival complex resultative construction. Therefore, my conclusion is that Early and Classical Latin (see footnote 1) disallows this type of complex resultative constructions.

Summing up, the scenario provided by Slavic and Latin, two alleged s-framed languages, could be a problem for the hypothesis that the availability of both adposition-based resultatives and adjective-based resultatives is due to the same fact, namely, that in s-framed

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could also combine with a particular adjective. This is the case of the verbs combining with *aeger* ‘ill’, or *pinguis/opimus* ‘fat’.

languages the Path may be expressed independently from the verb, as first proposed in the Talmy-Aske typology and l-syntactically interpreted by Mateu (2002) or Mateu & Rigau (2002). Moreover, the typological difference between s- and v-framed languages seems not to hold in the case of AP resultatives, since s-framed languages like Latin or Slavic pattern with v-framed languages like Romance in their disallowing those constructions.

### 3. Prefixation of the resultative predicate prevents complex AP resultatives

#### 3.1. An empirical observation

In this section I will present an empirical observation which may shed light on why s-framed languages like Slavic or Latin do not allow resultative constructions based on APs; it may also help us maintain a syntactic modelling of the Talmyan typology as the basic explanation for the availability of resultative constructions cross-linguistically, regardless of the category of their resultative secondary predicates. The observation is the one in (14) and is first illustrated in (15) and (16) for Latin and Russian, respectively:

- (14) Neither Latin nor Slavic, both disallowing AP resultatives, seem to allow the expression of complex resultative constructions without a result-conveying prefix attached onto the verb.

#### Latin

- (15) Repente    ad pabulatores    \*(ad-)volaverunt (Caes. *Gall.* 5, 17, 2)  
suddenly    at forager.ACC.PL    at-fly.PRF.3.PL  
'They flew upon the foragers suddenly.'

#### Bulgarian

- (16) \*(Iz)-kopah    sukrovishte (iz    dupkata)    (Angelina Markova, p. c.)  
out-dug    treasure    (out hole.ART)  
'I dug a treasure out of the hole.'

If the observation in (14) is on the right track, which I will attempt to show later, a possible way to make sense of it is through an implication construed in the following terms:

- (17) In some (s-framed) languages, there is a prefixation requirement on the element expressing the resultative predicate, so that it has to appear attached to the verb. This requirement impedes those languages to feature complex adjectival resultative constructions.

Now, if the prefixation operation affecting the resultative secondary predicate is taken as obligatory, possible or impossible, and if no other factor is taken into account, the implication in (17) yields a certain distribution of languages with respect to their allowance of complex resultatives based on APs:

- (18)

PREFIXATION OF THE RESULT PREDICATE ONTO V			
	IMPOSSIBLE	POSSIBLE	OBLIGATORY
AP RESULTATIVES	available	available	unavailable



We will go back to this typology later on. But first I would like to illustrate that the observation in (14) is empirically correct for Latin and Slavic.

### 3.2. Complex resultatives predicates are prefixed in Latin

I have conducted three searches in order to see whether Latin expresses complex resultative constructions always with the aid of a prefix. The first two searches aimed at finding complex resultative constructions formed by a PP and an unprefix surface contact verb, in the first search, and an unprefix sound emission verb, in the second search.<sup>7</sup> The search involving sound emission verbs, which was intended to find resultative constructions analogous to *Mary whistled Rover to her side* (Folli & Harley 2006), did not render any result whatsoever. The search involving surface contact verbs yielded 9 apparent examples, 2 of which are included here (the allegedly resultative PPs have been underlined):

#### Latin

(19) Clipeum=que            in pectora            calcat    (Stat. *Theb.* 8, 541)  
 shield.ACC.SG=and    in chest.ACC.PL    press.3.SG  
 ‘He presses his shield against his chest.’

(20) Piscis            hiberno                    ex aequore    verris    (Hor. *Sat.* 2, 3, 235)  
 fish.ACC.PL stormy.ABL.N.SG    out    see.ABL.SG sweep.2.SG  
 ‘You sweep the fish out of a stormy see.’

However, at least some of the examples I retrieved are not unequivocally complex resultative constructions, since they are not evidently telic (see § 3). In (19), for instance, the PP *in pectora* obviously represents an unbounded directional PP, since there is no entailment that the shield end up inside the soldier’s chest. Consequently, the predicate must be atelic. Other examples, such as (20) really appear to imply both a final location and telicity. However, it seemed necessary to conduct another search which overtly took into account the inner-aspectual dimension of the complex predicate in Latin. The search this time, involving complex predicates based on prefixed and unprefix manner-of-motion verbs and a battery of telicity-signalling expressions, intended to check the proportion of telic resultative predicates formed with either type of manner-of-motion verbs.<sup>8</sup> Here the results were significant, since unprefix verbs, unlike prefixed ones, almost never license a telic adverbial, despite appearing with a directional PP eligible as conveying the resultative predicate. Out of the 162 telic predicates yielded by the search, 8, represented through the

<sup>7</sup> The verbs used in these searches are the following ones. Unprefix surface contact verbs: *calco* ‘tread’, *frico* ‘rub’, *rado* ‘scrape, scratch; razor’, *tergeo* ‘wipe’, *tero* ‘rub, grind; thresh’, *verro* ‘sweep’. Unprefix sound emission verbs: *fremo* ‘roar’, *strideo* ‘yell’, *rideo* ‘laugh’, *sibilo* ‘whistle’, *latro* ‘bark’, *ululo* ‘howl’, *mugio* ‘moo’, *hinnio* ‘neigh’, *strepo* ‘make a lot of noise’, *grunnio* ‘grunt’, *rudo* ‘bray’, *ballo* ‘bleat’. The prepositions for both searches are: *ab* ‘off, away’, *ad* ‘at, beside, by’, *de* ‘downward; from, away’, *ex* ‘out of’, *in* ‘in’.

<sup>8</sup> The prefixed and unprefix manner-of-motion verbs used in this search are the following ones (<p> stands for any prefix): (p-)ambulo ‘walk’, (p-)curro ‘run’, (p-)equito ‘ride’, (p-)fluo ‘flow’, (p-)gredior ‘walk, step’, (p-)labor ‘slip’, (p-)navigo ‘sail’, (p-)repo ‘crawl’, (p-)salio ‘jump’, (p-)volo ‘fly’. Some of the telicity-signalling expressions are: *ut primum* ‘as soon as’, *statim*, *subito*, *subitum*, *repente*, *extemplo* ‘suddenly’, *duabus horis* (ablative) ‘in two hours’, etc.

sample in (21) are headed by unprefixed manner-of-motion verbs, while 154, represented through the sample in (22), are headed by prefixed ones:

### Latin

- (21) a. Non statim ad C. Aquilium cucurrisses? (Cic. *Quinct.* 53)  
 not at\_once at C. Aquilius.ACC run.PRF.SBJV.2.SG  
 ‘Wouldn’t you have run up to C. Aquilius at once?’  
 b. Si statim navigas, nos Leucade consequere (Cic. *Fam.* 16, 1, 2, 4)  
 if at\_once sail.2.SG us.ACC Leucas.ABL catch\_up.FUT.2.SG  
 ‘If you sail off at once, you will catch us up at Leucas.’
- (22) a. Repente ex equis de-siliunt (Liv. 22, 48, 2)  
 suddenly out horse.ABL.PL downward-leap.3.PL  
 ‘Suddenly they leapt down from their horses.’  
 b. Biduo est de-gressus (Liv. 40, 22, 7)  
 two\_days.ABL.SG be.3.SG downward-walk.PST.PART.M.NOM.S  
 ‘He walked down in two days.’

Interestingly, out of the 8 telic predicates headed by unprefixed manner-of-motion verbs 3 (one of them shown in (21a)) are headed by *curro*, ‘run’, a verb which we know exhibits a special behaviour in v-framed languages like Italian or French; indeed, it has been observed that these languages admit telic predicates headed by *correre* and *courir*, as illustrated by the Italian example of (23), which additionally shows that telic *correre* triggers selection of *be* as auxiliary for the perfect tense and must hence be considered as heading an unaccusative predicate expressing an accomplishment rather than an activity:

### Italian

- (23) Gianni è corso in spiaggia in/\*per un secondo (Folli & Ramchand 2005)  
 Gianni is run.PST.PART in beach in/ for one second  
 ‘Gianni ran to the beach in one second/\*for one second.’

These data suggest that *correre/courir* and its counterparts may be construed as light verbs, and not as manner-of-motion verbs.<sup>9</sup> Thus, there seems to be cross-linguistic evidence that *run* is able to behave as a light motion verb, as well as a manner-of-motion verb. Therefore, for Latin telic predicates headed by unprefixed *curro*, like (21a), it could be pleaded that *curro* may be in fact behaving as a light verb, and that these examples are therefore not cases of complex directed motion constructions.

In sum, a tentative conclusion is that the expression of complex resultative constructions in Latin is carried out through the prefixation of the resultative predicate.<sup>10</sup>

<sup>9</sup> There is evidence that English *run* itself could optionally behave like a light verb, as noted by Den Dikken (2008) through the contrast in *He ran/\*danced in the room* (with a directional reading): *run* but not *dance* may license a directional resultative predicate without the Path preposition *to* since, being a light verb, incorporates a null directional preposition (see Den Dikken 2008 more details).

<sup>10</sup> Haverling’s (2000) study of the semantics of Latin prefixed and unprefixed verbs confers great robustness to the already proposed idea that, at least at the earliest stages of the language, the prefixes were crucial in determining the Aktionsart of the predicate; this scenario is illustrated in (24), where the unprefixed perfect verb *tacui* is understood as a past activity, with a duration limited by the adverb *adhuc*, and the prefixed counterpart *conticuit* (see footnote 5 for the /a/-to-/i/ change) is interpreted as an achievement, licensing the adverbial *repente*, ‘suddenly’:

### 3.3. Complex resultatives predicates are prefixed in Slavic

Let us turn now to Slavic. To begin with, Slavic complex motion predicates are similar to Latin analogous predicates in that they typically feature a prefix encoding the final location of the motion path, as the Bulgarian, Czech and Serbo-Croatian examples in (25) through (27) respectively illustrate:

#### Bulgarian

- (25) Do-bjagah do bolnitsata (Markova & Padrosa-Trias 2008)  
 to-ran to hospital.ART  
 ‘I ran to the hospital.’

#### Czech

- (26) Při-nesl ze sklepa uhlí (Filip 1997)  
 to-carried.SG from.PREP basement.GEN.SG coal  
 ‘He brought (some) coal from the basement.’

#### Serbo-Croatian

- (27) Jovan je od-gurao prikolicu od prskalice (Arsenijević 2006)  
 Jovan AUX from\_at-push trailer from sprinkler  
 ‘Jovan pushed the car away from the sprinkler.’

A number of researchers have pointed out that the prefixation illustrated above must be in fact considered a general rule. Rojina (2004:29) states, for Russian at least, that “[...] the prefix is inseparable in Russian and the movement <in her terms, movement of the prefix from its original position as head of some ParticleP onto the verb: Acedo-Matellán> is obligatory.” As for Bulgarian, Angelina Markova, informs, in a personal communication, that the prefixation requirement for resultative predicates is general in this language:

#### Bulgarian

- (28) \*(Iz)-kopah sukrovishte (iz dupkata) (Angelina Markova, p. c.)  
 out-dug treasure (out hole.ART)  
 ‘I dug a treasure out of the hole.’

Finally, the observation in (14) which was made at the beginning of section 3.1 is made extensive to Slavic in general by Gehrke (2008:203), who states that “[...] there seems to be some morphological requirement to express resultativity on the verb in these languages <Slavic languages: Acedo-Matellán>. Indicative of this approach is that these Slavic languages lack adjectival resultatives of the English type (e.g. *hammer the metal flat*) but generally have to use accomplishment/achievement verbs (that are additionally marked for

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#### Latin

- (24) a. Tacui adhuc: nunc non tacebo (Plaut. *Truc.* 817)  
 be\_silent.PRF.1.SG until now not be\_silent.FUT.1.SG  
 ‘I have been silent until now: now I shall not be silent.’  
 b. Repente con-ticuit (Cic. *Catil.* 3, 10)  
 suddenly PREF-be\_silent.PRF.3.SG  
 ‘Suddenly he fell silent.’

resultativity by an internal prefix) and/or PPs in such constructions.”<sup>11</sup> She further notes (Gehrke 2008:203, footnote 14) that “in fact, it seems like resultativity is always expressed morphologically by an internal prefix on the verb.”

In sum, there is evidence that Slavic may indeed share with Latin the morphological requirement that I assumed in (14). I note furthermore that internal (see footnote 11) prefixes seem to convey the resulting state/location of the predicate and are thus analogous to adjectives in adjectival resultative constructions. This is proposed by Arsenijević (2006), Gehrke (2008) or Žaucer (2009). Thus, for instance, both in the Polish example in (29a) and in the Latin example in (29b) the prefix refers to a place which is the final location resulting from the event conveyed by the verb:<sup>12</sup>

#### *Polish and Latin*

- (29) a. Dzieci w-skoczyły do wody (Lindvall 2001, apud Svenonius 2004)  
 children in-jumped to water  
 ‘The children jumped into the water.’
- b. Tenuissimas radices ex-arabit (Cato, Agr. 61, 1)  
 slender.SUPERL.ACC.F.PL root.ACC.PL out-plough.FUT.3.SG  
 ‘He will plough out the most slender roots.’

On the other hand, as shown to happen in Latin, Slavic (internally) prefixed predicates are telic, as pointed out by Ramchand (2004), Svenonius (2004), Arsenijević (2006) or Gehrke (2008), among many others. This illustrated with the examples of (30), through the diagnostic of delimiting adverbials:

#### *Russian*

- (30) On pri-exal v Moskvu \*(za) den’ (Gehrke 2008)  
 he to-drive.PST in Moscow.ACC in day  
 ‘He arrived in Moscow in/\*for a day.’
- On ot-kryl okno \*(za) dve minuty (Gehrke 2008)  
 he from-cover.PST window.ACC in two minutes  
 ‘He opened the window in/\*for two minutes’

Thus, it seems that this kind of prefixed predicates meet the requirements for being considered complex resultative constructions.

<sup>11</sup> As for the notion of *internal* prefix, I will just note that Gehrke is referring to the prefixes which are assumed to originate inside the VP since they are able to affect the argument structure and inner-aspectual properties of the predicate. In this sense they are opposed to *external* prefixes, which I will not deal with here. For the internal/external difference within the domain of Slavic prefixes see Svenonius (2004), Romanova (2004), Milićević (2004), Gehrke (2008), among others. See Žaucer (2009) for an interesting discussion of Slovenian data which are apparently not in conformity with common assumptions on the syntax of internal prefixes.

<sup>12</sup> Of course result-conveying prefixes may express more abstract resulting locations. The Latin prefix *e(x)*- in (31), for instance, somehow indicates what is outside of someone’s psychological or physical state. This is analogous to the semantics of the English particle *off* in the translation:

#### *Latin*

- (31) E-dormi crapulam, inquam (Cic. Phil. 2, 30)  
 out-sleep.IMPERAT.2.SG hangover.ACC.SG say.1.SG  
 ‘Sleep off that hangover, I said.’

### 3.4. Interim conclusion: complex resultative predicates are prefixed in Latin and Slavic

The empirical observation made in (14) seems to be adequate for Latin and Slavic. In particular, I would like to argue that any complex resultative predicate in these languages has to be prefixed, even if provided with an allegedly resultative PP (see footnote 14). In these predicates the (unprefixed) verb lexicalises the event leading to the resulting state expressed by the prefix. Furthermore, these predicates have been shown to be telic, both in Latin and Slavic. This fact is important, because it distinguishes complex resultative predicates of Latin and Slavic from the constructions found in v-framed languages like Romance which are atelic and consist of a manner-of-motion verb and a directional PP.

Note, crucially, that only complex resultative predicates are stated to be prefixed; this means that other telic predicates may be unprefixed, as the Latin and Russian ones shown in (32):

#### *Latin and Russian*

- (32) a. Partem            paucis            diebus            sese  
 part.ACC.SG    few.ABL.M.PL    day.ABL.PL    REFL.ACC  
daturos                                  dixerunt    (Caes. *Gall.* 4, 27)  
 give.FUT.PART.ACC.M.PL    say.PRF.3.PL  
 ‘They said they would render a part of it in a few days.’
- b. On dal    ženšćine      knigu      \*(za) dve    minuty    (Gehrke 2008)  
 he    gave woman.DAT    book.ACC    \*(in) two    minutes  
 ‘He gave a/the woman a/the book in/\*for two minutes.’

This unidirectional implication between prefixation and telicity suggests that the prefix, rather than inducing telicity, only signals it. The same conclusion is arrived at by Arsenijević (2006) and Gehrke (2008). What I will claim in the next section is that in Latin and Slavic, the functional head responsible for the telicity in complex resultative predicates is morphologically specified as a prefix.

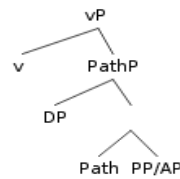
## 4. A morphophonological analysis

### 4.1. Basic assumptions on the syntactic building of argument structure

I work within a syntactic theory of argument structure, which, crucially, endeavours to do away with Hale & Keyser’s (1993f.) distinction between a lexical syntax and a properly syntactic syntax. As stated also in other frameworks such as Borer’s (2005) or Distributed Morphology (Halle & Marantz 1993; Marantz 1995, 1997; Harley & Noyer 1999; among other works), I believe it desirable to assume that a single computational module can account for the syntax of argument structure and beyond.

I will use two functional heads, *v* and Path, to characterise the structure of resultative predicates. *v* introduces an event, while Path introduces the semantics of change or transition, and, when the phrase it heads is sister to *v*, is responsible for a telic interpretation of the event:

(33)



Beside these relational elements, which are able to project, we find roots, which, alongside Mateu (2002) or Borer (2005), and pace Marantz (1997f.) or Harley (2005), I take to be non-relational and unable to project. In particular, roots are either complements or adjuncts, but never heads (or specifiers). Furthermore, following Distributed Morphology, I consider them acategorial. Along the lines of Marantz (2003), I suggest that roots may appear as adjuncts to the *v* head. The semantic import of such a configuration is the identification of the event introduced by *v* with the conceptual properties of the root. Thus, the configuration in (34) is read off as the event of singing. We will see that although *manner-adjunction*, is of course freely available in all languages, it is sometimes precluded in *v*-framed languages by independent reasons.<sup>13</sup>

(34)

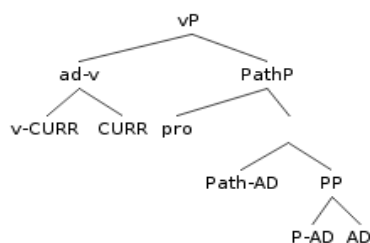


#### 4.2. Complex prefixed resultative predicates in Latin and Slavic

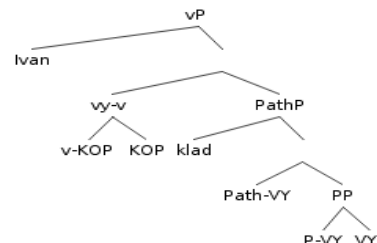
The complex resultative predicates we have seen in the preceding section are transitive or unaccusative structures where the *v* head is bundled with a root specifying the conceptual properties of the event, as just schematised in (34). I next provide an analysis of an unaccusative predicate in Latin (35a), and a transitive one in Russian (35b):

##### Latin and Russian

(35) a. Subito ac-currit  
suddenly at-run.3.SG  
'Suddenly, he himself arrives in haste'



b. Ivan vy-kopal klad  
Ivan out-dug.PRF treasure.ACC  
'Ivan dug out the treasure'



(35a) is an unaccusative structure, with no external argument merged as specifier of *v*. The root CURR is manner-adjoined to little *v* and specifies the manner component of the motion

<sup>13</sup> Note that in the representation of (34) the little *v* head is iterated as the root of the tree, as commonly assumed for adjunction structures. Roots can of course be taken as complements to little heads, but then the result is an *xP*, not an *x*. Unfortunately, I cannot hope to solve here the non trivial difficulty of distinguishing adjunction from complementation of roots within a Bare Phrase Structure approach to syntax.

event. The complement of *v* is the projection of the Path head, PathP, which is responsible for a dynamic and telic interpretation of the event. Path takes a specifier which is understood as the Figure of the motion event, in Talmy's terms. In turn, the complement of Path is a PlaceP conveying the final location of the Figure as a result of the event identified as a running event. This PlaceP is headed by an abstract preposition, Place, which indicates mere location and takes as its complement root AD, which may express the idea of proximity or just be, as is the case here, a deictic element to be equated with the speaker. In the analysis of the Russian example of (35b) we find the same elements, the only difference being the appearance of a specifier of little *v*, which is interpreted as the external argument of the event consisting of the treasure getting out of somewhere as a result of digging. In both (35a) and (35b) the heads *v*, Path and the preposition heading the PP, appear without a phonological matrix, and, following Hale & Keyser (1993, 1998, 2002), I propose that they get them through the mechanism of *conflation*; conflation selects a phonological matrix from a given node and inserts it into the sister empty or defective head. This is what happens with the manner roots CURR and KOP in (35a) and (35b), respectively, which fill up the empty matrix of little *v*. In a parallel way, the roots AD and VY provide their phonology into the P head and then into Path. In (35a) and (35b) a conflated root is represented as attached to the right of the target through a little dash. I take conflation to happen after syntax, at PF, following the Distributed Morphology idea that Morphology and Phonology interpret the output of syntax at Spell-out. At this point, I suggest that the Path head in Latin and Slavic is morphologically specified as a prefix, and that it must take a further step for the phonological representation to fully converge: namely, to get attached onto the verb. Note that this affixation operation is required by the properties of Path, and not by any need of the upper *v* head, which gets a phonological matrix already from the manner root adjoined onto it. This is also represented in (35a) and (35b) through a dash on the left of little *v*. These processes derive the fact that complex resultative predicates are prefixed in Latin and Slavic.<sup>14</sup> Additionally, the fact that the affixing requirement lays on Path itself and not on the properties of each root, explains why prefixation is not obligatory in cases of static, i.e., Path-less predicates with allegedly argumental, vP-internal PPs. This is illustrated through the Latin and Russian examples in (36a) and (36b), respectively:

*Latin and Russian*

- (36) a. Fuit                    certe            contentio            in senatu (Liv. 10, 24, 4)  
       be.PRF.3.SG    certainly    struggle.NOM.SG    in senate.ABL.SG  
       'There was in fact a struggle in the senate.'
- b. Kniga                byla    na stole (Freeze 1992)  
       book.NOM.SG    was    on table  
       'The book is on the table.'

Evidently, the same lack of prefixation is expected and attested in atelic non-directional manner-of-motion predicates with non-argumental PPs. This is exemplified and analysed

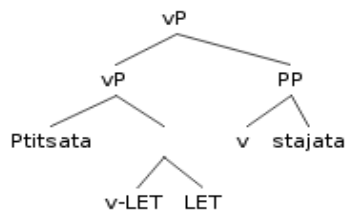
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<sup>14</sup> If the prefix in this kind of predicates is analysed as conveying the final location/state of the event, and PPs in the same predicates are taken to express the same notion (see, for instance, the PP *ex equis* in the Latin example of (22a) and the PP *do wody* in the Polish example of (29a)), the question naturally arises what the role and the structural source of PPs in resultative prefixed predicates is. My guess here is that, despite appearances, and pace Gehrke (2008), these PPs do not really occupy a place in the argument structure configuration, but are adjuncts to PlaceP which specify the final location introduced by the prefix. As a first argument in favour of that take, note that these PPs are not required for a complex resultative construction to be licensed as such (see examples (35a) and (35b)).

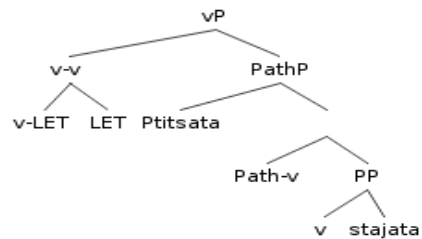
through the next Bulgarian contrast between non-directional and directional motion (Angelina Markova, p. c.):

*Bulgarian*

(37) a. Ptitsata leti v stajata  
 bird.ART flies in room.ART  
 ‘The bird flies inside/\*into the room.’

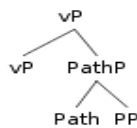


b. Ptitsata y-letja v stajata  
 bird.ART in-flies in room.ART  
 ‘The bird flew into/\*inside the room.’



Crucially, I take this prefixation operation to be an instance of head movement, pace Svenonius (2004), who proposes that prefixation in Slavic is phrasal movement. Note that if Path-prefixation is head movement we expect it to respect the Head Movement Constraint (Travis 1984, Baker 1988), that is, to always take place from one head  $H_1$  to the upper one  $H_2$ , in a stepwise fashion. Therefore, the prefixation operation we are dealing with will not be able to target the Path heading an adjunct to vP and not a sister to v:

(38)



Given the already made assumption that PathPs which are not sisters to v cannot induce telicity, the prediction emerges that atelic motion predicates with a directional semantics shall not feature a prefix, since the directional PathP could never render its Path up for prefixation from an adjunct position. A first indication that this is indeed the case comes from Russian and Czech, where, according to Gehrke 2008, the only prepositions which do not have a prefixal counterpart are those that convey an unbounded Path: *k* ‘toward’, in Russian, and *k* or *vůči* ‘toward’, in Czech. This morphological contrast between telic and atelic directional motion predicates is also attested in Bulgarian, as the following pair shows:

*Bulgarian*

(39) a. Toj pulzi kum vratata (Angelina Markova, p. c.)  
 he crawls towards door.ART  
 ‘He crawls towards the room.’

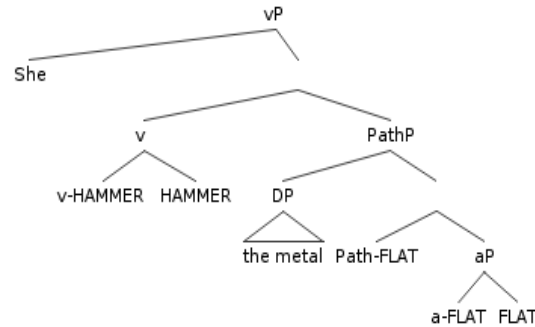
b. Toj do-pulzia do vratata (Angelina Markova, p. c.)  
 he to-crawled to door.ART  
 ‘He crawled up to the door.’



## 4.3. Complex AP resultative predicates

I have provided an account of the cases where Path takes a PP as complement. But what happens when it takes an AP as complement, that is, what happens when the complex resultative predicate is built on an AP? I will begin the discussion with the derivation of the English resultative construction *Sue hammered the metal flat* in (40):

(40)



In (40) we find a structure which closely parallels the ones proposed for the prefixed predicates in Latin in Russian in (35a) and (35b); again, we find a manner-conflation configuration where the root HAMMER provides its phonological material to its adjunct-sister v. v takes as its sister PathP, where the Path head projects a specifier which is to be read off as the Figure of the change-of-state event. Sister to Path is, in this case, an AP, headed by a little a (Marantz 2000), which introduces the semantics of property and the adjectival category. Sister to little a is the root FLAT, which provides the conceptual dimension of the adjective, and, also furnishes its phonological material up to little a and thence to Path. I propose that in English and other s-framed languages I will call *strong* s-framed languages, by contrast with *weak* s-framed languages like Latin or Slavic, the Path head is not specified as a prefix, so it can remain unattached to the verb and the output will be convergent at PF. Now, crucially, what is it that makes complex adjectival resultative constructions like the one just seen unavailable in Latin and Slavic? I propose that any such construction should have the derivation I have put forth for English in (40) but with one crucial difference: adjectives, both in Latin and Slavic always bear agreement markers with the same value as the phi-features born by the DP which the adjective is predicated of:

*Latin and Polish*

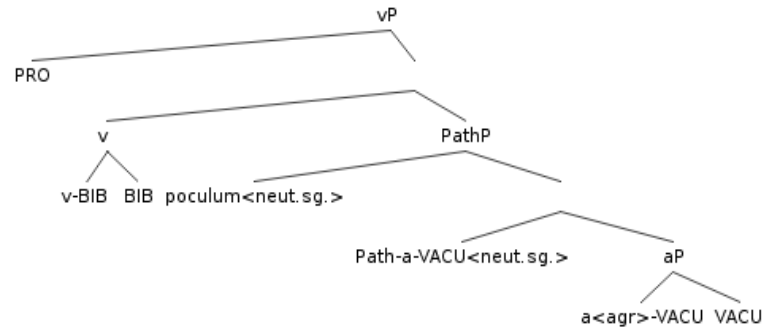
- (41) a. Inmodic-a/                      \*-us/                      \*-as                      ira  
           measureless-NOM.F.SG/ -NOM.M.SG/    -ACC.F.PL    rage.NOM.F.SG  
       b. Now-y/                            \*-e/                            \*-ą                      stół  
           new-INANIM.ACC.M.SG/ -INANIM.NOM.M.PL/ -ACC.F.SG    table-INANIM.ACC.M.SG

I propose that these agreement markers be treated as *dissociated morphemes*, first introduced by Embick (1998) in Distributed Morphology. Morphemes of this kind are inserted in the Morphology, in certain syntactic configurations, so they do not correspond to actual morphosyntactic features of Narrow Syntax. In the case of Latin and Slavic, where agreement morphology on the adjective is always obligatory in all syntactic environments, I will just assume that the dissociated morpheme is inserted onto little a in any case. With this in mind,

let us now imagine what the derivation of an adjectival complex resultative could look like in Latin and Slavic. I illustrate it for Latin:

*Latin*

(42) \*Poculum vacuum bibere  
 goblet.ACC.SG empty.ACC.SG drink.INF



The little *a* head presents, right-adjoined to it, a dissociated morpheme, suffixal in both languages, which is to be the host for the agreement morphology of the adjective. This means that the phonological matrix of the little *a*-plus-agreement complex is defective, since little *a* has no phonological matrix of its own, and the dissociated morpheme can first get it when the features of the DP sitting in Spec-Path position are copied into it. Through conflation little *a* acquires the phonological matrix of the root VACU, as represented in the tree. The Path head also needs a phonological matrix, and gets that from the complex of little *a*-plus-dissociated morpheme, into which are copied the phi-features of the DP in Spec position. However, the Path head is specified as prefixing in these languages, so it should be head-moved onto the *v* head in order for the derivation to converge at PF. But this is an impossible step, since head movement cannot prefix an already inflected form. As a result, the Path fails to get prefixed onto the verb, yielding an output which is not interpretable at PF.

Thus, the unavailability of AP complex resultative constructions in Latin and Slavic depends on two independently needed factors: on the one hand, the status of Path as a prefix, which also accounts for the lack of unprefixed complex resultative constructions; on the other hand, the fact that adjectives in these languages are morphologically complex, in that they bear agreement markers. By contrast, in other *s*-framed languages, AP complex resultatives are possible since the Path needs not get attached onto the verb. I illustrate with the Finnish, German and Norwegian examples in (43) through (45): the *a*-examples show that particles in complex resultative constructions in those languages are not required to get prefixed onto the verb, while the *b*-examples show the availability of complex adjectival resultative constructions:<sup>15</sup>

<sup>15</sup> Observe that the data from Finnish could constitute overt morphological evidence for the movement of the adjective onto Path, since the adjective in this languages, when it is used as a secondary resultative predicate, presents a case morph of directional semantics (see *-ksi* in (43b)), as pointed out in Fong 1997. The Path head would have a defective phonological matrix consisting merely in the suffix, and the adjective would fill up that matrix by conflation.

*Finnish*

- (43) a. Uolevi asui täällä, mutta hän muutti pois (Kolehmainen 2005)  
 Uolevi live.PST hier but he move.PST away  
 ‘Uolevi has lived here, but he has moved away.’
- b. Ravistin mato-n puhtaa-ksi (Fong 2003)  
 shake.PST.1.SG carpet-GEN clean-TRANS  
 ‘I shook a/the carpet clean.’

*German*

- (44) a. Sie malt die Figur auf (Stiebels & Wunderlich 1994)  
 she paints the statue up  
 ‘She paints the statue.’
- b. Die Teekanne leer trinken (Kratzer 2005)  
 the teapot empty drink  
 ‘To drink the teapot empty.’

*Norwegian*

- (45) a. Jon syklet til byen på en time (Tungseth 2003)  
 Jon biked into town in one hour  
 ‘Jon biked into town in an hour.’
- b. Per dansa Marit sur (Åfarli 2007)  
 Per danced Marit sour  
 ‘Per danced Marit sour.’

*4.4. The Icelandic case*

Additional empirical support for an analysis of AP resultatives taking into account prefixation and the agreement morphology on the adjective comes from Icelandic. This language allows the Path head to remain in situ, as shown in the PP resultative of (46):

*Icelandic*

- (46) Baturinn flýtur undir brúna (Whelpton 2006)  
 boat.ART float.3.SG under bridge.ART.ACC  
 ‘The boat is going under the bridge by floating.’

Interestingly, however, Icelandic presents two types of adjectival resultative constructions: those where the adjective remains detached from the verb, as in (47a), and those where it gets prefixed onto it, as in (47b):

*Icelandic*

- (47) a. Dóra æpti sig hás-a (Whelpton 2007)  
 Dóra screamed REFL.ACC.F.SG hoarse-ACC.F.SG  
 ‘Dóra screamed herself hoarse.’

- b. Hann hvít-bæsti rammann (Whelpton 2007)  
 he white-stained frame.ART  
 ‘He stained the frame white.’

The crucial observation here is that, while in (47a) the adjective is inflected, it is not in (47b). This dissociation could be interpreted as follows: the adjective in Icelandic complex resultative constructions is allowed to bear agreement morphology. If it does, it cannot get attached onto the verb, for the reasons already exposed, but this does not yield a deviant output, since the Path is not specified as prefixing in this language. When it does not bear agreement morphology, however, it may be prefixed onto the verb, since it is not morphologically complex. There are other facts linked to the difference between prefixed and unprefixed adjectival resultative constructions in Icelandic, as pointed out by Whelpton (2006), such as a much lesser productivity of the former than the latter, but I will not go into that matter here.

A further conclusion to draw from a comparison between Icelandic and, say, English, is that strong s-framed languages seem to be amenable to a two-way division: the ones where the Path is specified as -affixal, like English, and the ones where it is unspecified as to its affixal status, as in Icelandic.

#### 4.5. V-framed languages

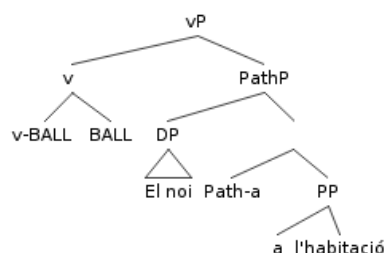
As for v-framed languages, we already know that complex resultative predicates are not allowed, regardless whether they are based on an AP or a PP:

##### Catalan

- (48) a. \*El noi ballà a l’habitació (directional reading) (Mateu 2002)  
 the boy danced at the=room  
 b. \*El gos bordà els pollastres desperts (Mateu 2002)  
 the dog barked the chickens awake

Following Mateu (2002), I propose that the Path in v-framed languages be marked as +conflating. The effect of this is to originate a phonological clash when there is another candidate for conflating into v, namely any root manner-adjoined to v. Since conflation is a strictly local operation and independently triggered by the needs of the upper empty head, the manner root which is adjoined to v conflates first, providing v with the phonological matrix it needs and precluding further conflation from Path. The phonological +conflating requirement of Path is therefore not satisfied, and the derivation crashes at PF. This is roughly the analysis proposed by Mateu (2001, 2002), and I schematise it in my terms in (49) for example (48a):

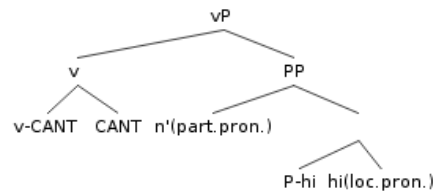
(49)



A welcome side-effect of permitting manner-adjunction as a universal option is, alongside the purely theoretical desirability of the lack of stipulated prohibitions, the fact that, as observed by Rigau (1997) and Mateu (2002), in v-framed languages like Catalan or Italian this option seems to be possible, crucially in stative, Path-less predicates, as shown and analysed in (50) for Catalan:

*Catalan*

- (50) En aquesta coral n'hi canten molts, de nens  
 in this choir PARTVE=LOC sing.3.PL many.PL of child.PL  
 'There are many children who sing in this choir.' (CATALAN: Mateu 2002)



In this example, the light verb *v* is free to get bundled with a manner-root which specifies an otherwise merely existential event. The derivation is convergent, since there is no Path head in these stative predicates trying to get conflated into *v*.

#### 4.6. A finer-grained typology

Summing up, the Path has been proposed to be marked as +conflating or non-conflating, and, within this category, as -affixal, +affixal or unspecified. This morphological specification, when combined with the overt morphological properties of the adjective in each language, produces a fine-grained typology which I hope to have argued captures the cross-linguistic variation in the expression of complex resultative predicates in a way more precise than that of Talmy-Aske-Mateu's or Snyder's (2001). It is represented in (51):<sup>16</sup>

(51)

		AVAILABILITY OF RESULTATIVE SECONDARY PREDICATES		AGREEMENT MORPHOLOGY ON THE PREDICATIVE ADJECTIVE	
		PARTICLES	AP		
MORPHOLOGICAL PROPERTIES OF PATH	-CONFLATING (S-FRAMED LANGUAGES)	+	(+) ?	+	
			-AFFIXAL (ENGLISH)	+	-
			UNSPECIFIED (ICELANDIC)	+	+
				-	-
			+AFFIXAL (LATIN, SLAVIC)	(+) ?	+
	+CONFLATING (V-FRAMED LANGUAGES)		-	-	

<sup>16</sup> Observe that, for clarity's sake, I have added no column for PP resultative predicates in the table of (51). As I pointed out in footnote 14, I believe that there are no true PP complex resultative constructions in weak s-framed languages like Latin or Slavic, the allegedly resultative PPs being in fact (low) adjuncts.

Note that there are two cells which remain to be checked for in (51): the one corresponding to languages with a -affixal Path and agreement morphology on the predicative adjective, and the one corresponding to languages with a +affixal Path and no agreement morphology on the predicative adjective. While the prediction is that both types of languages will allow for AP resultatives (since there would not be any clash between the morphological properties of Path and the adjective), that needs empirical confirmation.<sup>17</sup>

### 5. One potential problem: alleged complex AP resultative constructions in Korean

Son & Svenonius (2008), in a paper on cross-linguistic variation in the expression of complex resultative predicates, include data from Japanese and Korean which may jeopardise the analysis presented so far and the empirical generalisation arrived at in section 3.1. According to these authors, Japanese and Korean, in spite of disallowing complex directed motion constructions, allow for complex adjectival resultative constructions. Here I will concentrate on a revision of the Korean data, as seen in (52):<sup>18</sup>

#### Korean

- (52) a. \*Mary-ka cip-ey fttwi/kelg-ess-ta  
 Mary-NOM house-LOC run/walk-PST-DC  
 ‘Mary ran/walk to the house.’  
 b. Inho-ka kkangthong-ul napcakhya-key twutulki-ess-ta  
 Inho-NOM can-ACC flat-KEY pound-PST-DC  
 ‘Inho pounded the can flat.’

The data in (52) seem to go directly against the analysis I have proposed, which predicts that if a language generates AP resultatives, it will also generate PP resultatives, since the basic structure for both is the same and it is only the derivation of AP resultatives which can be blocked by independent features of the language. The analysis would not be endangered, though, if the AP resultative in (52b) were shown not to be a complex AP resultative of the Germanic type, as the ones I have examined. Here I would like to appeal to Shim and Den Dikken’s (2007) work on resultative constructions in Korean and English. An in-depth presentation of their position being impossible here, I will limit myself to focus on their observation that the *key*-suffixed AP typical of these constructions in Korean, and which is to be found also in (52b), behaves as an adjunct to VP, and, therefore, does not qualify as a true secondary predicate of the resultative type sitting inside the vP. Two of the diagnostics for the vP-external status of *key*-APs are illustrated in (53). On the one hand, as shown in (53a), *key*-APs may be stranded under VP-replacement by the proform *kuleh*, as opposed to resultative APs in English resultative constructions —cf. the literal English translation \*Jim dyed his hair yellow, and Susana did so red. On the other hand, as any other adjunct, *key*-APs may iterate, which is

<sup>17</sup> I observe last that from a diachronic point of view, the morphological specifications of Path fall into a phased sequence if, as discussed in Acedo-Matellán & Mateu (2008), Path starts out as an independent element, then it optionally attaches onto the verb, afterwards the affixation is obligatory, and last, it becomes phonologically undistinguishable from the verb. An interesting prediction is that early Indo-European (as Vedic or Hittite), in featuring a -conflating, unspecified Path, should license complex AP resultatives. That is, however, left for future research.

<sup>18</sup> See Washio (1997) for an influential early description of resultative constructions in Japanese.

never possible with English resultative APs —see (53b); cf. the literal English translation \*Jim painted the floor white shiny:

*Korean*

- (53) a. Jim-i        meli-lul    nolah-key    yemsaykha-ko    Susana-nun  
 Jim-NOM    hair-ACC    yellow-KEY    dye-CONJ        Susana-TOP  
ppalkah-key    kuleh-ess-ta  
 red-KEY        PROFM-PST-DECL
- b. Jim-i        patak-ul    hayah-key panccaki-key    chilha-ess-ta  
 Jim-NOM    floor-ACC    white-KEY    twinkle-KEY    paint-PST-DECL

This evidence would show that the *key*-AP does not qualify as a true vP-internal resultative predicate, and hence, that the constructions claimed by Son & Svenonius (2008) as complex AP resultatives in fact would not be so.

6. Overall conclusions and further challenges

I have tried to offer an explanation why some s-framed languages allow the formation of complex adjectival resultative constructions and some do not. This explanation was based in two independently needed properties of the morphophonology of the languages at stake: on the one hand, the specification of the Path head as a prefix, which also accounts for the non-existence of unprefixated complex resultative constructions, whether or not featuring a resultative PP; and, on the other hand, the obligatory presence of agreement morphology on the adjective. We have seen that these two factors conspire in banning formation of complex adjectival resultatives in languages like Latin or Slavic, and, also, how they can have a particular effect in languages like Icelandic, where the Path is not specified as a prefix but where the adjective may or may not bear agreement markers. As for v-framed languages, which allow neither adjectival nor any other kind of complex resultative constructions, the Path have been proposed to be specified as +conflating. Thus, a typology emerges where a distinction is made, first between v-framed languages and s-framed ones, and within the latter, between strong s-framed languages, like Germanic (with two varieties represented by English and Icelandic, respectively), and weak s-framed languages like Latin.

Future challenges include, first, considering the possible counterexamples, presented in section 3, to the claim that Latin does not allow complex resultative predicates in the absence of a prefix (see, for instance, example (20)). As a second challenge, some Hebrew, Indonesian or Malayalam data reported by Son & Svenonius (2008) should be revised, since they apparently show that these languages do not allow for AP resultatives, but do allow complex directed motion constructions where the manner-of-motion verb does not bear any affix conveying the final location of movement. This is illustrated for Javanese in (54):

*Javanese*

- (54) a. Mary nyacah daging \*(sampek) ajur  
 Mary beat meat until flat  
 ‘Mary beat the meat until it became flat.’

- b. Tika fmlaku/ mlayu/ mbrangkangg ning ngisor jembatan  
 Tika walk/ run/ crawl LOC bottom bridge  
 ‘Tika walked/ran/crawled under the bridge.’ (both locative and directional readings)

The data in Javanese are problematic for my analysis: (54b) allegedly shows that this language allows complex PP resultatives, and that precludes the possibility that Javanese be a v-framed language. On the other hand, as far as the glosses tell us, the verb heading the sentence in (54a) is morphologically simple, so we must conclude that the Path is not attached to it. But an s-framed language with an *in situ* Path should admit complex AP resultative constructions, contrary to the evidence in (54a).

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# Against the complex predicate analysis of secondary predication

Yuko Asada

This paper provides an empirical argument against the complex predicate analysis of the two types of secondary predication – depictive and resultative – put forth by Cormack & Smith (1999) and Rothstein (2003, 2004). The data coming from various languages present challenges to major assumptions on the syntactic structures of depictives and resultatives adopted by the complex predicate analysis. The issues disappear under the traditional approach to secondary predication, along with the novel argument that depictive secondary predicates are TP adjuncts. Finally, I show that the proposal makes a correct prediction on negated secondary predication in English and Japanese small clause constructions.

## 1. Introduction

Secondary predication is commonly classified into the two types of constructions — depictives and resultatives — illustrated in (1) and (2) respectively.

(1) *Depictives*

- a. *John* left the room *angry*.                    ‘subject-oriented’
- b. John ate *the fish* *raw*.                        ‘object-oriented’

(2) *Resultatives*

John hammered *the metal* *flat*.

A depictive predicate as in examples (1a-b) describes a state pertaining to an argument of the main predicate, whereas a resultative predicate as in sentence (2) describes the state of an argument resulting from the action determined by the main verb. In the analyses of these constructions, there are roughly two competing views. The first one is the traditional approach, under which a depictive predicate is analyzed as an adjunct (cf. Roberts 1988, Rothstein 1983, Williams 1980) and a resultative predicate as forming a small clause type constituent with a direct object of the sentence (cf. Bowers 1993, Hoekstra 1988, 1992). Under this approach, the two types of secondary predication are assigned two distinct syntactic structures (see also Zhang 2001 for the analysis of Chinese secondary predication). By contrast, the other approach, the complex predicate analysis, provides a more uniform treatment of these constructions: it combines the primary and the secondary predicates into one and forms a complex predicate.<sup>1</sup> One proponent of this analysis, Rothstein (2003, 2004), assigns structures (3) and (4) to the two types of secondary predication.

(3) *Depictives*

a. *John* [<sub>VP</sub> [<sub>V</sub> drove the car] *drunk*]

‘subject-oriented’

b. *Mary* [<sub>VP</sub> [<sub>V</sub> drank *the coffee* *hot* ]]

‘object-oriented’

(Rothstein 2003, 2004)

(4) *Resultatives*

*John* [<sub>VP</sub> [<sub>V</sub> painted *the house* *red* ]]

(Rothstein 2003, 2004)

Similarly, Cormack & Smith (1999) propose structures (5) and (6), using a conjunction null operator which combines the primary and the secondary predicates into a complex one.

(5) *Depictives*

a. *John* [<sub>SP</sub>[\$ [angry]]] [<sub>VP</sub> left the room]

‘subject-oriented’

b. *Lucy* drinks both tea and coffee [<sub>V</sub>[\$ cold] [<sub>V</sub> *t* ]]

‘object-oriented’

(Cormack & Smith 1999)

(6) *Resultatives*

*Harry* painted both his car and his van [<sub>V</sub>[\$ red] [<sub>V</sub> *t* ]]

(Cormack & Smith 1999)

The complex predicate analysis proposed by these authors differs from the traditional approach to secondary predication in the following two key assumptions.

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<sup>1</sup> See Embick (2004), Larson (1988), Neeleman & van de Koot (2002), Synder (2001), and others for various arguments in favor of the complex predicate analysis.

- (7) The object-oriented depictive and resultative constructions are not to be distinguished in terms of their syntactic structures.
- (8) The secondary predicate does not form a constituent with its predication subject.

As the data show, Rothstein's structures (3b) and (4), as well as Cormack & Smith's (5b) and (6), are syntactically non-distinct. Also, we observe that in neither Rothstein's nor Cormack & Smith's structures, do the secondary predicate and its predication subject of which it is predicated form a constituent.

The goal of this paper is to challenge these two assumptions of the complex predicate analysis, and to propose an alternative analysis of secondary predication.

The paper is organized as follows. Section 2 presents the four sets of data coming from various languages that are problematic with the two key assumptions adopted by the complex predicate analysis. In consideration of these problems, an alternative analysis for depictive and resultative constructions is then provided in section 3. Section 4 applies this proposal to the cross-linguistic phenomena discussed in section 2. In section 4, the analysis of Shim & Den Dikken (2007) of Korean resultative constructions, which appears to conflict with my proposal, is also discussed. Section 5 shows that my proposal of secondary predication makes a correct prediction on two types of phenomena, the one from English and the other from Japanese. Section 6 concludes the discussion.

## 2. Problems

### 2.1. Problems with assumption (7)

The complex predicate analysis, although its simpler syntactic configurations assigned to the two types of secondary predication is appealing, is not free of problems which arise from wider cross-linguistic observations. The following three sets of data, the one from English and the other two from Japanese, present empirical issues to assumption (7) of the complex predicate analysis.

First, English has the verbal participle *-ing* that appears in depictive sentences as illustrated in (9) and (10).

(9) *Depictive 'subject-oriented'*

John decided to leave, thinking the party was over.

(Williams 1975:249)

(10) *Depictive 'object-oriented'*

a. She likes to drink tea, boiling hot.

b. We found John at last, sleeping in the library.

(Rothstein 1983:149)

In contrast, the *-ing* form is not allowed to occur in resultative predicates, as shown in examples (11).

(11) *Resultative*

a. \*I cried my eyes sparkling. (Fabb 1984:106)

b. \*I cooked it disgusting. (Kratzer 2005:33)

These sentences are ungrammatical, although it is semantically possible, for instance, to express the meaning that my eyes became sparkling as a result of crying and likewise, the interpretation that I cooked something which became disgusting.

Such a contrast in grammaticality between depictives and resultatives is difficult to explain, if we assume that the syntactic structures of object-oriented depictives and resultatives are non-distinct.

Second, a similar problem arises with the Japanese data. The nominal secondary predicates in Japanese are marked by two different particles, *de* and *ni*, and they are in complementary distribution. In a depictive sentence like (12), the predicate is marked by *de*, not *ni*, whereas the resultative predicate as in (13) is marked by *ni*, not *de*.

(12) *Depictive*

John-ga sakana-o [ nama\*-ni /-de ] tabe-ta.

John-NOM fish-ACC raw -NI/-DE eat-PAST

‘John ate the fish raw.’

(13) *Resultative*

John-ga kabe-o [ makka-ni /\*-de ] nut-ta.

John-NOM wall-ACC very red-NI/-DE paint-PAST

‘John painted the wall very red.’

This morphological variation suggests that the two types of secondary predication represent different syntactic structures.

The difference between depictives and resultatives is further confirmed by the third set of data, again from Japanese, which involves quantifier floating. (14) and (15) show examples of the two types of secondary predication that contain a numeral quantifier (NQ), *ni-dai* ‘2-CL,’ which is associated with the direct object, *kuruma-o* ‘car-ACC.’

(14) *Depictive*

John-ga kuruma-o ni-dai sinpin-de kat-ta.

John-NOM car-ACC two-CL new-DE buy-PAST

‘John bought two cars new.’

(15) *Resultative*

John-ga kuruma-o ni-dai makka-ni nut-ta.  
 John-NOM car-ACC two-CL very red-NI paint-PAST  
 ‘John painted two cars very red.’

In these examples the NQ and its associated NP are adjacent to each other, and the sentences are both grammatical. However, when the NQ in (14)-(15) gets ‘floated’ out of their canonical position, that is, when the NQ is not adjacent to its associated NP, the contrast emerges, as shown in examples (16) and (17).

(16) *Depictive*

John-ga kuruma-o sinpin-de ni-dai kat-ta.  
 John-NOM book-ACC new-DE two-CL buy-PAST  
 ‘John bought two cars new.’

(17) *Resultative*

\*John-ga kuruma-o makka-ni ni-dai nut-ta.  
 John-NOM car-ACC very red-NI two-CL paint-PAST  
 ‘John painted two cars very red.’

(Takezawa 1993:63)

The depictive sentence (16) remains acceptable, while the resultative counterpart is deviant as observed by Takezawa (1993). This contrast suggests that object-oriented depictive and resultative constructions have distinct syntactic statuses.

All these data show that object-oriented depictive and resultative constructions differ in their syntactic structures, thereby presenting a challenge to one of the key assumptions of the complex predicate analysis, namely (7): the object-oriented depictive and resultative constructions are not to be distinguished in terms of their syntactic structures.

## 2.2. Problem with assumption (8)

The second key assumption of the complex predicate analysis (8), i.e., that the secondary predicate and its predication subject do not form a constituent, raises a problem with the agreement phenomenon observed, for example, in Romance languages. Romance languages exhibit overt agreement in number and gender between the secondary predicate and its subject, as illustrated in examples (18) from French and (19a-b) from Italian.<sup>2</sup>

<sup>2</sup> There is a cross-linguistic variation in terms of agreement between the secondary predicate and its subject. German, for example, does not exhibit agreement in the secondary predicate constructions, even if it shows agreement in attributive use. This is illustrated in examples (i)-(ii), which I owe to an anonymous reviewer.

(18) *Depictive*

Pierre mange la viande crue.  
 eat-3s the meat-*fem.sg.* raw-*fem.sg.*  
 ‘Pierre eats the meat raw.’

(Legendre 1997:45)

(19) a. *Depictive*

Gianni mangiata la carne cruda.  
 eat-3s the meat-*fem.sg.* raw-*fem.sg.*  
 ‘Gianni eats the meat raw.’

(Napoli 1989:92)

b. *Resultative*

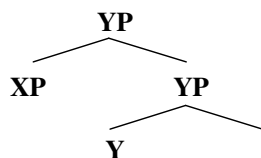
Ho dipinto l’armadio troppo scuro.  
 have paint-1s the closet-*m.sg.* too dark-*m.sg.*  
 ‘I painted the closet too dark.’

(Napoli 1992:82)

In the minimalist framework, one major proposal as to what configuration leads to agreement is a Spec-head relation (Chomsky 1995, Koopman 2006) as depicted in (20).<sup>3</sup>

(20) *Spec-head relation*

If XP agrees with Y, YP has merged with XP in the course of the derivation.



(Koopman 2006:4)

- 
- |      |  |
|------|--|
| (i)  | Pierre isst roh-es Fleisch. ( <i>attributive</i> ) |
|      | Pierre eats raw- <i>neut.sg. accusative</i> meat   |
|      | ‘Pierre eats raw meat.’                            |
| (ii) | Pierre isst Fleisch roh. ( <i>depictive</i> )      |
|      | Pierre eats meat raw                               |
|      | ‘Pierre eats meat raw.’                            |

It is not immediately clear to me how to account for the difference across languages with respect to the presence of agreement in secondary predication. I leave this issue for further research, and thank the reviewer for pointing out to me this issue.

<sup>3</sup> An alternative approach to the mechanism of agreement is pursued by Chomsky (2000). He proposes that the operation *Agree* establishes between the agreement bearing head and a triggering phrase that is in the local domain of the head, where ‘the local domain’ is defined in terms of ‘closest c-command’ (ibid.:122). Cormack & Smith’s (1999) model still remains problematic under this proposal. The target NP is not in the local domain of the agreement bearing head of the secondary predicate. Further, the ternary structure of Rothstein’s (2003, 2004) may not conflict with *Agree* since the matching NP is in the local domain of the secondary predicate, while this structure is not compatible with another minimalist assumption: syntactic structures are built by the binary operation, *Merge* (Chomsky 1995, 2000).



On this assumption, the configuration under the complex predicate analysis has difficulty to account for the agreement attested in Romance languages. In the structures proposed by Rothstein (2003, 2004) and Cormack & Smith (1999), the secondary predicate does not form a constituent with its subject, and therefore, the head of the secondary predicate phrase, corresponding to Y in (20) cannot enter into a Spec-head relation with its agreement target NP.<sup>4</sup>

Therefore, given the fact that the secondary predicate and its predication subject undergo agreement in languages like Romance, we may have to drop either one of the two assumptions — the one of the complex predicate analysis (8) or the independently motivated minimalist assumption (20).

In this section, I presented a number of problems faced with the two major assumptions, (7) and (8), adopted by the complex predicate analysis. Section 3 provides an alternative account of secondary predication to circumvent these shortcomings.

### 3. Proposal

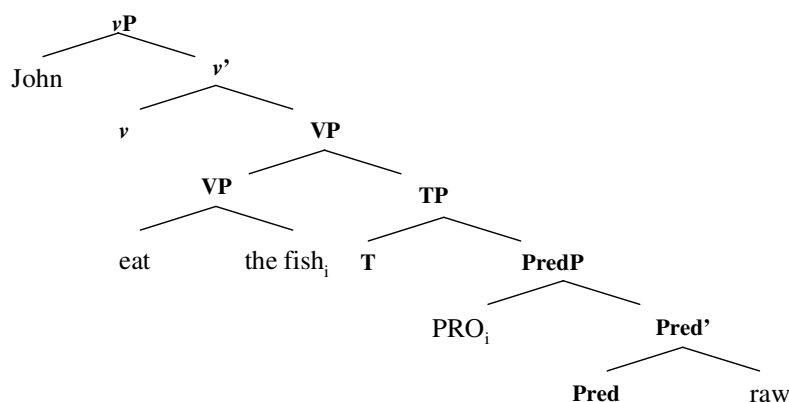
#### 3.1. The “distinct syntax” analysis

In consideration of the cross-linguistic data presented in the previous section, I propose to return to the conventional analysis, which posits two distinct structures for the two types of secondary predication: the “adjunct” analysis for depictive constructions on the one hand, and the “small clause” analysis for resultatives, on the other. My proposal of the syntactic structures of depictive and resultative constructions is given in (21) and (22), respectively.<sup>5</sup>

#### (21) *Depictive*

a. John ate *the fish raw*.

b.

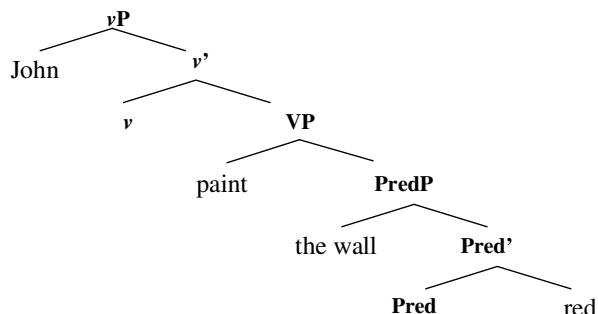


<sup>4</sup> In the present paper, I do not distinguish between NP and DP.

<sup>5</sup> I assume here that PRO is controlled by the local antecedent, where locality is defined based on the notion of “closest c-command” (Chomsky 2000).

(22) *Resultative*a. John painted *the wall red*.

b.



One point in this proposal that crucially differs from earlier theories is that the adjoined depictive constituent is TP.<sup>6</sup> Here, the depictive predicate phrase as in (21), which I assume to be a small clause of the type PredP (cf. Bowers 1993), is selected by a local tense head, and this TP is adjoined to the matrix VP. In the structure of resultatives, such a T-node is not present, and the PredP is directly selected by the matrix V, as is assumed by Hoekstra (1988, 1992) among others.

A major motivation for this account comes from the observation of the temporal properties of the two types of secondary predication. The subsequent subsection is dedicated to this topic.

### 3.2. *Temporal properties of secondary predication*

Recent works on secondary predication have revealed that depictive and resultative constructions represent two distinct aspectual structures (Rothstein 2003, 2004, Tenny 1994).

In a discussion on depictive constructions, Rothstein argues that these constructions contain the temporal dependency constraint between the matrix and the secondary events: the two events must go on simultaneously, their run times must coincide. As an example that clearly delineates this constraint, she cites example (23).

(23) John drove the car drunk for an hour.

(Rothstein 2004:68)

The sentence asserts that the events of John's driving the car and of his being drunk took place at the same time, and the two events lasted for an hour each. Thus, for this sentence to be true, the secondary event, namely John's being drunk, as well as the matrix event, must have a duration of the same one hour.

<sup>6</sup> See Ike-uchi (2003) for independent arguments on the TP status of depictives.

In contrast, a shared view on resultative constructions is that they are telic: that is, they describe events with a definite endpoint, and at this endpoint, the state denoted by a resultative predicate is attained (see Cormack & Smith 1999, Rothstein 2003, 2004, Wechsler 1997, 2005, and others). The sentence in (24), a run-of-the-mill example of this construction, entails that as the hammering event progresses, the metal becomes flatter and flatter, and finally it attains flatness, at which point the event ends.

(24) John hammered *the metal flat*.

In other words, the state described by a resultative predicate is associated with an incremental process, and thus its temporal property is unspecified at the starting point of the matrix event. This characteristic of resultatives is confirmed by, for example, the fact that *John is hammering the metal flat* does not entail that *John hammered the metal flat* (Wechsler 2005:258). These observations indicate that resultative constructions do not have the same aspectual properties as depictives. While the event denoted by a depictive predicate has its own run time, the resultative event does not have such a duration. A well-known test with adverbials, *for an hour* and *in an hour*, shows this difference in aspectual property between the two types of secondary predicates.

(25) He painted the car a brilliant red in an hour/\*for an hour.

(Tenny 1994:153)

The resultative sentence (25) is not compatible with an expression of duration, as opposed to the depictive example (23).<sup>7</sup> One way to syntactically represent this variation in temporal properties between the two types of secondary predication is to introduce a tense projection for depictives, and not for resultatives, so that only the former can indicate their temporal duration. This idea provides the basis of the presence of TP in depictive phrases as proposed in structure (21), which departs from the conventional adjunct analysis of depictives. Let us formulate this claim as (26).

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<sup>7</sup> The resultative sentence that occurs with a durative adverbial like the example (25) becomes more acceptable in a context where the accomplishment event denoted by the verb is not culminated, as in example (i) provided by a reviewer.

(i) He painted the car red for an hour and then decided to paint it blue.

Another instantiation of this effect is the contrast between the sentences (iia) and (iib), cited from Rothstein (2003). Only the latter, which contains an indefinite NP, yields an atelic reading of the event, thereby allowing the adverbial *for an hour*.

(ii) a. John sang 3 babies asleep \*for hours/in an hour last night.  
b. John sang babies asleep for hours/\*in an hour last night.

(ibid.:81)

Thanks to the reviewer for calling my attention to this point.

- (26) The event denoted by a depictive secondary predicate has its temporal duration, and this semantic property is syntactically represented by the T that heads a depictive phrase.

In this section, an alternative proposal of secondary predication to the complex predicate analysis has been presented. Based on this proposal, I will provide an explanation of the data from the various languages discussed earlier.

#### 4. Accounts for the cross-linguistic data

##### 4.1. English: *-ing* participle

In section 2, I presented four sets of data across languages that challenge the key assumptions adopted by the complex predicate analysis. These phenomena are captured in a principled way by my proposal. The account for each phenomenon is in order.

The first case that I discussed is the contrast in grammaticality between English depictive phrases and resultatives containing the verbal participle *-ing*. Examples are repeated as below.

- (27) *Depictive*

We found John at last, sleeping in the library. = (10b)

- (28) *Resultative*

\*I cooked it disgusting. = (11b)

The occurrence of the participle *-ing* with depictive sentences as in (27) is expected, with the general assumption that the suffix *-ing* is some functional head related to tense and the further assumption that the head of adjunct tense projections is realized as *-ing* when it selects a verbal projection in English.<sup>8</sup> The relevant structure of (27) under my proposal is shown in (29).

- (29) *Structure of (27)*

[<sub>VP</sub>[<sub>VP</sub> find John<sub>i</sub> at last][<sub>TP</sub>-ing [<sub>VP</sub> PRO<sub>i</sub> [<sub>VP</sub> sleep in the library]]]]

In contrast, the *-ing* participle does not occur with resultative predicates, since this type of secondary predicates lack a T-node. The ungrammaticality of sentences like (28) naturally follows from this account.

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<sup>8</sup> See Kratzer (2005) for a different account of the constraint for the *-ing* participle in resultatives.

4.2. Japanese: *de* and *ni*

Second, the proposal also provides an account for the complementary distribution of *de* and *ni*, the two particles that mark secondary predicates in Japanese. As shown in examples (30) - (31), depictive predicates are followed by *de*, while resultatives end with *ni*, and not vice versa.

(30) *Depictive*

John-ga sakana-o [ nama\*-ni /-de ] tabe-ta. = 0  
 John-NOM fish-ACC raw -NI/-DE eat-PAST  
 ‘John ate the fish raw.’

(31) *Resultative*

John-ga kabe-o [ makka-ni /\*-de ] nut-ta. = 0  
 John-NOM wall-ACC very red-NI/ -DE paint-PAST  
 ‘John painted the wall very red.’

This morphological variation between the two constructions is explained by my proposal coupled with the assumption given in (32).

(32) The *de* that follows a nominal predicate is the phonologically contracted form of two lexical items — *ni*, a Pred, and *te*, a T.

Historical evidence supports this assumption. In Old Japanese, more specifically, before the mid-Heian period (9-10C), the particle *de* was not used; instead, the sequence *ni-te* was used (cf. Konoshima 1966, Hashimoto 1969). (33) is an example cited from *the Tale of Genji*, written in the Heian period, illustrating the use of *ni-te* in a depictive sentence.

(33) utiki-sugata-ni-te tati tamahe-ru hito ari.  
 informal-dress-NI-TE stand POLITE-pres person be  
 ‘There was a lady standing in informal dress.’

(The Tale of Genji:Wakana)

This historical fact suggests that the particle *de* is lexically composed of *ni* and *te*. The data from Modern Japanese as in (34) confirms this view.

(34) a. Taroo-ga [ hadaka-de/?-ni-te ] deteit-ta.  
 Taro-NOM naked-DE/ -NI-TE leave-PAST  
 ‘Taro left naked.’

- b. Taroo-ga katuo-o [ mottomo oisii-zyootai-de/-ni-te] tabe-ta.  
 Taro-NOM bonito-ACC tastiest-condition-DE/-NI-TE eat-PAST  
 ‘Taro ate the bonito as its best.’

(adapted from Koizumi 1994:40)

The sentence in (34a) is not totally unacceptable when *de* is replaced by *ni-te*, although stylistically it is slightly awkward. The acceptability increases, though in a more formal context, in an example like (34b). These data indicate that the *de* contains *ni* and *te*. Then what are *ni* and *te*? We have seen that *ni* marks an ending of resultative nominal predicates. Following Nishiyama (1999), I assume that the *ni* is a Pred. Then, what is the other element that composes *de*, namely *te*? Nakatani (2003, 2004) argues that the *te* is a T head (see also Tsujimura 1993). *Te* appears, for example, in sentences like (35), functioning as an aspectual marker.

- (35) a. watasi-wa gohan-o tabe-te ki-masi-ta.  
 I-TOP meal-ACC eat-TE come-POLITE-PAST  
 ‘I have already had a meal (before I came here).’

(Nakatani 2004:18)

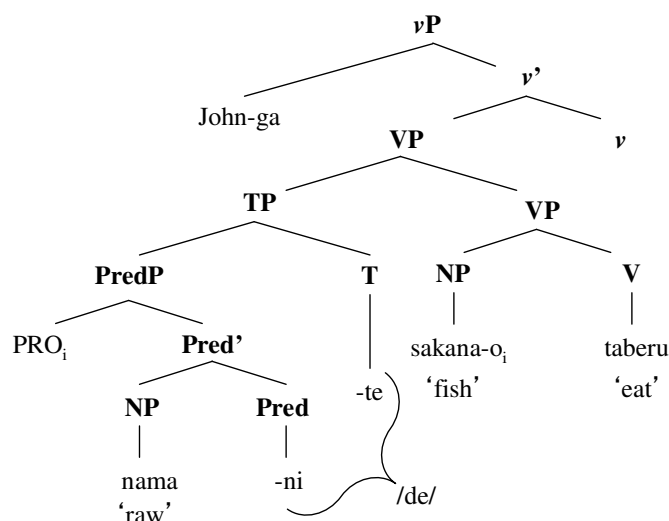
- b. tori-ga saezut-te i-ru.  
 bird-NOM chirp-TE exist-PRES  
 ‘The bird is chirping.’

(ibid.:96)

In (35a), the *te* refers to the perfective aspect of the event described by the VP ‘have a meal,’ inducing the sequential reading ‘I have eaten before I came.’ In (35b), the *te*, located between the verbs ‘chirp’ and ‘exist,’ denotes the progressive aspect of the event described by the first verb ‘chirp.’

The assumption (32) states that the two different endings of nominal secondary predicates, *de* and *ni*, differ minimally in one respect: the *de* contains *te*, while the *ni* does not. On this view, I could offer an account for the occurrence of the *de* in Japanese depictive sentences. The depictive sentence (30) is analyzed as in structure (36).

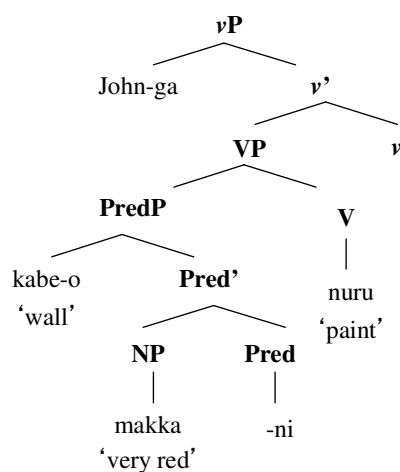
(36)



According to my claim (26), the temporal property of a depictive predicate of sentence (30) is syntactically represented by *te*, the T. Then, as shown in (36), the depictive phrase, a PredP headed by *ni*, is selected by the *te*, and we get the linear sequence *ni-te*, giving rise to the phonological realization of *de*.

Also, the occurrence of the particle *ni* as an ending of resultative predicates follows from the proposal. The relevant structure of example (31) is given below.

(37)



I argue that resultative predicates lack a temporal structure. Thus, the resultative phrase, PredP, appears as is, without the presence of a T. The sequence *ni-te* is therefore not realized in this structure, correctly excluding the occurrence of *de* as a resultative predicate marker.

In this subsection it is shown that the different morphological manifestation attested in Japanese secondary predicates is explained by my proposal: the two types of secondary predicates differ in the presence or the non-presence of a T-node.<sup>9</sup>

<sup>9</sup> One might argue that the different morphological manifestations, *de* in depictives and *ni* in resultatives, are

## 4.3. Japanese: quantifier float

The current analysis correctly explains the difference in behavior between Japanese depictive and resultative sentences with regard to quantifier float. The examples showing the crucial contrast between the two types of constructions are repeated as (38) and (39).

(38) *Depictive*

John-ga kuruma-o sinpin-de ni-dai kat-ta. = (16)

John-NOM book-ACC new-DE two-CL buy-PAST

‘John bought two cars new.’

(39) *Resultative*

\*John-ga kuruma-o makka-ni ni-dai nut-ta. = (17)

John-NOM car-ACC very red-NI two-CL paint-PAST

‘John painted two cars very red.’

The observed fact is that when the numeral quantifier (NQ) and its associated NP are not adjacent to each other, the resultative sentence in (39) becomes degraded, while the depictive (38) remains acceptable. The relevant structures assigned to the two sentences are (40) and (41), respectively.

(40) [<sub>VP</sub> John-ga [<sub>VP</sub> kuruma-o<sub>i</sub> [<sub>TP</sub> [<sub>PredP</sub> PRO<sub>i</sub> sinpin-ni]-te] [<sub>VP</sub> [<sub>QP</sub> e<sub>i</sub> ni-dai ] kau]]]  
 John-NOM car-ACC new-NI-TE two-CL buy

(41) [<sub>VP</sub> John-ga [<sub>VP</sub> [<sub>PredP</sub> kuruma-o makka-ni ] ni-dai nuru]]  
 John-NOM car-ACC very red-NI two-CL paint

In (40), the NQ, *ni-dai* ‘2-CL,’ and its associate, *kuruma-o* ‘car-ACC,’ are base-generated in the lower VP, forming a constituent, Quantifier Phrase. Then, *kuruma-o* is scrambled out of this QP across the TP adjunct, *sinpin-ni-te* ‘new,’ to the VP initial position, leaving its trace behind.<sup>10</sup> It is widely held that an NQ and its associated NP (or its trace) must mutually c-command each other (Miyagawa 1989). In structure (40), the mutual c-command requirement between the NQ and the NP is satisfied, thereby offering an acceptable configuration. By contrast, in structure (41), the NQ, *ni-dai*, and its associate, *kuruma-o*, do not

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the realizations of two distinct heads, Dep proposed in Pytkäinen (2002) and Cause proposed in Kratzer (2005), respectively (thanks to a ConSOLE XVII reviewer for pointing this out to me). However, this account cannot provide a unified analysis of the *de* and *ni* that occur in contexts other than secondary predication, such as the *ni* that appears in the small clause complement of epistemic verbs like *omou* ‘consider’ (see the data (52-54)). In contrast, my proposal properly captures the distribution of the particles in these constructions.

<sup>10</sup> The base position of the accusative NP in structure (40) is indicated by *e*. I leave open the question whether or not scrambling in Japanese involves movement, which does not affect the argument of the present work.



form a constituent. Rather, the accusative NP forms a constituent with the resultative predicate, *aka-ni* ‘red,’ and the whole constituent forms a PredP. In this structure, the mutual c-command condition between the NQ and its associate is not met, correctly analyzing the resultative sentence (39) as ungrammatical.

#### 4.4. Romance languages: agreement

Finally, the problem with the agreement phenomenon observed in Romance languages disappears under my proposal. The proposed structures of depictives and resultatives offer a configuration for the agreement to be established between the secondary predicate and its predication subject, complying with the minimalist assumption (20). To illustrate, a French depictive example is repeated as (42a), and its relevant structure is shown in (42b).

- (42) a. Pierre mange la viande crue. = (18)  
       ‘Pierre eats the meat raw.’  
       b. [<sub>VP</sub> Pierre [<sub>VP</sub> [<sub>VP</sub> mange la viande<sub>i</sub>] [<sub>TP</sub> [<sub>PredP</sub> PRO<sub>i</sub> [<sub>Pred'</sub> Pred crue]]]]]

In (42b), the direct object, *la viande* ‘the meat,’ transmits its gender and number features to the PRO in the Spec of PredP, which, in turn, agrees with the functional head, Pred.

#### 4.5. Korean

In this subsection, I take up the analysis of Korean resultatives proposed by Shim & Den Dikken (2007), which appears to be problematic to my proposal.<sup>11</sup>

On the basis of a variety of empirical arguments, the authors claim that resultative secondary predicates in Korean are TP adjuncts. One of their arguments deals with (43).

- (43) a. Jim-i        mok-i/\*-ul        swi-key        wul-ess-ta.  
       Jim-NOM throat-NOM/-ACC become.hoarse-KEY cry-PAST-Dcl  
       ‘Jim cried his throat hoarse.’  
       b. Jim-i        (pap-ul)    pay-ka/\*-lul        theci-key        mek-ess-ta.  
       Jim-NOM rice-ACC belly-NOM/-ACC explode-KEY eat-PAST-Dcl  
       ‘Jim ate his belly full.’

(Shim & Den Dikken 2007:5)

<sup>11</sup> I thank one of the anonymous reviewers of ConSOLE XVII, whose comments helped me in the development of this subsection.

In these sentences, the subject of the secondary predicates, ‘throat/belly,’ is nominative and not accusative. According to Shim & Den Dikken, this Case marking pattern indicates that Korean resultative predicates are associated with a local tense, on their assumption that the distribution of nominative Case in Korean is closely connected to that of tense.

Shim & Den Dikken’s claim that resultative secondary predicates in Korean are TP adjuncts seems to conflict with my argument that resultative secondary predicates are not associated with a tense.

More recently, however, Son (2008) proposes that not all resultative secondary predicates in Korean involve TP adjuncts. She observes that the “TP adjunct” data presented by Shim & Den Dikken (2007) are *eventive*-resultatives and Korean has another type of resultatives, namely *stative*-resultatives, which do not syntactically behave in the same way as the *eventive* ones. Son provides a wide range of data showing the syntactic differences between the two types of resultatives, and argues that *eventive* resultatives are adjoined to VP, in accordance with Shim & Den Dikken, while *statives* are small-clause complements of VP. One distinction between the two varieties of resultatives is the different type of Case assigned to the subject of resultative phrases. Observe Son’s examples of *stative* resultatives in (44).

(44) *Stative-resultative*

- a. Chelswu-ka chaysang-ul/\*i kkaykkusha-key takk-ass-ta.  
 Chelswu-NOM desk-ACC/\*NOM clean-KEY wipe-PAST-Dcl  
 ‘Chelswu wiped the table clean.’
- b. Inho-ka chelpan-ul/\*i napcakha-key twutulki-ess-ta.  
 Inho-NOM iron.plate-ACC/\*NOM flat-KEY pound-PAST-Dcl  
 ‘Inho pounded the iron plate flat.’

(Son 2008)

The subject of the secondary predicates in these sentences is marked accusative and not nominative, displaying the opposite Case-marking pattern to that of Shim & Den Dikken’s examples in (43).

Interestingly, she also observes the difference in temporal properties between the two varieties of resultatives. Examples (45a-b) illustrate this contrast.

(45) a. *Stative-resultative*

- Inho-ka o-pwun<sup>??</sup>-tongun/-maney chelpan-ul  
 Inho-NOM five-minute-for/-in iron.plate-ACC  
 napcakha-key twutulki-ess-ta  
 flat-KEY pound-PAST-Dcl  
 ‘Inho pounded the iron plate flat for five minutes/in five minutes.’

b. *Eventive-resultative*

Inho-ka o-pwun-tongun/?-maney sinpal-i  
 Inho-NOM five-minute-for/-in shoes-NOM  
 talh-key ttwi-ess-ta.  
 wear.threadbare run-PAST-Dcl  
 ‘Inho ran his shoes threadbare for five minutes/in five minutes.’

On the one hand, the *stative* resultative sentence (45a) is not perfectly compatible with the adverbial of duration ‘for five minutes,’ while it is allowed to occur with a telic adverbial ‘in five minutes.’ On the other hand, the *eventive* resultative sentence in (45b) does not conflict with expression ‘for five minutes,’ whereas the oddity arises with ‘in five minutes.’ Son further compares this set of data to the English counterparts shown in (46).

- (46) a. John pounded the iron plate flat \*for five minutes/in five minutes.  
 b. John ran his shoes threadbare \*for five minutes/in five minutes.

The English resultatives (46), both *stative* (a) and *eventive* (b), pattern with the Korean *stative* resultative in (45a), and not with the *eventive* example (45b).

Therefore, if Son’s observation is correct, the *eventive* resultatives in Korean as in (43) and (45b) differ in their temporal property from that of the *stative* resultatives in Korean as in (44) and (45a) and of English resultatives like (46): the Korean *eventive* resultatives can occur with an expression of temporal duration. In this respect, this type of secondary predicate behaves like a standard depictive predicate (see section 3.2). Then, following Son’s analysis, the data that serve as the basis of the TP adjunct analysis of Shim & Den Dikken, are the *eventive* resultatives, and hence, they are not counterexamples to my claim that resultative secondary predicates lack a temporal structure. I thus conclude that the argument presented by Shim & Den Dikken (2007) does not present an immediate challenge to my proposal.<sup>12</sup>

In this section, I provided an account for the four sets of cross-linguistic data discussed in section 2, under the distinct syntax approach to secondary predication — the “adjunct” analysis for depictive constructions and the “small clause” analysis for resultatives — coupled with the

<sup>12</sup> Furthermore, M.-J. Kim (2002) and Y.-T. Kim (2007) observe that the suffix *-key*, which attaches to resultative predicates, also provides a depictive reading, as shown in example (i).

(i) John-un encena pap-ul ttukep-key mek-nun-ta.  
 John-TOP always rice-ACC hot-KEY eat-PRES-Dcl  
 ‘John always eats rice hot.’

(M.-J. Kim 2002:15)

Along with the data on *key*-marked *eventive* resultatives provided by Son (2008), this fact shows that some of the *key*-marked secondary predicates in Korean do not behave like English-type resultative predicates, but rather pattern like depictives. This confirms the description that not all Korean resultatives pattern with standard resultative sentences, suggesting that the syntactic status of the *key*-marked secondary predicates in Korean calls for a fuller explanation.

novel argument that the adjoined depictive phrases are TP. The observed phenomena across languages which are problematic under the complex predicate analysis receive an adequate explanation on my account.

Another advantage of the present analysis is that the syntax-semantics mapping is maintained as one to one. This is not the case with the complex predicate analysis proposed by Rothstein (2003, 2004) nor the one proposed by Cormack & Smith (1999). On their assumption, the semantic difference between the object-oriented depictive and resultative constructions is not reflected in their syntactic structures. Thus, the complex predicate analysis requires a more complex semantic calculation to distinguish the two interpretations of object-oriented depictive and resultative sentences. Under the traditional approach, such complexity in semantics could be avoided.

## 5. Predictions

### 5.1. Negation

The analysis presented in this paper makes a correct prediction on the following two contexts. The first one is negation of secondary predication in English. I argue that depictive secondary predicate phrases are TP. Then, if we follow Zanuttini (1996) and assume that clausal negation requires TP, we predict that the T which heads a depictive phrase can license negation. This prediction is borne out by examples (47) - (48).

#### (47) *Depictives*

- a. ?John left the room not drunk.
- b. John left the meeting not satisfied with the decision.
- c. John met Mary not angry at herself.
- d. ??John ate the meat not rare.

(Ike-uchi 2003:156) [judgments: original]

#### (48) *Depictives*

- a. He arrived not proud of what he had done.
- b. Not happy with the results, they abandoned the experiment.

(Fabb 1984:108)

Moreover, Guéron & Hoekstra (1995) observe that the adjuncts with *-ing* participle can license negation, as shown in (49).

- (49) a. Not knowing the answer, John felt at a loss.
- b. This student, not having written any paper, should be flunked.

(ibid.:95)

On the assumption that these sentential adjuncts are subject-oriented depictives, the fact that they allow negation is naturally expected.<sup>13</sup>

By contrast, we expect that resultative predicates cannot feature negation, since, as I argue, this type of secondary predicate phrases do not include the local T which can license negation. This is indeed the case, as shown in examples (50).

(50) *Resultatives*

- a. \*John painted the wall not red.
- b. \*John hammered the metal not flat.
- c. \*John ran his shoes not threadbare.
- d. \*John laughed Mary not silly.

While some of these examples may be interpreted as involving contrastive negation, (e.g., *John painted the wall not red but blue*), it is not possible to analyze them as featuring a clausal negation of resultative secondary predication. This is not the case with depictive sentences as in (47) - (49).

## 5.2. Small clauses in Japanese

The other expected phenomenon under my analysis is the distribution of the particles *ni* and *de* that attaches to nominal predicates in Japanese small clause constructions. In my account for the occurrence of *ni* and *de* in Japanese secondary predicates (section 4.2), I crucially adopted assumption (32), repeated here.

(51) The *de* that follows a nominal predicate is the phonologically contracted form of two lexical items — *ni*, a Pred, and *te*, a T. = (32)

Now, on the general assumption that small clauses lack tense (cf. Hoekstra 1988, Stowell 1991), it is expected that the nominal predicate that appears in the small clause complement of verbs like *consider* is marked by *ni*, and not *de*, since *de* contains a T. This is what we observe in examples (52).

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<sup>13</sup> While I assume that the present participle adjuncts are subject-oriented depictives like the one in example (i), the adjunction site of the depictive phrase as in (i) may not be the same as that of the depictive predicates as in (ii).  
 (i) *Angry at John*, *Bill* didn't leave the room. Depictive > Neg only (Roberts 1988:707)  
 (ii) *Bill* didn't leave the room *angry at John*. Neg > depictive only (ibid.)  
 The scope facts with regard to negation indicate that the two types of depictives are to be distinguished syntactically (see Ike-uchi 2003 for discussion on this issue). I am grateful to a reviewer for noting this contrast.

(52) *Consider-type verbs*

- a. John-ga Mary-o totemo miryokuteki-ni/\*-de omot-ta.  
 John-NOM Mary-ACC very attractive-NI/-DEconsider-PAST  
 ‘John considered Mary very attractive.’

(Takezawa 1993:53; note 18)

- b. John-ga Mary-o totemo miryokuteki-ni/\*-de kanzi-ta.  
 John-NOM Mary-ACC very attractive-NI/-DEfeel-PAST  
 ‘John found Mary very attractive.’

Inchoative and causative constructions further provide data that confirm our prediction. It is commonly held that inchoative and causative verbs take a small clause complement. Observe Japanese examples in (53) and (54).

(53) *Inchoative*

- John-ga sensei-ni/\*-de nat-ta.  
 John-NOM teacher-NI/-DE become-PAST  
 ‘John became a teacher.’

(54) *Causative*

- John-ga Mary-o siawase-ni/\*-de si-ta.  
 John-NOM Mary-ACC happy-NI/-DE make-PAST  
 ‘John made Mary happy.’

The nominal predicate in these examples is marked by *ni* and not *de*, again as correctly predicted by assumption (51).

In this section, I discussed two phenomena, negation of secondary predication in English and small clauses in Japanese. In both cases, the analysis provided in this paper makes a correct prediction.

## 6. Conclusion

In this paper, I presented an empirical argument based on evidence that comes from different languages against the complex predicate analysis of the two types of secondary predication — depictive and resultative — put forth by Cormack & Smith (1999) and Rothstein (2003, 2004). It is shown that the “two distinct syntax” approach to secondary predication, coupled with the novel argument that depictive secondary predicates are TP adjuncts, works better than the

complex predicate analysis, in that the former, not the latter, captures in a principled way the presented cross-linguistic data.

While I fully acknowledge that most of the arguments in favor of the complex predicate analysis in the literature are left unaddressed in this paper, I hope to have shown that there are some shortcomings to the complex predicate approach and that the traditional “distinct syntax” analysis remains valid in some respects.

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# Long-distance relativization in varieties of Dutch

Eefje Boef

Dutch dialects show a wealth of variation in the distribution and the status of the elements *die* and *dat* in the left periphery of (long-distance) restrictive relative clauses. This paper presents a clear description of the (limits of) variation within long-distance relatives, and it will furthermore focus in detail on two systems of relativization that make use of a subject relative that is different from the object relative (subject/object asymmetries). The proposed analysis of these data is primarily based on the observation that there is a significant positive correlation between subject/object asymmetries in relative clauses and complementizer agreement.

## 1. Introduction

This paper investigates the syntactic microvariation in long-distance relativization structures in varieties of Dutch. The empirical basis for this study is formed by the SAND data on relativization, which show a considerable amount of variation regarding long-distance restrictive relative clauses in the Dutch speaking language area.<sup>1</sup> More specifically, they show variation along the following four dimensions: (i) the form of the element that introduces the relative clause (*die/dat*), (ii) the form of the element that introduces the most deeply embedded clause (*die/dat*), (iii) the presence/absence of a complementizer in addition to the relative pronoun (i.e. doubly filled COMP), and (iv) the presence/absence of an overt subject/object (*resumptive pronoun*) at the extraction site. The aim of this paper is twofold. First, I give a detailed overview of the data in order to get some insights into (the limits) of variation, and second, I focus on six systems of relativization that differ with respect to the distribution and status of the elements *die* and *dat* (i.e. dimensions (i) and (ii) above). Of these six systems of relativization, I am primarily concerned with two systems that show a *subject/object asymmetry*, that is, systems in which the pattern that is used for subject extraction is different from the pattern that is used with object extraction.

The paper is organized as follows. Section 2 gives some background information on the properties of restrictive (long-distance) relative clauses in Standard Dutch, and section 3 presents the SAND data on relativization that form the empirical basis for this paper, namely

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<sup>1</sup> SAND = *Syntactic Atlas of the Dutch Dialects* (cf. Barbiers et al. 2005, 2006). For more information on the SAND project, see [www.meertens.knaw.nl/projecten/sand](http://www.meertens.knaw.nl/projecten/sand).

six systems that differ with respect to the distribution and status of elements *die* and *dat*. Section 4 provides an analysis of two patterns of relativization that show a subject/object asymmetry. I show that these two systems can be derived by two micro-parameters: the spell-out/non-spell-out of an agreement relation between the most deeply embedded  $C^0$  and (the copy of) the subject relative item in its specifier position (micro-parameter 1), and the presence/absence of relative pronouns (micro-parameter 2). These two parameters predict the existence of two other systems of relativization (out of the six systems we started out with). Section 5 deals with patterns of relativization in which no subject/object asymmetries are displayed, and briefly touches upon the issue of the correct analysis of restrictive relative clauses. Section 6 summarizes the main points and concludes the paper.

## 2. Background: relative clauses in Standard Dutch

A restrictive relative clause is a finite subordinate clause that modifies a noun – the *head* of the relative clause. Relative clauses in Standard Dutch are head-initial, they always need to be introduced by a relative pronoun, and the relativized item leaves a gap at the extraction site (not a resumptive pronoun).<sup>2</sup> Thus, Dutch restrictive relative clauses obey the format in (1).<sup>3</sup>

- (1) [MATRIX CLAUSE ... head [RELATIVE CLAUSE \*(relative pronoun)<sub>i</sub> ... t<sub>i</sub> ... ]]

With respect to long-distance relative clauses I will assume that the relative pronoun successive-cyclically moves to the relative CP, as indicated in (2).<sup>4,5</sup> As (2) shows, in Standard Dutch long-distance relative clauses, the relative clause itself is introduced by the relative pronoun and the lower finite embedded clause is introduced by the declarative complementizer *dat*.

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<sup>2</sup> This paper restricts its attention to Dutch headed restrictive relative clauses that require *die* in Standard Dutch: in all (test) sentences the antecedent is 3rd person non-neuter (neuter singular antecedents require *dat*, which is identical in form to the Dutch finite complementizer).

<sup>3</sup> The most prominent analyses of relative clauses in the current literature – the *head raising analysis* (cf. Vergnaud 1974, Kayne 1994, Bianchi 1999, 2000, and De Vries 2002 amongst others) and the *matching analysis* (cf. Citko 2001, Bhatt 2002, Sauerland 2003, Salzmann 2006a amongst others) – assume that relative clauses are derived by A'-movement of the head noun together with the relative pronoun, i.e. there is a relative clause-internal representation of the external head. For now, as nothing hinges on this, I will simply assume that only the relative pronoun undergoes A'-movement in the relative clause (cf. the traditional *head external analysis* of relative clauses; cf. Chomsky 1977, Borsley 1997). In paragraph 5 I will come back to the issue of what constitutes the most adequate analysis of relative clauses.

<sup>4</sup> I will simply assume there is successive-cyclic A'-movement (cf. Felser 2004 for an overview of the arguments in favor of successive-cyclicality), but I will leave the issue of what triggers the intermediate movement step of the relativized item to a non-relative C-domain for future research.

<sup>5</sup> An alternative to the successive cyclic *wh*-movement analysis of long-distance relativization has recently been proposed by Koopman & Sportiche (2008). Although this theory seems to adequately account for the French *que/qui* alternation, as it stands, it cannot straightforwardly be extended to Dutch. That is, it is unclear whether Koopman & Sportiche's theory can account for all the Dutch data, because it is primarily based on French relative clauses and pseudo relative small clauses (PRSC), but Dutch doesn't have PRSCs (of the same type) as in French. Moreover, Koopman & Sportiche's account of special *qui/die* contexts seems to make some wrong predictions with respect to the possible patterns of long-distance relativization in Dutch, e.g. it predicts one of the patterns in footnote 25 to be impossible, contrary to fact (although this pattern is only marginally attested and should be further investigated). For reasons of space I cannot go into the details of their analysis or the exact predictions it makes for Dutch, but see Boef (2008) for a more elaborated evaluation of their proposal.

- (2) ... head [<sub>RELATIVE CLAUSE</sub> \*(relative pronoun)<sub>i</sub>] ... [t<sub>i</sub> \*(complementizer) ... t<sub>i</sub> ... ]]

Variation with respect to long-distance relative clauses in Dutch predominantly concerns the form of the element that introduces the relative clause (*die* or *dat*) and the element that introduces the lower clause (*die* or *dat*). The current standard assumption is that in the left periphery of relative clauses there is one position for the relative pronoun (Spec,CP), and one position for the complementizer (C<sup>0</sup>). Languages differ with respect to which of the two positions is filled. In contrast to languages like English in which *zero-relativization* is allowed, in Standard Dutch (and all its dialects (cf. *infra* section 3)) at least one element needs to be overtly present in the C-domain (cf. Dekkers 1999).

- (3) a. Dat is de man [ *die dat* het verhaal heeft verteld].  
 that is the man who that the story has told  
 ‘That is the man who told the story.’  
 [Lier Dutch]
- b. Dat is de man [*dat* het verhaal heeft verteld].  
 [Brussel Dutch]
- c. Dat is de man [*die* het verhaal heeft verteld].  
 [Standard Dutch]
- d. \*Dat is de man [ \_\_\_ het verhaal heeft verteld].  
*zero-relativization*

### 3. Long-distance relative clauses in Dutch - the SAND data

The variation that is found in the following two Standard Dutch sentences forms the empirical basis for this paper. Example (4a) shows a long subject relative, and example (4b) shows a long object relative. In the SAND interviews both sentences were presented to the informants as a translation task.

- (4) a. Dat is de man [ **die** ik denk [ **dat** het verhaal heeft verteld].  
 that is the man *die* I think that the story had told  
 ‘That is the man who I think told the story.’
- b. Dat is de man [ **die** ik denk [ **dat** ze geroepen hebben].  
 that is the man *die* I think that they called have  
 ‘That is the man who I think they have called.’

The attested variation with respect to the sentences in (4) is given in (5) for long subject extraction and in (6) for long object extraction. It should be noted that I have abstracted away from any phonological variation, e.g. forms like *dee*, *dei* or *der* are taken to be form variants of *die*. The total amount of dialects for which there are reliable data is 218 for long subject relatives and 216 for long object relatives; note that in a given location more than one variant can be attested.

- (5) a. ... de man **die** ik denk **dat** Ø het verhaal heeft verteld. (63)  
 b. ... de man **die** ik denk **dat hij/die** het verhaal heeft verteld. (24)  
 c. ... de man **dat** ik denk **die** Ø het verhaal heeft verteld. (36)  
 d. ... de man **die** ik denk **die** Ø het verhaal heeft verteld. (32)

- e. ... de man **dat** ik denk **dat** Ø het verhaal heeft verteld. (20)
- f. ... de man **dat** ik denk **dat** **hij/die** het verhaal heeft verteld. (10)
- g. ... de man **waarvan** ik denk **dat** **hij/die** het verhaal heeft verteld. (69)  
the man whereof I think that he/die the story has told
- h. ... de man **waarvan** ik denk **dat** Ø het verhaal heeft verteld. (10)  
the man whereof I think that the story has told
- (6) a. ... de man **die** ik denk **dat** ze Ø geroepen hebben. (99)
- b. ... de man **die** ik denk **dat** ze **die/'m** geroepen hebben. (9)
- c. ... de man **dat** ik denk **dat** ze Ø geroepen hebben. (50)
- d. ... de man **die** ik denk **die** ze Ø geroepen hebben. (24)
- e. ... de man **dat** ik denk **die** ze Ø geroepen hebben. (11)
- f. ... de man **dat** ik denk **dat** ze **die/'m** geroepen hebben. (5)
- g. ... de man **waarvan** ik denk **dat** ze **die/'m** geroepen hebben. (48)  
the man whereof I think that they *die/him* called have
- h. ... de man **waarvan** ik denk **dat** ze Ø geroepen hebben. (8)  
the man whereof I think that they called have

Abstracting away from the so-called PP-relatives in (5g,h) and (6g,h) these data show that long-distance relative clauses vary along the following four dimensions.

- (7) a. the form of the element that introduces the relative clause (*die* or *dat*)
- b. the form of the element that introduces the most deeply embedded clause (*die* or *dat*)
- c. the presence/absence of an overt subject/object in the most deeply embedded clause (i.e. the presence/absence of a *resumptive pronoun*)
- d. the presence/absence of a complementizer in addition to the relative pronoun (*doubly filled COMP*)

For expository reasons I have not included the latter parameter in the examples in (5) and (6). The sentences in (8) illustrate that in addition to the relative pronoun *die*, a complementizer can be present in the higher clause (8a), as well as in the lower clause (8b).

- (8) a. Da s de man **dieë** **dat** denk dassé geroepen emme.  
that is the man *die* that-I think that-they called have  
'That is the man who I think they have called.'  
[Zandhoven Dutch]
- b. Da s de vent **die** **dat** denk **die** **asse** geroepen emme.  
that is the man *die* I think *die* that-they called have  
[Nieuwmoer Dutch]

As mentioned before, no variety of Dutch allows zero-relativization: there always needs to be (at least) one element (*die* or *dat*) overtly present in the C-domain.

In this paper I will abstract away from PP-relatives and resumption,<sup>6</sup> and I will only be concerned with long-distance relative clauses that show variation with respect to the dimensions of variation in (7a) and (7b). The focus will thus be on variation in the distribution and the status of the elements *die* and *dat*. With this restriction of the field of research, we are

<sup>6</sup> For a recent, detailed description and analysis of PP-relatives in German, Dutch and Zurich German, see Salzmann (2006a).

left with the four subject relatives in (9) and the four object relatives in (10). With these relative clauses we predict  $4^2 (=16)$  logically possible patterns of long-distance relativization. However, only the six patterns (henceforth *systems*) in table 1 are attested. This table shows for each of the six systems of long relativization the corresponding system of short relativization.<sup>7</sup>

- (9) a. Dat is de man **die** ik denk **dat** het verhaal verteld heeft.  
 b. Dat is de man **die** ik denk **die** het verhaal verteld heeft.  
 c. Dat is de man **dat** ik denk **die** het verhaal verteld heeft.  
 d. Dat is de man **dat** ik denk **dat** het verhaal verteld heeft.
- (10) a. Dat is de man **die** ik denk **dat** ze geroepen hebben.  
 b. Dat is de man **die** ik denk **die** ze geroepen hebben.  
 c. Dat is de man **dat** ik denk **die** ze geroepen hebben.  
 d. Dat is de man **dat** ik denk **dat** ze geroepen hebben.

	<i>short subject</i>	<i>short object</i>	<i>long subject</i>	<i>long object</i>
system I (46/47)	die	die	die-dat	die-dat
system II (20)	die	dat	dat-die	dat-dat
system III (19)	die	die	die-die	die-dat
system IV (10/16)	dat	dat	dat-dat	dat-dat
system V (9)	die	die	die-die	die-die
system VI (7)	die	die	dat-die	dat-die

Table 1: six systems of (long-distance) relativization

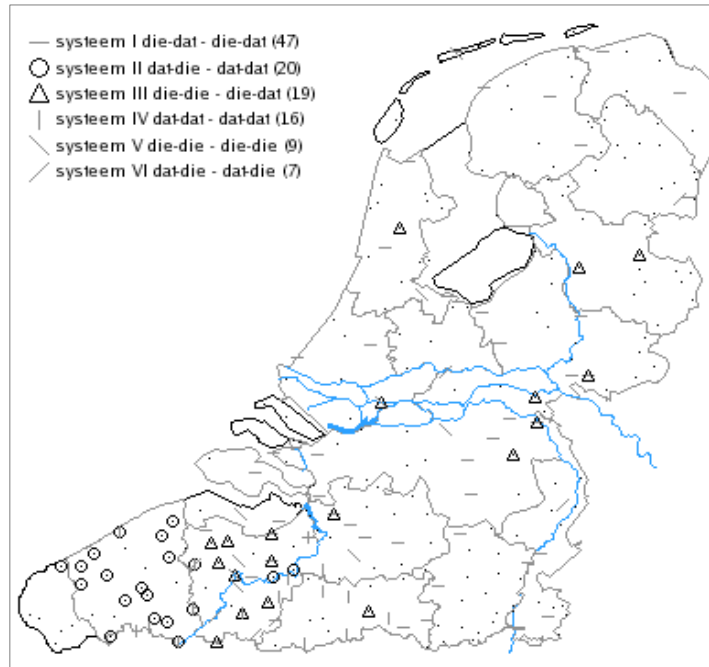
On the basis of the observation that of all logically possible combinations of subject and object relatives only the six patterns in table 1 are attested, the following descriptive generalizations are formulated.

(11) *Generalizations*

- A** long-distance relativization without a subject/object asymmetry shows all logically possible variants: *die-dat* - *die-dat* (I), *dat-die* - *dat-die* (VI), *die-die* - *die-die* (V), *dat-dat* - *dat-dat* (IV)
- B** a subject/object asymmetry can only appear in the CP containing the extraction site (i.e. a subject/object asymmetry in the higher clause is (almost) never attested)
- C** in case of a subject/object asymmetry (in the most deeply embedded clause), *die* occurs with subject extraction, whereas *dat* occurs with object extraction

The main goal of this paper is to provide an analysis of the six attested systems of (long-distance) relativization, and to give an explanation for the three generalizations in (11).

<sup>7</sup> The numbers in the leftmost column of table 1 indicate the amount of locations in which a given system is found. When there are two numbers, the last number indicates the amount of locations in which the system of long relativization is attested, and the first number indicates for how many systems of these long relativization systems the given short relativization pattern is attested. Thus, sometimes (system IV) not all dialects that show the given pattern for long relativization also show the given pattern for short relativization.



Map 1: geographic distribution of six systems of long relativization

#### 4. Subject/object asymmetries

##### 4.1. The data and a fourth generalization

Of the six systems of (long-distance) relativization are two systems that show a subject/object asymmetry (systems II and III), i.e. in these systems the pattern for subject relativization is different from the pattern for object relativization. In long-distance relativization in system II, as illustrated in (12), the relative clause is introduced by the element *dat*<sup>8</sup>, and the embedded finite clause is introduced by *die* in case of subject extraction and by *dat* in case of object extraction.<sup>9</sup> Moreover, the asymmetry is also attested with short relativization. System II is attested in 20 locations and it is found almost exclusively in West-Flanders, as can be seen on map 1. In system III, as illustrated in (13), the higher clause in long-distance relativization is introduced by *die* and the lower clause is introduced by *die* in case of subject extraction and by *dat* in case of object extraction. This asymmetry is, however, not attested with short relativization, in which case *die* introduces both subject and object relative clauses.<sup>10</sup> System

<sup>8</sup> In the dialects that make use of system II and III, the complementizer can have the form *dat* or *da*. In the main text I will simply use *dat* for both form variants.

<sup>9</sup> Notice that the West-Flemish data (12) are reminiscent of the well-known *que/qui* alternation in French (in long-distance relativization), i.e. *die* seems to have the same distribution as *qui*: it appears only in the left periphery of clauses from which the subject is extracted; cf. Bennis & Haegeman (1984:36):

- (i) a. ... l'homme que tu crois \*que/qui t viendra    *subject extraction*  
           the man that you think    that/who t will come  
       b. ... l'homme que tu crois que/\*qui j'aime t    *object extraction*  
           the man that you think    that/who I love t

<sup>10</sup> Actually, the data for system III are less clear than illustrated here. Of the 19 dialects that make use of system III, all 19 dialects make use of *die* in short subject and object relatives. But of these 19 dialects there are also 10 dialects that, in addition to *die*, can also use *dat* in short object relatives (and sometimes there are even more alternatives possible for both object and subject relatives). However, in what follows, I will assume that the

III is attested 19 times and its geographic distribution is somewhat less clear than that of system II; it is attested in the main part of East-Flanders and several times in the Netherlands.

- (12) a. Da s de vent **da** k peizen **die** da graptje verteld eet. *subject*  
 that is the man that I think *die* that joke told has  
 ‘That is the man who I think told that joke.’  
 b. Da s de vent **da** k peizen **da-n** ze geroepen en. *object*  
 that is the man that I think that.<sub>3P.PL</sub> they called have  
 ‘That is the man who I think they have called.’

[Brugge Dutch]

- (13) a. Da s de man **die** k peize **die** t verhaal verteld ee. *subject*  
 that is the man *die* I think *die* the story told has  
 ‘That is the man who I think told the story.’  
 b. Da s de man **die** k peize **da-n** ze geroepen en. *object*  
 that is the man *die* I think that.<sub>3P.PL</sub> they called have  
 ‘That is the man who I think they have called.’

[Gent Dutch]

In the remainder of this paper I will refer to the element *die* that is found in the most deeply embedded clause of (long) subject relatives in dialects that show the subject/object asymmetry with ‘special *die*’ (cf. Koopman & Sportiche 2008).<sup>11</sup>

The variable under study, i.e. the presence of a subject/object asymmetry in the CP that contains the extraction site, covaries with another variable in Dutch, namely the presence of *complementizer agreement* (cf. also Bennis & Haegeman 1984). That is to say, the geographic distribution of systems that show a subject/object asymmetry (systems II and III) shows a significant positive correlation with the geographic distribution of *complementizer agreement*.<sup>12</sup> Complementizer agreement is a well-known phenomenon in West-Germanic languages by which the complementizer overtly agrees with the subject of the clause it introduces; examples of this can be found in (12b) and (13b). Map 2 shows the geographic distribution of complementizer agreement; more specifically, the map shows for each dialect which members of the paradigm show complementizer agreement.

Comparing this map with map 1, we see that the geographic distribution of dialects that show a subject/object asymmetry in long-distance relative clauses correlates with the geographic distribution of complementizer agreement. Therefore, the following generalization is put forward.<sup>13</sup>

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pattern for system III as given in table 1 is in fact the core pattern, that is, a system that shows a subject/object asymmetry with long-distance relativization, but no asymmetry with short relativization. The correctness of this assumption is corroborated by the observation that of the few dialects that only make use of system III, the majority shows the pattern in table 1 (based on the data from the oral interviews).

<sup>11</sup> Note however that whereas I claim that special *die* is only found with subject extraction, Koopman & Sportiche (2008) assume that in some varieties of Dutch special *die* can also be found with object extraction.

<sup>12</sup> The two variables, (i) +/- subject/object asymmetry, and (ii) +/- complementizer agreement (as scale variable), show a positive correlation ( $r=0.363$  with  $n=203$ ). This correlation is significant at the  $p<0.01$  level (one-sided;  $p=0.00$ ).

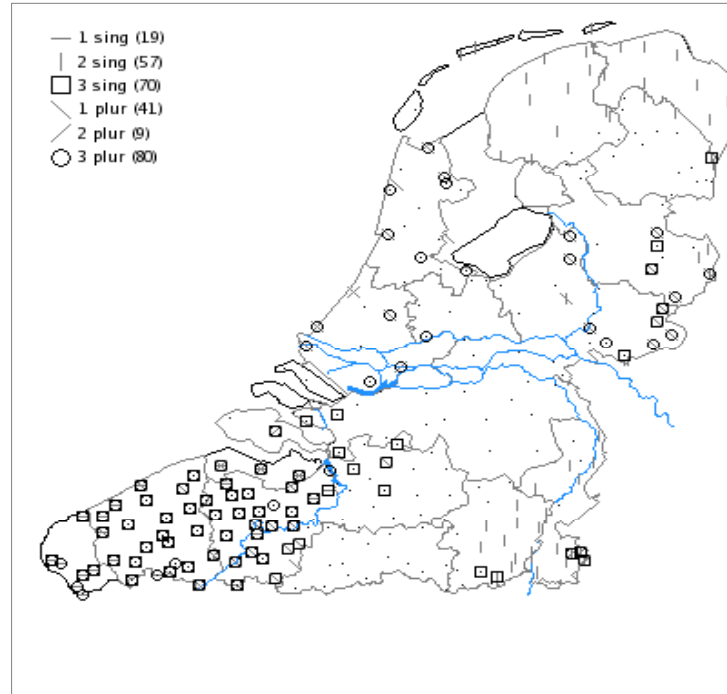
<sup>13</sup> Notice that this generalization is in fact a one-way generalization, i.e. dialects that exhibit subject/object asymmetries in relative clauses often also show complementizer agreement, but it is not true that dialects that exhibit complementizer agreement also always exhibit subject/object-asymmetries in relative clauses.



(14) *Generalization correlation complementizer agreement*

**D** the presence of a subject/object asymmetry in long-distance relativization correlates with complementizer agreement

Recently, a similar claim has been made by Mayr (to appear) for Bavarian (cf. also Kayne 1976, Rizzi 1990). Mayr's proposal forms the starting point for the analysis of subject/object asymmetries in (long-distance) relative clauses in varieties of Dutch, and is discussed in the next subsection.



Map 2: geographic distribution of complementizer agreement (Barbiers et al. 2005)

#### 4.2. Towards an analysis of subject/object asymmetries – Mayr (to appear)

On the basis of the Bavarian data in (15)-(17), Mayr shows that complementizer agreement correlates with extraction of subjects in Bavarian. The sentences in (15) show that the subject can be extracted from the embedded clause only when there is complementizer agreement, whereas (16) shows that complementizer agreement is not fully obligatory when there is no subject extraction. The sentences in (17) show that in contrast to subjects, objects can freely extract, independent from complementizer agreement (17a), and that objects in fact cannot agree with the local complementizer (17b).

- (15) a. [ Es Kinda ]<sub>i</sub> hot da Hans gfrogt [ \_\_<sub>i</sub> ob-s \_\_<sub>i</sub> hamkummts].  
           you children has the Hans asked if-2P.PL home come  
           ‘Hans asked if you children will come home.’  
       b. \* [ Es Kinda ]<sub>i</sub> hot da Hans gfrogt [ \_\_<sub>i</sub> ob-Ø \_\_<sub>i</sub> hamkummts].  
           you children has the Hans asked if-Ø home come

(16) ? Da Hans hot gfrogd [ ob-Ø es Kinda hamkummts].  
 the Hans has asked if you children home come  
 ‘Hans asked if you children will come home.’

(17) a. [ Die Bauan]<sub>i</sub> hot da Hans gfrogd  
 the farmers has the Hans asked  
 [ \_\_<sub>i</sub> ob-s/ob-Ø es Kölna endlich \_\_<sub>i</sub> bedients].  
 if<sub>2P.PL</sub>/if-Ø you waiters finally serve  
 ‘Hans asked if you waiters will finally serve the farmers.’

b. \* [ Eich Bauan]<sub>i</sub> hot da Hans gfrogd  
 you farmers has the Hans asked  
 [ \_\_<sub>i</sub> ob-s die Kölna endlich \_\_<sub>i</sub> bedienen].  
 if<sub>2P.PL</sub> the waiters finally serve  
 ‘Hans asked if the waiters will finally serve you farmers.’

Traditionally, subject/non-subject asymmetries were explained by appealing to the *Empty Category Principle* (ECP; e.g. Chomsky 1981, Bennis & Haegeman 1984, Rizzi 1990). Simply put, this principle states that traces need to be properly licensed. Whereas traces of moved objects are automatically licensed by the selecting verb, the licensing of subject traces requires an additional mechanism. In Mayr’s account, the mechanism that is required to license subject extraction is complementizer agreement.<sup>14</sup>

Assuming that complementizer agreement licenses subject extraction – giving rise to subject/object-asymmetries – the question that needs to be answered is: what is the difference between extraction from Spec,TP and extraction from the complement position of V? Mayr argues that subject/object-asymmetries are a consequence of the manner in which Merge proceeds. More precisely, the fact that subjects are merged later in the derivation than objects gives rise to the contrast. He introduces the following definition of Internal Merge, i.e. attraction of a goal by a probe.

(18) *Internal Merge*

IM at derivational stage  $\Sigma_i$  applies to nodes on the same projection line as the head H probing under c-command, thus a node formed at stage  $\Sigma_{i-1}, \Sigma_{i-2}, \dots \Sigma_1$ , where  $i > 1$ .

(19) *Projection Line*

X and Y are on the same projection line, iff the head X selects for YP. If the head Y selects for ZP, then by transitivity X and Z are on the same projection line.

Given these definitions, it should be clear that objects can always undergo movement because they are on the same projection line as a higher probing head. However, when an object has moved to the specifier position of a designated head, the question is how it can move any further, since a specifier is not on the projection line of a higher probing head (i.e. it is not selected for by that particular head). Mayr assumes that objects can always undergo movement, because they are on the projection line in their *external* merge position. That is to say, since objects are merged onto the projection line when they are merged into the derivation, they can always undergo movement. Subjects, on the other hand, are never on the

<sup>14</sup> For a comprehensive overview of the Bavarian data that support the claim that subject extraction is licensed by complementizer agreement, I refer the reader to Mayr (to appear); see Boef (2008) for a review of these arguments.

same projection line as a given head because they are not selected for by any head (they are specifiers). Therefore, given the definitions in (18)-(19), subjects cannot act as goals, i.e. they can never be probed. In order for subjects to be probed, Mayr proposes (20), according to which subjects in Spec,TP can be extracted by virtue of agreeing in  $\varphi$ -features with  $T^0$ . However, we need to derive that it is agreement with  $C^0$  that licenses long-distance subject extraction. Therefore, Mayr proposes (21).

(20) *The role of  $\varphi$ -features:*

Agreement in  $\varphi$ -features connects an element to the projection line, of which the agreeing head H is a part.

(21) *Checking conditions at Spell-Out for long-distance extractions:*

- a. At Spell-Out, which is induced by a phase head, e.g. C, it is checked, whether the dislocated element is on the relevant projection line due to its external merge position or not. If not,
- b. then a  $\varphi$ -relation with the phase head reconnects the dislocated element to the relevant projection line.
- c. Any previous  $\varphi$ -relation is deleted after Spell-Out.

The conditions in (21) ensure that only  $\varphi$ -agreement with  $C^0$  influences subject extraction. More specifically, when a subject has moved to the C-domain, its  $\varphi$ -agreement relation with  $T^0$  is no longer visible and as a consequence, another  $\varphi$ -agreement relation with  $C^0$  is established.

Assuming that selectional requirements need to be visible at all stages of the derivation, the following principle ensures that once the subject has entered into a  $\varphi$ -agreement relation with the local  $C^0$ , it is on the relevant projection line once and forever (similar to objects) and need not enter into further  $\varphi$ -agreement relations.

(22) *Secondary selection*

If V selects for a CP with DP in Spec,CP which agrees with the head of this CP in  $\varphi$ -features, then the DP behaves as if it were directly selected by V.

Notice that the principle in (22) predicts that subject/object asymmetries are always found in the clause that contains the extraction site of the moved item. This is in line with generalization B, which states that a subject/object asymmetry in long-distance relative clauses is (almost) only attested in the most deeply embedded clause.

#### 4.3. *The analysis of subject/object asymmetries*

In order to account for the patterns of long-distance relativization in dialects that show a subject/object asymmetry, I will follow Mayr in claiming that agreement in  $\varphi$ -features with the local  $C^0$  licenses subject extraction. More specifically, I will argue that special *die* in the dialects under discussion is in fact a manifestation of this agreement relation. That is, special *die* is not a (weak) relative pronoun, as is argued for in Bennis & Haegeman (1984), and recently in Sportiche (2008), but rather, special *die* is an agreeing form of the complementizer *dat* (in line with traditional analyses of the French *que/qui* alternation like Kayne 1976 and Rizzi 1990). In short, special *die* is the spell out of complementizer *dat* together with

agreement of  $\phi$ -features.<sup>15</sup>

As presented in section 3, not all dialects show a subject/object asymmetry. Following the null hypothesis that states that whenever there is subject extraction, the subject needs to agree with the local complementizer, it seems straightforward to assume that dialects differ with respect to whether they spell out this agreement relation or not. Therefore, the following micro-parameter is formulated.

(23) micro-parameter 1: +/- spell out of agreement relation with  $C^0$

An important consequence of this proposal is that it implies that in case of subject extraction there always needs to be complementizer agreement in Dutch, independent of whether this is spelled out or not.<sup>16</sup> To account for the fact that the majority of the Dutch dialects do not spell out this agreement relation, the most straightforward assumption is that not all dialects have complementizer *die* in their lexicon, in which case complementizer *dat* will be used as the default.<sup>17</sup> Variation between dialects that show the subject/object asymmetry and dialects without the asymmetry then seems to be reduced to the lexicon (or the level of Morphology), in line with Chomsky (1995) who argues that the lexicon is the locus of microvariation.

At this point we can give an explanation for generalizations B, C and D, which are here repeated for convenience as (24)-(26).

- (24) **B** a subject/object asymmetry can only appear in the CP containing the extraction site (i.e. a subject/object asymmetry in the higher clause is (almost) never attested)
- (25) **C** in case of a subject/object asymmetry (in the most deeply embedded clause), *die* occurs with subject extraction, whereas *dat* occurs with object extraction
- (26) **D** the presence of a subject/object asymmetry in long-distance relativization correlates with complementizer agreement

As already mentioned above, generalization B can be accounted for by Mayr's mechanism of *Secondary Selection* (cf. *supra* (22)): once the subject has entered into an agreement relation with the most deeply embedded  $C^0$ , it acts as though it is directly selected for by the verb, i.e. it is on the projection line once and forever and therefore it can be extracted and need not enter into further agreement relations with higher heads. Generalization C can be accounted

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<sup>15</sup> In some West-Flemish dialects special *die* appears with an obligatory additional agreement affix when the antecedent is not third person singular, as illustrated in the following sentence for a 3rd person plural antecedent.

(i) ... de mensen dan ze zeggen die-n/\*die da gekocht een  
 the people that.<sub>3P.PL</sub> they say *die*.<sub>??3P.PL</sub> that bought has

The question immediately arises why there is such additional complementizer agreement if we assume that special *die* is an agreeing form of the complementizer. If *-n* in (i) indeed shows 'additional' complementizer agreement similar to what we find on complementizer *da*, we expect to find *diet* instead of *die* in case the relative subject is third person singular; *-t* being the agreement affix that shows up with third person singular subjects (in all Dutch dialects that have complementizer agreement marking third person singular, cf. Barbiers et al. (2005:19)). However, this is not the case. Most likely, the affix *-n* is the phonological realization of a feature that is different from the features that are phonologically realized by *die*; perhaps *-n* is a number marker marking plural. Further research into the whole paradigm of 'additional' complementizer agreement affixes on special *die* is necessary to see if data as in (i) are representative for the whole dialect area that makes use of special *die*.

<sup>16</sup> Interestingly, Postma (2006) makes a similar claim on different grounds (a study on number neutralization of the subject pronoun *ze* 'she'/'they' in Dutch). He argues that although the ( $\phi$ -)agreement features in  $C^0$  are always (abstractly) present (in asymmetrical V2 languages), they are not spelled out in all dialects.

<sup>17</sup> This proposal could easily be formally implemented in the framework of *Distributed Morphology* (cf. Halle & Marantz 1993, Halle 1997, Harley & Noyer 1999).

for by the assumption that special *die* is an agreeing form of complementizer *dat*, and the assumption that only the subject, not the object, needs to agree with the local complementizer in order to be on the projection line and get extracted, i.e. special *die* only occurs with subject extraction. Finally, when we assume that special *die* is an agreeing variant of the complementizer, generalization D is explained: specifically dialects that show agreement morphology on the complementizer in other contexts (see map 2), spell out the agreement relation with the local complementizer in relative clauses.

Although the proposed analysis can account for generalizations B, C and D, we still cannot account for the difference between systems II and III. That is to say, micro-parameter 1 only distinguishes dialects that show a subject/object asymmetry (systems II and III) from dialects that do not show the asymmetry (systems I, IV, V and VI). As the proposed analysis only has something to say about what is going on in the most deeply embedded clause of long-distance relative clauses: *die*<sub>COMP</sub> with subjects and *dat*<sub>COMP</sub> with objects, we are still in need of an explanation of the patterns in the higher clause of long-distance relative clauses: *die* in system III and *dat* in system II for both subjects and objects. In order to answer the question of what is going on in the higher clause, the patterns for short relativization in table 2 and the format that Standard Dutch relative clauses obey, repeated here as (27), become important.

	<i>short subject</i>	<i>short object</i>	<i>long subject</i>	<i>long object</i>
system II	die	dat	dat-die	dat-dat
system III	die	die	die-die	die-dat

Table 2 : systems that show a subject/object asymmetry - systems II and III

- (27) a. ... head [RELATIVE CLAUSE \*(relative pronoun)<sub>i</sub> ... t<sub>i</sub> ... ]  
 b. ... head [RELATIVE CLAUSE \*(relative pronoun)<sub>i</sub> ... [t<sub>i</sub> \*(complementizer) ... t<sub>i</sub> ... ]]

Assuming for the moment that not only Standard Dutch, but all varieties of Dutch obey the format of restrictive relative clauses as given in (27), we predict the patterns as given in table 3 and 4 for long-distance relativization in systems II and III on the basis of the pattern in table 2, i.e. we predict the patterns of short relativization to be identical to the patterns in the higher clause of long relativization.

	<i>higher clause</i>	<i>lower clause</i>
subject	die <sub>REL</sub>	die <sub>COMP</sub>
object	dat <sub>REL</sub>	dat <sub>COMP</sub>

Table 3: predictions for system II

	<i>higher clause</i>	<i>lower clause</i>
subject	die <sub>REL</sub>	die <sub>COMP</sub>
object	die <sub>REL</sub>	dat <sub>COMP</sub>

Table 4: predictions for system III

In contrast to the predictions for system III in table 4, the predicted pattern for system II in table 3 is incorrect: long relativization in system II does not show an asymmetry in the higher clause (see table 2).<sup>18</sup> This observation seems to force the conclusion that the assumption that all dialects of Dutch require a relative pronoun to introduce the relative clause is incorrect. The pattern of system II can then be explained by assuming that system II, in contrast to system III, does not make use of relative pronouns. Together with the observation from section 2 that in Dutch dialects there always needs to be at least one overt element in the complementizer domain, we have an explanation of why we find *dat* in the higher clause of

<sup>18</sup> The predicted pattern in table 3 is in fact attested in the SAND corpus. In footnote 25 I will briefly touch upon this issue.

system II: because system II does not make use of relative pronouns and because there needs to be an element present, the complementizer *dat* appears, as shown in table 5.<sup>19</sup>

	<i>short relativization</i>	<i>long relativization</i>	
		<i>higher clause</i>	<i>lower clause</i>
subject	die <sub>COMP</sub>	dat <sub>COMP</sub>	die <sub>COMP</sub>
object	dat <sub>COMP</sub>	dat <sub>COMP</sub>	dat <sub>COMP</sub>

Table 5: pattern of system II; no relative pronouns

For dialects that show the pattern of system III, I will simply assume that they make use of relative pronouns, namely *die* for both subjects and objects. The pattern for this system then follows naturally: because the relative clause is always introduced by the relative pronoun *die*, there is no asymmetry in short relativization (only the pronoun is visible, not the complementizer) and the higher clause in long relativization is introduced by the relative pronoun *die*.<sup>20</sup> This is summarized in table 6.

	<i>short relativization</i>	<i>long relativization</i>	
		<i>higher clause</i>	<i>lower clause</i>
subject	die <sub>REL</sub>	die <sub>REL</sub>	die <sub>COMP</sub>
object	die <sub>REL</sub>	die <sub>REL</sub>	dat <sub>COMP</sub>

Table 6: pattern of system III; + relative pronouns

The difference between systems II and III can thus be attributed to the use of relative pronouns, as described by the following micro-parameter.

(28) micro-parameter 2: +/- presence of relative pronouns

On the basis of the two proposed micro-parameters, the properties of systems II and III can be summarized as follows. Both systems spell out the agreement relation with the local complementizer in case of subject extraction, but the systems differ with respect to whether they use relative pronouns or not.

<sup>19</sup> Notice that I assume – given the observation that we also find a subject/object asymmetry in short relativization – that the relative pronoun always enters into a  $\phi$ -agreement relation with the local  $C^0$ , i.e. the  $\phi$ -agreement relation with  $C^0$  is established in both short and long relativization. This is in line with what is claimed by Mayr (to appear). Recall that according to Mayr, at the point the subject is in Spec,CP, the agreement relation with  $T^0$  has become inaccessible due to Spell Out, and another  $\phi$ -agreement relation with  $C^0$  needs to be established. It is thus predicted that (overt) complementizer agreement in relative clauses also occurs in short relatives.

<sup>20</sup> One could imagine that there are dialects that make use of system III and at the same time allow doubly filled COMP. Although such dialects are predicted to have the string *die<sub>REL</sub> die<sub>COMP</sub>* in the C-domain of short subject relative clauses, such strings are not attested. A plausible explanation for this is that natural languages have the tendency to avoid the (accidental) repetition of identical morphemes within a particular syntactic environment (cf. Neeleman & Van de Koot 2006). As a result of this, only one *die* in the string *die<sub>REL</sub> die<sub>COMP</sub>* in short subject relatives is maintained, but it is unclear which *die* this is. As for table 6, I simply assumed that it is relative pronoun *die* that is maintained in short subject relatives. Further research is necessary to investigate the exact conditions under which identical morphemes may or may not occur adjacently.

	+ spell out agreement with C <sup>0</sup>
+ relative pronouns ( <i>die</i> <sub>SUBJ</sub> , <i>die</i> <sub>OBJ</sub> )	<i>die</i> <sub>REL</sub> - <i>die</i> <sub>COMP</sub> , <i>die</i> <sub>REL</sub> - <i>dat</i> <sub>COMP</sub> (III)
- relative pronouns	<i>dat</i> <sub>COMP</sub> - <i>die</i> <sub>COMP</sub> , <i>dat</i> <sub>COMP</sub> - <i>dat</i> <sub>COMP</sub> (II)

Table 7: properties of systems II and III

## 4.4 Predictions of the analysis

The proposed analysis makes several predictions. First, under the assumption that there is only one position for the finite declarative complementizer per clause (cf. Zwart 2000), we predict that dialects that make use of special *die*, never allow the string *die dat* in contexts with special *die*. This prediction is largely borne out by the SAND data: 38 out of the 39 dialects that make use of special *die* in (long-distance) relative clauses do not show the string *die dat* in special *die* contexts.<sup>21,22</sup> This conclusion is corroborated by the observation that in West-Flemish subordinate clauses the complementizer is always overt, independent of the presence of an element in Spec,CP (Haegeman 1992:57). This is illustrated in (29)-(30).

- (29) a. Kpeizen **da** Valère a weg is.  
 I think that Valère already away is  
 ‘I think that Valère is already gone.’  
 b. \*Kpeizen Ø Valère a weg is.
- (30) a. Kweten niet **wannièr da** Valère goa werekommen.  
 I know not when that Valère goes return  
 ‘I don’t know when Valère is going to return.’  
 b. \*Kweten niet **wannièr** Valère goa werekommen.

[Haegeman 1992:57]

However, in West-Flemish relative clauses the complementizer is never overt, cf. Haegeman (1983). Whereas this observation would be puzzling under the assumption that special *die* is a relative pronoun, it follows straightforwardly from the analysis of special *die* as a complementizer: under that assumption, the generalization that in West-Flemish subordinate clauses the complementizer always needs to be overt can be maintained.

Second, under the assumption that A'-movement of interrogative pronouns and A'-movement of relative pronouns target the same position, we predict the proposed analysis to be extended to other constructions involving long-distance A'-movement, and we predict these constructions to have the same geographic distribution. More specifically, languages that make use of special *die* in (long-distance) subject relative clauses are predicted to make use of the construction in (31a) when forming long-distance subject *wh*-questions. In (31a) the

<sup>21</sup> It is important to note that not all SAND informants were explicitly asked about the potential presence of a complementizer in addition to the element *die*. Therefore, the prediction that a complementizer cannot be present in special *die* contexts cannot be adequately tested on the basis of the SAND corpus. Moreover, the data in the online database are not always clear about the absence of string *die dat*. It thus seems more adequate to state that *at most* 38 out of 39 dialects do not allow the string *die dat* in special *die* contexts. Needless to say, further research into the issue of doubly filled COMP (in special *die* contexts) in relative clauses is necessary (cf. also footnote 22).

<sup>22</sup> Interestingly, according to Mayr (to appear), besides complementizer agreement, there is another way to license subject extraction, namely via doubly filled COMP. However, the data Mayr uses to make this point are not entirely uncontroversial (cf. Boef 2008), so it remains to be seen if doubly filled COMP can in fact license subject extraction.

question is introduced by the *wh*-word *wie* ‘who’, and the embedded clause is introduced by *die*, which, according to the analysis above, is taken to be a complementizer spelled-out as *die* by virtue of  $\phi$ -agreement with the copy of the *wh*-subject *wie*.

- (31) a. **Wie** denk je **die** het verhaal verteld heeft?  
           who think you *die* the story told has  
           ‘Who do you think told the story?’  
       b. **Wie** denk je **dat** het verhaal verteld heeft?  
           who think you that the story told has

At least at first sight, the prediction that the construction in (31a) should be used by dialects that exhibit special *die* in (long-distance) relativization, seems correct given the data on long-distance A’-dependencies in Dutch as reported by Schippers (2006). The construction in (31a) is indeed attested in West-Flanders, and several times in East-Flanders.<sup>23</sup> Moreover, as noted by Sportiche (2008:12), sentences like (31a) are acceptable in Nijmegen Dutch, which is exactly the area in which several attestations of subject/object asymmetries in long-distance relative clauses (system III) were found (cf. map 1). Unfortunately, the prediction cannot be tested on the basis of the SAND corpus, as in the SAND project sentences like (31a) were only questioned with a *wh*-object, and not with *wh*-subjects.<sup>24</sup> Further research needs to settle the issue of the (mis)matches between long-distance relatives and long-distance *wh*-questions.

Third, the proposed analysis predicts the existence of the four systems of relativization as given in table 8. This prediction is borne out: all four systems of relativization are attested within the SAND corpus.<sup>25</sup> In fact, the two proposed micro-parameters generate four out of the six systems of (long-distance) relativization that were presented in section 3. In other words, in addition to the patterns of the systems that show a subject/object asymmetry (systems II and III) the patterns of system I and system IV follow naturally from the proposed

<sup>23</sup> Importantly, however, this construction is certainly not the only possible one found in this area; the construction in (31b) – the Standard Dutch variant with a complementizer introducing the subordinate clause – is also attested several times in Flanders. This result might lead one to suggest that long-distance *wh*-questions and long-distance relativization structures cannot be analyzed on a par. A similar conclusion is also suggested by other SAND data on long-distance *wh*-dependencies, and the data gathered by Schippers (2006). Moreover, structures involving A’-movement of interrogative pronouns and A’-movement of relative pronouns have different semantic properties (e.g. reconstruction effects), suggesting that they should receive a different analysis (cf. Salzmann 2006a, Koopman & Sportiche 2008). Note that under the assumption that A’-movement of interrogative pronouns targets a different position than A’-movement of relative pronouns (cf. *infra*), possible mismatches between long A’-dependencies in questions and in relative clauses can be explained.

<sup>24</sup> Note that according to the proposed analysis, nothing excludes the existence of systems that show the *wie-die* pattern with *wh*-objects; cf. Strik (2008:325) who found that the *wie-die* pattern occurs quite a lot with long-distance *wh*-object questions in spoken Dutch. However, such systems are predicted to show the same pattern with subjects, and the proposed analysis predicts that *die* in these cases is not an agreeing complementizer (but these predictions remain to be tested). A possible analysis for such systems is the *partial copying* analysis of Barbiers, Koenenman & Lekakou (2009).

<sup>25</sup> Due to the fact that micro-parameter 2 (+/- relative pronouns) does not say anything about the form of the relative pronouns, we in fact predict the existence of two more systems of relativization, namely systems that make use of relative pronoun *die* for subject relatives, and relative pronoun *dat* for object relatives. The patterns of these two systems are given in the table below.

+ spell out agreement- spell out agreement+ relative pronouns ( $die_{SUBJ}$ ,  $dat_{OBJ}$ )*die-die*, *dat-datdie-dat*, *dat-dat* Although these systems do not occur very frequently (the observant reader might have seen that in fact they violate generalization B), the patterns in this table are attested in the SAND corpus. A plausible reason for the low amount of attestations of these systems is the observation that there are not many dialects that exclusively make use of the relative pronoun *die* for subject relatives and the relative pronoun *dat* for object relatives.



analysis.

	+ spell out agreement	- spell out agreement
- relative pronouns	dat-die, dat-dat (II)	dat-dat, dat-dat (IV)
+ relative pronouns ( <i>die</i> <sub>SUBJ</sub> , <i>die</i> <sub>OBJ</sub> )	die-die, die-dat (III)	die-dat, die-dat (I)

Table 8: two micro-parameters

In addition to this prediction regarding the *existence* of different systems of relativization, the analysis also makes a prediction with respect to the *non-existence* of certain systems: because the analysis only predicts the existence of the systems in table 8, at the same time the analysis predicts the non-existence of all other logically possible patterns of (long-distance) relativization that show a subject/object asymmetry. Table 9 shows that this prediction is borne out: all other systems that are ruled out by the proposed analysis are not or only very marginally attested in the SAND corpus. In sum, it can thus be stated that the proposed analysis correctly predicts the existence of *all* and *only* those patterns of long-distance relativization with a subject/object asymmetry that are attested in the SAND corpus.

<i>long subject</i>	<i>long object</i>	gen. B	gen. C	# attestations in SAND corpus
die-dat	die-die	+	-	2
dat-dat	dat-die	+	-	0
die-die	dat-die	-	+	0
dat-die	die-die	-	-	1
dat-dat	die-die	-	-	0
dat-die	die-dat	-	-	0
dat-dat	die-dat	-	-	1
die-dat	dat-die	-	-	0

Table 9: patterns of long-distance relativization that are excluded by the proposed analysis

#### 4.5 Unresolved issues

An important issue that presents itself is the morphological plausibility of *die* as an agreeing complementizer. That is to say, dialects that show the subject/object asymmetry in (long-distance) relative clauses often also show complementizer agreement (generalization D). However, outside of relative clauses this complementizer agreement does not manifest itself as *die* but as an agreement morpheme on complementizer *da* (e.g. (12b), (13b)). From a cross-linguistic perspective, however, the proposed analysis of *die* as an agreeing instance of *dat* is not implausible as there are more languages that have a special form of the complementizer that is only used in relative clauses (see De Vries 2002 for a comprehensive overview), like Czech, Polish, Indonesian, and Tyrolean (cf. Alber 2008). Moreover, following Hoekstra (1993), Hoekstra & Zwart (1994, 1997), Bennis (1997), and Zwart (2000) amongst others, in assuming that the CP (at least in Dutch) consists of two or more layers of complementizer phrases, and taking these layers to each represent a different landing site for different types of constituents – like *wh*-pronouns and relative pronouns – the complementizer that heads the CP-layer that is the target for A'-movement of relative pronouns might be different from the complementizer that heads the CP-layer that is the target for A'-movement of other elements. Thus, the complementizer we find in relative clauses might be specific to relative clauses, which can explain the observation that complementizer agreement with this complementizer is morphologically different from complementizer agreement with non-relative constituents.

Regarding the form of special *die*, it should be noted that it is not unique that a complementizer has the same form as a pronoun, cf. Dutch *dat* and English *that*. Moreover, it seems to be the case that pronouns can be ‘reanalyzed’ as heads in certain circumstances. For example, Bayer & Brandner (2006) show that in certain dialects of German the simplex interrogative pronoun in questions seems to appear in the position of the complementizer, making doubly filled COMP impossible (cf. Westergaard & Vangsnes 2005, and Vangsnes 2006 for a similar observation in certain Norwegian dialects, e.g. the dialect of Tromsø). A similar ‘reanalysis’ could be responsible for the form, the properties and the distribution of special *die*.

### 5. No subject/object asymmetries – a tentative proposal

Recall that we started out with the following six systems of relativization. In the previous section, I proposed an analysis that could account for four of these six systems, namely systems I, II, III and IV (cf. *supra* table 8). We are thus left with the two systems in the bottom row of table 1: system V and system VI. This section will present a tentative proposal of these two systems of (long-distance) relativization.

	<i>short subject</i>	<i>short object</i>	<i>long subject</i>	<i>long object</i>
system I (46/47)	die	die	die-dat	die-dat
system II (20)	die	dat	dat-die	dat-dat
system III (19)	die	die	die-die	die-dat
system IV (10/16)	dat	dat	dat-dat	dat-dat
system V (9)	die	die	die-die	die-die
system VI (7)	die	die	dat-die	dat-die

Table 1: six systems of (long-distance) relativization

It is important to note that the element *die* that appears in the patterns of both short and long relativization in systems V and VI cannot be an agreeing form of the complementizer, because we find *die* both with subject and with object extraction; *die* is thus a relative pronoun in these systems. Assuming that a moved element leaves an identical copy of itself (instead of a trace) in its extraction site (*copy theory of movement*, Chomsky 1995), a long-distance relative clause in Dutch is predicted to look as follows. The relative pronoun first moves to the intermediate Spec,CP (cf. *supra* footnote 4), and then moves further to the higher Spec,CP.

(32) ... de man [<sub>CP</sub> **die**<sub>i</sub> (dat) ik denk [<sub>CP</sub> **die**<sub>i</sub> (dat) ze **die**<sub>i</sub> geroepen hebben]]

Let us assume that – as in most movement chains – only the highest copy of the pronoun gets spelled out (cf. Nunes 2004). Then, given that zero-relativization is not allowed in Dutch, the complementizer in the lower clause is spelled out. This gives us the Standard Dutch system (I), as illustrated in (33).

(33) ... de man [<sub>CP</sub> **die**<sub>i</sub> (dat) ik denk [<sub>CP</sub> ~~die~~<sub>i</sub> **dat** ze ~~die~~<sub>i</sub> geroepen hebben]]

When we follow the null hypothesis and assume that the core syntax of (long-distance)

relative clauses is the same for all variants in table 1,<sup>26</sup> i.e. the underlying structure of all variants of long-distance relativization looks like (32) before PF-deletion has taken place, it seems rather straightforward to assume that the patterns of systems V and VI are the result of different parameter settings with respect to which copies of the relative pronoun are targeted by PF-deletion (in contrast to the patterns of systems I and III in which only the highest copy of the relative pronoun gets spelled out). If we assume the conditions of PF-deletion as given in (34), we derive *all* and *only* the attested systems of long-distance relativization that make use of relative pronouns, as illustrated in tables 10 and 11.

- (34) a. condition  $\alpha$ : the tail of a movement chain cannot be spelled out<sup>27</sup>  
 b. condition  $\beta$ : at least one link in a movement chain needs to be spelled out<sup>28</sup>

chain (subject & object)	condition $\alpha$	condition $\beta$	system
[die <del>die</del> ]	+	+	√ system I, III, V and VI
[die die ]	-	+	*
[ <del>die</del> die ]	+	-	*
[ <del>die</del> die ]	-	+	*

Table 10: PF-deletion in the relative chain in short relativization

chain (subject & object)	condition $\alpha$	condition $\beta$	system
[die [ <del>die</del> die ]]	+	+	√ system I, III
[die [die <del>die</del> ]]	+	+	√ system V
[die [ <del>die</del> die ]]	-	+	*
[die [die die ]]	-	+	*
[ <del>die</del> [die die ]]	-	+	*
[ <del>die</del> [die <del>die</del> ]]	-	+	*
[ <del>die</del> [die die ]]	+	+	√ system VI
[ <del>die</del> [ <del>die</del> die ]]	+	-	*

Table 11: PF-deletion in the relative chain in long relativization

We make the right predictions with the two relatively uncontroversial conditions in (34), and we could implement this proposal by formulating a third micro-parameter that states that some dialects allow the spell out of multiple copies (system V), whereas others do not. In the latter case PF has a choice as to which of the two highest copies to spell out: either the higher one, resulting in systems I and III, or the lower one, giving us system VI.

An analysis like the one proposed above needs to assume that in relative clauses only the relative pronoun moves, whereas the relative head noun is base generated in a position outside the relative clause (the *head external analysis* of relative clauses), giving rise to movement

<sup>26</sup> However, dialects may differ with respect to whether or not they merge a relative pronoun (micro-parameter 2).

<sup>27</sup> It seems a general tendency in natural languages to not spell out the lowest copy in a chain (cf. Barbiers, Koenenman & Lekakou 2009, and Nunes 2004:39 for *wh*-chains). Note however, that I am abstracting away from *resumption* as the question remains to be answered whether resumption should be analyzed as the spell out of the lowest copy in a chain; but even so, the dialects under discussion (almost) never make use of resumptive pronouns, so it seems reasonable to expect that PF does not have the option of spelling out the lowest chain link in these varieties.

<sup>28</sup> Note that this condition is very reminiscent of (yet not identical to) the principle of *recoverability of deletion* (cf. Chomsky & Lasnik 1977 among others).

chains like the one in (32). However, as mentioned before (*supra* footnote 3), the most prominent analyses of relative clauses in the current literature all assume that there is a clause-internal representation of the relative head (cf. Kayne 1994, Zwart 2000, Bianchi 1999, 2000, De Vries 2002, Citko 2001, Bhatt 2002, Sauerland 2003, Salzmann 2006a amongst others). If that is indeed the case, we need to answer the question of how to account for the fact that the copy of the relative DP in the embedded Spec,CP is never fully spelled out (this was already pointed out in Schippers 2006), i.e. the NP *man* is never spelled out in lower copies (cf. (5) and (6)), as illustrated in (35) for a head raising analysis of relative clauses.

- (35) a. de man<sub>i</sub> [CP [~~die man~~] ik denk [CP [~~die man~~] ze [~~die man~~] geroepen hebben]  
 b. \*de man<sub>i</sub> [CP [~~die man~~] ik denk [CP [~~die man~~] ze [~~die man~~] geroepen hebben]

In the literature, several proposals have been put forward that can account for this and related issues (see e.g. Nunes 2004, Van Koppen 2005, 2007, Neeleman & Van de Koot 2007). However, another option that immediately comes to mind is to rethink the arguments in (dis)favor of the traditional head external analysis of relative clauses. In the remainder of this section some of these arguments will be briefly evaluated.

The most serious arguments in favor of a head *internal* analysis of relative clauses have to do with reconstruction effects that show the need for an internal representation of the external head inside the relative clause.<sup>29</sup> Examples of reconstruction for Principle A, variable binding, and idiom interpretation respectively are given in (36).

- (36) a. ... het [ gerucht over zichzelf<sub>i</sub> ] dat Jan<sub>i</sub> niet \_\_ verdragen kan  
 the rumour about SE-SELF that Jan not bear can  
 ‘... the rumour about himself<sub>i</sub> that Jan<sub>i</sub> cannot bear’  
 b. ... de [ foto van zijn<sub>i</sub> geliefde ] die iedere man<sub>i</sub> \_\_ in zijn  
 the picture of his beloved which every man in his  
 portefeuille heeft<sup>30</sup>  
 wallet has  
 ‘... the picture of his beloved that every man keeps in his wallet’  
 c. De [ *streek* ] die hij me leverde, riep om wraak.  
 the nasty joke which he me delivered, cried for revenge  
 [De Vries 2002:78]

Interestingly, we seem to need ‘reconstruction without copies’ anyway (cf. Hoekstra & Zwart 1997, Hoekstra 1999, Sharvit & Guerzoni 2003, Van Craenenbroeck 2004, Van de Koot 2004, Van Koppen 2007), as exemplified in the following two examples. In (37) the left dislocated PP *naar zijn promotie* ‘to his defense’ needs to be reconstructed into the trace position in order for the pronoun *zijn* ‘his’ to be bound by *iedere taalkundige* ‘every linguist’. However, the trace position is a DP-position (it is the complement of the preposition *naar* ‘to’), not a PP-position. A similar thing holds for the example in (38). The infinitival constituent *elkaar helpen* ‘help each other’ may not occur as the complement of *doen* ‘do’ as shown in (38b), because this verb doesn’t select for an IP (38c). However, when the infinitival constituent occurs in a left dislocated position (38a), the reciprocal *elkaar* ‘each other’ within this constituent can be bound by the lower subject pronoun *ze* ‘they’.

<sup>29</sup> Note that this argument only holds under the assumption that reconstruction is best analyzed as the interpretation of a lower copy in a syntactic chain.

<sup>30</sup> (36a) and (36b) are translated from German examples in Salzmann (2006b:66).

- (37) Naar zijn<sub>i</sub> promotie, daar kijkt [ iedere taalkundige]<sub>i</sub> naar t<sub>daar</sub> uit.  
 to his defense there looks every linguist to out  
 ‘Every linguist looks forward to his defense.’

[Hoekstra & Zwart 1997]

- (38) a. [ Elkaar<sub>i</sub> helpen] (dat) doen ze<sub>i</sub> hier niet t<sub>dat</sub>.  
 each other help *topic pronoun* do they here not  
 ‘Help each other, they don’t do that here.’  
 b. \* Ze doen hier niet elkaar helpen.  
 they do here not each other help  
 ‘They don’t help each other here.’  
 c. Ze doen dat niet.  
 they do *topic pronoun* not  
 ‘They don’t do that.’

[Hoekstra 1999:65]

Moreover, the reconstruction effects in relative clauses are less clear than presented here. Relative clauses show ‘lack of Principle C effects’ (cf. Citko 2001, Bhatt 2002, Sauerland 2003, Salzmann 2006a), as exemplified for Dutch in (39): because of reconstruction of the head of the relative clause *verhaal over Jan* ‘story about Jan’, we predict the sentence to be ungrammatical on a coindexed reading, because the pronoun *hij* ‘he’ c-commands the name. However, this sentence is perfectly fine. Although the lack of Principle C effects in relative clauses is compatible with the matching analysis, an additional mechanism is required under the head raising analysis (e.g. *vehicle change*).

- (39) ... het [ verhaal over Jan<sub>i</sub> ] dat hij<sub>i</sub> mij \_\_\_ liever niet verteld had.  
 the story about Jan that he me rather not told had

More importantly, the argument in favor of the presence of an internal head in relative clauses that comes from idiom reconstruction is in fact rather weak. As observed by Lasnik & Fiengo (1974:541), a lot of object NPs of VP idioms cannot relativize (in contrast to the famous VP idiom *making headway*). This observation is unexpected under a raising/matching analysis of relative clauses according to which the object NP is merged as the complement of the verb, and there is obligatory reconstruction, i.e. we would expect the sentences in (41) to be grammatical. The observation becomes even stronger when we take into account the grammaticality of the sentences in (42). Note that the contrast in acceptability between (41) and (42) follows under a head external analysis of relative clauses.

- (40) The headway that we made was sufficient.

- (41) a. \*The tabs that we kept paid off.  
 b. \*The advantage that we took of Mary was frowned upon.  
 c. \*The heed that we paid to that warning was slight.  
 d. \*The attention that we paid to the lecture was careful.

- (42) a. Tabs were kept on Mary.  
 b. Advantage was taken of Mary.

- c. Heed was paid to our warning.
- d. Attention was paid to our problems.

An argument in favor of the head *external* analysis of relative clauses has to do with Case mismatches between (i) the relative head noun and the relative clause internal gap, and/or between (ii) the relative pronoun and the nominal head (43). Such mismatches are hard to account for under any analysis that assumes the relative pronoun and the nominal head are merged as one constituent (but see amongst others Kayne 1994, Bianchi 1999, 2000, Citko 2001, De Vries 2002, and Salzmann 2006a for potential solutions), but follow straightforwardly under a head external analysis of relative pronouns.

- (43) Widzialem tego pana który zbil ci szybe.  
 saw-1SG this-ACC man-ACC which-NOM broke you glass  
 ‘I saw the man who broke your glass.’

[Polish; Borsley 1997:638]

Another argument in favor of a head external analysis is the observation that in contrast to what we predict on the basis of an analysis that takes relative pronouns to be determiners, relative pronouns do not have the same selectional properties as their determiner counterparts, as illustrated for the Dutch element *die* in (44).

- (44) a. dat/\*die meisje heb ik gezien *determiner*  
 that/*die* girl have I seen  
 ‘I have seen that girl.’  
 b. het meisje dat/?die ik gezien heb *relative pronoun*  
 the girl that/*die* I seen have  
 ‘the girl that I have seen’

A final argument in favor of a head external analysis has to do with the observation that restrictive relative clauses can have multiple heads, as first observed by Ross & Perlmutter (1970). As exemplified in (45) and (46), the antecedent of such plural relative clauses in sentence-final position is a discontinuous noun phrase: in (45) the antecedent is *a man ... (and) a woman*, in (46) the antecedent is *a man ... a woman ... (and) a boy*.

- (45) A man entered the room and a woman went out who were quite similar.  
 [Ross & Perlmutter 1970:350]

- (46) First a man arrived, then a woman arrived, and finally a boy arrived who all looked like zombies.

[Hoeksema 1986:69]

Although the existence of this construction poses a problem for all existing theories of the syntax of relative clauses, it should be clear that it is particularly problematic for theories that assume that the antecedent of the relative clause originates in a relative clause-internal position.

## 6. Summary and conclusions

This paper provided a description and an analysis of the syntactic variation in long-distance relative clauses in varieties of Dutch. I primarily focused on six systems of relativization that differ with respect to the distribution of the elements *die* and *dat*. I argued that dialects differ with respect to the spell out of the agreement relation between the extracted subject and the local complementizer (micro-parameter 1). This could account for subject/object asymmetries. Dialects were moreover assumed to differ in their use of relative pronouns (micro-parameter 2). With these two relatively simple micro-parameters I was able to account for four of the six systems of relativization we started out with. The two remaining systems can probably best be analyzed in terms of the spell out of different copies of the relative pronoun. Future research should settle the issue of which analysis of relative clauses is best compatible with this idea.

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# The Psychological Reality of Hidden Lexical Entries

## Evidence from Hebrew

Julie Fadlon

In this paper I discuss *hidden lexical entries*: forms that are assumed to be represented in the mental lexicon even though they do not exist in the actual vocabulary, in order to account for derivational gaps. This mechanism is often criticized to be an ad-hoc, unfalsifiable theoretical tool and to lack psychological reality. I argue that in view of the commonly assumed interface between the mental lexicon and the conceptual system, *hidden lexical entries* are not unfalsifiable. To demonstrate this argument, I present an experiment designed to detect the hidden existence of gaps in the transitive-unaccusative alternation.

### 1. Introduction: derivational gaps and hidden lexical entries mechanisms

When attempting to reach a generalization regarding word formation, one often encounters the phenomenon of sporadic derivational gaps: cases in which an assumed structure or a derivational rule predicts the existence of a word, which (for no apparent phonological or semantic reason) does not exist.

One word formation theory which explicitly addresses this issue is Halle (1973). Halle presents a lexicalist model of the mental lexicon, i.e., a model that views the mental lexicon as an active component of grammar which includes rule governed derivational relations between the entries listed in it (see also Chomsky 1970, Jackendoff 1975, Aronoff 1976, Reinhart 1996, 2002, Siloni 2002, among others). For example, according to this model, the nouns *approbation* and *proposition* are created from the verbs *approve* and *propose* in the mental lexicon by the following derivational rule ('word formation rule'):

- (1) [VERB+ ation]<sub>N</sub>  
a. [*approve*+ ation]<sub>N</sub> (*approbation*)  
   [*propose*+ ation]<sub>N</sub> (*proposition*)

Nevertheless, this word formation rule also predicts the existence of forms like *\*arrivation* and *\*refusation*, which are absent from the vocabulary of English:

- (1) b. [*arrive*+ ation]<sub>N</sub> (*\*arrivation*)  
   [*refuse*+ ation]<sub>N</sub> (*\*refusation*)

There is no particular independent reason to exclude these outputs. Therefore, a model which assumes the word formation rule in (1) must provide an account for the fact that some of this rule's potential outputs are absent from the English vocabulary.

According to Halle (1973), *\*refusation* and *\*arrivation* are cases of 'accidental gaps' between the lexicon and 'the list of actual words' in English. They are potential outputs of lexical rules that, just like existing words, are represented in the mental lexicon by a corresponding lexical entry. However, they are also arbitrarily marked [-lexical insertion], which results in their exclusion from the list of actual words by a special filter that prevents them from emerging in utterances. A simplified scheme of Halle's model of the mental lexicon is presented in figure 1 below.

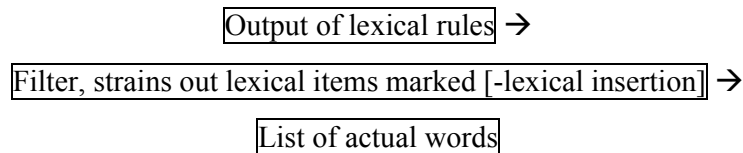


Figure 1

Another lexicalist model which addresses derivational gaps is Jackendoff (1975). According to this model, when two or more lexical items contain less independent information than a random grouping of lexical items, they are connected by a 'lexical redundancy rule', for example:

- (2) Rule:  
V+ ion → a noun, the abstract result of the act of V-ing
- a. Lexical items connected by it:  
*decide*+ ion → *decision*  
*delete*+ ion → *deletion*

However, the redundancy rule in (2) also implies that the noun *retribution* is derived from the non-existent verb *\*retribute* and that the noun *aggression* is derived from non-existent verb *\*aggress*:

- (2) b. *\*retribute*+ ion → *retribution*  
*\*aggress*+ ion → *aggression*

According to Jackendoff, it is speakers' intuition that words with a predictable morphology like *retribution* and *aggression* are not basic forms, and a theory of the lexicon must reflect this intuition. Hence, in his model non-existing inputs of lexical redundancy rules like *\*retribute* and *\*aggress* are represented in the mental lexicon. However, their lexical representation is not in the form of a corresponding independent lexical entry, as an actual word would be lexically represented, but as a reference to the lexical ancestors of the forms derived from them (i.e., as part of the information coded in the entries of *retribution* and *aggression*). As a result of lacking a corresponding independent lexical entry, forms like *\*retribute* and *\*aggress* do not exist as actual words.

### 1.1. Hidden lexical entries

The accounts Halle (1973) and Jackendoff (1975) provide for derivational gaps are similar in the sense that they both assume that the missing forms do exist in the mental lexicon, but are somehow prevented from appearing as actual words ([-lexical insertion] marking, lack of an independent lexical entry).

To refer this type of account for derivational gaps, I use the unifying term *hidden lexical entries*. I adopt Horvath & Siloni's (2008) terminology which separates *the mental lexicon* (a component of the grammar) from *the actual vocabulary* of a particular language (the sum of words speakers can utter) and accordingly define *hidden lexical entries* as follows:

- (3) *Hidden lexical entries*: forms that do not exist in the vocabulary of a language but are assumed to have a representation in the mental lexicon.

### 1.2. Psychological reality?

The assumption of lexical entries that lack a corresponding vocabulary item is often perceived as an ad hoc, unfalsifiable theoretical tool (Aronoff 1976, Anderson 1992, Doron & Rappaport-Hovav 2007). Lacking a corresponding vocabulary item, these forms are always hidden in the mental lexicon and are never used in an utterance. Consequently, there appears to be no way of providing theory-independent evidence for their existence.

I argue that given the common assumption that the lexical component of language is in interface with the conceptual system (Fodor 1975, Pinker 1994, Sperber & Wilson 1997, among others), it is not unreasonable to assume that lexical encoding will have an effect on the perception of the matching concept.<sup>1,2</sup> Therefore, as a result of the language faculty's interaction with other components of the human knowledge, the existence of a lexical entry should be traceable even when there is no direct evidence for it.

Based on this assumption, I conducted an experiment to explore the psychological reality of hidden lexical entries. The experiment used the Hebrew transitive-unaccusative derivational alternation as a case study and its results provide evidence in favor of hidden lexical entries in this context. As a whole, this research shows that even though hidden lexical entries are missing from the physical, articulatory end of language, they are not unfalsifiable theoretical entities.

The remainder of the paper is structured as follows: in section 2 I briefly introduce the subject of unaccusative verbs and the hidden lexical entries mechanism assumed in order to account for gaps in the transitive-unaccusative derivational alternation (Chierchia 1989, Reinhart 2002, Horvath & Siloni 2008); section 3 presents an alternative way to account for these gaps and discusses the differences between the two possible approaches; section 4 is a summary of a distinction, made by Horvath & Siloni (2008), between two types of

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<sup>1</sup> When I use the term *concepts* in this paper, I refer to concepts in the wider sense - notions; things that a person is able to think about. I distinguish between *concepts* and *lexical items* and treat the mental lexicon as including a linguistic representation for a subset of all the things a person is able to think about, i.e. concepts. Thus, use of the term concept in this paper does not denote 'a mental primitive' and reference for a mental entity as 'concept' simply implies that it is perceivable by human beings (cf. Fodor 1975, Sperber & Wilson 1997, in which the use of this term often entails 'individual concepts' as opposed to 'complex conceptual expressions').

<sup>2</sup> The effect the existence of a parallel vocabulary item has on a concept is a well studied topic in psycholinguistic research regarding color terms, starting with Berlin & Kay (1969). Hays et al (1972), for example, found that color terms that have a parallel word in all languages (white, black and red) are universally more salient than color terms with parallel words in fewer languages.

unaccusative verbs with no transitive alternate in the vocabulary, which enables the empirical examination of the hidden lexical entries mechanism presented in section 2; in section 5, I present a general hypothesis regarding the relation between lexical entries and the conceptual system and the predictions this hypothesis makes in the context of the transitive-unaccusative alternation; section 6 presents an experiment which tested these predictions, including a detailed description and a statistical analysis of its results; in section 7 I discuss the consequences of these results; section 8 summarizes this paper and presents its conclusion; in appendix A I suggest a way to account for derivational gaps within the framework of Distributed Morphology (Halle & Marantz 1993, Marantz 1997); appendix B includes the list of Hebrew verbs used in the experiment.

## 2. Case study: Gaps in the transitive-unaccusative alternation

A very general definition of unaccusative verbs (Perlmutter 1978, Chierchia 1989, Burzio 1986, Reinhart 1991, Borer 1994, Levin & Rappaport-Hovav 1995, Pesetsky 1995, Reinhart 2002, Reinhart & Siloni 2005, among others) is that they are intransitive verbs which (a) assign a theme theta-role to an internal argument, (b) lack the ability to assign accusative case and (c) in most cases, have a transitive alternate. For example:

English:

- (4) a. The window broke. (unaccusative)  
 b. The girl /stone /wind broke the window. (transitive)

Hebrew:

- (5) a. ha-kadur hitgalgel  
 the-ball rolled-UNACC  
 'The ball rolled.'  
 b. Roza/ha-ru'ax gilgela 'et ha-kadur  
 Roza/the-wind rolled-TRANS ACC the-ball  
 'Rosa rolled the ball.'
- (6) a. ha-agartal nafal  
 the-vase fell-UNACC  
 'The vase fell.'  
 b. ha-tinok/kadur hipil 'et ha-'agartal  
 the-baby/ball fall-TRANS ACC the-vase  
 'The baby/the ball caused the vase fall.'

However, as demonstrated in (7)-(9) below, there are also cases in which the transitive counterpart of a certain unaccusative is idiosyncratically missing from the vocabulary of a given language; these are cases of gaps in the transitive-unaccusative alternation.

English:

- (7) a. The vase fell.  
 b. \*The wind/the girl fell the vase.

- (8) a. The spot vanished.  
b. \*The cleaners/the detergent vanished the spot.

Hebrew:

- (9) a. ha-cemax naval.  
the-plant wilted  
'The plant wilted.'  
b. \* ha-zman/ganan hinbil/nibel'et ha-cemax<sup>3</sup>  
the-time/gardner wilt- TRANS ACC the-plant  
'Time/the gardener caused the plant to wilt.'

The gaps in the transitive-unaccusative alternation discussed in this paper are sporadic. These are not cases where a form's absence from the vocabulary can be given a semantic explanation, since these gaps idiosyncratically occur in some languages while in others the alternation is complete. Consider (7)-(9) above for example. *fall* and *vanish* have a transitive alternate in Hebrew (*hipil* and *he'elim* accordingly) and *wilt* has a transitive alternate in Hungarian (*elhervaszt*). It seems unreasonable to argue that the semantics of a transitive lexical entry would prevent it from having a corresponding vocabulary item in one language, while a semantically identical transitive verb can be found in the vocabulary of another. There is also no phonological generalization that can provide an explanation for the absence of these transitive forms; for example, the Hebrew unaccusative *nafal* 'fall' has a transitive counterpart in the vocabulary (*hipil*) while the almost phonetically identical *naval* 'wilt' does not.<sup>4</sup>

### 2.1. A current hidden lexical entries mechanism: frozen lexical entries

The analysis of unaccusative verbs brought by Reinhart (2002) and Reinhart & Siloni (2003, 2005) is a lexicalist one. According to this analysis, unaccusative verbs are derived from transitive verbs with a Cause external theta role via the lexical operation of 'decausativization', which, as demonstrated in (10) below, eliminates the transitive entry's external theta role and its accusative case feature:

- (10) Decausativization:  $V_{ACC} (\theta_{Cause} \theta_{theme}) \rightarrow V \theta_{theme}$ <sup>5</sup>

<sup>3</sup> These non-existent Hebrew verbs are presented here in CiCeC and hiC.CiC verbal templates, which are typical for Hebrew transitive verbs.

<sup>4</sup> Also notice that these are not cases that can be accounted for by the process of 'blocking' suggested by Aronoff (1976). According to Aronoff, the existence of one word in the mental lexicon can prevent the application of a rule which creates its semantic equivalent. However, consider the cases presented in (7)-(9). There is no semantic equivalent for the transitive counterpart of *naval* 'wilt' in Hebrew, and no semantic equivalents for the transitive counterpart of *vanish* in English.

<sup>5</sup> Unlike the Agent external theta role, the Cause role is indifferent with regard to animacy. It could be assigned to either an animate or an inanimate argument. The difference between Agent and Cause is demonstrated in (i-iii) below. *open* and *break* assign a Cause theta role to their external argument while *eat* assigns an Agent theta role to its external argument. Consequently, (iii)a, which has an animate argument in the external position is grammatical while (iii)b, which has an inanimate argument in this position, is not:

- (i) a. Max opened the door  
b. The wind opened the door  
(ii) a. Max broke the window  
b. The stone broke the window  
(iii) a. Max ate the soup  
b. \*The spoon ate the soup  
c. \* The soup ate

Important for the purpose of this paper is that, in order to account for derivational gaps, this analysis assumes a hidden lexical entries mechanism, namely ‘frozen lexical entries’.

Reinhart (2002) and Horvath & Sioni (2007, 2008) argue that one-place unaccusatives are never basic entries and that they are always derived via decausativization. Gaps in the transitive-unaccusative alternation, like the ones demonstrated in (7)-(9) above, are not considered to be a problem for this generalization. Following Fodor (1975), they assume that lexical information is largely universal, i.e., that apart from phonological forms, the information encoded in the mental lexicon is the same for all natural languages. Therefore, according to these accounts, as long as an alternation is complete in one language or another, it exists in the mental lexicon of all speakers universally.

Reinhart (2002) and Horvath & Sioni (2008) note that all one-place unaccusative verbs have a transitive alternate in one language or another, or had one in earlier stages of a given language. Thus, a one-place unaccusative will always have a transitive alternate listed in the mental lexicon, even in languages where the transitive form does not exist as an actual vocabulary item.

For example, consider the cross-linguistic data from Horvath & Sioni in (11) below. Given the assumption above, the fact that *vanish*, and *collapse* have a transitive alternate in Hebrew and *wilt* has a transitive alternate in Hungarian, is evidence for the universal existence of these transitive forms in the mental lexicon.

(11) <b>collapse</b>	<b>wilt</b>	<b>vanish</b>
Hungarian:	Hebrew:	English:
Unaccusative: <i>összeesik</i>	Unaccusative: <i>naval</i>	Unaccusative: <i>vanish</i>
No transitive alternate	No transitive alternate	No transitive alternate
Hebrew:	Hungarian:	Hebrew:
Unaccusative: <i>hitmotet</i>	Unaccusative: <i>elhervad</i>	Unaccusative: <i>ne'elam</i>
Transitive: <i>motet</i>	Transitive: <i>elhervaszt</i>	Transitive: <i>he'elim</i>

In order to account for the absence of these forms from the vocabulary of some languages and following Chierchia (1989), Reinhart (2002) and Horvath and Sioni (2008) argues that their representation in the mental lexicon is ‘frozen’, or, in the current paper's terms, that they are hidden in the mental lexicon.

Frozen lexical entries are lexical entries that cannot be inserted into syntax in a particular language, and hence are not part of the actual vocabulary of this language. However, being represented in the mental lexicon, they can still feed lexical operations and serve as frozen input. Thus, the missing transitive alternates of one-place unaccusatives are frozen lexical entries that, as illustrated in (12), can feed the lexical operation of decausativization:

- (12) Decausativization:  $V_{ACC} (\theta_{Cause}, \theta_{theme}) \rightarrow V \theta_{theme}$   
 break-TRANS  $\rightarrow$  break- UNACC  
 And also:  
 \*fall-TRANS  $\rightarrow$  fall- UNACC

### 3. Hidden listing vs. non-existence

The word formation theories discussed above use the assumption of hidden lexical entries to account for derivational gaps. However, the assumption of the hidden existence of the missing

part is not a prerequisite for explaining a gap in an alternation. It is possible to do so without assuming that a missing part of the alternation is hidden in the mental lexicon.

An alternative way to address this subject is to argue that the forms absent from a certain language's vocabulary do not exist in this particular language. This approach would argue that for English speakers, for example, the transitive alternates of *fall* and *vanish* simply do not exist: not as words in the vocabulary of the language, and not as mental-lexicon representations.<sup>6</sup>

In other words, a gap in the transitive-unaccusative alternation can be viewed in one of the two following ways:

- A. The missing transitive alternate is listed in the mental lexicon in a hidden way and serves as input for unaccusative verb formation, but cannot be inserted into syntax.
- B. These gaps occur when an existing unaccusative simply does not have a parallel transitive form in a particular language.

The various differences between these two explanations can be reduced into a single fundamental one: explanation A views the missing transitive as existing, while explanation B views it as non-existent.

Explanation B seems like the null hypothesis – a transitive form is missing from a certain language because it simply does not exist in this language. Nevertheless, if there is theory-independent evidence to justify the assumption of *hidden* lexical entries – of listings in the mental lexicon that cannot be used in an utterance – then explanation A should be preferred.

However, it seems that there is no way to distinguish a hidden lexical entry from a non-existing one. After all, what sort of evidence can suggest that a form missing from the vocabulary is actually listed in the mental lexicon?

Notice that within a model which assumes only two types of unaccusative verbs – (1) derived from a transitive entry with a corresponding word and (2) derived from a frozen (hidden) transitive entry – the assumption of hidden lexical entries would indeed be unfalsifiable. Within such a model, any linguistic behavior typical to the second type will not provide a meaningful insight regarding the existence of hidden lexical entries of the transitive alternate. This is because there would be no way of telling if the behavior an unaccusative verb with no transitive alternate in the vocabulary is the result of its having a hidden transitive alternate or of not having a transitive alternate at all.

#### 4. Two types of unaccusatives without a transitive alternate (Horvath & Siloni 2008)

A contrast between unaccusatives which have a hidden transitive alternate and unaccusatives which have no transitive alternate at all is given by Horvath & Siloni (2008), who provide a starting point for a research devoted to identify hidden lexical entries. This is since it predicts a hidden existence of a lexical entry to have particular observable implications that should be absent if the form is not at all listed in the mental lexicon.

Following Jaeggli (1986), Horvath & Siloni (2008) note that, crosslinguistically, unaccusatives with no transitive alternates in the vocabulary never have a corresponding

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<sup>6</sup> In appendix B I argue that this is how the non-lexicalist model of Distributed morphology (Halle & Marantz 1993, Marantz 1997) can account for derivational gaps and present a suggestion for such an account within this framework.



verbal passive form, while there are such unaccusatives with an adjectival passive alternate. Some examples for unaccusatives that have a corresponding adjectival passive are:

- |  |  |  |
|--|--|--|
| (13) English                                 | (14) Hebrew  | (15) Hungarian   |
| <i>arrive, faint, fall,</i><br><i>vanish</i> | <i>nirkav / hirkiv</i> ‘got<br><i>naval</i> ‘wilted’ | <i>kialszik</i> ‘extinguish’,<br><i>elhunyik</i> ‘pass away/ die’<br><i>összeesik</i> ‘collapse’ |

These unaccusative verbs, which Horvath & Siloni (2008) name the ‘arrive class’, have transitive alternates in other languages (see (11) above for a partial demonstration). Their transitive alternates are only idiosyncratically absent from the vocabulary of certain languages. This leads Horvath & Siloni (2008) to conclude that arrive class unaccusatives have frozen (hidden) transitive alternates.

The second class of unaccusatives with no transitive alternate in the vocabulary is a group of verbs Horvath and Siloni name the ‘appeal class’, which consists of two-place object-experiencer verbs. Appeal class verbs have a nominative argument that is generated internally (a theme) (Belletti & Rizzi 1988, Pesetsky 1995) and an experiencer, which (in most cases) is an object bearing oblique case (Pesetsky 1995). For example (Hebrew and English):

- (16) *ha-ra'ayon xamak mimeni*  
the-idea escaped from+me  
‘The idea escaped me.’
- (17) *ze medaber elay*  
it talks to+me  
‘It appeals to me.’

Unlike verbs in the arrive class, for which we can find a transitive alternate in the vocabulary of one language or another, appeal class unaccusatives do not have transitive counterparts in any language (Reinhart 2002). Also unlike the arrive class, they never appear to be morphologically derived and never have an adjectival passive counterpart (Pesetsky 1995). In addition, the appeal class is a crosslinguistic phenomenon. In languages where these verbs exist, they roughly refer to the same concepts. In other words, for this class of unaccusatives there is no crosslinguistic evidence to support the existence of frozen transitive alternates.

Based on these observations, Horvath & Siloni (2008) conclude that appeal class unaccusatives are crosslinguistically not derived. They have no corresponding transitive entry, not even a frozen one.<sup>7</sup>

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<sup>7</sup> Horvath & Siloni suggest that the operation forming adjectival passives applies in the mental lexicon and requires transitive entries as input. This can account for the correlation between the lack of crosslinguistic evidence for transitive entries and the absence of adjectival passive alternates. For further details see Horvath and Siloni (2008).

5. *Predictions: The GABLE hypothesis*

In light of the fact that the mental lexicon is commonly assumed to have an interface with the conceptual system, I will now present a general hypothesis regarding the connection between lexical encoding and the salience of concepts.

(18) GABLE (graded accessibility by lexical encoding) (Fadlon 2008):

The relative salience level of a concept is affected by its lexical encoding.

- (a) The existence of a word in the vocabulary of a language X is an accessibility enhancer for the concept it represents.
- (b) The existence of a lexical entry (visible or hidden) is also an accessibility enhancer for the concept it represents.

According to the GABLE hypothesis a concept with a corresponding vocabulary item will be more accessible than a concept without one and a concept with a corresponding lexical entry will be more accessible than a concept without one. In other words, it predicts a difference between hidden and non-existent lexical entries. Thus, provided with a method designed to measure the accessibility levels of concepts, the GABLE hypothesis can be used to detect the existence of hidden lexical entries.

Returning to our case study, given Horvath and Siloni's distinction, there are three possible types of unaccusative verbs:

- (i) Derived, with a transitive alternate in the vocabulary (*break*)
- (ii) Derived, with a frozen transitive alternate (*fall, vanish*)
- (iii) Underived, with no transitive alternate at all (*appeal, escape*)

These three types of unaccusatives are related to the following three types of logically possible transitive concepts:

- (I) Transitive concepts with a corresponding vocabulary item (*making something break*)
- (II) Transitive concepts with a corresponding frozen lexical entry (*making something vanish*)
- (III) Transitive concepts with no lexical entry at all, not even a frozen one (*making something escape the mind of someone*)

With regard to (i)-(iii) and (I)-(III) the GABLE hypothesis makes two predictions. First, if frozen lexical entries are psychologically real, the GABLE hypothesis predicts a three-way contrast. If type (ii) unaccusatives indeed have a hidden transitive alternate, in contrast with type (iii) unaccusatives, the GABLE hypothesis predicts that type (I) transitive concepts, with two accessibility enhancers (vocabulary item and lexical entry) will be more accessible than type (II) transitive verbal concepts, with one accessibility enhancer (a hidden lexical entry). In turn, type (II) concepts should be more accessible than type (III) transitive concepts, which have no accessibility enhancers.

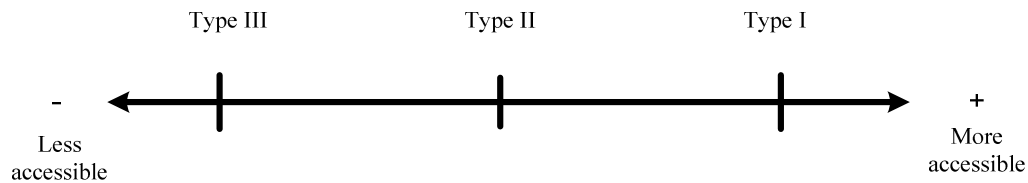


Figure 2

The observation of a three-way contrast, in which concepts with a corresponding lexical entry are distinguished from concepts with no lexical representation at all, would be consistent with explanation (A) above, which uses the notion of hidden lexical entries to explain gaps in the transitive-unaccusative alternation. In contrast, a three-way contrast would be inconsistent with explanation (B), which only distinguishes between forms with a corresponding vocabulary item and forms without one.

The Second prediction the GABLE hypothesis makes in this context, is that if frozen lexical entries are not psychologically real, a two-way contrast should be observed. If type (ii) unaccusatives do not have a hidden transitive alternate and are identical to type (iii) unaccusatives with regard to lexical encoding, the GABLE hypothesis predicts that type (I) transitive concepts (two accessibility enhancers) should be more accessible than type (II) and (III) transitive concepts (no accessibility enhancers). In other words, the accessibility levels of type (II) and (III) are not expected to be different.

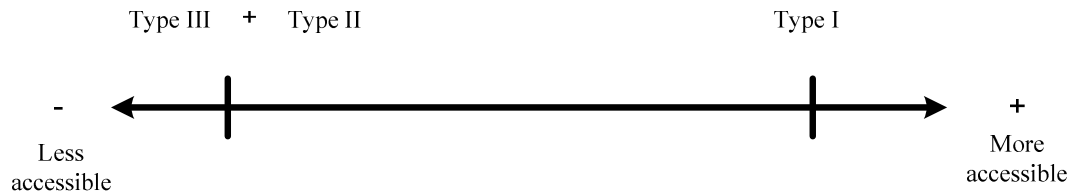


Figure 3

The observation of a two-way contrast would be inconsistent with a theory like explanation (A) that relies on the mechanism of hidden lexical entries, since such a theory predicts a three-way contrast. However, a two-way contrast would be consistent with explanation B which predicts a two-way contrast between unaccusatives with a corresponding transitive in the vocabulary (word) and unaccusatives without one.

## 6. Experiment

The following experiment was designed to measure the relative salience levels of type (I), (II) and (III) transitive concepts once a speaker is exposed to their unaccusative verbal variant.

### 6.1. Participants

Participants included 20 adult native Hebrew speakers, 10 male and 10 female, with an education level of 13 years or more. Participants' age ranged between of 19 and 29 (mean age 24.1). None had any relevant prior linguistic education.

### 6.2. Stimuli

The stimuli consisted of 8 unaccusative verbs with a corresponding transitive in the vocabulary (type i); 7 unaccusative verbs with no transitive alternate in the vocabulary, but with a parallel adjectival passive alternate (which, following Horvath & Sioni 2008 was taken to suggest they have a frozen/hidden transitive alternate) (type ii); and 6 unaccusative verbs with no transitive alternate, no adjectival passive alternates and no known cases of transitive alternates in the vocabulary of languages other than Hebrew (type iii).<sup>8</sup>

### 6.3. Task

For each of the 21 unaccusative verbs, a very short story was composed. The stories included scenarios that established the status of a participant as the causer of some event through relevant content. In each story the event was only labeled towards the end of the vignette, using a sentence with one of the unaccusative verbs ('the unaccusative sentence'). The very last sentence of the story described an outcome of the event ('the outcome sentence').

Subjects were then asked to give a 1 (least acceptable) to 8 (most acceptable) rating to describe the extent to which they perceive the causer of the event to be 'the executer of a specific action' that resulted in whatever the 'outcome sentence' described. The rationale behind this task was that the more accessible the transitive concept of the relevant event is for speakers, the higher they will rate its causer as the 'the executer of a specific action'. Examples (19) and (20) are translated examples of short stories and stimuli sentences composed around the unaccusatives *hipil* 'fall', a Hebrew type (i) unaccusative, and *xamak me-* 'escape (the mind of)', a type (iii) unaccusative.

- (19) *John and Mary are twins. They are a bit similar and a bit different. John, for example, is a clumsy boy who always drops stuff and Mary is not clumsy at all. In fact, teasing John about his clumsiness is Mary's very annoying habit. Last week they were on their way to Grandma's house for a holiday dinner. John carried the cake and his sister Mary carried a glass dish that contained some fish. The dish was cold, slippery and heavy. Mary felt how it began to slip out of her grip.*

*The dish **fell** on the sidewalk. John gloated.*

*On a scale of 1-8, how acceptable/conceivable for you is to consider Mary as the executer of **one specific action** that resulted in the gloating of John?*

In order to grade Mary's role as the executer of an action which led to John's gloating, subjects must activate the transitive concept parallel to the unaccusative *fall* – 'the concept of making something fall'. The more accessible this concept is for them, the higher they should rate Mary as the causer.

- (20) *Danny was very happy he was elected to be the chairman of the prom's decoration committee. He was excited about the chance to finally express his creative side. Unfortunately, as the date approached, Danny found it very difficult to come up with a good enough decorating idea. It was about two days before prom night when Danny made himself sit in his room and think real hard. After sitting there for nearly two hours,*

<sup>8</sup> For a full list of unaccusative verbs used in the experiment see appendix A.

*it seemed to him that this method might be working and that an idea should pop up very soon. But – alas! in the exact moment he felt an idea getting structured in his mind, his sister shouted from the other room: ‘Danny, would you be a dear and make me a sandwich?’*

*The idea **escaped** Danny's mind. It was now completely empty.*

*On a scale of 1-8, how acceptable/conceivable for you is to consider Danny's sister to be the executer of **one specific action** that resulted in the emptiness of his mind?*

In order to grade Danny's sister's role as the executer of an action which led to Danny's mind being empty, subjects must activate the transitive concept parallel to the unaccusative *escape* – ‘the concept of making something escape the mind of someone’.

The GABLE hypothesis predicts that for each unaccusative, if the relevant transitive concept has a parallel vocabulary item in Hebrew, it should be more salient than transitive concepts without a parallel vocabulary, and that if the relevant transitive concept does not have a parallel vocabulary item, but does have a parallel lexical entry (a hidden lexical entry) it should be more salient than a concept with no representation in the mental lexicon at all. These different salience levels should positively correspond with the acceptability ratings that subjects give to the causers of the events described by the unaccusative verbs.

#### 6.4. Procedure

Each subject participated in a short, one on one, training session which included two items. Subjects that did not show a full understanding of the judgment they were asked to give were excluded from the experiment.

The experiment was performed using a Power-Point slide show and an answer form. The first slide included the first story. Only in the next slide, after pressing the ENTER key, did the subject encounter the ‘unaccusative sentence’, the ‘outcome sentence’<sup>9</sup> and the task. This separation was maintained in order to ensure that it is the unaccusative's verbal concept subjects was considering while answering and not any other verbal concept mentioned in the story. Also for this purpose, subjects were instructed that once they have moved on to the next slide they cannot go back to any previous one. In addition, the unaccusative verbs appeared in boldface.

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<sup>9</sup> Outcome sentences were used as mediators between the event described by the target unaccusative and the tasks for two reasons. First, a task which includes a direct question regarding the event described by the unaccusative would have to include the linguistic counterpart of the transitive concept whose salience we wish to measure (*how acceptable/conceivable is it for you to consider Mary as causing the dish to fall?*). This might interrupt the purpose of the experiment, since it could make all three types of transitive concepts highly accessible, which would blur the differences between them. Second, the use of outcome sentences as mediator differentiates the task from simple content questions. In order for subjects to concentrate on giving the intuitive judgment they were asked to give, it was important that they will not feel as though they are being quizzed on the content of the short story they have just read.

6.5. Results

A t-test for correlated samples, performed on a list of 20 mean ratings (one average per subject) showed that the mean ratings for the group of type (i) unaccusatives were significantly higher than the mean ratings for the group of the type (ii) unaccusatives (one-tailed:  $t(19)=6.12$ ,  $p<0.0001$ ), and that the mean ratings for the group of type (ii) unaccusatives are significantly higher than the mean ratings for the group of type (iii) unaccusatives (one tailed:  $t(19)=3.12$ ,  $p=0.003$ ).

The same pattern was revealed by a Wilcoxon signed-rank test comparing speakers' median acceptability rating for each verb type: type (i) unaccusatives were rated significantly higher than type (ii) unaccusatives (one tailed:  $W(17)= 0$ ,  $p<0.0001$ ), and type (ii) unaccusatives are significantly higher rated than type (iii) unaccusatives (one tailed:  $W(17)=31$ ,  $p=0.017$ ).

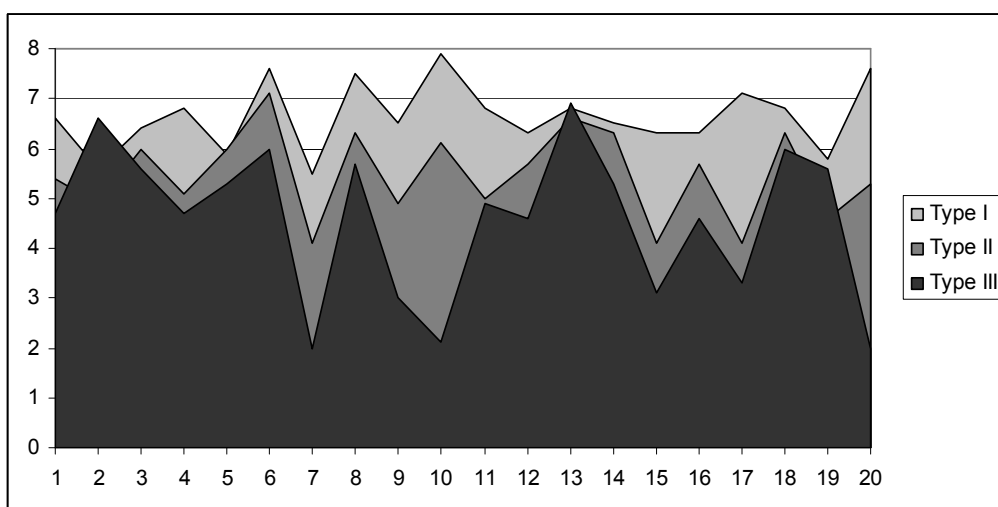


Chart 1: means of ratings per subject

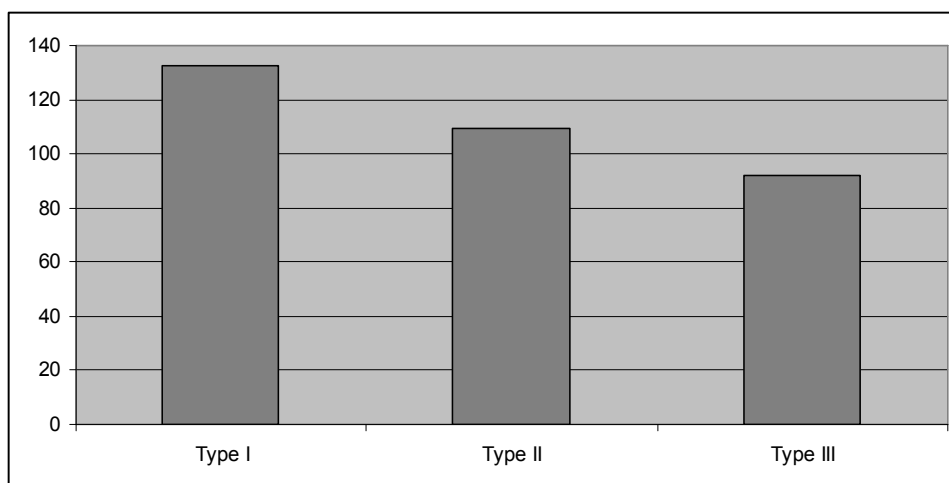


Chart 2: sums of average ratings

### 7. Discussion

As demonstrated above, a statistical analysis of the results found the ratings for type (I) concepts significantly higher than the ratings for type (II) concepts, and the latter significantly higher than the ratings for type (III) concepts. In other words, subjects graded causers of events described by type (i) unaccusatives higher than causers of events described by type (ii) unaccusatives, and causers of events described by type (ii) unaccusatives higher than causers of events described by type (iii) unaccusatives. These results are in accordance with the GABLE hypothesis's prediction given the assumption that frozen lexical entries are psychologically real: namely, a three-way contrast between type (I), type (II) and type (III) transitive verbal concepts.

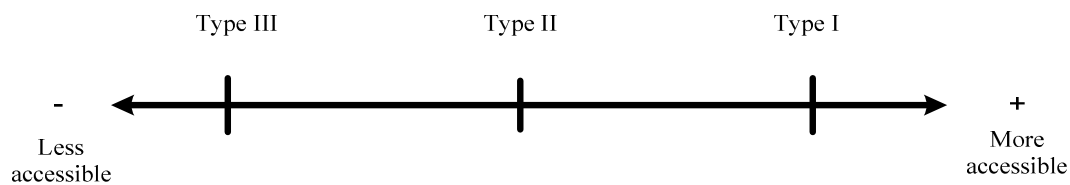


Figure 4

Considering the pattern of salience observed, it is clear that some property, uniquely possessed by type (II) transitive concepts renders them more accessible than type (III) transitive concepts, even though both types lack a corresponding word in the vocabulary of Hebrew. This is evidence in favor of explanation A, which uses a hidden lexical entries mechanism to explain gaps in the transitive-unaccusative alternation, and against explanation B, which does not.

Explanation B cannot account for the significant difference found between type (II) and type (III) transitive concepts. Recall that according to this type of theory, derivational gaps occur when a part of a derivational alternation simply does not exist in a particular language. Consequently, as far as explanation B is concerned, type (II) and type (III) transitive concepts are the same. They both lack a corresponding vocabulary item as well as a mental lexicon representation.

On the other hand, according to the hidden lexical entries mechanism assumed by explanation A (Reinhart 2002, Horvath & Siloni 2008), while type (II) transitive verbal concepts are listed in the mental lexicon in a hidden way (idiosyncratically marked as unable to be inserted into syntax), type (III) verbal concepts are not listed at all. As a result, under the assumption that the lexical encoding of a concept has an effect on that concept's perception, this explanation predicts that the salience levels of type (III) concepts will be lower than the salience levels of type (II) concepts. As stated, this prediction is consistent with the results of the current experiment.

### 8. Summary and conclusion

This paper has shown that the assumption of hidden lexical entries is not unfalsifiable. Keeping in mind that distinct faculties of human knowledge interact with one another, it is easy to see that even though hidden lexical entries are not used in utterances, there is a way to tap into their psychological reality. According to the GABLE hypothesis, the lexical encoding

of a concept has an effect on its salience level. As a result, the existence of a hidden lexical entry can be revealed through an estimation of its parallel concept's salience level.

With regard to the case study of gaps in the transitive-unaccusative alternation and the particular hidden lexical entries analysis of frozen lexical entries (Reinhart 2002, Horvath & Siloni 2008), the GABLE hypothesis predicts that transitive concepts related to unaccusatives with a transitive counterpart in a speaker's vocabulary will be more accessible than transitive concepts related to unaccusative that are derived from frozen lexical entries, and that latter will be more accessible than transitive concepts related to underived unaccusatives. The results of the current experiment are consistent with these predictions, thus providing evidence in favor of this particular mechanism and against the alternative account (explanation B).

The results of the experiment constitute a step toward showing that the inclusion of hidden lexical entries in a theory of the lexicon is more than a useful theoretical tool; the next step is a cross-linguistic research. It is important to remember that even though derivational gaps are a universal phenomenon, they have language specific manifestations; for instance, the set of type (ii) unaccusatives is not the same across languages. Therefore, relevant independent evidence from other languages is essential in probing the psychological reality of hidden lexical entries.

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## Appendix A: Hebrew unaccusatives used in the experiment

<b>Type (i) Unaccusative</b>	<b>Transitive (Cause external theta role)</b>
<i>hitkavec</i> ‘shrank’	<i>kivec</i>
<i>nafal</i> ‘fell’	<i>hipil</i>
<i>hitkamet</i> ‘got wrinkled’	<i>kimet</i>
<i>hitlaxlex</i> ‘got dirty’	<i>lixlex</i>
<i>nigmar</i> ‘was finished’	<i>gamar</i>
<i>nisbar</i> ‘broke’	<i>savar</i>
<i>nisraf</i> ‘got burnt’	<i>saraf</i>
<i>nirtav</i> ‘got wet’	<i>hirtiv</i>
<b>Type(ii) unaccusatives</b>	<b>Adjectival Passive</b>
<i>hirkiv</i> ‘got rotten’	<i>rakuv</i>
<i>kamas</i> <sup>✓</sup> ‘withered’	<i>kamus</i> <sup>✓</sup>
<i>naval</i> ‘wilted’	<i>navul</i>
<i>daha</i> ‘faded’	<i>dahuy</i>
<i>hexmic</i> <sup>10</sup> ‘turned sour’	<i>xamuc</i>
<i>hexlid</i> ‘became rusty’	<i>xalud</i>
<i>hitrofef</i> ‘loosen’	<i>rafuy</i>
<b>Type (iii) unaccusatives</b>	
<i>medaber</i> ( <i>el</i> ) ‘appeals (to)’	
<i>mešane</i> ( <i>le</i> ) ‘matters (to)’	
<i>xamak</i> ( <i>me</i> ) ‘escapes from’	
<i>xaser</i> ( <i>le</i> ) ‘misses (to)’	
<i>xore</i> ( <i>le</i> ) ‘unpleasing (to)’	
<i>maca xen</i> ( <i>be-einey</i> ) ‘appeals (to the eyes of)’	

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<sup>10</sup> ‘*hexmic*’ is ambiguous between ‘turned sour’ (an unaccusative) and ‘made-pickle’ (a transitive). Nevertheless, the second is not the transitive alternate of the first.

*Appendix B: derivational gaps and non-lexicalist approaches*

The need for a mechanism which deals with gaps in an alternation is not exclusive to lexicalist models. Rather, every theory that assumes derivational relations (especially universal ones) between different realizations of the same concept should include such a mechanism.

Consider for example the English unaccusatives *break* (which has a transitive counterpart with a Cause external argument in the vocabulary), and *fall* (which does not). A theory that views *break* as derivationally related to its transitive counterpart would have to account for the case of *fall* in one of the two following ways:

- (a) Argue that this derivational relation applies in the case of *break*, but does not apply in the case of *fall*, and provide a language specific explanation for this difference.
- (b) Assume the existence of *fall's* transitive counterpart and provide an explanation for its absence from the vocabulary of English.

In the remainder of this section I suggest a way to deal with derivational gaps within a non-lexicalist model of grammar, namely Distributed Morphology (Halle & Marantz 1993, Marantz 1997).

According to the non-lexicalist model of Distributed Morphology the mental lexicon consists of 3 disjoint non-computational lists of atomic elements:

- i. List 1 ('narrow lexicon') - atomic 'roots' unspecified for lexical category and atomic bundles of grammatical features able to be combine only by syntax. For example, an unaccusative form and a transitive form of the same verbal concept (the Hebrew *nafal* 'fall' and *hipil* 'made-fall') are not listed separately. They are both represented by the root which encodes the broad concept related to them ('falling').
- ii. List 2 (the 'vocabulary') - phonological forms for terminal nodes from syntax. List 2 items compete for insertion and the most highly specified item that doesn't conflict in features with the terminal node 'wins'.
- iii. List 3 (the 'encyclopedia') - special meanings of particular roots relative to the syntactic contexts in which they appear.

The Assumption that distinct thematic instantiations of the same root are derivationally related is inherent to Distributed Morphology. Consequently, derivational gaps are a phenomenon this model must address. If an unaccusative and a transitive form of the same verbal concept are different syntactic manifestations of the same root, this model cannot disregard cases of gaps in this alternation. At the same time, the account such a model would give for derivational gaps cannot be a hidden lexical entries mechanism. It cannot include the assumption that the arbitrarily missing parts of the alternation are listed in the mental lexicon in a hidden way. This is since list 1 includes unspecified roots and not verbs of any type, and list 3 includes forms, i.e., only items with corresponding phonological forms can be listed in it. Also, due to the arbitrary and sporadic nature of these gaps, their exclusion from utterances cannot be marked in list 3. This is since there seems to be no semantic generalization to explain their absence from the vocabulary. Nonetheless, there is a way to account for derivational gaps within this framework.

Let us take a closer look at list 2. This component of the disjoint mental lexicon determines the connection between terminal nodes and their phonological realization. Therefore, it is obviously a language-specific list. Even though it has an interface with phonology, it is not necessarily governed by phonological rules. Being a listing of forms, there is no reason to assume all that list 2 related phenomena must be phonologically motivated. In other words, in this stage of the derivation certain forms can be arbitrarily ruled out from utterances.

Therefore, within the framework of Distributed Morphology, one can argue that in the case of a gap in an alternation, a form suitable for insertion at a certain terminal node is missing from a language's list 2, rendering this terminal node unable to be assigned a form and thus excluded from utterances of this language. Accordingly, in English, the terminal node created from the root that encodes the broad concept of 'falling' and the grammatical features responsible for unaccusativity, would have a suitable list 2 item, while the terminal node created from the same root and the grammatical features responsible for transitivity would not.<sup>11</sup>

This account is a type (B) explanation, since it is only able to distinguish between possible members of an alternation with a parallel form (word) and possible members of an alternation without a parallel form. As shown, this type of accounts is inconsistent with the results of the experiment presented in section 6 above.

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<sup>11</sup> Notice that for such an account to hold, the form *fall* should be marked as suitable for insertion only at unaccusative terminal nodes.

## How vowels point to syntactic structure: roots and skeletons in Hebrew and Italian

Noam Faust & Nicola Lampitelli

In this paper, we discuss theme vowels in Modern Hebrew and Italian. These vowels reveal a morphological phenomenon that cannot be attributed to the specific root or to the category-assigning head. In Modern Hebrew, theme vowels are positioned between the last two radicals, whereas in Italian they are suffixed to the root. We claim that both languages have an intermediate level which introduces these theme vowels. The difference in position follows from the non-syllabified status of Modern Hebrew roots at insertion. We further show that both languages exhibit direct merger of *n* and the root.

### 1. Introduction

Modern Hebrew (henceforth MH) displays non-concatenative morphology. The same three radicals appear in several semantically related lexical items with different interdigitated vocalizations, affixes, and prosody (=surface syllabic structure). This (usually tripartite) non-linearized morpheme is referred to as the root. We show some examples of MH roots in (1). Stress is final if not marked:

- (1) The Modern Hebrew root  $\sqrt{\text{msr}}$ <sup>1</sup>
- | Nouns              |                      | Verbs                              |
|--------------------|----------------------|------------------------------------|
| a. <i>méser</i>    | ‘message’            | <i>masar</i> ‘pass on, deliver’    |
| b. <i>mi-msar</i>  | ‘intermed. transfer’ | <i>hit-maser</i> ‘pass (football)’ |
| c. <i>masór-et</i> | ‘tradition’          |                                    |
| d. <i>ta-msir</i>  | ‘handout’            |                                    |
| e. <i>masur</i>    | ‘devoted’            |                                    |

By definition, the Semitic root can never be detected in isolation; only additional material specifies it semantically and formally.

In Italian, non-derived variable nouns consist of a stem and an additional theme vowel, which may or may not carry grammatical information (*-i* is always plural; *-o* is always masculine; but *-e* may be masculine, feminine or plural):<sup>2</sup>

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<sup>1</sup> We use the traditional notation for MH, rather than the IPA. <c, š> correspond to IPA [ts, ʃ] respectively. For Italian we use the standard spelling.

<sup>2</sup> There is one exception to the *-o* generalization: *mano – mani* ‘hand(s)’ is feminine.

## (2) Italian variable nouns

	singular	gender	plural	gender	gloss
a.	<i>lup<u>o</u></i>	M	<i>lup<u>i</u></i>	M	'wolf'
b.	<i>ros<u>a</u></i>	F	<i>ros<u>e</u></i>	F	'rose'
c.	<i>can<u>e</u></i>	M	<i>can<u>i</u></i>	M	'dog'
d.	<i>nav<u>e</u></i>	F	<i>nav<u>i</u></i>	F	'ship'
e.	<i>poet<u>a</u></i>	M	<i>poet<u>i</u></i>	M	'poet'
f.	<i>al<u>a</u></i>	F	<i>al<u>i</u></i>	F	'wing'
g.	* <i>lup</i>	M or F	* <i>lup</i>	M or F	
h.	* <i>ros</i>	M or F	* <i>ros</i>	M or F	

The Italian stem, like the MH root, is also never found in isolation (cf. 2.g,h). Recent work in morphology (Marantz 1997) has adopted the tradition of calling such stems Roots.

This short paper examines this parallelism. We aim to characterize the lowest morphological architecture and the nature of its ingredients: the Root, the first category assigning head and the relation between them. We propose that this relation is often not direct, but rather that there is an intermediate level, which we call Tem(plate)P, between a nominal head and the root it is attached to. This is the level at which the Romance theme-vowel/Semitic vocalization is introduced.

Some lexical items (in both languages) do not bear the components we characterize as contributed by TemP. We propose that the existence of this intermediate level is not obligatory; these items, we suggest, are best analyzed as cases of direct merger of *n*-head and root. This analysis accounts for other, seemingly independent aspects of their distribution. This is how vowels point to syntactic structure.

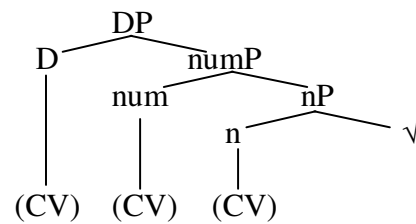
The effects of TemP or its absence are different in the two languages. We propose that this difference is the consequence of a morphophonological feature of roots: MH roots lack skeletal material (a CV tier), whereas Italian roots are fully skeletalized. While this statement is far from controversial, its implications have not been examined in detail. They are shown to be crucial in the case of such direct merger of the *n*-head and the Root.

The paper proceeds as follows. Section 2 presents some theoretical preliminaries. Section 3 motivates the introduction of TemP independently in MH and Italian. In section 4, we show that the same structure (with TemP) neatly accounts for the vast majority of nouns in both languages, if one assumes the difference between their respected roots mentioned above. Section 5 explores a case of what we claim is direct merger, and shows that it exists in both languages. Section 6 summarizes our proposals.

## 2. Theoretical preliminaries

Working within the piece-based framework of Distributed Morphology (Halle & Marantz 1993; Marantz 1997, 2001), we assume the basic form of a noun in (3). The present study will be concerned only with the range between NumP and the Root. As the diagram shows, the spelled-out forms of a head may be skeletalized (i.e. syllabified) or not. We assume, following Lowenstamm (1996), that the only skeletal constituent is a CV unit. In concentrating on the relation between syntactic nodes and minimal phonological material, we follow the specific implementation of Distributed Morphology found in Piggott & Newell (2006) and Lowenstamm (2008), and similar work.

## (3) Basic Nominal structure



The spelled-out form of a certain head or feature can be of several kinds. Leaving aside empty and expletive markers, the following three types of spell-out are identified in Bendjaballah & Haiden (2008):

## (4) Types of spell-out

a. segmental (floating)	b. skeletal	c. segmental & skeletal
ka		k a
	CV	CV

Especially important in the understanding of our analysis is the Theory of Elements (Kaye et al. 1985, henceforth KLV). According to that theory, segments consist of elements and their combination. Here we will only be concerned with the inner structure of vowels, presented in (5). Simplex vowels consist of only one element; complex vowels are the combination of two elements.

## (5) Simplex and Complex vowels

- a. [a] = /A/    b. [i] = /I/    c. [u] = /U/  
 d. [e] = /I.A/    e. [o] = /U.A/

3. An intermediate level between *n* and the root

## 3.1. Modern Hebrew and Italian roots

We have seen in (4) that a spelled-out form can be segmental, skeletal or both. The question is now raised as to the nature of the spelled-out form of the root: we will see that MH and Italian differ in this respect.

Let us start with Italian roots and make the following observations. First, the root is a pronounceable stem.<sup>3</sup> Secondly, such radical stems may not be infixed: they are linearized and impenetrable. Thirdly, there is no limitation on the number of consonants or vowels in this stem (phonological constraints aside).

These characteristics are not found in MH roots. These are non-pronounceable, abstract morphemes. They are limited (in principle) to three radicals which *a priori* associate to consonantal positions. As a consequence, additional material can be inserted *between* the radicals.

These points are not new. That MH (and Semitic) roots, unlike Romance roots, are discontinuous is evident. But what does this difference point to? We propose that the discontinuity exhibited by Semitic roots (and, in fact, non-concatenative morphology in

<sup>3</sup> By pronounceable, we mean that they are syllabified sequences.

general) is a consequence of their exclusively segmental (i.e. not skeletalized) nature. An MH root enters the derivation with no skeletal material. Italian roots, on the contrary, are fully skeletalized.<sup>4</sup>

If so, the skeleton for the root in MH has a different origin than in Italian. As we will see, skeletal material is provided by the structure that the root is inserted into. This view is supported by another difference between MH and Italian *lexical items* (as opposed to roots): in Italian, the internal prosody of the item is not indicative of its lexical category: a CCVCV sequence may be a noun, a verb, an adjective etc. This follows if Italian roots are inserted with their skeleton. But in MH, as we will see, the internal prosody *may* be indicative of lexical category: a sequence CCVC in isolation can only be a noun, but CaCVC can be either a noun or an adjective. This fact can only be explained if one assumes that the root lacks skeletal material, and that this material is provided by additional structure.

We will come back to these points later. We now move to the examination of the data and structures of both languages.

### 3.2. “Theme Vowels” in Modern Hebrew

As mentioned, Semitic roots cannot appear in isolation; they surface only as combined with further lexical material, referred to as the *template*. Every template has at least one lexical site between the radicals, which hosts the distinctive *vocalization* of that lexical item. For example, the items *katan* ‘small’ and *katin* ‘juvenile person’ have the same template QaTV<sub>2</sub>L, with a lexical site in V<sub>2</sub>. (Q,T,L standing for the root consonants). The only difference is the quality of the vowel (in bold) that fills that position, i.e. their vocalization.

V<sub>2</sub> is the lexical site in all templates in Modern Hebrew, as elsewhere in Semitic: any vowel may be found in this position. The vowel in V<sub>1</sub>, in contrast, is very rarely other than *i* or *a*. The vowels in V<sub>2</sub> are thus referred to as the “theme” of the lexical item (e.g. Goldenberg 1994).

#### (6) MH lexical items (cf. Faust in prep.)

##### a. nouns

<i>Template</i>	<i>singular</i>	<i>gender</i>	<i>gloss</i>
/QTvL/	a. klaf	M	‘card’
	b. bdixa	F	‘joke’
/maQTvL/	e. macpun	M	‘conscience’
	f. makpeca	F	‘trampoline’
/miQTvL/	g. mixšol	M	‘obstacle’
	h. mištara	F	‘police’

##### b. nouns/adjectives

/QaTvL/	c. ragiš	M	‘sensitive’
	d. kvuca	F	‘group’

##### c. verbs

(h)vQTiL	i. hikpic~yakpic		‘jump (caus.)’
QvTTeL	j. kipec~yekapec		‘jump (iter.)’
QaTvL	l. kafac~yikfoc		‘jump’

<sup>4</sup> The idea of Semitic roots as lacking a skeleton originates in Lowenstamm (2004). In that handout, the author derives the apparent triradicality restriction on Semitic roots from the syntactic structure rather than from some independent principle.



Indeed, the  $V_2$  vocalizations in (6) behave not unlike Romance theme vowels. On the one hand, we observe that the presence of a lexically determined vowel in  $V_2$  is true regardless of the category. It is thus not a property of one specific category-defining head. On the other hand, one cannot say that  $V_2$  is part of the root, since the same root may appear with different vocalizations in  $V_2$ . These facts lead us to conclude that there is an intermediate projection between the root and the category-assigning head that introduces  $V_2$ . We call this projection *Temp*.

### 3.3. “Theme Vowels” in Italian

As shown in (2) above, a general feature of Italian nouns is that they must end in a vowel, the only exception being loanwords such as *film* ‘movie’. This final vowel changes according to the number feature of the noun. An important property distinguishes two different groups of nouns. If the final vowel is stressed, then [a], [i], [e], [o] and [u] are allowed, and the noun is invariable - the final vowel is the same for both plural and singular (*virtú* ‘virtue (sg. or pl.)’). On the other hand, if the final vowel is unstressed, then i) the vowel [u] is excluded; and ii) the noun is variable. The data in (2), which represent the vast majority of Italian nouns, belong to this second set. Their final vowels are often referred to as “theme” vowels (cf. Benveniste 1984). We will refer to them as  $V_{fin}$ . The set of possible  $V_{fin}$  vowels is presented in (7):

- (7)  $V_{fin}$  inventory
- |  |   |
|--|---|
| <p>a. <i>Singular</i></p> <p style="text-align: center;">e                      o</p> <p style="text-align: center;">                            a</p> | <p>b. <i>Plural</i></p> <p style="text-align: center;">i</p> <p style="text-align: center;">                            e</p> |
|--|---|

We recast these data as in (8), decomposing each  $V_{fin}$  according to the Theory of Elements (cf. *supra* (5)):

- (8) Decomposed  $V_{fin}$  inventory
- |  |   |
|--|---|
| <p>a. <i>Singular</i></p> <p style="text-align: center;">A.I                      A.U</p> <p style="text-align: center;">                            A</p> | <p>b. <i>Plural</i></p> <p style="text-align: center;">I</p> <p style="text-align: center;">                            A.I</p> |
|--|---|

The analysis in (8) points to the following generalization: /A/ must always be present in the singular, while /I/ is always present in the plural. Both /A/ and /I/ may appear alone, whereas the element /U/ cannot. Lampitelli (2009) proposes the following definition for /A/ and /I/:<sup>5</sup>

- (9) Number markers
- a. /A/ marks the singular (henceforth  $A_{sg}$ )
  - b. /I/ marks the plural (henceforth  $I_{pl}$ )

In the light of (9), we propose to regard  $V_{fin}$  as the result of the phonological fusion between two distinct elements, one being the number marker the other being an unpredictable and undefined element as shown in (10):

<sup>5</sup> Passino (2008a) explores a similar analytic path. Acquaviva (2008b) does not decompose these theme vowels, but argues that they represent the fusion between gender and number.

(10) Decomposing  $V_{fin}$ 

sg.	gender	pl.	gender	class	examples
$V_{fin}$		$V_{fin}$			(from (2))
a. $A_{sg} + \underline{U}$	M	$I_{pl} + \underline{U}$ <sup>6</sup>	M	{1}	<i>lupo</i>
b. $A_{sg} + \underline{A}$ <sup>7</sup>	F	$I_{pl} + \underline{A}$	F	{1}	<i>rosa</i>
c. $A_{sg} + \underline{I}$	M/F	$I_{pl} + \underline{I}$	M/F	{2}	<i>cane, nave</i>
d. $A_{sg} + \emptyset$	M/F	$I_{pl} + \emptyset$	M/F	-	<i>poeta, ala</i>
↑     ↑		↑     ↑			
regular	unpredictable	regular	unpredictable		

Number markers are totally predictable: in the structure in (3) above, they are introduced by the projection  $numP$ . The main point made by (10) is that the exponent of gender is not stable: for instance, a [feminine] feature can correspond to either /A/ or /I/ (10.b,c), and this is not a predictable property. We thus propose the classification in (10): in class {1}, /U/ and /A/ express M and F, respectively; in class {2}, /I/ expresses both genders. In other words, there are class diacritics involved. Assuming that all that nouns contribute to the structure is gender, these diacritics cannot be carried by the  $n$ -head.<sup>8</sup> On the other hand, they cannot belong to the root either, as roots may not bear diacritics or labels (cf. Marantz 2001; Acquaviva 2008a; contra Embick & Halle 2005). Therefore, an intermediate level is required.

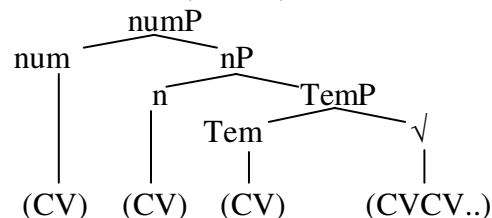
Proposing an intermediate level entails making the claim that  $V_{fin}$  is not just a property of nouns, because it is introduced lower than the category-assigning head, and could in principle serve as the basis for other categories. Indeed, Italian adjectives and verbs also carry such thematic vowels. We conclude that the information on class is introduced in the structure by the projection  $Temp$ .

## 4. The unified analysis: MH and Italian use the same ingredients

The previous section showed that both MH and Italian have an intermediate level between the root and the nominalizing head. This level was labeled  $Temp$ . This section shows how this idea is implemented and how the syntactic structure is linearized in each language.

We analyze the nouns that carry the morphological markers introduced by  $Temp$  as involving the structure in (11):

## (11) MH and IT nouns structure (default):



In the structures that follow, the head  $Temp$  introduces that lexical material which on the one hand is not indicative of a lexical category and on the other does not belong to the root.

<sup>6</sup> KLV's Theory of Elements predicts that in a five-vowel language like Italian, front rounded vowels can't exist (\*[y], \*[œ], \*[ø], etc.): the combination /I.U/ would result in this illicit [y]. Cf. Passino (2008a).

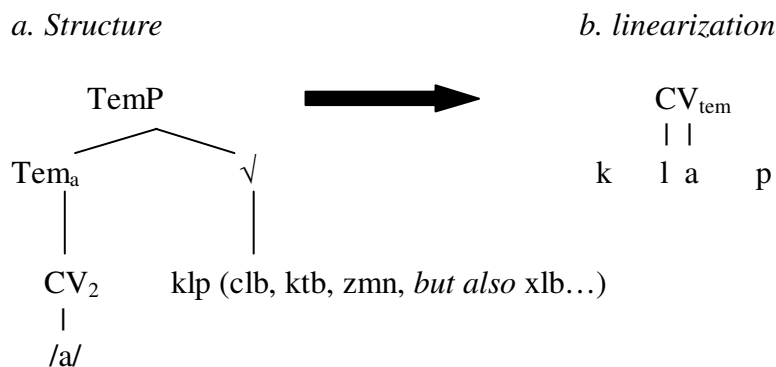
<sup>7</sup> Lampitelli (2009) assumes that two different /A/ exist in the structure of *rosa*-type nouns.

<sup>8</sup> Kihm (2002) and Lowenstamm (2008) argue that gender is a property of  $n$ .

Merger of the root and *Temp*P results in the arbitrary aspects of the form of a lexical item. Higher in the structure, the category and number markers (in the case of nouns) are added.

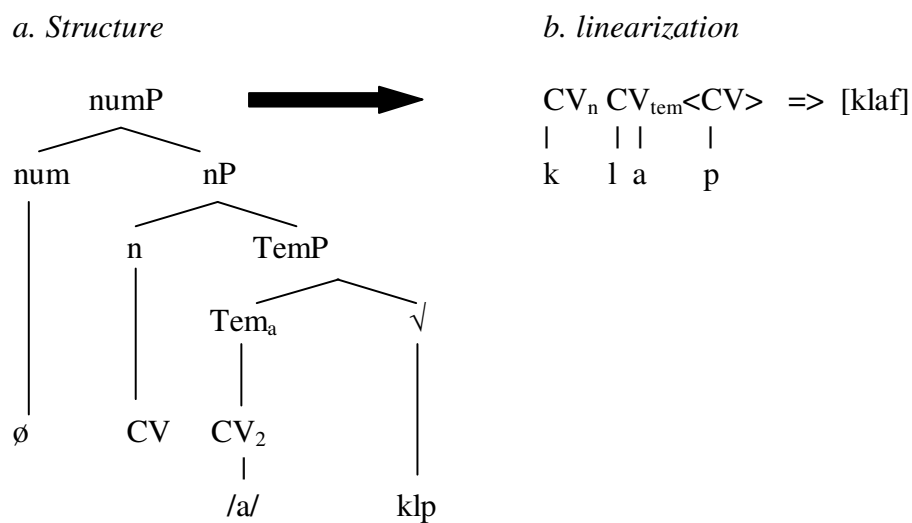
Let us start with an example from MH. In (12), a derivational site is activated within the root by the merger of a templatic head. By assumption, all templatic heads activate the CV<sub>2</sub> position. The vowel that accompanies CV<sub>2</sub> spells out a class diacritic on the *Tem* head. Only a subset of MH roots can be inserted in this structure (see Acquaviva 2008a). This subset of roots includes  $\sqrt{\text{klp}}$ ,  $\sqrt{\text{zmn}}$  and  $\sqrt{\text{ktb}}$ , which will eventually result in the nouns *klaf* ‘card’ *zman* ‘time’ and *ktab* ‘writing’; but the set also includes the basic ingredients for other nouns (*bacal* ‘onion’) or adjectives (*katan* ‘small’). Whatever the root is, one skeletal CV constituent is provided, and that constituent *always* ends up hosting the penultimate consonant (*l* in (12b)), alongside the lexical vowel.

(12) *klaf* ‘card’, M sg.



The linearization in (12b) is only given for representational purposes. In fact, *Temp*P cannot be spelled out on its own, because it is not a lexical category. Lexical categories are introduced by additional structure, as (13) demonstrates for a noun derived from (12):

(13) *klaf* ‘card’, M sg.



Nominal heads, like other major lexical categories, introduce at least one CV unit (Lowenstamm 1999, 2008). When the template is nominalized, another CV constituent is

added to the structure (marked above as  $CV_n$ ).<sup>9</sup> Further up in the structure, [sg] is just the lack of any feature on the *num* head in MH. Therefore, no CV constituent is added by the *num* head.

This structure is linearized as in (13b). The initial radical *k* is linked to  $CV_n$ . The final radical *p*, which does not have a skeletal position to be linked to, is provided one by an automatic process of *skeletalization*: such positions are marked with angled brackets <CV>. This process is strictly phonological, and does not participate in morphological process; parts of the word that are thus skeletalized correspond to what is referred to as *extrametrical* syllables.

To summarize, the root enters the derivation with no skeletal material. A projection *Temp* provides a thematic vowel linked to a skeletal position. These are mapped to an internal position between the last two radicals. A category-assigning head merges with this structure, adding at least one other CV unit. Only CV positions provided by the structure are available for morphological processes. This is the end of morphological derivation; what follows is just phonological computation.

Moving on to Italian, consider the noun *lupo* ‘wolf’ in (14). Like in MH, the root initially merges with a templatic head *Tem*. This head adds its own CV constituent ( $CV_{fin}$  below). As in MH, this head carries a diacritic (“<sub>{1}</sub>” below) that determines which roots may be combined with it. In (14) it is the root  $\sqrt{\text{lup}}$ , but the same structure underlies *trampolo* ‘stilt’ *strato* ‘layer’ *nucleo* ‘nucleus’.

As in MH, *Temp* is only the partial structure of a noun. In Italian this is even clearer, since the spelled-out form of the class diacritic may be conditioned by the gender value on the nominal head. It is the element /U/ if *n* has the feature [-f], /A/ if it has the feature [+f]. In (14), the nominal head carries the gender [-f], and so the class vowel will be spelled out as /U/. Higher in the structure, number is specified as [-pl]. Regardless of class, this feature is always spelled out as /A/.

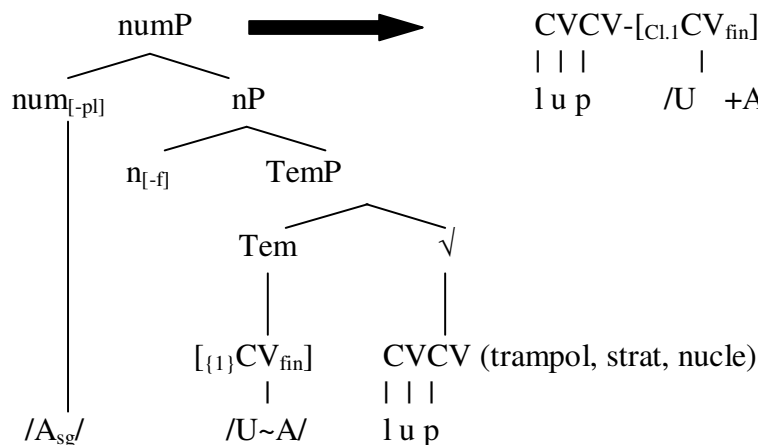
This structure is linearized as in (14b). The elements /U/ and /A/ combine to give the  $V_{fin}$  vowel [o].

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<sup>9</sup> We ignore here the question of the position of  $CV_n$ . Rucart (2006) evokes the contrast between Merge and Move for the realization of an affix as a prefix (Merge) or a suffix (Move). If he is correct, then there is no movement involved in the merger of *n* and *Temp*. Indeed, as we will see, *n* does not carry uninterpretable features that could motivate such movement, as it can be merged directly with the root as well.

(14) *lupo* ‘wolf’, M sg.

a. structure



b. linearization

CVCV-[<sub>Cl.1</sub>CV<sub>fin</sub>] => [lupo]  
 | | | |  
 l u p /U +A<sub>sg</sub>/

The only fundamental difference between MH and Italian is that in the former roots enter the structure with no skeletal positions, whereas in the latter they are inserted with such a skeleton.<sup>10</sup> That the positions and vowels introduced by TemP are internal in MH and external (suffixal) in Italian is a consequence of this fact. That aside, the structure of basic nouns in the two languages is strikingly analogous.

The next section attempts to broaden this analogy further.

### 5. Direct merger

We have seen that most items in MH have a lexical vowel in V<sub>2</sub>. A certain group of nouns stands out in this respect. It is presented in (15):

(15) MH Q<sub>v1</sub>Te<sub>2</sub>L

<i>sg.</i>		<i>fm.</i>	
a. séfer	‘book’	sifra	‘digit’
b. kélev	‘dog’	kalba	‘bitch’
c. xófeš	‘liberty’	xufša	‘vacation’
d. váʔad	‘council’	vaʔada	‘committee’
e. zóhar	‘shining’	-	(cf. zohama ‘filth’)

The lexical items in (15) are traditionally called “segholates” (owing to the name of the symbol for the vowel [e] in Biblical Hebrew). Abstracting away from the alternating quality of their V<sub>1</sub> vowel, it is quite clear that *this* position, and not V<sub>2</sub>, is the lexical site in these nouns. Indeed, the vowel in V<sub>2</sub> alternates with zero, and its quality is predictable from the surrounding consonants: next to a radical /a/ (or a historical guttural) it is [a] (15d,e); elsewhere it is [e] (15a-c). In the terms of Government Phonology (Kaye et al. 1990), there is

<sup>10</sup> That said, non-Semitic languages may come to have roots that are only *partially* skeletalized: this is arguably the case of French  $\sqrt{\text{chat}}$  ‘cat’, which is pronounced [ša] but clearly carries a final *t*, detectable in [šatō] ‘kitten’.

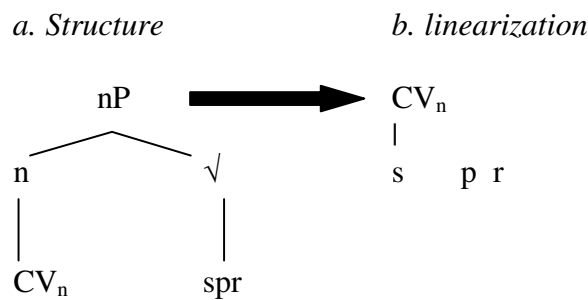
*no* vowel in  $V_2$ : if the empty nucleus between the last two radical is pronounced, it is due to a restriction on word final consonant clusters.<sup>11</sup>

Finally, segholates can only be nouns. They are thus a class apart because of three properties: i) their non-final stress; ii) their lack of  $V_2$  vocalization; and iii) their exclusively nominal nature.

Properties i) and ii) are clearly related: stress cannot be assigned to  $V_2$  because there is no vowel there. It is assigned to  $V_1$  (*sefr* => *séfr*) and then phonology applies and the cluster is broken (*séfr* => *séfer*). This analysis has two drawbacks. First, it assumes that stress applies before epenthesis. The derivational view that this implies is not uncontroversial. Second, and more importantly, it fails to explain the third property. Why should it be that items with a lexical  $V_1$  are exclusively nominal?

A view that builds templates gradually in the manner described above can account for these three properties at once. According to such a view, the lack of a lexical  $V_2$  is an indication as to the structure of segholates: they do not involve Temp. Instead, they spell out a direct merger of a category defining head and the bare root (16):

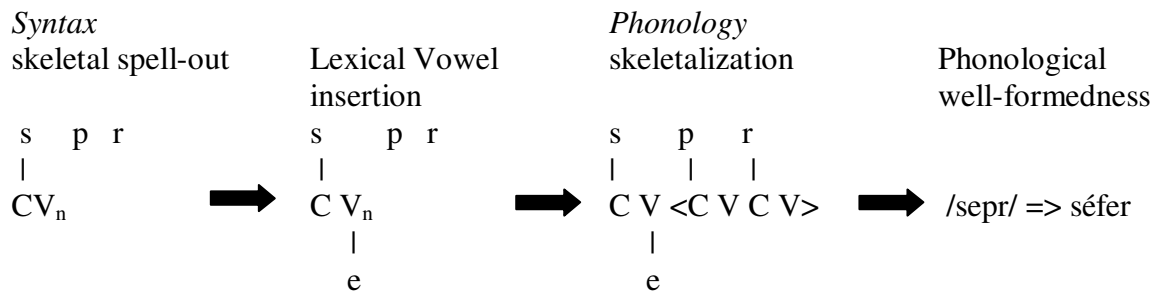
(16) Syntactic structure of segholate *séfer* ‘book’, M sg.



Such an analysis naturally explains the exclusively nominal nature of segholates: direct merger of *n* with the root can only yield a noun. Direct merger can also explain the *position* of the lexical vowel. Like in (13) above, the *n*-head is spelled out as a single CV position, and this position is linked to the first radical. Because of the lack of Temp, this is the only piece of skeleton provided by the structure. It will thus be the only position that the morphological process of stress can make reference to.

The linearization of the structure in (16) proceeds as in (17). The vowel that fills the only lexical position is inserted for the given root. This output is sent to the phonology. The first radical is linked to the syntactic skeletal position (by assumption), and the two remaining radical are skeletalized by default. This part of the skeleton is thus an extrametrical edge; it is ignored by stress.

<sup>11</sup> That empty nuclei in MH are realized as described can be seen in the following data. The  $V_1$  vowel /a/ of QaTvL adjectives only surfaces in the penultimate syllable. When the plural suffix *-im* is added,  $V_1$  is no longer penultimate, and the vowel does not surface: *paxús-pxusím* ‘flattened (ms.-pl.)’. If this results in an illicit cluster, the cluster is broken by a vowel [e]: *laxúc-lexucím* ‘pressed (ms.-pl.)’. If the first consonant of the cluster is a guttural, it is the vowel [a] that surfaces in the  $V_1$  position: *harús-harusím*. ‘destroyed (ms.-pl.)’.

(17) Linearization of segholate *séfer* ‘book’, M sg.

A segholate, if so, is the nominalization of a bare root, with no further information. One may still ask how the vocalization of a segholate is determined, that is, how do we know if V<sub>1</sub> will have an underlying /a/ (*kélev-kalba*) or an underlying /i/ (*séfer-sifra*). We have seen that TempP bore class diacritics that pointed to the vowel to be inserted in V<sub>2</sub>. This was important in accounting for the appearance of the same root with two different V<sub>2</sub> vocalizations (*katan* ‘small’ *katin* ‘underage’). The *n*-head, however, cannot carry such diacritics; that would amount to “hiding” TempP information under *n*.

Fortunately, this is not necessary. Because segholates are direct nominalizations of roots, the implicit claim is made that the V<sub>1</sub> vocalization they surface with is just a matter of a list: it may not be contrastive. And indeed, not one case was found where the same root surfaces with two different vowels in V<sub>1</sub>.<sup>12</sup> The only significant fact about segholates is that they don’t have a vowel in V<sub>2</sub>.<sup>13</sup>

The analogy to MH predicts that direct merger should also be possible in Italian. This analogy also tells us what to look for: if in MH we examined lexical items with no V<sub>2</sub>, we should look for cases with no V<sub>fin</sub> in Italian.

Such cases are absent from the “native” vocabulary of Italian. But they abound in loans. (18) demonstrates:

## (18) Consonant final nouns in Italian

- |                                   |                                       |
|-----------------------------------|---------------------------------------|
| a. <i>film</i> ‘film’             | d. <i>bancomat</i> ‘cash distributor’ |
| b. <i>šot</i> ‘shot (of alcohol)’ | e. <i>kebab</i> ‘shawarma sandwich’   |
| c. <i>ananas</i> ‘pineapple’      |                                       |

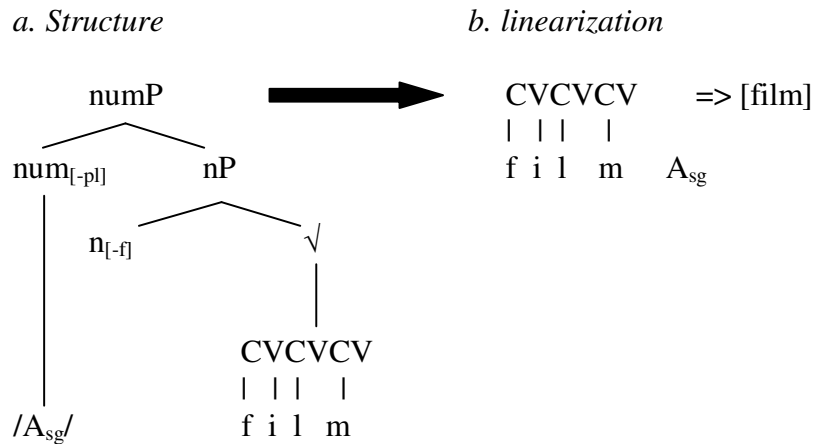
Interestingly, these nouns are invariable, i.e. they do not exhibit additional class or number morphology. This is surprising because the unattested *\*filmo* would be a perfectly legitimate noun in Italian (other, older loans have been integrated and do carry number morphology: *bistecca* ‘beef steak’). This invariability must stem from structural restrictions; notice that other languages have perfectly regular number morphology on loanwords (MH *ámbreks-ámbreksim* ‘armbreak (sg.-pl.)’; Spanish: *líder-líderes* ‘leader (sg.-pl.)’). An account of the behaviour of the nouns in (18) has to explain the lack of number morphology.

<sup>12</sup> There are no QaTL~QiTL pairs. Two pairs QéTeL-QóTeL were found, but they are less clearly derived from the same root: *réga* ‘moment’ vs. *róga* ‘calmness’; *émek* ‘valley’ vs. *ómek* ‘depth’. Moreover, in these case the vocalization with *o* does add information to the item (something like “abstract”).

<sup>13</sup> The view of the vowel in V<sub>1</sub> as unimportant is supported by other aspects of the morpho-phonological behavior of segholates, three of which we will only mention here: i. the predictability of V<sub>1</sub> before gutturals (it is always [a] as in (15d)), typical of “filler” vowels; ii. The disappearance of V<sub>1</sub> in plural formations (*kélev-klavim* ‘dog (sg.-pl.)’); and iii) the low resistance of segholate vocalisation to imposed templatic vocalization (*dégel* ‘flag’, *diḡli* ‘my flag’ but *daglan* ‘flag bearer’, *\*diḡlan*).

The analogy to *segholates* does exactly that. A noun like *film* is represented as in (19):

(19) Direct merger in Italian: *film* M sg.



The only thing that nouns like *film* lack when contrasted to other nouns is the position  $V_{fin}$ . This lack has a consequence: recall that it was this position that accommodated both the class and the number marking of regular nouns. Nouns with no  $TemP$  are therefore prevented from showing any number morphology. This is illustrated in (19b), where the singular number marking remains afloat because of the lack of the position  $V_{fin}$  (cf. (20) and the related discussion).

The claim that loanwords in Italian lack the specific piece of structure we have been calling  $TemP$  is thus doubly motivated: it explains both the lack of class marking and the lack of number marking.<sup>14,15</sup>

Before we conclude, we would like to make one further point, which will remain internal to Italian. The above analysis of *film* presupposes that number marking on the noun is dependent on a position  $V_{fin}$ , provided only by the head *Tem*. Still, one might claim that it is not the position (i.e. the skeletal material) that conditions number marking, but rather the segmental existence of a class marker (i.e. the segmental material: U, A, I). According to such a view,  $A_{sg}$  would not be realized in (19b) because it has no class vowel to combine with.

Now consider a noun like *problema* ‘problem’. Indeed, such nouns seem to be problematic: they are almost all masculine, but carry a  $V_{fin}$  marking /A/ that is usually associated with feminine nouns. This problem vanishes if we assume that in this case  $TemP$  provides only a *position*, with no segmental spell-out. The final A is *not* a class marker; it is the regular spell-out of a number head,  $A_{sg}$ .

This view is supported by the alternation of this final vowel with  $I_{pl}$  in *problemi*. If the final A were a regular class vowel, we’d expect it to behave like the vowel of *rosa* ‘rose’, to which  $I_{pl}$  is only added to yield *rose*. But if the final vowel of *problema* is simply the exponent of singular number, then we *predict* its absence from the plural form *problemi*. The

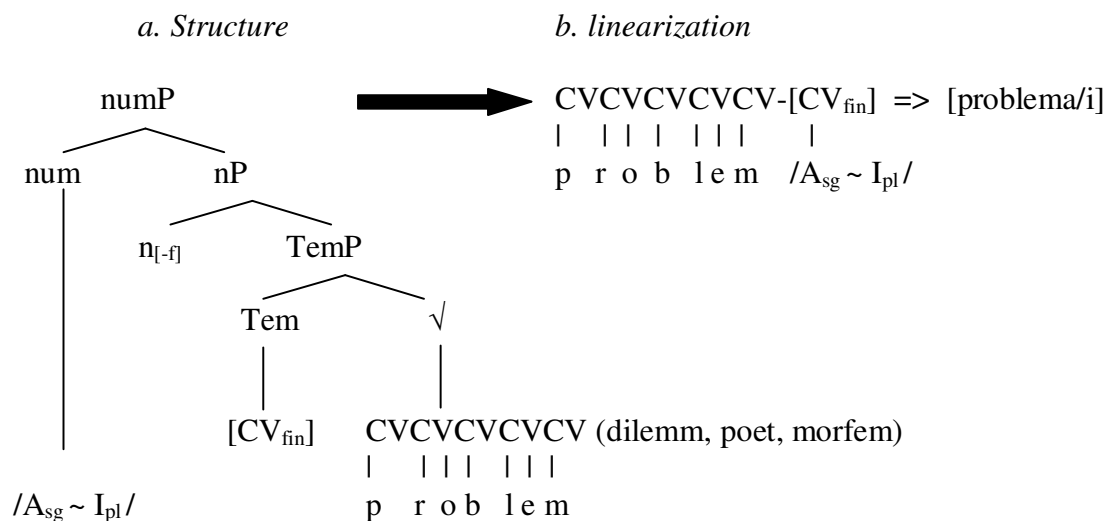
<sup>14</sup> The final consonant of loans such as *šot* ‘shot (alcohol)’ is often geminated [šott]. Passino (2008b) is an in-depth phonological analysis of this phenomenon. In this book, the author claims that the source of this gemination is “an empty CV projected by syntax to mark the end of the utterance” (p. 91). That as may be, this phenomenon is *not* to be confused with  $TemP$ . If this additional CV position were analogous to the one provided by the *Tem* head, we would not be able to explain the invariability of loans with respect to number.

<sup>15</sup> As a reviewer notes, the analogy is made between the most basic nouns in MH and loanwords in Italian.



structure of *problema* is shown in (20). As in (10) above, we assume that such nouns simply have no diacritic on the *Tem* head.

(20) *problema* ‘problem’ M sg.



To summarize, *problema*-type nouns support i) the view of *Tem* as providing skeletal – but not necessarily segmental – material; and ii) the analysis of *film* as not having this skeletal material, whence the absence of number morphology.<sup>16</sup>

This section has shown that there are cases in both MH and Italian that favour an analysis in terms of direct merger of a nominal head and the root. These structures do not have the intermediate level which we have called *Temp*. Even though the “problem” – direct merger – is the same in both languages, the effects are different: in MH, this results in restrictions on stress and lexical category, whereas in Italian it results in lack of inflectional marking.

## 6. Conclusion

This paper compared two unrelated languages, Modern Hebrew (Semitic) and Italian (Indo-European). It concentrated on the way roots enter the structure in these languages, and the implications that differences in this respect may have.

Semitic roots are notorious for their disregard for syllabic structure and vocalization. Indo-European roots rarely (if ever) behave in this manner. We have formalized this difference by claiming that Semitic roots enter the structure with no skeletal material, whereas Indo-European roots are (at least partially) syllabified at the stage of their initial insertion.

Provided this distinction, all other morphological differences are not fundamental and follow quite naturally. We have seen that both languages obey a morphological well-formedness condition having to do with a “theme vowel”. This pointed to a structural similarity: both languages have an intermediate level between the root and the first category assigning head. We have called this level *Temp*. *Temp* triggers a morphological process that applies to the root in order for it to be a regular lexical item in the language. The languages

<sup>16</sup> A reviewer raises the question of how our theory would account for the few Italian nouns of the *uovo-uova* ‘egg-eggs’ type. For a discussion of this problem, we refer the reader to Acquaviva 2008c.

are thus not different in this perspective: that the theme vowel is suffixal in Italian but infixal in MH is a consequence of the nature of their roots at insertion.

A further similarity was found in that some lexical items in both languages spell-out direct merger with the root. This is not without price: because of the lack of Temp, such cases are predicted to show morpho-phonological peculiarities, which they do.

Finally, we have seen that both languages have all three types of spelled-out exponents: exclusively skeletal, exclusively segmental, and both segmental and skeletal. It is often said that Semitic languages are “templatic”; we have reduced this generalization to one property of Semitic roots. Beyond this fact, there is no sense in which MH is more templatic than Italian. We hope this paper will constitute a (modest) step towards the comprehension of the differences – and similarities – between concatenative and non-concatenative morphological systems.

To conclude, let us say a word about the strategy employed in the present analysis. We began by treating the form of lexical items, and observing regularities in form that are essentially *not* phonological. These observations allowed us to gain insight into the structural similarities between the two languages. In other words, minute aspects of the form of an item were interpreted as reflecting its syntactic structure. This is how vowels point to syntactic structures. We believe this is a path worth taking.

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# A uniform analysis of global & local argument encoding patterns

## A local and cyclic approach

Doreen Georgi

In languages with global case splits (GCS), an overt case marker shows up on one of the arguments of a transitive verb if the internal argument is higher on a Silverstein hierarchy than the external argument. Thus, it seems that case assignment in vP either requires look-ahead or has to apply counter-cyclically. In this paper, I present a strictly local analysis of GCS in a derivational framework. To this end, I depart from the traditional assumption that case depends on the  $\phi$ -features of both arguments of a transitive verb in GCS languages. Furthermore, I show that the approach can also be extended to capture Local Case Splits, non-split case systems as well as direct-inverse marking. Besides, Burzio's Generalization falls out as a byproduct of the analysis.

### 1. Introduction

Case marking is a strategy to encode the grammatical function of arguments. The characteristic property of split systems is that an argument in the syntactic position X does not always bear the same case Y. Instead, the case exponent varies with respect to the location of the argument's properties on a Silverstein hierarchy. Arguments high on a scale are marked differently from those lower on that scale, a phenomenon called differential argument encoding.

(1) *Silverstein hierarchies (Silverstein 1976)*

- a. person scale: 1st  $\succ$  2nd  $\succ$  3rd
- b. animacy scale: human  $\succ$  animate  $\succ$  inanimate
- c. definiteness scale: pronoun  $\succ$  proper name  $\succ$  definite  $\succ$  indefinite specific  $\succ$  non-specific

Silverstein (1976) distinguishes between *local* and *global case splits* (LCS and GCS, respectively). A split is called local when the case marking of an argument solely depends on its own properties. In Hebrew, for example, an object is overtly case marked by *?et* if it is a pronoun, a name or definite. In any other configuration, it is zero-marked a (nominative):

(2) *Local Case Split in Hebrew (Aissen 2003:448)*

- a. Ha-seret herʔa ʔet-ha-milxama  
 DEF-movie showed ACC-DEF-war  
 ‘The movie showed the war.’
- b. Ha-seret herʔa \*(ʔet)-milxama  
 DEF-movie showed (ACC-)war  
 ‘The movie showed a war.’

Global case splits (GCS) differ from LCS in that the case exponent of an argument in transitive contexts does not only depend on the argument’s properties but also on those of its coargument. In the languages that will be considered in this paper, both arguments of a transitive verb are usually zero marked, but if the internal argument ( $DP_{int}$ ) is higher on a Silverstein hierarchy than the external argument ( $DP_{ext}$ ), one of both arguments receives an overt case marker. Languages differ with respect to the relevant hierarchy and the argument on which the split shows up.

In Yurok, for example,  $DP_{int}$  bears an object marker (glossed as accusative) if it is higher on the person scale than  $DP_{ext}$ , the hierarchy being 1st/2nd person  $\succ$  3rd person.

(3) *Yurok (Robins 1958:21)*

- a. keʔl nek ki newoh-paʔ  
 2SG.NOM 1SG.NOM FUT see-2<sub>i</sub>1SG  
 ‘You will see me.’
- b. yoʔ nek-ac ki newoh-peʔn  
 3SG.NOM 1SG-ACC FUT see-3SG<sub>i</sub>1SG  
 ‘He will see me.’

The following table shows a survey of GCS languages, but of course only some of them are discussed in this article.

Language	Reference
Arizona Tewa (Kiowa-Tanoan)	Kroskirty (1978, 1985)
Awtuw (Sepik-Ramu)	Feldman (1986)
Fore (Trans-New Guinea)	Scott (1978)
Kashmiri (Indo-European)	Wali & Koul (1997)
Kolyma Yukaghir (Yukaghir)	Maslova (2003)
Tauya (Trans-New Guinea)	MacDonald (1990)
Umatilla Sahaptin (Penutian)	Rigsby & Rude (1996)
Yurok (Algic)	Robins (1958)

*Table 1: Languages with Global Case Splits*

Local case splits have been discussed a lot in the literature (cf. among others Silverstein 1976; Comrie 1979; Lazard 1984; Bossong 1985; Aissen 1999, 2003; Keine & Müller 2008), but there are only a few articles on global case splits Bějar & Řezč (2007); De Hoop & Malchukov

(2008), although the latter are more problematic for derivational syntactic theories because it seems that the decision which case to assign needs a non-local representation of structure that includes both coarguments and the case assigner. Besides, all of these approaches are non-local and/or not compatible with a derivational syntax.<sup>1</sup> In the following 2 sections I present an analysis of GCS which is both local and cyclic. In section 4 I go through the derivations in detail and in the following section I show how the analysis proposed for GCS can also capture local case splits, non-split case systems and direct-inverse marking. Section 6 concludes.

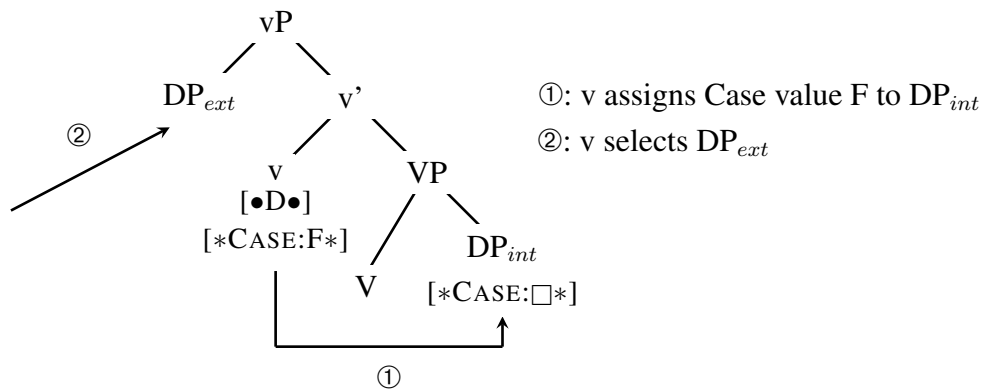
## 2. Global case splits as a challenge for locality

Under the minimalist theory of case assignment as proposed in Chomsky (2000, 2001); Adger (2003), GCS are a challenge for locality and cyclicity. The reason is that the case assigner must be able to compare the properties of two arguments in order to be able to decide which case to assign to one of them, hence a non-local (global) representation has to be accessible. However, in recent minimalist approaches it is a goal to reduce globality and to model restrictions within small subparts of the derivation. In order to understand the problems, let me first summarize the main assumptions about structure-building and case assignment in recent developments of Minimalism:

- (4) *General assumptions on structure building and case assignment:*
- a. syntactic structure unfolds bottom-up
  - b. structure building (Merge) is feature driven (by c-selection features [ $\bullet F \bullet$ ])
  - c. unvalued features [ $*F: \square *$ ] have to be valued by Agree (probe-goal approach)
  - d. DPs are assigned a case value by a c-commanding functional head
  - e.  $v$  assigns case to  $DP_{int}$  and selects  $DP_{ext}$ <sup>2</sup>.
- (5) *Structure of  $vP$  in transitive contexts:*

<sup>1</sup>See Keine (this volume) and Keine (to appear) for another local approach to GCS.

<sup>2</sup>This assumption is crucial for the analysis I will present in this paper. It is, however, incompatible with an approach that dissociates the tasks of  $v$  and postulates two different heads for each - one head which checks the case of the internal argument and another one which introduces the external argument (cf. among others Kratzer 1996; Collins 1997; Harley 2009)



If GCS are derived in the same way, a dilemma arises. For concreteness, assume we want to derive GCS in a language like Yurok with the split on the internal argument.  $v$  can assign accusative to DP<sub>int</sub> before DP<sub>ext</sub> is merged (①  $\succ$  ②), but then case valuation requires **look-ahead**, because the case value of DP<sub>int</sub> also depends on the properties of DP<sub>ext</sub> which is not merged. Alternatively, DP<sub>ext</sub> could be merged before  $v$  assigns case to DP<sub>int</sub> (②  $\succ$  ①). There is no look-ahead problem anymore, but case valuation is **counter-cyclic** given the Strict Cycle Condition in (6).

(6) *Strict Cycle Condition (SCC, cf. Chomsky 1973):*

- a. No operation can apply to a domain dominated by a cyclic node  $\alpha$  in such a way as to affect solely a proper subdomain of  $\alpha$  dominated by a node  $\beta$  which is also a cyclic node.
- b. Every projection is a cyclic node.

Apart from the look-ahead and the cyclicity problem a further issue arises: How can the case assigner  $v$  communicate with the features of both of its arguments? It must "know" about the properties of the coarguments in order to be able to decide which case to assign. Furthermore, the question arises how the case value on  $v$  is finally fixed (by a feature changing operation or an insertion rule that violates Inclusiveness).

Hence, none of the orders of operations on  $v$  is in accordance with the assumptions in (4). In the following sections I present an analysis which reconciles GCS with the derivational Minimalist framework.

### 3. Analysis

#### 3.1. A change of perspective

In order to circumvent the look-ahead problem I propose to adopt a different perspective with respect to the GCS data. GCS has always been described such that the case value of an argument depends on the features of this argument and its coargument. It is this assumption which unavoidably leads to the look-ahead problem. I suggest to abandon this point of view and to characterize the data as follows:

Table 2: GCS in Yurok

person of DP <sub>ext</sub>	case of DP <sub>ext</sub>	person of DP <sub>int</sub>	case of DP <sub>int</sub>
1st/2nd	Nom	1st/2nd	Nom
3rd person	Nom	1st/2nd	Acc
1st/2nd	Nom	3rd	Nom
3rd	Nom	3rd	Nom

(7) *A different perspective on GCS:*

It is not case marking that depends on the properties of the coarguments. The properties of DP<sub>int</sub> determine what properties DP<sub>ext</sub> can have. This means that v's c-selection properties are restricted by the properties of DP<sub>int</sub>.

Take Yurok as an example, where DP<sub>int</sub> bears an overt case marker if it is higher on the person hierarchy in (8) than DP<sub>ext</sub> (see the table below).

(8) *Person hierarchy in Yurok:*

1st/2nd  $\succ$  3rd

Under the new perspective GCS in Yurok can be described as follows: If DP<sub>int</sub> is 1st/2nd person nominative, DP<sub>ext</sub> has to be 1st/2nd person as well; if DP<sub>int</sub> is 1st/2nd person accusative, DP<sub>ext</sub> has to be 3rd person; if DP<sub>int</sub> is 3rd person, there are no restrictions on the person of DP<sub>ext</sub>. Thus, the person of DP<sub>ext</sub> is the dependent feature now, but not the case of DP<sub>int</sub> anymore. This change of perspective enables a local derivation of GCS if there is a way to let the first merged argument DP<sub>int</sub> influence the properties of DP<sub>ext</sub>, depending on the case and features of DP<sub>int</sub>. To this end, two questions have to be answered: i) How can DP<sub>int</sub> influence the nature of DP<sub>ext</sub>? ii) What drives the occurrence of the overt case marker? My answers to these questions is that there is a repair operation, called *Maraudage*, which withdraws features originally provided for DP<sub>ext</sub> depending on the features of DP<sub>int</sub>. The overt case marker is a reflex of *Maraudage* which is realized postsyntactically.

3.2. *Theoretical background*

In this section I lay down the background assumptions for the analysis of GCS. I adopt a derivational theory in which syntactic structure unfolds bottom-up in accordance with the Stric Cycle Condition in (6). The syntactic operations Merge and Agree are feature driven ([•F•] triggers Merge, [\*F\*] triggers Agree, (cf. Sternefeld 2006; Heck & Müller 2007)). Agree is defined as in (9).

(9) *Agree (based on Chomsky 2000):*

Agree between a probe P and a goal G applies if

- a. P c-commands G



- b. G is the closest goal to P
- c. P and G have matching feature values (Match = feature identity).
- d. Result: P and G check their matching features.

As Match is a prerequisite for Agree it has to apply before the actual Agree operation. This will become relevant in what follows.

Furthermore, I assume that both arguments of a transitive verb are checked against one and the same head (cf. the *two arguments against one head*-configuration in Anagnostopoulou 2003; Adger & Harbour 2007; Heck & Richards 2007; Béjar & Řezč 2007; Řezč 2008; Richards 2008a; Keine to appear). This head is *v* because it is the element that communicates with both DPs: it can check  $DP_{int}$  and it introduces  $DP_{ext}$ .<sup>3</sup> This solves the communication problem identified in the last section. Hence, apart from the subcategorization features for VP and DP, *v* provides two sets of probe features  $[*F*]$  – one for Agree with  $DP_{ext}$  and another one for Agree with  $DP_{int}$ :

$$(10) \quad v \{ [\bullet V \bullet] \succ [\bullet D \bullet], [*F*]_{ext}, [*F*]_{int} \} \quad (X \succ Y \text{ means that } X \text{ is discharged before } Y)$$

A derivation can only converge if all probe features are checked (cf. *Full Interpretation*, Chomsky 1995).

Moreover, I propose that *v* in languages which show scale effects has expectations about what features the arguments with which it agrees will have:  $DP_{int}$  should be lower on the scale than  $DP_{ext}$  – this is the (morphologically) unmarked combination in these languages. In order to distinguish the location of features on the scale, I follow Béjar (2003); Béjar & Řezč (2007) and assume that person and animacy features are complex objects which are bundles of privative features. (11) gives an example for the decomposition of person features:

$$(11) \quad \begin{array}{ccc} \underbrace{1st \ person} & 2nd \ person & 3rd \ person \\ \underbrace{[speaker] \cong [Speak]} & & \\ \underbrace{\quad \quad \quad [participant] \cong [Part]} & & \\ \underbrace{\quad \quad \quad \quad \quad [person] \cong [\pi]} & & \end{array}$$

There is a general person feature  $[\pi]$ , a speech act participant feature  $[Part]$ , and a feature for the speaker. 1st person is thus encoded by three privative features because it is the speaker, a speech act participant and a person value. As 3rd person is neither the speaker nor a speech act participant, it is encoded by a single feature,  $[\pi]$ .

$$(12) \quad 1st: \begin{bmatrix} [\pi] \\ [Part] \\ [Speak] \end{bmatrix} \quad 2nd: \begin{bmatrix} [\pi] \\ [Part] \end{bmatrix} \quad 3rd: \begin{bmatrix} [\pi] \end{bmatrix}$$

The following correlation arises from this decomposition: Values which are high on a scale are encoded by a superset of privative features compared to values which are lower on the same scale. This can be generalized to other features like animacy, definiteness, etc. except that

<sup>3</sup>In order to ensure Agree under c-command between *v* and  $DP_{ext}$  to be possible I assume that the features of *v* percolate to *v'*.

the privative features must have different meanings. In order to capture generalization across languages and different features later on I name the features by letters. Person features, for example, are decomposed as in (13):

- (13) *Encoding of person features:*
- a. [C] = general person feature
  - b. [B] = participant feature
  - c. [A] = speaker

The decomposition applies to goals and probes. In a language with a case split depending on a binary person scale like Yurok probes and goals bear the following features:

- (14) *Person values in Yurok (1st/2nd > 3rd):*
- a. 3rd person: [C]
  - b. 1st/2nd person: [BC]

The feature [A] is not relevant because at least with respect to the case split, the language does not distinguish between 1st and 2nd person.

I claimed that what is special in GCS languages is that *v* has expectations about the nature of  $DP_{int}$  and  $DP_{ext}$ . The former should be lower on a scale than the latter. This expectation can be encoded in the two probe feature sets on *v* which agree each with one of the arguments: The set for  $DP_{int}$  contains a subset of features of the set provided for  $DP_{ext}$ . In the case of Yurok, *v* looks as follows before the derivation starts:

- (15)  $v \{[*BC*]_{ext}, [*C*]_{int}\}$  ( $DP_{ext}$  expected to be 1st/2nd,  $DP_{int}$  to be 3rd).

Probe features have to be checked if the derivation is to converge. In addition, I assume that certain goal features have to be checked as well, formulated in the constraint FEATURE CHECKING (cf. the *Person Licensing Condition* in Béjar & Ěezĉ (2007)).

- (16) FEATURE CHECKING (FC):  
Certain goal features have to be checked (person, animacy, obviation, etc; depending on the relevant scale in a given language).

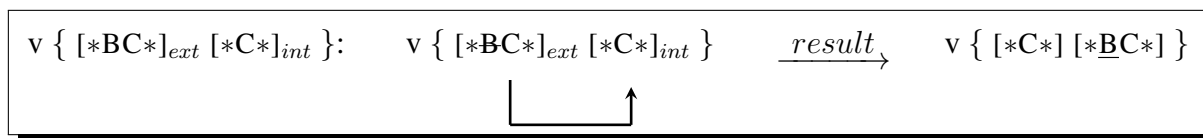
Taken together, these assumptions have the following consequences. Because of incremental structure building,  $DP_{int}$  agrees first with *v*. If  $DP_{int}$  is atypical in that it possesses more features than *v* provides for it (viz., if  $DP_{int}$  is higher on a scale than expected), it cannot check all of its features and violates FC. Take Yurok as an example; *v* is repeated in (17).

- (17) *v in Yurok:*  $v \{[*BC*]_{ext}, [*C*]_{int}\}$

*v* expects  $DP_{int}$  to be 3rd person [C], but if it is 1st or 2nd person [BC], the feature [B] of the goal could not be checked and FC would be violated. I propose that there is a repair strategy, called *Maraudage*, which can apply in order to avoid the violation of FC (see also Georgi et al. (2009) for the application of *Maraudage* to Basque agreement displacement and to operator islands):

(18) *Maraudage*: Features on  $v$  can be displaced from probe set<sub>A</sub> to probe set<sub>B</sub>.

This means that features which were originally provided for checking with  $DP_{ext}$  are displaced from set  $[* *]_{ext}$  into set  $[* *]_{int}$  in order to fulfill FC for  $DP_{int}$ :



Note that *Maraudage* cannot apply freely at any time. It is a repair strategy which can only take place if it is necessary to fulfill FC, but it is usually prohibited. Whether *Maraudage* takes place or not has different consequences for what remains in set  $[* *]_{ext}$  on  $v$ . If it applies, only [B] remains in set  $[* *]_{ext}$  and therefore,  $DP_{ext}$  cannot be 1st or 2nd person [BC]; if it were, [B] of  $DP_{ext}$  could not be checked because the probe feature [B] had been checked by  $DP_{int}$  after *Maraudage*. Hence, FC is violated. In this way, the restrictions on  $DP_{ext}$  are brought about. If *Maraudage* does not apply,  $DP_{ext}$  can be 1st/2nd person because [BC] remains in set  $[* *]_{ext}$ .

In order to guarantee a local derivation of GCS, FC is checked at each derivational step. Because of this fact, there is a stage in the derivation at which  $v'$  has been built and at which FC applies. But at this stage  $DP_{int}$  is the *only* argument in the structure and can trigger *Maraudage* *before*  $DP_{ext}$  is merged.  $DP_{ext}$  has to cope with the remaining features. Hence,  $DP_{ext}$  depends on the properties of  $DP_{int}$ .

### 3.3. Exponence

In this subsection I address the question what drives the (non-)occurrence of the overt case marker. The context described in the abstract example in the box above ( $DP_{int}$  is 1st/2nd person,  $DP_{ext}$  is 3rd person) is exactly the one in which an overt case marker shows up. Thus, I take the overt case marker to be the indicator that *Maraudage* has taken place. I suggest that whenever *Maraudage* takes place, a diacritic " \_ " is generated and it is added to a feature [F] on  $v$ : [F]. Let us assume for the moment that it attaches to the marauded feature, as in the shaded box above. As case is expressed on the arguments and not on the verbal head, this diacritic is passed on (copied) to the argument that checks the marauded feature via Agree. The overt case marker is then the morphological realization of this diacritic on a DP. This can be modeled in a post-syntactic, realizational model of morphology, like Distributed Morphology (*DM*, Halle & Marantz (1993), Halle & Marantz (1994), Harley & Noyer (1999)). In *DM*, syntax operates solely on feature bundles, but lacks any phonological elements. After the syntactic computation, vocabulary items (VI) are inserted into terminal nodes of the syntactic structure. VIs pair morpho-syntactic features with phonological information and are inserted in accordance with the *Subset Principle* and *Specificity*. This means that the most specific vocabulary item which matches the features of a terminal node is inserted into this node. In GCS languages, there is a VI which is sensitive for the diacritic generated by *Maraudage*:

(19) *Vocabulary items*:

- a. /X/ ↔ [F]

b. ( $\emptyset \leftrightarrow [ \ ]$ )

If the diacritic is present on an argument the first VI is inserted because it is more specific than the second. If there is no diacritic only the second VI is a subset of the terminal node. Alternatively, there could be no zero morpheme and no VI is inserted.

Remember that GCS languages differ in the location of the split: It can either show up on  $DP_{int}$  (accusative) or  $DP_{ext}$  (ergative). We have seen in Yurok that an accusative pattern arises when the diacritic is generated in the set to which the marauded feature is displaced, hence set  $[* \ *]_{int}$ . It is then transferred to  $DP_{int}$  via Agree. I propose that an ergative pattern arises if the diacritic is generated in the set  $[* \ *]_{ext}$  from which it is of course transferred via Agree to  $DP_{ext}$ . Thus, the difference between an ergative and accusative alignment of the split lies in the location in which the diacritic is generated. Put differently, either a language marks the situation that a feature has been "stolen" from a set ( $= [* \ *]_{ext}$ ) or that a feature has been added to a set that it did not provide originally ( $= [* \ *]_{int}$ ).

I will briefly summarize how the problems discussed in the last section are solved by the new analysis of GCS. First,  $v$  agrees with both arguments which solves the communication problem. Furthermore, the properties of  $DP_{int}$  influence the properties of  $DP_{ext}$  under the new perspective on the data such that a cyclic incremental derivation is possible which avoids look-ahead. Case assignment is in accordance with the Strict Cycle Condition because the "assignment" (the generation and transfer of the diacritic which leads to postsyntactic spell-out of a case affix) takes place when only  $DP_{int}$  but not  $DP_{ext}$  is merged in the structure. However, as we saw in our Yurok example, an overt case marker does not always appear when  $DP_{int}$  is high on the hierarchy (viz. 1st/2nd person) person. The application of Maraudage thus has to be further restricted which will be discussed in the following section.

#### 4. Sample derivations

In the following subsections I will go through the derivations for Yurok, Tauya, Umatilla Sahaptin, and Fore. In the former three languages the split depends on a binary Silverstein scale whereas in the latter it depends on a tripartite scale. The variation between GCS languages is shown to arise from a parameter on the application of Maraudage, the decomposition of features and the locus of morphological realization of the diacritic as discussed above.

##### 4.1. Binary scale effects

###### 4.1.1. Yurok and Umatilla Sahaptin

In this subsection I go through the derivation of binary scale effects in detail. The first example is Yurok. We already saw that  $DP_{int}$  bears an overt case marker if it is higher on the person scale in (20) than  $DP_{ext}$  (see the table below).

(20) *Person hierarchy in Yurok:*  
1st/2nd  $\succ$  3rd

Table 3: GCS in Yurok

	person of DP <sub>ext</sub>	case of DP <sub>ext</sub>	person of DP <sub>int</sub>	case of DP <sub>int</sub>
pattern 1	1st/2nd	Nom	1st/2nd	Nom
pattern 2	3rd person	Nom	1st/2nd	Acc
pattern 3	1st/2nd	Nom	3rd	Nom
pattern 4	3rd	Nom	3rd	Nom

The split is driven by a binary scale that only distinguishes speech act participants from non-participants, hence, only the general person feature [C] and the participant feature [B] play a role for GCS.  $v$  expects DP<sub>ext</sub> to be higher on the person scale than DP<sub>int</sub>.

(21) *Person values in Yurok (1st/2nd > 3rd):*

- a. 3rd person: [C]
- b. 1st/2nd person: [BC]

(22) *v in Yurok:*

$v$  [[\*BC\*]<sub>ext</sub>, [\*C\*]<sub>int</sub>]

I propose that there are two parameters in GCS (and also in non-GCS) languages: The first is whether Maraudage applies obligatorily or optionally. The second concerns the violability of FC.

(23) *Parameters:*

- a. Maraudage: optional or obligatory
- b. FC is violable i) only if the application of Maraudage can be avoided in this way or ii) it is always violable when Maraudage is not an option.

In Yurok Maraudage has to be optional because we do not see an overt case marker whenever DP<sub>int</sub> is 1st(2nd person. If DP<sub>ext</sub> is 1st/2nd person, too, DP<sub>int</sub> is zero marked for case. In a derivational system like the one adopted here, however, the properties of DP<sub>ext</sub> play no role at the stage of the derivation  $v'$  when the decision is made whether Maraudage applies or not. Hence, Maraudage must be optionally and the patterns which are not attested but predicted by this optionality have to be filtered out.

In sum, there are six possible derivations if all combinations of DP<sub>int</sub> and DP<sub>ext</sub> are taken into account, but only four of them are attested, see Table 3. In the first scenario DP<sub>int</sub> is 3rd person [C]. When  $v$  merges with VP it can agree with DP<sub>int</sub>. Then FC is checked at the  $v'$ -stage and as  $v$  expected DP<sub>int</sub> to be 3rd person [C] FC is fulfilled. Maraudage is not necessary and therefore prohibited. Hence, no diacritic is generated and no overt case marker will show up whenever DP<sub>int</sub> is 3rd person. [BC] remains for checking with DP<sub>ext</sub>.

(24) *Stage of the derivation =  $v'$  (checking indicated by a strike-through):*

$v$	DP <sub>int</sub>	
1	<del>[[*BC*]<sub>ext</sub>, [*C*]<sub>int</sub></del> [C]	no Maraudage

Two options arise by the choice of  $DP_{ext}$  in this context. If  $DP_{ext}$  is 1st/2nd person [BC] it can check all of  $v$ 's remaining features such that FC and Full Interpretation are fulfilled. This derives pattern 3 in Table 3 (see option 1' in (26)). If, however,  $DP_{ext}$  is 3rd person [C], [ $*B*$ ] of  $v$  cannot be checked and this violates Full Interpretation. But the derivation has to converge because the pattern 3-3 without overt case marking is attested (see pattern 4 in table 3). The crucial observation is that if both arguments are 3rd person [C], it is already clear before the derivation starts that [ $*B*$ ] can never be checked because neither  $DP_{int}$  nor  $DP_{ext}$  possesses a feature [B]. I suggest that such a situation can be detected in the numeration and a repair mechanism applies in order to avoid the crash:

- (25) *F-deletion*:<sup>4</sup> A probe feature [ $*F*$ ] can be deleted on a head  $\alpha$  in the numeration if it is impossible to check F in the first place, because none of the arguments of  $\alpha$  possesses a matching feature F (where F is a variable over the privative values A, B, and C).

Thus, in case of two 3rd person arguments, [ $*B*$ ] is deleted in the numeration and the derivation converges:  $v$  provides only [ $*C*$ ] because [ $*B*$ ] has been deleted in the numeration and  $DP_{ext}$  being 3rd person possesses only the feature [C] (see 1'' in (26)). Hence, FC and Full Interpretation are fulfilled and pattern 4 is derived.

- (26) *Stage of the derivation* =  $vP$ :<sup>5</sup>

	$DP_{ext}$	$v$
1'	[BC]	[ $*BC*$ ] <sub>ext</sub> , [ $*C*$ ] <sub>int</sub>
1''	[C]	[ $*C*$ ] <sub>ext</sub> , [ $*C*$ ] <sub>int</sub>

In the second scenario  $DP_{int}$  is 1st/2nd person [BC]. At the  $v'$ -cycle,  $v$  provides fewer features in the set [ $* *$ ]<sub>int</sub> than  $DP_{int}$  needs to check and Maraudage can apply. But as Maraudage is optional there are two ways to continue the derivation. Assume first it does not take place. FC is then violated by  $DP_{int}$  because [B] is not checked. A violation of FC usually leads to the crash of the derivation which is needed to filter out some candidates that can be generated (see the tables below), but here it must not. Therefore, I proposed that FC is minimally violable if this avoids the application of Maraudage, see parameter b.i) in (23). Without Maraudage, [ $*BC*$ ] remain on  $v$  for checking with  $DP_{ext}$  (see 2a in (27)). The second option is to apply Maraudage at the stage  $v'$  and to displace [ $*B*$ ] from [ $* *$ ]<sub>ext</sub> to [ $* *$ ]<sub>int</sub>. As a consequence only [ $*C*$ ] remains in set [ $* *$ ]<sub>ext</sub> (see 2b in (27)). Unchecked features are put in a box, a marauded feature is indicated as  $\bar{F}$  in its original set.

- (27) *Stage of the derivation* =  $v'$ :

	$v$	$DP_{int}$	
2a	[ $*BC*$ ] <sub>ext</sub> , [ $*C*$ ] <sub>int</sub>	[ $\bar{B}$ ][C]	no Maraudage
2b	[ $*\bar{B}C*$ ] <sub>ext</sub> , [ $*BC*$ ] <sub>int</sub>	[BC]	Maraudage

<sup>4</sup>See Heck & Müller (2003) for arguments that access to elements in the numeration is not another instance of look-ahead.

<sup>5</sup>Note that the checking of feature [C] in [ $* *$ ]<sub>int</sub> originates from checking with  $DP_{int}$  at the  $v'$ -level, relevant in this table is only checking of the features of  $DP_{ext}$  and of the features in set [ $* *$ ]<sub>ext</sub>

Maraudage generates a diacritic on *v*. In Yurok which has an accusative case marking pattern it is generated in the set  $[* *]_{int}$  and transmitted via Agree to  $DP_{int}$  (cf. section 3.3).

Afterwards,  $DP_{ext}$  is merged. As  $DP_{ext}$  can be 1st/2nd person or 3rd person and two derivations survive at level  $v'$ , there are 4 possible derivations:

(28) *Stage of the derivation = vP:*

	DP <sub>ext</sub>	v	
2a'	[BC]	[*BC*] <sub>ext</sub> , [*C*] <sub>int</sub>	pattern 1 in Table 3
2a''	[C]	[*B C*] <sub>ext</sub> , [*C*] <sub>int</sub>	crash, *Full Int.
2b'	[B C]	[*BC*] <sub>ext</sub> , [*BC*] <sub>int</sub>	crash: *FC
2b''	[C]	[*BC*] <sub>ext</sub> , [*BC*] <sub>int</sub>	pattern 2 in Table 3

If no Maraudage applied at the v' cycle (cf. 2a in (27)), [\*BC\*] remains for checking with DP<sub>ext</sub> at the vP cycle. If DP<sub>ext</sub> is 1st/2nd person [BC] (see 2a' in (28)), FC is fulfilled and pattern 1 in Table 3 is derived: If both arguments are 1st/2nd person, no overt case marker shows up on DP<sub>int</sub>. If DP<sub>ext</sub> is 3rd person (see 2a'' in (28)), the derivation crashes because [\*B\*] remains unchecked and violates Full Interpretation (which means that there is no pattern in which DP<sub>int</sub> is 1st/2nd person, DP<sub>ext</sub> is 3rd person and there is no overt case marker on DP<sub>int</sub>).

If 2b in (27) is continued, only [\*C\*] remains for checking with DP<sub>ext</sub>; [\*B\*] has been marauded at the v' level. Now, if DP<sub>ext</sub> is 3rd person, it can check its feature [C] and pattern 2 in Table 3 is derived: If DP<sub>int</sub> is 1st/2nd person and DP<sub>ext</sub> is 3rd person an overt case marker must show up (derivation 2a'' with the same features for the arguments and without Maraudage crashes). However, if DP<sub>ext</sub> were 1st/2nd person [BC], [B] could not be checked. In this case the violation of FC leads to the crash of the derivation because Maraudage is not an option and therefore, FC cannot be violated (see the second parameter). Hence, there is no pattern in which both arguments are 1st/2nd person and DP<sub>int</sub> bears an overt case marker.

The VI for the overt case exponent in Yurok is given below (it attaches only to singular arguments, hence the context restriction; [D] means that it realizes the diacritic on an item of category D and not on v):

(29) *Case exponent in Yurok:*  
 -ac ↔ [ ] / [D, sg]

Finally, I summarize the local derivation of GCS in Yurok. Whether Maraudage takes place or not (and hence whether there will be an overt case marker or not) primarily depends only on the properties of DP<sub>int</sub>: An atypical DP<sub>int</sub> which has more features than v expects it to have triggers Maraudage. The properties of DP<sub>int</sub> do not play a role in the derivation afterwards. It is indirectly represented by the number of features which remain in the set [\* \*]<sub>ext</sub> on v. Some of the derivations which can be generated crash at the vP-cycle because of the properties of DP<sub>ext</sub>. The reason is that DP<sub>ext</sub> is the second merged argument and therefore has to cope with what features are left for it after the v'-cycle is completed. At no point does v have access to the properties of both of its arguments at the same time. The derivation of patterns 1 to 4 is thus local and cyclic.

Another language with a binary split is Umatilla Sahaptin (Penutian). In this language DP<sub>ext</sub> bears an ergative marker (=INV.ERG) if DP<sub>int</sub> is higher on the person scale than DP<sub>ext</sub>.

(30) *Person hierarchy in Umatilla Sahaptin:*  
 1st/2nd > 3rd



(31) *GCS in Umatilla Sahaptin (Rigsby & Rude 1996:676, 677):*

- a. *iwnš i-tuxnana yamaš-na*  
 man 3NOM-shot mule.deer-OBJ  
 ‘The man shot a mule deer.’ 3rd sg → 3rd
- b. *n=aš -q’inu-ša awnš-in-aman.*  
 1SG.NOM=1SG 3-see-IMPV men-DU-OBJ.PL  
 ‘I see the two men.’ 1st → 3rd
- c. *iwnš-nim=nam i-q’nu-ša*  
 man-INV.ERG=2SG 3NOM-see-IMPV  
 ‘The man sees you.’ 3rd sg → 2nd
- d. *iwnš-nim=naš i-wynawi-yawan-a*  
 man-INV.ERG=1SG 3SG-arrive-APPL-PST  
 ‘The man came to me / my place.’ 3rd sg → 1st
- e. *Čw=nam paaman -yk-ša?*  
 NEG=2SG 3PL.OBJ 3-hear-IMPV  
 ‘Don’t you hear them?’ 2nd → 3rd

This leads to the following attested patterns:

*Table 4: Person/case combinations in Umatilla Sahaptin*

	person of DP <sub>ext</sub>	case of DP <sub>ext</sub>	person of DP <sub>int</sub>	case of DP <sub>int</sub>
Pattern 1:	1st/2nd	Abs	1st/2nd	Abs
Pattern 2:	3rd	Erg	1st/2nd	Abs
Pattern 3:	1st/2nd	Abs	3rd	Abs
Pattern 4:	3rd	Abs	3rd	Abs

Obviously, the patterns in Umatilla Sahaptin are exactly the same as those in Yurok (compare Table 3): There is an overt case marker whenever DP<sub>int</sub> outranks DP<sub>ext</sub> on a binary person scale. The derivations are thus exactly the same. The only difference between the two languages is the location of the split: aIn YUrok it shows up on DP<sub>int</sub> whereas it appears on DP<sub>ext</sub> in Umatilla Sahaptin. In section 3.3 I proposed that this difference is handled by a parameter which concerns the emergence of the Maraudage diacritic: The diacritic emerges in the set [\* \*]<sub>int</sub> in Yurok (attached to the marauded feature), but it emerges in the set [\* \*]<sub>ext</sub> (attached to the remaining features in this set) in Umatilla Sahaptin and is consequently passed on to DP<sub>ext</sub> in the latter.

(32) *Case exponent in Umatilla Sahaptin:*<sup>6</sup>  
 /nim/ ↔ [ ] / [D, sg]

<sup>6</sup>As in Yurok, the marker only attaches to singular arguments, hence the context restriction.

4.1.2. *Tauya*

*Tauya* differs minimally from *Yurok* and *Umatilla Sahaptin*. In this language  $DP_{ext}$  bears an ergative marker whenever  $DP_{int}$  is high on the binary animacy hierarchy (human).

(33) *Animacy hierarchy in Tauya:*

human  $\succ$  non-human

(34) *GCS in Tauya (MacDonald 1990:120, 121, 316):*

- a. ya-ni/\* $\emptyset$  fanu  $\emptyset$ -yau-e-ʔa  
 1SG-ERG/\*ABS man 3SG-see-1/2-IND  
 ‘I saw the man.’ *hum*  $\rightarrow$  *hum*
- b. ya- $\emptyset$  pai yau-e-ʔa  
 1SG-ABS pig see-1/2-IND  
 ‘I saw the pig.’ *hum*  $\rightarrow$  *non-hum*

Table 5: Animacy/case combinations in *Tauya*

	animacy of $DP_{ext}$	case of $DP_{ext}$	animacy of $DP_{int}$	case of $DP_{int}$
Pattern 1:	hum/non-hum	Erg	hum	Abs
Pattern 2:	hum/non-hum	Abs	non-hum	Abs

Although the presence of the overt case marker only depends on the properties of  $DP_{int}$  in *Tauya* the split is not a local split as in Hebrew. In Hebrew the overt case marker occurs on the same argument whose properties (definite or indefinite) are relevant for the split, namely  $DP_{int}$ . In *Tauya*, however, the overt case marker shows up on the coargument ( $DP_{ext}$ ) of the argument whose properties are decisive for the split ( $DP_{int}$ : human or non-human). The presence of the case marker in *Tauya* still depends on the properties of the coargument and *Tauya* thus exemplifies a global split.

*Tauya* patterns with *Umatilla Sahaptin* in that the split is realized on  $DP_{ext}$ . It differs from *Umatilla* and *Yurok* in that the relevant feature is animacy and not person. Because of this we first need to decompose animacy: [C] is a general animacy feature (animacy as opposed to person, number, ...) and [B] means [+human]. Hence, a human argument is [BC] and a non-human one is only [C].

(35) *Representation of animacy features*

- a. [C] encodes a non-human referent  
 b. [BC] encodes a human referent

(36) *v in Tauya:*

v  $[[*BC*]_{ext}, [*C*]_{int}]$

In order to capture the observation that Maraudage takes place *whenever*  $DP_{int}$  is high on the animacy scale, Maraudage in Tauya must be *obligatory* if  $DP_{int}$  wants to check more features than  $v$  provides for it (cf. the first parameter in GCS languages (see (23))).

In the first scenario  $DP_{int}$  is non-human [C].  $v$  provides [\*C\*] for  $DP_{ext}$  and hence Maraudage is not necessary at the  $v'$ -stage. The derivation converges no matter if  $DP_{ext}$  is human or non-human. In the former case  $v$  provides enough features for checking (namely [BC]), in the latter case the feature [\*B\*] in the set [\* \*]<sub>ext</sub> is deleted in the numeration (cf. (25)) and [\*C\*] is checked by  $DP_{ext}$ . This derives pattern 2 in Table 5.

In the second scenario  $DP_{int}$  is human and wants to check more features than  $v$  provides for it. As Maraudage is obligatory in Tauya, [\*B\*] is displaced to set [\* \*]<sub>ext</sub> which leads to the generation of a diacritic in the set [\* \*]<sub>ext</sub> from which the feature is taken away, just as in Tauya. The diacritic will be passed on to  $DP_{ext}$  by Agree and is realized by a case affix. Consequently, only [\*C\*] remains for checking  $DP_{ext}$  on  $v$ . If  $DP_{ext}$  is non-human [C] it can check its feature and the derivation converges and half of pattern 1 in Table 5 is derived. If, however,  $DP_{ext}$  is also human [BC], [B] cannot be checked because  $v$  only provides [\*C\*] after Maraudage and the derivation should crash, but the pattern does exist. To avoid a violation of FC I suggest that in Tauya the looser condition for the violability of FC is active (see the second value of the second parameter in (23)): FC is always violable if Maraudage is not an option. This means that a derivation with two human arguments does not crash although feature [B] of  $DP_{ext}$  cannot be checked because at the stage of the derivation where  $DP_{ext}$  is merged Maraudage cannot apply anymore - all feature of the set [\* \*]<sub>int</sub> are already checked by  $DP_{int}$ . Therefore, FC can be violated in Tauya. this derives the second half of pattern 1 in Table 5. Hence, there is always an overt case marker when  $DP_{int}$  is human, regardless of the features of  $DP_{ext}$ .

#### 4.2. Tripartite scale effects

In this section I turn to languages in which the case split depends on a tripartite scale. These include Fore, Kashmiri and Awtuw. I concentrate on Fore (Trans-New Guinea) here. In Fore  $DP_{ext}$  bears an overt case marker if  $DP_{int}$  outranks  $DP_{ext}$  on the tripartite animacy scale in (37) (see the table below).

(37) *Animacy hierarchy in Fore:*  
human > animate > inanimate

(38) *GCS in Fore (Scott 1978:116):*

a. Yagaa-wama w aegye.  
pig-ERG man hit  
'The pig hits the man.'

*anim* → *hum*

b. Yagaa w aegye.  
pig man hit  
'The man hits (or kills) the pig.'

*hum* → *anim*

The only difference between binary and tripartite scale effects is the decomposition of features: Three privative features are needed for tripartite scales and two privative features for binary

Table 6: Animacy/case combinations in Fore

	animacy of DP <sub>ext</sub>	case of DP <sub>ext</sub>	animacy of DP <sub>int</sub>	case of DP <sub>int</sub>
Pattern 1:	hum	Abs	hum	Abs
Pattern 2:	anim	Erg	hum	Abs
Pattern 3:	inanim	Erg	hum	Abs
Pattern 4:	hum	Abs	anim	Abs
Pattern 5:	anim	Abs	anim	Abs
Pattern 6:	inanim	Erg	anim	Abs
Pattern 7:	hum	Abs	inanim	Abs
Pattern 8:	anim	Abs	inanim	Abs
Pattern 9:	inanim	Abs	inanim	Abs

scales. In Fore, [C] is a general animacy feature (as opposed to person, number, ...), [B] encodes [+anim[ate]] (as opposed to [-animate]), and [A] means [+human]. The following encodings result:

(39) *Representation of animacy features:*

- a. [C] encodes inanimates.
- b. [BC] encodes animates.
- c. [ABC] encodes humans.

Of course, the content of  $v$  differs, too.  $v$  still expects DP<sub>ext</sub> to be higher on the scale and hence to have more features than DP<sub>int</sub>, but the set  $[* *]_{int}$  can contain up to three features.

(40) *Lexical entry of  $v$  in Fore:*

$v$   $[[*ABC*]_{ext}, [*C*]_{int}]$

The case split in Fore is global, hence Maraudage must apply optional. With these settings the patterns in Fore are derived in exactly the same way as those in Yurok and Umatilla Sahaptin, except for the fact that there are more combinations of DP<sub>int</sub> and DP<sub>ext</sub> which can be generated. Let me go through some of them. If DP<sub>int</sub> is inanimate [C], Maraudage need and cannot apply. Every DP<sub>ext</sub> which merges with the resulting  $v'$  can check its features and the derivations converge (if DP<sub>ext</sub> possesses less features than  $v$  provides for it, deletion applies in the numeration, cf. (25)). This derives patterns 7-9 in Table 6. If DP<sub>int</sub> is animate [BC] or human [ABC], Maraudage can apply optionally. In general, if it does not apply DP<sub>ext</sub> has to check all of the features in  $[* *]_{ext}$  which are not deleted by (25) in order to fulfill Full Interpretation. If Maraudage does apply, DP<sub>ext</sub> has to match exactly the remaining features. If it has more features, FC would be violated; if it had fewer features, probe features would remain unchecked and this time these features cannot be deleted in the numeration because DP<sub>int</sub> provided the matching features. For example, if DP<sub>int</sub> is animate [BC] and Maraudage applies to  $[*B*]$ , DP<sub>ext</sub> has to be inanimate [C] because an animate and human DP<sub>ext</sub> could not check their feature [B] and FC is violated; if Maraudage does not apply,  $[*ABC*]$  remains on  $v$  and DP<sub>ext</sub> can be animate

[BC] ([\*A\*] deleted in the numeration) or human [ABC]. An inanimate could not check [\*B\*] and the derivation crashes ([\*B\*] cannot be deleted because  $DP_{int}$  provides [B]). This derives the remaining patterns in Table 6.

## 5. Other case marking strategies

### 5.1. Non-GCS languages

Although the present system might seem to be designed for the relatively small number of languages with GCS, it can also handle languages with local case splits (LCS) and non-split systems. The crucial difference is that Maraudage has to apply obligatorily in these languages and therefore, the overt case marker shows up more often than in GCS languages.

I begin with local case splits, exemplified by Hebrew, cf. (2). In Hebrew,  $DP_{int}$  bears a case marker if it is definite (a pronoun, a name or a definite noun). In any other configuration, it is zero-marked (nominative, absolutive):

- (41) *Definiteness hierarchy in Hebrew:*  
definite  $\succ$  indefinite

Table 7: Definiteness/case combinations in Hebrew

	definiteness of $DP_{ext}$	case of $DP_{ext}$	definiteness of $DP_{int}$	case of $DP_{int}$
Pattern 1:	def/indef	Nom	def	Acc
Pattern 2:	def/indef	Nom	indef	Nom

First, definiteness is decomposed into privative features. [C] is a general definiteness feature (animacy as opposed to person, number, ...) and [B] means [+definite]. Hence, the following feature bundles for definites and indefinites arise:

- (42) *Representation of definiteness features*
- [C] encodes an indefinite referent
  - [BC] encodes a definite referent

- (43) *v in Hebrew:*  
 $v$  [[\*BC\*]<sub>ext</sub>, [\*C\*]<sub>int</sub>]

Hebrew is like Yurok in that the split shows up on  $DP_{int}$ . It differs from Yurok and Umatilla Sahaptin in that the split is not global because the appearance of the case marker does not depend on the properties of  $DP_{ext}$ , only on those of  $DP_{int}$ : Every time a human  $DP_{int}$  is present, Maraudage has to apply which is reflected in overt case marking. In this respect Hebrew patterns with Tauya. For this reason Maraudage in Hebrew must be obligatory as in Tauya.

If  $DP_{int}$  is indefinite [C], Maraudage need not and cannot apply.  $DP_{ext}$  can be definite or indefinite,  $v$  provides enough matching features for  $DP_{ext}$ , viz.  $[*BC*]_{ext}$ . If both arguments are definite [C], deletion of  $[*B*]$  on  $v$  must take place in the numeration just as in Yurok and Umatilla Sahaptin, cf. (25). This derives pattern 2 in Table 7.

If, however,  $DP_{int}$  is definite [BC], FC cannot be fulfilled at the  $v'$ -level for  $DP_{int}$  because  $v$  provides only  $[*C*]_{int}$ . As Maraudage is obligatory in Tauya,  $[*B*]$  is marauded and a diacritic is generated in the set  $[* *]_{int}$ , which is then passed on to  $DP_{int}$  via Agree (to derive the accusative case marking pattern). As a consequence, only  $[*C*]$  remains for checking with  $DP_{ext}$ . If  $DP_{ext}$  is indefinite [C], all features of  $v$  are checked, FC and Full Interpretation are fulfilled and half of pattern 1 in Table 7 is derived. But if  $DP_{ext}$  is definite [BC], [B] of the argument cannot be checked. However, remember that the looser condition on FC is active in Hebrew which says that FC is always violable if Maraudage is not an option. As all features in set  $[* *]_{int}$  have already been checked by  $DP_{int}$ , Maraudage cannot apply and FC is violable. This derives the second half of pattern 1 in Table 7.

Although Tauya was classified as a global split above, Tauya and Hebrew are treated alike in the present system, namely as local splits with obligatory Maraudage. Differences with respect to the location of the overt case marker on either  $DP_{ext}$  (Tauya) or  $DP_{int}$  (Hebrew) are handled in exactly the same way as in languages with global splits (and optional Maraudage): The diacritic generated when Maraudage applies can be attached to features in  $[* *]_{ext}$  or  $[* *]_{int}$ . In this way a pattern like in Tauya falls out nicely from the system, although it seemed to be a special case at first sight, because it appears to be in between local and global splits, having properties of both. It is now a local split with a different locus of case realization, a parameter which is expected to occur when looking at global splits.

In languages without case splits, an argument A always shows the same case marker. Let us see how such non-split systems can be integrated into the present account. Maraudage has to apply anytime, regardless of the nature of  $DP_{int}$ . In German, for example,  $DP_{int}$  of a transitive verb always bears accusative case (apart from lexically determined case which is demanded by certain verbs and overwrites the default accusative). This can be derived when  $v$  looks as follows:

$$(44) \quad v \{ [* (A)(B)C*]_{ext}, [* *]_{int} \}$$

First, the probe set  $[* *]_{int}$  has to be empty in order to ensure that Maraudage is an option even when  $DP_{int}$  bears only [C], the unmarked value of a feature (person, animacy, etc.). This is in accordance with what was said above: In general the features in  $[* *]_{int}$  are a subset of those of  $[* *]_{ext}$  and this is also true if  $[* *]_{int}$  is empty. Secondly, Maraudage has to apply obligatorily as in all languages without global splits, cf. Tauya and Hebrew with a local split. This is all that has to be said to derive the omnipresence of the overt case marker: As  $[* *]_{int}$  is empty and Maraudage is obligatory, Maraudage has to apply regardless of whether  $DP_{int}$  is encoded by [C] or more features and a diacritic is generated on  $v$  – in the case of German it is generated on the marauded feature, in non-split ergative languages it is generated in the probe set  $[* *]_{ext}$  – again as before in the split languages. The nature of  $DP_{ext}$  is of no importance, just as in the LCS languages with the looser condition on the violability of FC: There is only

a single derivation  $\alpha$  at the  $v'$ -cycle and hence, FC is generally violable if Maraudage is not an option because there is no alternative derivation  $\beta$  which could converge and express a certain feature combination of  $DP_{int}$  and  $DP_{ext}$ . As the reader may verify, it does not matter whether  $[* *]_{ext}$  contains  $[*A*]$  and  $[*B*]$  besides  $[*C*]$ , the outcome is always the same. In the worst case, if  $[* *]_{ext}$  contains more features than  $[*C*]$ , the deletion rule in (25) applies in some cases before the derivation starts.

We have seen that the approach developed for the apparently "exotic" global case splits can also account for local case splits (which depend on the properties of  $DP_{int}$ ) and case patterns in non-split languages with the parameters in (23).

### 5.2. Burzio's Generalization

The aim of this subsection is to show that Burzio's Generalization falls out from the analysis developed for languages with case splits. Stated in more modern minimalist terminology, Burzio (1986) states about languages with a nominative-accusative case marking pattern that a  $v$  head which does not select an external argument can also not assign accusative case to the internal argument. Hence, there is a dependency between the ability to assign case and the selection of an argument. Exactly this dependency is encoded in my analysis of case splits: Overt case marking is a reflex of Maraudage from set  $[* *]_{ext}$  to  $[* *]_{int}$ . If Maraudage does not take place, no diacritic is generated and thus no overt case VI can be realized.

First, consider an intransitive verb. The sole argument of intransitives bears nominative which is usually not overtly marked. Assume that  $v$  in an intransitive context provides only a single probe feature set as there is only a single argument in the structure (a second probe feature set could never be checked and would violate Full Interpretation). If there is only a single probe set, Maraudage is logically impossible - there is no other set from which features could be displaced and hence, no diacritic is generated can be spelled out.<sup>7</sup> Second, in passive clauses the accusative marked internal argument of a transitive verb becomes the sole argument of an intransitive verb, bearing nominative. Passive is brought about (at least) by argument reduction. This can be implemented in the present framework by deletion of the  $c$ -selection feature  $[\bullet D \bullet]$  on  $v$  before the derivation starts. Assume that argument reduction also deletes the external probe set  $[* *]_{ext}$  on  $v$ , which seems reasonable, because without an external argument this set could not be checked anyway - it depends on the presence of  $[\bullet D \bullet]$ . The result is again a  $v$  which only possesses a single probe feature set when the derivation starts. Again, Maraudage is then precluded and the sole argument cannot bear an overt case marker (accusative).<sup>8</sup>

<sup>7</sup>Strictly speaking, Burzio's generalization only makes a statement about unaccusative verbs which do not select an external argument. The present account also explains why intransitives in general - including unergatives - do not assign accusative case.

<sup>8</sup>This section shows how Burzio's generalization can be made follow from my account for the languages in which it is indeed valid. There are of course exceptions to Burzio's generalizations, e.g. in Ukrainian or Czech where the sole argument of an intransitive can still bear accusative case after passivization. Variation can be derived by assuming for example that argument reduction does not absorb the probe feature set  $[* *]_{ext}$ , but only the  $c$ -selection feature in these languages. But it is not the aim of this section to account for all the exceptions of Burzio's generalization.

## 5.3. Direction marking

In this subsection I show that direction marking also falls out from the analysis proposed in the last sections. In languages with a direct-inverse distinction, the verb bears an inverse marker if  $DP_{int}$  is higher on a scale than  $DP_{ext}$ , which is not expected. The verb in a direct context is usually zero-marked. Thus, direction marking differs from Global Case Splits only in the locus of the exponent: Whereas GCS languages use dependent marking, direction is marked on the verbal head (cf. Nichols 1986). But both are global in the sense that the properties of two coarguments seem to determine whether an overt marker occurs or not. Therefore, direction marking can be analysed in exactly the same local and cyclic way as GCS (Zñiga 2006; Drellishak 2008).

In Nocte (Sino-Tibetan, Aissen (1999)) the direct, zero marked verb form is used when  $DP_{ext}$  is higher on the person scale than  $DP_{int}$  and if both are 3rd person and non-coreferent; the inverse marker *-h* is attached to the verb when  $DP_{int}$  is higher on the person scale than  $DP_{ext}$ .

(45) *Person scale in Nocte:*

1st  $\succ$  2nd  $\succ$  3rd

(46) *Person hierarchy effects in Nocte (Das Gupta 1971:21)*

- |    |   |   |
|----|---|---|
| a. | hetho-min<br>teach-1PL<br>'I will teach you.'         | <i>1st</i> $\rightarrow$ <i>2nd</i>     |
| b. | hetho-o<br>teach-2<br>'You will teach them.'          | <i>2nd</i> $\rightarrow$ <i>3rd</i>     |
| c. | hetho-h-ang<br>teach-INV-1<br>'You/he will teach me.' | <i>2nd/3rd</i> $\rightarrow$ <i>1st</i> |
| d. | hetho-h-o<br>teach-INV-2<br>'He will teach you.'      | <i>3rd</i> $\rightarrow$ <i>2nd</i>     |

In contrast to e.g. Yurok, the person scale is tripartite in Nocte, which leads to the following privative feature bundles (see (13) for the meaning of the features):

(47) *Representation of person features:*

- a. [C] encodes 3rd person
- b. [BC] encodes 2nd person
- c. [ABC] encodes 1st person

Note that the abstract patterns of Nocte are the same as those of Fore which can be seen due to the abstract feature decomposition, although in Nocte person is the relevant category and in Fore it is animacy:<sup>9</sup>

<sup>9</sup>The gaps in Nocte arise because these are reflexive contexts which are of course absent in Fore as the features in Fore encode animacy and not person.



(48) *Patterns of Fore and Nocte:*

Fore		Nocte	
ergative	∅	inverse	∅
[BC]–[ABC]	[ABC]–[ABC]	[BC]–[ABC]	–
[C]–[ABC]	[ABC]–[BC]	[C]–[ABC]	[ABC]–[BC]
[C]–[BC]	[BC]–[BC]	[C]–[BC]	–
	[ABC]–[C]		[ABC]–[C]
	[BC]–[C]		[BC]–[C]
	[C]–[C]		[C]–[C]

The patterns of Nocte can thus be derived as in Fore. The only difference is that the diacritic in Nocte is spelled out on *v*, whereas it is realized on an argument in Fore. This is indeed expected in the present analysis because the diacritic emerges on *v*. Its absence on arguments in direction marking languages like Nocte can be handled by saying that the diacritic is not passed on to an argument via Agree in direction marking languages or that it is passed on, too, but that it is simply not spelled out on arguments, just as it is not spelled out on *v* in GCS languages.

Similar correspondences between head marking and GCS languages can be found elsewhere. For example, direct/inverse marking in Ojibwe (Algonquian) is based on obviation features and corresponds to the abstract patterns of Yurok and Umatilla Sahaptin: In a context with two 3rd person arguments, there is a binary scale (proximate  $\succ$  obviative) and an overt marker occurs (inverse marker on the verb in Ojibwe, case marker on an argument in Yurok/Umatilla Sahaptin) only if  $DP_{int}$  outranks  $DP_{ext}$  on this scale (Rhodes 1976; Dryer 1992; Fadden 2000). If obviation features are decomposed into bundles of privative features ([C] as a general obviation feature and [B] meaning [+topic]), the pattern can be derived as in Yurok.

5.4. *Predictions about exponence*

The diacritic which is generated by Maraudage and which is realized postsyntactically arises as a consequence of Maraudage on *v* and can be passed on to an argument that checks features with *v*. Several morphological marking strategies are thus expected to occur in languages:

1. marking on the verb (head): = *inverse marking*
2. marking on an argument (dependent): = *GCS*
3. marking on both the verb and an argument (double): = *inverse+GCS, Arizona Tewa*
4. marking on both arguments of a transitive verb: = *Nez Perce*

Indeed, all of these possibilities are attested. The first two options have already been encountered: Either the diacritic is only spelled out on the head where it emerges (= head-marking, direct-inverse languages) or only on an argument to which it is transferred (= dependent-marking, GCS languages). The third option is one in which the diacritic is spelled out on an argument *and* the verb. Arizona Tewa (Kiowa-Tanoan) exhibits this pattern. The relevant scale is 1st/2nd  $\succ$  3rd<sup>10</sup>. Transitive verbs in Arizona Tewa bear a portmanteau prefix which encodes person and

<sup>10</sup>If both arguments are 3rd person, animacy becomes the relevant factor: anim  $\succ$  inanim.

number of  $DP_{ext}$  and  $DP_{int}$ . There are two sets of prefixes, active and passive prefixes (Kroskrity (1985:308), Zñiga (2006:182))

(49) *Active prefixes, implies 3rd person  $DP_{int}$ :*

$DP_{ext}$	1sg	1du	1pl	2sg	2du	2pl	3sg	3du	3pl
	d-	'n	'i:-	n:-	den-	ob:n-	mn-	den-	d:-

(50) *Passive prefixes:*

$DP_{int}$ \ $DP_{ext}$	1	2	3
1	–	d-	d-
2	w-	–	
2sg			w:-
2du			w:bn-
2pl			w:b-
3sg			'i:-
3du			'i:bn-
3pl			:b-

If  $DP_{ext}$  is higher on the person scale than  $DP_{int}$ , the active prefixes are attached to the verb. But if  $DP_{int}$  outranks  $DP_{ext}$  the passive prefix are attached instead and crucially,  $DP_{ext}$  also bears the case suffix *-d*. Hence, the diacritic is spelled out on  $DP_{ext}$  by *d* and prefixes selection is sensitive to the diacritic on *v*.<sup>11</sup> Thus, there is GCS and inverse marking at the same time, but both arise from a single Maraudage operation in the present analysis.

(51) *Verbal prefixes and case marking (Kroskrity (1985:311, 314), Zñiga (2006:182))*

- a. NÉ'i k<sup>w</sup>iy d-tay.  
 this woman 1SG;3-know  
 'I know this woman.' 1st → 3rd
- b. Hẹ'i sen-**di** 'u w:-kẹgen-n.  
 that man-OBL you 3;2SG.PASS-help-COMP  
 'That man helped you.' 3rd → 2nd

The fourth pattern can arise because nothing precludes that the diacritic is generated in *both* probe sets of *v* and passed on to both arguments. This is attested in Nez Perce.<sup>12</sup> In this language, both arguments of a transitive verb are zero marked if  $DP_{int}$  is a weak indefinite (a pattern called "Pseudo-Antipassive" by Woolford (1997)), otherwise the subject bears the ergative marker *-nim* and the object bears the objective case marker *-ne*.

<sup>11</sup>Kroskrity argued that the alternation in Arizona Tewa is an active-passive alternation (cf. Kroskrity 1985:310f.), hence the labels given to the prefixes. Klaiman (1991), however, shows that the different encoding of arguments is indeed an instance of an inverse system and not an active-passive-alternation with argument reduction.

<sup>12</sup>I thank Stefan Keine for pointing out this example to me.

- (52) *Local case split in Nez Perce (Deal (2010:85), see also Crook (1999:238)):*  
 Context: One house in Lewiston is red, and yesterday, John found that house.
- a. Caan-nim paa-'yaax-na ini-ne  
 John-ERG 3/3-find-PERF house-OBJ  
 'John found a (certain) house.'
- b. Caan hi-'yaax-na init  
 John 3SUBJ-find-PERF house  
 'John found a house.'
- Comment: "It's not referring to the red house or anything, it's just he just found a house that he's been looking for"

Thus, the pattern could be described as follows: There is a local case split in Nez Perce, depending on the location of  $DP_{int}$  on a binary definiteness scale definite  $\succ$  indefinite. This follows if definiteness is decomposed into privative features, cf. the discussion of Hebrew above.

- (53) *Encoding of definiteness features:*
- a. [C] encodes indefinites  
 b. [BC] encodes definites  
 c. Consequence: indefinite referent = [C], definite referent = [BC]

Obviously,  $v$  expects  $DP_{int}$  to be indefinite:  $v$   $[[*BC*]_{ext}, [*C*]_{int}]$ . If  $DP_{int}$  outranks  $DP_{ext}$  on the definiteness hierarchy because it is definite [BC], Maraudage takes place obligatorily. This leads to the generation of the diacritic in *both* sets of  $v$  and it is passed on to the arguments where the overt case suffixes show up.

Another option is that the diacritic is not realized at all, but this case is of no interest because if there is no overt reflex of Maraudage, we would not identify a hierarchy effect in the first place.

## 6. Conclusion

In this paper I have provided a local analysis of global case splits in a strictly derivational framework. The local derivation is made possible by a change of perspective on the data: It is not the case value of an argument which is the dependent feature, but the properties of the external argument are dependent on the properties and case of the internal argument. I implemented this as follows: As the internal argument is the first argument in the structure in a derivational bottom-up syntax it can consume as many features as it needs to check its inherent features, even features which were originally provided for the external argument (by Maraudage). This is possible because both arguments are checked by the same head  $v$ . The external argument has to cope with the features which the internal argument left for it and it is thus restricted in its properties. Overt case marking is a reflex of Maraudage. Furthermore, I showed that the analysis developed for GCS can also derive local case splits, case assignment in non-split languages, as well as direction-marking which is the head-marking counterpart of GCS. Finally, Burzio's generalization can be accounted for.

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## Case splits and the order of elementary operations

Stefan Keine

In this paper I propose a unified account for both local and global case splits in terms of local impoverishment. My main claim is that impoverishment takes place within narrow syntax and may thus interact with the operation Agree. This reassessment of the place of impoverishment in the grammar has the immediate consequence that Agree may apply *after* impoverishment. Such ‘late’ Agree may then percolate the effects of local impoverishment within the structure, giving rise to the impression of non-local mechanisms at work. A second, logically independent, claim is that impoverishment is brought about by harmonic alignment of Silverstein hierarchies.

The paper is structured as follows: Section 1 lays the empirical groundwork by exemplifying local and global case splits and showing that they adhere to the same generalization. Section 2 introduces the theoretical background assumptions and puts forth my proposal. Sections 3 and 4 put the account to work on local and global splits, respectively. Section 5 illustrates an empirical prediction of the proposed system regarding the interaction of local and global splits with verbal agreement. Finally, section 6 draws the conclusion.

### 1. Empirical overview

Differential case marking refers to systematic alternations of case markers triggered by DP-internal properties.<sup>1</sup> Although the typological and theoretical literature has focused mainly on local case splits, ‘global’ instances of the phenomenon are attested as well (Silverstein 1976).

- If the DP that conditions the marker alternation is also the locus of the alternation, I will refer to the split as *local*. Hence, case marking of an argument can be determined solely on the basis of other properties of that argument.

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<sup>1</sup> Case marker alternations may also be conditioned by other features within the clause, such as tense or aspect. In this paper, I will restrict my attention to splits induced by nominal properties. See Keine (to appear) for an application to various other types of alternations.

- Case splits are considered *global* if the DP hosting the contextual feature and the one containing the affected feature are disjoint. Consequently, case marking of a DP cannot be determined with reference to other properties of that DP alone.<sup>2</sup>

This section provides various examples of local and global splits, which will then serve as the basis for developing and illustrating the analysis pursued here.

### 1.1. Local case splits

#### 1.1.1. Object case marking in Hindi

Hindi gives a first illustration of a local case split. There exist two object case markers (*-ko* and *-∅*), the choice among which is conditioned by humanness and specificity: If the object is non-human and non-specific it is zero marked, otherwise *-ko* is attached (cf. (1) and (2)).<sup>3</sup>

#### (1) DEFINITENESS

- a. *nadya-ne gari-∅ cala-yi hε*  
 Nadya.F.SG-ERG car.F.SG-NOM drive-PERF.F.SG be.PRES.3SG  
 ‘Nadya has driven a car.’
- b. *nadya-ne gari-ko cala-ya hε*  
 Nadya.F.SG-ERG car.F.SG-ACC drive-PERF.M.SG be.PRES.3SG  
 ‘Nadya has driven the car.’

(Butt & King 2004:161)

#### (2) HUMANNES

- a. *ilaa-ne ek bacce-ko / \*-∅ ut<sup>h</sup>aayaa*  
 Ila-ERG one child-ACC / -NOM lift/carry.PERF  
 ‘Ila lifted a child.’
- b. *ilaa-ne ek haar-∅ / \*-ko ut<sup>h</sup>aayaa*  
 Ila-ERG one necklace-NOM / -ACC lift.PERF  
 ‘Ila lifted a necklace.’

(Mohanani 1994:79)

#### 1.1.2. Object case marking in Trumai

Trumai is a language isolate spoken in the central area of Brazil (Guirardello 1999). There exist three dative markers, all of which are non-zero: *-(V)tl*, *-ki*, *-(V)s*. Their choice is conditioned by the factors *individuation* and *prominence* (see table 1). Examples are given in (3).

- (3) a. *ha hu'tsa chi\_in kasoro-tl*  
 I see FOC/TENS dog-DAT  
 ‘I saw the dog (I know it).’

<sup>2</sup> Despite their relevance for the questions of syntactic locality, global case splits have attracted relatively little attention. Notable treatments include Aissen (1999), de Hoop & Malchukov (2008), Béjar & Rezac (2009), and Georgi (this volume).

<sup>3</sup> The following abbreviations are used in the glosses: ABL ‘ablative’, ACC ‘accusative’, ANR ‘action nominalizer’, DAT ‘dative’, ERG ‘ergative’, INV.ERG ‘inverse ergative’, NOM ‘nominative’, OBJV ‘objective’.

**Tab. 1** Distribution of dative markers in Trumai (Guirardello 1999:280)

-(V)tl	>	-ki	>	-(V)s
· individuated	· individuated but not identifiable		· not individuated, not identifiable	
· identifiable	· individuated but not prominent		· not individuated, not prominent	
· prominent	· not individuated, identifiable			

- b. *ha hu'tsa chi\_in kasoro yi-ki*  
 I see FOC/TENS dog YI-DAT  
 'I saw a dog/the dog (I do not know it well).'
- c. *ha hu'tsa chi\_in kasoro-s*  
 I see FOC/TENS dog-DAT  
 'I saw dogs.'

(Guirardello 1999:276)

## 1.2. Global case splits

## 1.2.1. Yurok

In Yurok (Algic), the object is marked with *-ac* if the subject is 3<sup>rd</sup> person and the object 1<sup>st</sup> or 2<sup>nd</sup>. In all other cases, it is unmarked (Robins 1958; Bickel in press).<sup>4</sup> The alternation qualifies as global since case marking of the object cannot be determined on the basis of properties of the object alone. Instead, some reference to the subject is necessary. See (4) for illustration.

- (4) a. *keʔ nek ki newoh-paʔ*                      b. *yoʔ nek-ac ki newoh-peʔn*  
 2SG.NOM 1SG.NOM FUT see-2>1SG                      3SG.NOM 1SG-OBJV FUT see-3SG>1SG  
 'You will see me.'    'He will see me.' (Robins 1958:21)

## 1.2.2. Umatilla Sahaptin

In Umatilla Sahaptin (Penutian; Washington, Oregon) the subject is marked with *-nim* (the so-called 'inverse ergative') if it is 3<sup>rd</sup> person singular and the object is 1<sup>st</sup> or 2<sup>nd</sup> person. In all other cases the subject is zero marked (Rigsby & Rude 1996; Rude 1997; Zúñiga 2002). Notice that – as far as person is concerned – the environment of case marking is identical to Yurok above. Both languages differ, however, with respect to the locus of case marking: While in Yurok object case marking is at stake, it is subject marking in Sahaptin.

- (5) a. *ín=aš á-q'ínu-ša payúwii-na tmáma-an*  
 I=1SG 3ABS-see-IMPV sick-OBJV old.woman-OBJV  
 'I see the sick old woman.' (Rigsby & Rude 1996:674)
- b. *i-q'ínu-ša=aš iwínš-nim*  
 3NOM-see-IMPV=1SG man-INV.ERG  
 'The man sees me.' (Rude 1997:332)

<sup>4</sup> I am grateful to Juliette Blevins for confirming this distribution to me.



- c. *iwínš i-tuxnána yáamaš-na*  
 man 3<sub>NOM</sub>-shot mule.deer-OBJV  
 ‘The man shot a mule deer.’

(Rigsby &amp; Rude 1996:676)

The subject receives *-nim* in [3<sub>SG</sub>>1/2] configurations. In (5a) the subject *ín* ‘I’ is 1<sup>st</sup> person, and hence zero marked.<sup>5</sup> Compare this to the [3<sub>SG</sub>>1<sub>SG</sub>] configuration in (5b), whose subject *iwínš-nim* ‘man-INV.ERG’ is overtly case marked. (5c) makes it clear that object properties play a role in determining the subject’s case marking: The subject *iwínš* is 3<sub>SG</sub>, as in (5b), but nevertheless receives no overt marking due to the 3<sub>SG</sub> object.

### 1.2.3. Kolyma Yukaghir

The most complex global case split that I know of is found in the Siberian language Kolyma Yukaghir (Maslova 2003).<sup>6</sup> The whole system consists of four object case markers, only one of which is zero. The distribution of these markers is given in (6). Some examples are provided in (7).

- (6) a. *-gele:*                      b. *-le:*                              c. *-ul:*                              d. *-∅:*  
       [3 > 1/2/3.def]                [3 > 3.indef]                      [1/2 > 1/2]                      [1/2 > 3]

- (7) a. *tet kimnī met-kele kudede-m*  
       your whip me-ACC kill-TR:3<sub>SG</sub>  
       ‘Your whip has killed me.’  
       b. *met-ul amde-l-get polde-mek*  
       me-ACC die-PERF-ANR-ABL save-TR:2<sub>SG</sub>  
       ‘You have saved me from death.’  
       c. *n’umud’ī-le mid’-u-m*  
       ax-INSTR take-0-TR:3<sub>SG</sub>  
       ‘He took an ax.’  
       d. *met tudel-∅ ju∅*  
       I he see(TR:1<sub>SG</sub>)  
       ‘I saw him.’

(Maslova 2003:93,95,227)

A relevant minimal pair demonstrating that object case marking indeed depends on subject properties is (7a) versus (7b). In both cases the direct object is *met* ‘1<sub>SG</sub>’. The subject is however 3<sup>rd</sup> person in (7a) and 2<sup>nd</sup> person in (7b). This distinction leads to differences in the case marking of the object.

### 1.3. Common properties

There are two striking similarities between local and global case splits: First, both instantiate case marker alternations triggered by some properties of verbal arguments.<sup>7</sup> They differ only with regard to whether the conditioning and affected features are distributed over several arguments or present within one. A pervasive property is that the alternation is by no means arbitrary but

<sup>5</sup> The clitic *-aš* ‘1<sub>SG</sub>’ instantiates agreement.

<sup>6</sup> I am indebted to Lennart Bierkandt for bringing Kolyma Yukaghir to my attention.

<sup>7</sup> Both the global splits in Yurok and Umatilla Sahaptin are triggered by person features. That person features may in principle also condition local splits is exemplified by Marathi, where 3<sup>rd</sup> person subjects are marked with *-ne* in the perfective, thus contrasting with 1<sup>st</sup> and 2<sup>nd</sup> person subjects, which are zero marked (Pandharipande 1997).

systematically conforms to Hale/Silverstein hierarchies (Hale 1972; Silverstein 1976) such as those given in (8).

- (8) a. *Grammatical Function (GF) scale*  
 Subject > Object  
 b. *Person scale*  
 1<sup>st</sup> > 2<sup>nd</sup> > 3<sup>rd</sup> person  
 c. *Prominence scale*  
 X > x (discourse-prominent argument > non-discourse-prominent argument)  
 d. *Animacy scale*  
 Human > Animate > Inanimate  
 e. *Definiteness scale*  
 Pronoun > Proper noun > Definite > Indefinite Specific > Non-specific

Markedness of subjects and objects in terms of the scales in (8) is measured along the following lines: Subjects count as unmarked if they have properties high on the scales in (8); marked subjects, on the other hand, are associated with low-ranked properties. Consider the person scale (8b) as an example: Canonical subjects are 1<sup>st</sup> person; 3<sup>rd</sup> person subjects, on the other hand, are hierarchically marked. The opposite holds for objects: Canonically, objects have properties low on these scales. While unmarked objects are 3<sup>rd</sup> person, 1<sup>st</sup> person objects are highly marked. Upon closer scrutiny, it is not just the case that all the splits encountered so far are conditioned by scale features, but furthermore that *hierarchical markedness corresponds to morphological marking*: It is always the hierarchically marked configuration that receives more morphological marking in the sense that it is associated with a phonologically more complex marker if measured against the SONORITY HIERARCHY. By contrast, zero marking always occurs in canonical configurations. To see this, recall that in Hindi, objects with canonical properties (i.e., those that are non-human and non-specific, ranked low on the animacy and definiteness scale) are unmarked morphologically. Hierarchically marked objects bear the overt exponent *-ko*. In Trumai the phonological complexity of the case markers correlates precisely with the relative markedness of the objects that they appear on: The most marked objects bear the most complex marker *-(V)tl*; the least marked objects are least marked morphologically (by *-(V)s*). In Yurok, it is a marked combination of subject and object – comprising a 3<sup>rd</sup> person subject and a 1<sup>st</sup>/2<sup>nd</sup> person object – that receives overt marking. If subject, object or both are unmarked, no morphological marking occurs. The same holds for Umatilla Sahaptin. Finally, in Kolyma Yukaghir the most complex marker morphologically (*-gele*) occurs in the most marked combination of subject and object properties, the less complex markers *-le* and *-ul* in intermediate configurations, and the least complex exponent  $-\emptyset$  shows up in combinations of canonical subjects and objects.

- (i) a. *mī* / *āmhī* / *tū* / *tumhī* *gāñī* *mhaṭlī*  
 I.ABS / we.ABS / you.SG.ABS / you.PL.ABS SONG.3PL.NEUT.ABS SING.PAST.3PL.NEUT  
 ‘I/we/you sang songs.’  
 b. *tyā-ne* / *tyān-nī* / *ti-ne* / *tyān-nī* *gāñī* *mhaṭlī*  
 he-ERG / they.MASC-ERG / she-ERG / they.FEM-ERG SONG.3PL.NEUT.ABS SING.PAST.3PL.NEUT

(Pandharipande 1997:131)

This shows that there is no principled difference as to which features trigger which kind of split. Rather, both local and global splits may be conditioned by identical features.

Second, local and global case splits may either exhibit a zero/non-zero alternation or involve several overt markers. In addition, they may be gradual in the sense that more than two markers may enter the alternation, each associated with a certain range of hierarchical markedness that is reliably correlated with their phonological complexity.

These similarities are summarized in (9).

(9) *Common empirical properties of local and global case splits*

- a. Internal phonological complexity of the exponents correlates with hierarchical markedness of the context that they appear in.
- b. They may involve two or more overt markers.

These common properties strongly suggest a unified approach to local and global case splits, attributing their identical behaviour to the fact that they emerge from the same mechanism. It remains, of course, to be determined what leads to the different loci of the alternation. Specifically in the case of global splits, the relation between the conditioning and the affected elements appears peculiar. As there does not appear to be any evidence that subject and object enter into a direct (Agree) relation with each other, there is no independently motivated dependency between the two arguments that could be used to implement the sensitivity of one element to the other. Given that it is preferable to avoid an entirely *ad hoc* relation between the two arguments – used solely to account for global splits –, I submit that this reasoning entails that an account for global case splits must be indirect in the sense that it involves a third element, which undergoes an Agree operation with both subject and object and thus serves as a link between the properties of the former and the latter.

## 2. Proposal

In order to account for the properties of local and global case splits observed in the previous section I will argue that both are the result of highly local case feature impoverishment triggered by scale features (see Keine & Müller 2008, 2009 for an application of this idea to local case splits). Treating both types of splits as reflexes of the same underlying operation derives without further ado the striking similarities between them. Furthermore, I argue that impoverishment is not an arbitrary rule but the result of an OT-style constraint interaction. The constraint ranking is brought about by harmonic alignment of markedness scales. Restrictions on impoverishment then follow from independent restrictions on possible constraint rankings. This derives the observed influence of markedness scales on the distribution of the case markers.

Given that local and global splits differ in the relation between the DP undergoing the alternation and the one hosting the conditioning features, I will propose that they differ with respect to the ordering between impoverishment and various Agree operations. The account developed below challenges the commonly held position that impoverishment is solely a post-syntactic operation, hence applying after all Agree operations. In contrast, I claim that impoverishment applies within syntax proper and may hence be intermingled with Agree operations, giving rise to various kinds of interaction. Most importantly, Agree may apply *after* impoverishment. Suppose impoverishment affects a fully specified feature  $\alpha_+$  on a head  $\Gamma$ , yielding the reduced feature

$\alpha_-$ . Subsequent Agree percolates  $\alpha_-$  to a second head  $\Sigma$ . On  $\Sigma$ , vocabulary insertion takes place, realizing  $\alpha_-$ . In this derivation, Agree has percolated the effects of impoverishment to a second head, giving rise to the impression that vocabulary insertion into  $\Sigma$  was sensitive to features on  $\Gamma$ . The result is a global case split. If, on the other hand, case feature impoverishment takes place *after* case assignment, the impoverished case feature is not percolated to another head, so that the effects of impoverishment show up on the same head that impoverishment has applied to. This leads to a local split. The difference between local and global case splits is thus reduced to a different order of operations. Globality in some sense reduces to an early local operation, the effects of which are subsequently percolated within the structure.

### 2.1. Distributed Morphology

I presuppose the general grammatical architecture of Distributed Morphology (Halle & Marantz 1993, 1994 and subsequent work). Specifically, vocabulary insertion takes place post-syntactically, is realizational in nature and determined by the SUBSET PRINCIPLE (10) and SPECIFICITY (11).

(10) SUBSET PRINCIPLE

A vocabulary item V is inserted into a functional morpheme M iff (i) and (ii) hold:

- (i) The morpho-syntactic features of V are a subset of the morpho-syntactic features of M.
- (ii) V is the most specific vocabulary item that satisfies (i).

(11) SPECIFICITY<sup>8</sup>

A vocabulary item  $V_1$  is more specific than a vocabulary item  $V_2$  iff  $V_1$  contains more morpho-syntactic features than  $V_2$ .

In order to capture natural classes of syntactic categories I follow Bierwisch (1967) in assuming that (case) features are made up of more primitive *subfeatures*.

Prior to vocabulary insertion, impoverishment may delete morpho-syntactic features and thereby influence marker insertion (Bonet 1991; Halle & Marantz 1993, 1994; Noyer 1998; Bobaljik 2002; Frampton 2002). After impoverishment has taken place a less specific marker is inserted than would otherwise have been chosen. Impoverishment thus leads to a *retreat to the general case*. Because of decomposition of case features impoverishment does not necessarily yield a total deletion of case features. Rather, only certain subfeatures are deleted. The remaining subfeatures may still be realized by a (less specific) marker.

Impoverishment is strictly local in that both the contextual as well as the affected features must reside on single head, as formulated in (12). As for the timing of operations, impoverishment takes priority. Thus, if the context of impoverishment is met, it has to apply immediately, even if other operations, such as Agree, could take place as well.

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<sup>8</sup> Cf. Halle (1997); specificity of vocabulary items may in principle also be defined with reference to an explicit ordering stipulation (Halle & Marantz 1993) or a feature hierarchy which has priority over mere set cardinality (cf., e.g., Lumsden 1992; Noyer 1992; Müller 2004b). I will stick to this arguably simpler definition here.

(12) LOCALITY CONDITION ON IMPOVERISHMENT<sup>9</sup>

Impoverishment may only be sensitive to features within a single syntactic head and its structural configuration.

2.2. *Impoverishment by harmonic alignment of markedness scales*

Contrary to the widely held position, I assume that impoverishment is *not* stated as an arbitrary rule that applies as soon as its context is brought about by an optimality-theoretic interaction between faithfulness and markedness constraints (cf. Keine & Müller 2008, 2009, based on Aissen 1999, 2003). The ranking is not free but systematically restricted by HARMONIC ALIGNMENT of the markedness scales in (8) above. Harmonic alignment is defined in (13).

## (13) HARMONIC ALIGNMENT (Prince &amp; Smolensky 2004:161)

Suppose given a binary dimension  $D_1$  with the scale  $X > Y$  on its elements  $\{X, Y\}$ , and another dimension  $D_2$  with a scale  $a > b > \dots > z$  on its elements  $\{a, b, \dots, z\}$ . The *harmonic alignment* of  $D_1$  and  $D_2$  is the pair of harmony scales  $H_X, H_Y$ :

a.  $H_X: X/a > X/b > \dots > X/z$

b.  $H_Y: Y/z > \dots > Y/b > Y/a$

The *constraint alignment* is the pair of constraint hierarchies  $C_X, C_Y$ :

a.  $C_X: *X/z \gg \dots \gg *X/b \gg *X/a$

b.  $C_Y: *Y/a \gg *Y/b \gg \dots \gg *Y/z$

The resulting ranking is locally conjoined with the faithfulness constraint MAX-CASE, which penalizes case feature deletion, yielding a ranking of faithfulness constraints. The crucial property of this ranking is that canonical arguments are associated with a low-ranked faithfulness constraint; non-canonical ones have a high-ranked constraint. Into this ranking is inserted a markedness constraint against a case subfeature, which triggers deletion of this subfeature in the output. Configurations associated with a faithfulness constraint ranked lower than the markedness constraint are impoverished, all others are not. Because of their low-ranked faithfulness constraint canonical arguments are impoverished more easily than non-canonical ones. The effect of this conception of impoverishment is the implication in (14).

## (14) If impoverishment applies to a given type of argument, it also applies to all less marked ones.

<sup>9</sup> As mentioned in the main text, the purpose of (12) is to disallow global impoverishment. Specifically, impoverishment of nominal features triggered by features in the verbal domain or vice versa is ruled out. The restriction embodied in (12) may ultimately turn out to be too strong. Consider the nominal domain. Under the plausible assumption that humanness and specificity features reside on different heads, a problem arises. As we have seen for Hindi in section 1.1.1, case exponence depends on both types of features, apparently in violation of (12). Various solutions seem tenable. First, one may stick to (12) and assume some sort of nominal concord for these features. Second, if reprojection is involved within the nominal domain (see Georgi & Müller 2010 and references cited there), the number of distinct heads is severely cut down, thus making (12) a viable option again. Finally, (12) may be relaxed to refer only to extended projections (Grimshaw 2000) or to phasal domains. Since I will ignore here the internal structures of DPs, I will stick to (12), merely noting that further refinements may prove necessary.

As a consequence of (14), the alternation pattern derived by impoverishment is not arbitrary but corresponds to markedness in terms of Hale/Silverstein hierarchies. A concrete derivation for the constraint ranking will be given for Hindi in section 3.1.

### 2.3. Iconicity

To ensure that it is a smaller marker that is inserted after impoverishment has taken place, I assume the meta-grammatical principle of ICONICITY.

(15) ICONICITY (Wiese 1999, 2003, 2004)

Similarity of form implies similarity of function (within a certain domain, and unless there is evidence to the contrary).

*Function* in (15) refers to a marker's morpho-syntactic complexity; *form* denotes its phonological complexity as measured against the sonority hierarchy. (15) states that both properties are correlated. After impoverishment has taken place a marker with fewer morpho-syntactic features has to be inserted due to the Subset Principle (10). By iconicity, this marker is phonologically less complex and hence 'smaller' than the marker otherwise attached. Impoverishment thus leads to a retreat to a phonologically 'smaller' marker, possibly the zero marker. Combined with the restrictions on impoverishment above, this system captures the unifying property of local and global case splits: Morphological marking and hierarchical markedness coincide.

### 2.4. Agree

Apart from impoverishment, the second crucial operation employed is AGREE as defined in (16). I take Agree to be a maximally general operation that copies a feature value from one head to another (based on, but deviating somewhat from, Chomsky 2000, 2001, 2004; see fn. 10).

(16) AGREE<sup>10</sup>

Agree is a function that maps an unordered pair ( $[F : \_]$ ,  $[F : \delta]$ ) into ( $[F : \delta]$ ,  $[F : \delta]$ ) iff (a)–(d) hold.

- (a)  $[F : \_]$  and  $[F : \delta]$  are features of separate heads  $\Sigma$  and  $\Gamma$ ,
- (b)  $\Sigma$  c-commands  $\Gamma$ ,
- (c)  $\Gamma$  is *transparent* for probes on  $\Sigma$ , and
- (d) locality is respected.

---

<sup>10</sup> Some remarks are in order concerning (16). As defined, Agree does not require the unvalued feature to c-command its valued counterpart. (16) specifies that  $\Sigma$  c-commands  $\Gamma$  and that both features are distributed over these two heads; it does not, however, determine which head comprises which feature. An immediate consequence is that Agree is symmetrical: Probes on  $\Sigma$  may be valued by goals on  $\Gamma$  and vice versa. While I will base the technical considerations in the remainder of this paper on the definition in (16) it is important to notice that it is not mandatory. The present account can equally well be made to work with an asymmetric conception of Agree, as long as additional movement steps are assumed. Since, however, such movement steps only complicate the derivations, I will adopt (16), allowing me to concentrate on the main proposal. The *locality* requirement in (d) refers

I will term Agree applied to  $\phi$ -features ‘ $\phi$ -AGREE’ and use ‘ $\kappa$ -AGREE’ to refer to Agree applied to case features.<sup>11</sup> Both  $\phi$ - and  $\kappa$ -Agree are instantiations of the general operation Agree.

A result of the locality restriction on impoverishment in (12) is that under certain conditions impoverishment depends on prior Agree. To see this, suppose that an impoverishment operation is sensitive to features  $\alpha$  and  $\beta$ , distributed over two domains  $\Delta$  and  $\Omega$ . Because of (12) impoverishment may not take place. Now suppose that an Agree operation percolates  $\beta$  from  $\Omega$  to  $\Delta$ . As a result, both  $\alpha$  and  $\beta$  are now present within  $\Delta$ , rendering impoverishment possible. Thus, Agree may feed impoverishment, delaying it until Agree has taken place.

The account developed below crucially relies on impoverishment taking place in between  $\kappa$ - and  $\phi$ -Agree and vice versa. It is thus mandatory that these are two *disjoint* Agree operations (*contra*, e.g., Chomsky 2000, 2001, who treats case and  $\phi$ -agreement as reflexes of a single operation Agree).

### 2.5. The order of operations

I follow Heck & Müller (2007) and Müller (2009) in assuming that the syntactic derivation is subject to *extremely local optimization*, where every derivational step is evaluated in an optimality-theoretic fashion. Deletion of unvalued features is triggered by markedness constraints against unvalued  $\phi$ - and case features, respectively, cf. (17). I assume these two constraints to be universal. One of the tenets of OT is to reduce variation among languages to differences in constraint rankings. Accordingly, I will argue below that the difference between local and global splits can ultimately be traced back to different constraint rankings.

- |   |  |
|---|--|
| (17) a. * $[\phi: \_ ]$<br>‘penalizes the presence of<br>unvalued $\phi$ -features’ | b. * $[\text{CASE}: \_ ]$<br>‘penalizes the presence of<br>unvalued case features’ |
|---|--|

If a stage in the derivation comprises both unvalued  $\phi$ - and case features it is not *a priori* clear which features undergo Agree first. In such a situation the ranking between the markedness constraints in (17) is decisive.<sup>12</sup>

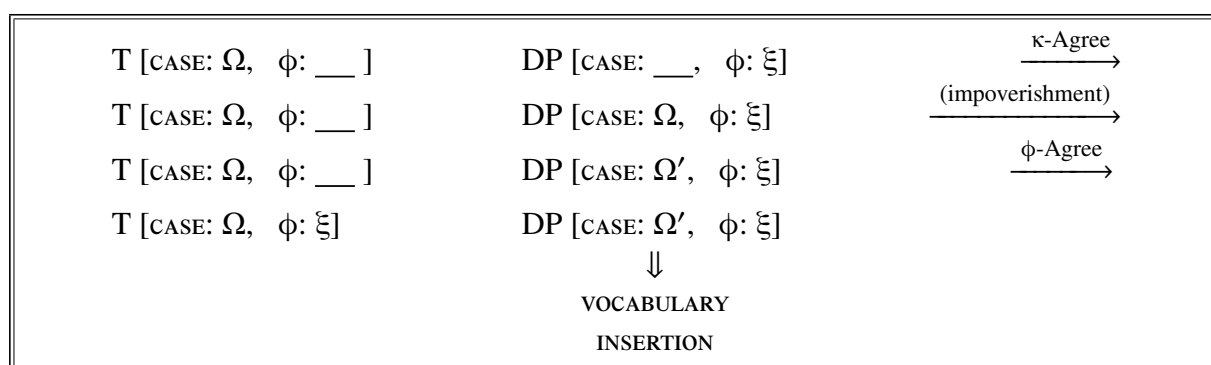
As detailed in the next two sections, it is precisely these ranking differences and their distinct interaction with impoverishment that accounts for the distinctions between local and global case splits. In a nutshell, the ranking \* $[\text{CASE}: \_ ] \gg *[\phi: \_ ]$  will result in a local case split; the inverse ranking \* $[\phi: \_ ] \gg *[\text{CASE}: \_ ]$  will give rise to a global case split.

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to the standard constraints on Agree, namely phase boundedness (Chomsky 2000, 2001, 2008) and Relativized Minimality/Minimal Link Condition (Rizzi 1990; Chomsky 1995).

<sup>11</sup> This notation is used to highlight the claim that both case assignment and  $\phi$ -agreement are instances of Agree. It is hence of expository purpose only.

<sup>12</sup> See Müller (2004a) and Müller (2009) for an analogous argument based on the interaction of Merge and Agree.



**Fig. 1** – General derivation for local case splits

### 3. Derivation of local case splits

This section shows how the system proposed in the previous section accounts for local case splits in Hindi and Trumai.

#### 3.1. Hindi object marking

##### 3.1.1. Deriving the case marker alternation

As exemplified by (1) and (2) above, objects in Hindi may bear either *-ko* or  $-\emptyset$ . Highly canonical objects – i.e., being both non-specific and non-human – bear the zero marker, to all others *-ko* is attached (Mahajan 1990; Mohanan 1994; Woolford 2001; Stiebels 2002; Lee 2003; Butt & King 2004; Anand & Nevins 2006; Keine 2007). To derive this empirical pattern, I will first illustrate how impoverishment may lead to case marking alternations by invoking a standard – viz., stipulated – impoverishment rule. I will then go on to show how impoverishment construed as the effect of constraint interaction yields a more elegant and more restrictive account of the same phenomenon.

As the point of departure, consider the system in (18). As a result of the constraint ranking in (18a), whenever a structure comprises both unvalued case and  $\phi$ -features, case feature valuation takes place first. For the sake of exposition, suppose that T in Hindi contains both unvalued  $\phi$ -features and the case to be assigned to the object (called ‘accusative’ here), which contains the subfeature [+obj] (cf. (18b)).<sup>13</sup> Two markers (*-ko* and  $-\emptyset$ ) compete for insertion. *-ko* outranks  $-\emptyset$  due to specificity. In the case of non-human, non-specific objects the impoverishment rule in (18d) applies, deleting the subfeature [+obj] and thereby bleeding insertion of *-ko*. As a result, only the elsewhere zero marker can be attached. This derivation is schematized in figure 1.<sup>14</sup>

<sup>13</sup> Nothing hinges on that particular assumption. It is, however, necessary that the case assigning head comes no later into the structure than the  $\phi$ -probing one, as case has to be assigned first. Thus, the accusative could equally well be assigned by, e.g., *v*. There is independent evidence that the  $\phi$ -probe in Hindi resides on T. If both subject and object are accessible (i.e., zero-marked), only the subject may control agreement, which appears to be a minimality effect.

<sup>14</sup> The role of  $\phi$ -Agree in the derivation in figure 1 will become relevant in section 5.



- (18) a. *Ranking*  
 \*[CASE: \_\_\_] >> \*[\phi: \_\_\_]  
 b. *Case decomposition*  
 ACCUSATIVE: [+obj]
- c. *Vocabulary items*  
 /-ko/ ↔ [+obj]  
 /-∅/ ↔ [ ]  
 d. *Impoverishment rule for objects*  
 [+obj] → ∅ / [-def, -hum]

Since in Hindi  $\kappa$ -Agree takes place before  $\phi$ -Agree, the structural description of the impoverishment rule (18d) is met within the object domain. Impoverishment and subsequent vocabulary insertion into the case feature thus apply to the same head (the object). Consequently, the effects of impoverishment are morphologically visible on the same head that impoverishment has applied to. Therefore, the effect is *local*, giving rise to a local case split.<sup>15</sup>

The system in (18) derives the correct empirical pattern. However, it arguably suffers from a lack of restrictiveness. If impoverishment is stated as an explicit rule as in (18d), there is no principled reason why it should be restricted to hierarchically unmarked configurations. Thus, in the system proposed above, impoverishment may in principle equally well apply to highly marked objects alone. As a consequence, impoverishment stated as an arbitrary rule faces the considerable drawback that it does not capture the recurring generalization of virtually all case splits: Hierarchical markedness and morphological marking coincide. To derive this strong correlation, impoverishment has to be restricted in a principled fashion so as to apply to unmarked configurations alone. This is achieved if impoverishment is restricted by harmonic alignment of markedness scales, as illustrated below.

Consider the scales in (19). Harmonic alignment (13) applied to the GF and the animacy scale yields the harmony scales in (20a); application to the GF and the definiteness scales results in (20b). What, e.g., (20ai) states is that subjects are typically human and non-human subjects are marked. These markedness relations can be transformed into constraint alignments against the relevant configurations (cf. (21)). Note that the order is reversed because (21) instantiates *prohibitions* against certain configurations.

- (19) a. *Animacy Scale*  
 Human >  $\overbrace{\text{Animate} > \text{Inanimate}}^{\text{Non-Human}}$
- b. *Definiteness Scale*  
 ... > Specific > Non-Specific
- c. *GF Scale*<sup>16</sup>  
 Subject > Object
- (20) HARMONY SCALES
- a. (i) Subj/Hum > Subj/NHum  
 (ii) Obj/NHum > Obj/Hum
- b. (i) Subj/Spec > Subj/NSpec  
 (ii) Obj/NSpec > Obj/Spec
- (21) CONSTRAINT ALIGNMENTS
- a. (i) \*Subj/NHum >> \*Subj/Hum  
 (ii) \*Obj/Hum >> \*Obj/NHum
- b. (i) \*Subj/NSpec >> \*Subj/Spec  
 (ii) \*Obj/Spec >> \*Obj/NSpec

<sup>15</sup> An analogous treatment can be given for the aspect-driven alternation between *-ne* and *-∅* on subjects. See Keine (2007, to appear) for discussion.

<sup>16</sup> One might wonder how the notions ‘subject’ and ‘object’ are represented. There are several ways to accomplish that. I will assume that it is the phrase-structural environment which encodes the information about grammatical function (Chomsky 1965). ‘Subject’ can then be defined as “Spec-*v*P” and ‘direct object’ as “Comp-VP” (Chomsky 2001).

Take (21ai) as an example: There is one constraint against non-human subjects and another against human subjects. Because human subjects are less marked than non-human ones, the constraint penalizing this type of subject is ranked lower than the constraint against non-human subjects. The differences in hierarchical markedness of certain types of arguments is thus represented via different positions in constraint rankings.

LOCAL CONJUNCTION<sup>17</sup> applied to the constraint rankings in (21) combines the humanness and specificity properties of the objects (the subject constraints are neglected from here on as they are irrelevant for the question of object marking). This yields two-dimensional argument encoding as these constraints are specified for two properties of the object (namely, humanness and specificity). The result is the ranking in (22).

(22) LOCAL CONJUNCTION

- a. \*Obj/Hum & \*Obj/Spec  $\gg$  \*Obj/Hum & \*Obj/NSpec
- b. \*Obj/NHum & \*Obj/Spec  $\gg$  \*Obj/NHum & \*Obj/NSpec
- c. \*Obj/Spec & \*Obj/Hum  $\gg$  \*Obj/Spec & \*Obj/NHum
- d. \*Obj/NSpec & \*Obj/Hum  $\gg$  \*Obj/NSpec & \*Obj/NHum

(22a) is a ranking of two markedness constraints. The first penalizes objects which are human and specific, the second is violated if an object is human and non-specific. Crucially, the former is ranked higher than the latter, corresponding to the difference in hierarchical markedness between the two: human and specific objects are more marked than human, non-specific ones and correspondingly violate a higher ranked constraint. The ranking in (22) can be notationally simplified as in (23).

(23) NOTATIONAL SIMPLIFICATION OF (22)

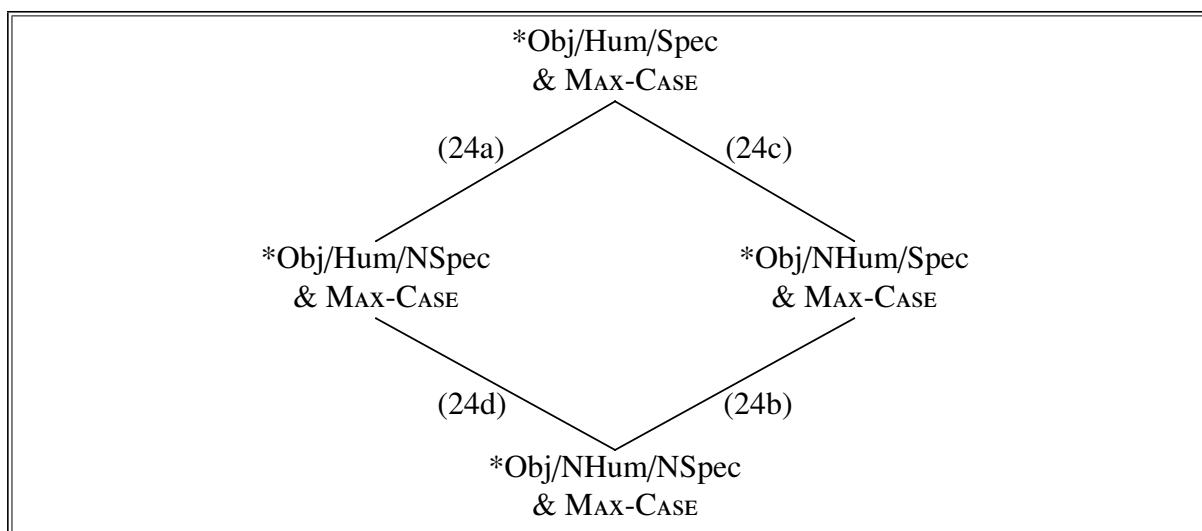
- a. \*Obj/Hum/Spec  $\gg$  \*Obj/Hum/NSpec
- b. \*Obj/NHum/Spec  $\gg$  \*Obj/NHum/NSpec
- c. \*Obj/Hum/Spec  $\gg$  \*Obj/NHum/Spec
- d. \*Obj/Hum/NSpec  $\gg$  \*Obj/NHum/NSpec

Note that all the constraints in (23) are markedness constraints that penalize certain types of objects. Since I am only concerned with case marking here, (23) has to be restricted to case features. This is achieved by local conjunction with the faithfulness constraint MAX-CASE, which is violated if a case (sub)feature is deleted from input to output. This then gives the ranking of faithfulness constraints in (24).

(24) LOCAL CONJUNCTION WITH MAX-CASE

- a. \*Obj/Hum/Spec & MAX-C  $\gg$  \*Obj/Hum/NSpec & MAX-C
- b. \*Obj/NHum/Spec & MAX-C  $\gg$  \*Obj/NHum/NSpec & MAX-C
- c. \*Obj/Hum/Spec & MAX-C  $\gg$  \*Obj/NHum/Spec & MAX-C
- d. \*Obj/Hum/NSpec & MAX-C  $\gg$  \*Obj/NHum/NSpec & MAX-C

<sup>17</sup> The LOCAL CONJUNCTION of  $C_1$  and  $C_2$  in domain  $D$ ,  $C_1 \& C_2$ , is violated when there is some domain of type  $D$  in which both  $C_1$  and  $C_2$  are violated. Universally,  $C_1 \& C_2 \gg C_1, C_2$  (Smolensky 1995:4).



**Fig. 2** – Inherent ranking of the faithfulness constraints in (24)

The ranking between these constraints is not arbitrary but fixed to a large extent. It was arrived at by general means and follows from the algorithms of harmonic alignment and local conjunction themselves. The inherent ranking relations are illustrated in figure 2, where top-down order corresponds to ranking relations. This ranking is *not* subject to cross-linguistic variation, because the mechanisms by which it is derived by their very definition impose severe restrictions on the ranking relations.

The topmost constraint in figure 2 is violated if any case feature of a human, specific object is deleted from input to output. It is crucial to note that the inherent ranking between these constraints corresponds to hierarchical markedness: Because human, specific objects are the most marked type their faithfulness constraint is ranked the highest. Conversely, since non-human, non-specific objects are the most canonical objects they have the lowest ranked constraint. *\*Obj/Hum/NSpec & Max-Case* and *\*Obj/NHum/Spec & Max-Case* are ranked between the former two constraints but not ranked with respect to each other.

In order to trigger case feature deletion (and thereby yield the effects of impoverishment) the markedness constraint (25) is inserted into the ranking in figure 2. (25) penalizes the presence of the case subfeature [+obj] within the output. The final ranking for Hindi is given in (26).

(25) *Markedness constraint triggering case feature deletion*  
 \* [+obj]

(26) *Final ranking for Hindi*

$$\left\{ \begin{array}{l} *Obj/Hum/Spec \ \& \ MAX-C, \\ *Obj/Hum/NSpec \ \& \ MAX-C, \\ *Obj/NHum/Spec \ \& \ MAX-C \end{array} \right\} \gg * [+obj] \gg *Obj/NHum/NSpec \ \& \ MAX-C$$

The ranking in (26) has the same effect as the impoverishment rule in (18d): Non-human, non-specific objects are impoverished. However, (26) derives the fact that only canonical objects undergo impoverishment. Insertion of the markedness constraint \* [+obj] establishes a cut-off point. Every object whose faithfulness constraint is ranked lower than this point is impoverished;

objects with a higher ranked faithfulness constraint remain unaltered. As noted above, the inherent ranking of the faithfulness constraints in (26) corresponds to hierarchical markedness. While the concrete insertion point of the markedness constraint \*[+obj] is language-specific, the following implication is universal: If impoverishment applies to a certain type of object (i.e., if there is a markedness constraint ranked higher than the faithfulness constraint for that type of object), it also applies to all less marked types, as these types have even lower ranked faithfulness constraints (cf. (14)). The ranking in (26) thus captures the generalization that there is a strong correspondence between morphological markedness and hierarchical marking. Impoverishment of marked objects alone is impossible in this system, as desired.<sup>18</sup>

The competition for (1a) and (1b) is provided in (27) and (28), respectively.

(27) *Tableau for (1a)*

<i>Input:</i> [obj,-hum,-spec][+obj]	* <sub>o</sub> /+h/+s & MAX-C	* <sub>o</sub> /+h/-s & MAX-C	* <sub>o</sub> /-h/+s & MAX-C	*[+obj]	* <sub>o</sub> /-h/-s & MAX-C
☞ [obj,-hum,-spec]					*
[obj,-hum,-spec][+obj]				*!	

(28) *Tableau for (1b)*

<i>Input:</i> [obj,-hum,+spec][+obj]	* <sub>o</sub> /+h/+s & MAX-C	* <sub>o</sub> /+h/-s & MAX-C	* <sub>o</sub> /-h/+s & MAX-C	*[+obj]	* <sub>o</sub> /-h/-s & MAX-C
[obj,-hum,+spec]			*!		
☞ [obj,-hum,+spec][+obj]				*	

As a consequence of (27), non-human, non-specific objects are impoverished. As outlined above, deletion of [+obj] bleeds the insertion of *-ko*, leading to zero case exponence.

To summarize, in this section I have illustrated on the basis of Hindi how the system proposed here accounts for local case splits. The proposal identifies impoverishment as the source of the case marker alternation. Impoverishment is viewed as being triggered by the interaction of faithfulness and markedness constraints. Restrictions on impoverishment are imposed as restrictions on possible constraint rankings. Harmonic alignment of markedness scales provides a general means to limit constraint rankings and is therefore capable of confining impoverishment to adhere to (14). Impoverishment thus conceived derives the empirical observation (9a). Local splits are the result of  $\kappa$ -Agree taking place before  $\phi$ -Agree due to the ranking in (18a). Hence, impoverishment affects the object, into which, at a later stage in the derivation, the case marker is inserted. Case feature impoverishment therefore has local effects in Hindi.

### 3.2. Differential object marking in Trumai

The present analysis attributes differential case marking to impoverishment rather than a total deletion of a DP's case (as, e.g., in Aissen's 1999, 2003 approach). Given the independently motivated assumption that case features are composed of smaller subfeatures, impoverishment

<sup>18</sup> One might rightfully object that this pattern *could* be derived after all if (23) is conjoined with the markedness constraint (25) and MAX-CASE is inserted into this ranking. The present analysis inherits this problem from Aissen (1999, 2003), who is forced to rule it out by stipulation. I have nothing illuminating to say about this puzzle.



The next section will show how local impoverishment accounts for global case splits by merely changing the order of the Agree operations.

#### 4. Derivation of global case splits

The empirical considerations in section 1 led to the conclusion that global and local case splits adhere to the same empirical generalizations. Given that an account based on impoverishment derives these properties (section 3), it is desirable to extend the analysis to global splits as well.

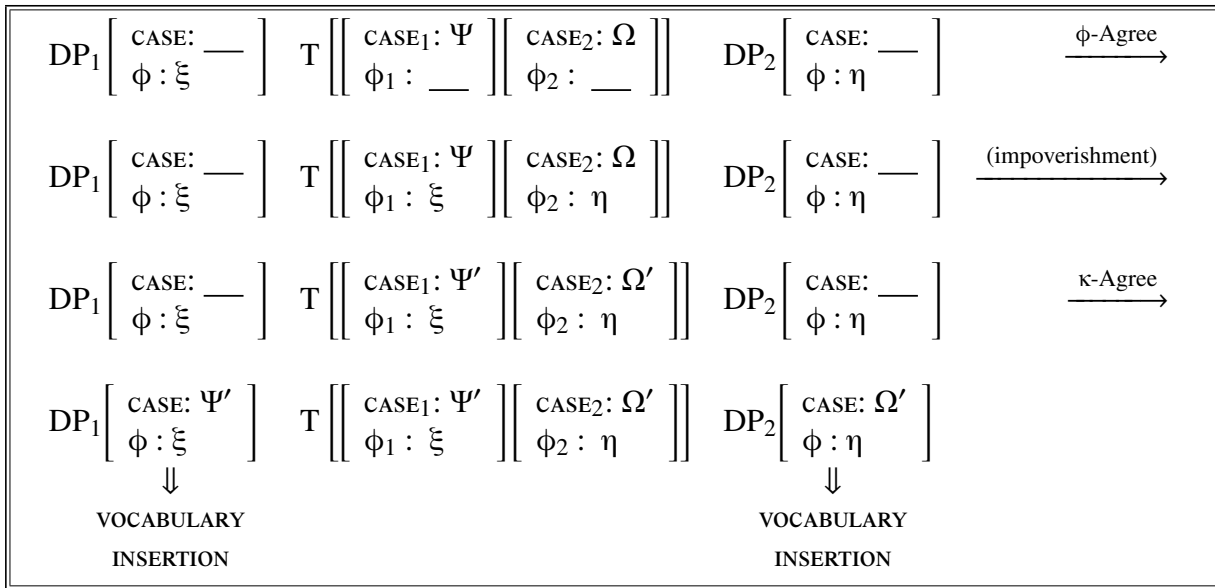
Global splits, at first glance, seem to make necessary global impoverishment, operating on larger chunks of (possibly non-adjacent) structure. To take Yurok as an example, the subject's  $\phi$ -features are never present on the object (there is no independently motivated Agree operation between the two arguments, these  $\phi$ -features are never overtly realized on the object, etc.), impoverishment appears to affect the object's case features in the presence of subject properties. Such a global characterization of impoverishment would, however, tremendously increase its power. Furthermore, it messes up the whole system as *any* feature in the structure could lead to impoverishment of *any* other feature in the structure, predicting a whole range of interaction that is not attested. To escape this undesirable conclusion I will stick to (12), which entails that impoverishment takes place on a head other than the object, comprising all the relevant features. This head, I will assume, is T. As the morphological effect of this impoverishment operation shows up on the object, some mechanism is called for that percolates the information that impoverishment has taken place (i.e. the impoverished features matrix) to another head. This mechanism, I submit, will be  $\phi$ -Agree.

In a nutshell, the account for global splits to be elaborated in this section makes the followings claims. Local and global splits involve the same operations (Agree and impoverishment) but a different order between them: While in the case of local splits impoverishment *follows* case assignment, it *precedes* case assignment in global splits. This difference in the application of operations is not directly stated but follows from a ranking difference between the markedness constraints against unvalued case features on the one hand and unvalued  $\phi$ -features on the other.

Thus, from the present perspective a strictly local impoverishment operation may appear to apply to a much larger domain if its interaction with Agree leads to *spreading* of its effects. As a consequence of such spreading, the morphological effects of impoverishment may show up on another head than impoverishment has applied to. In a nutshell, an apparent global operation reduces to the effects of an early local one.

##### 4.1. Yurok

As illustrated by (4) above, objects in Yurok bear the marker *-ac* if they are 1<sup>st</sup> or 2<sup>nd</sup> person and the subject is 3<sup>rd</sup> person. Otherwise, they receive no overt case marking. Verbal agreement in (4) makes it clear that the verb agrees with both subject and object for  $\phi$ -features. There is thus evidence that the person feature of the subject is represented within the verbal domain, by assumption on T. It can hence trigger case feature impoverishment on T, in conformity with the locality condition (12).



**Fig. 3** – General derivation for global case splits

The crucial assumption is that the ranking between the constraints  $*[\text{CASE: } \_]$  and  $*[\phi: \_]$  in Yurok differs from Hindi – the latter constraint is ranked higher than the former (cf. (33)).

(33) *Ranking for global case splits*

$$*[\phi: \_] \gg *[\text{CASE: } \_]$$

This ranking leads to the general derivation in figure 3, which is identical for all instances of global case splits. For reasons to be discussed immediately, I assume that the verbal  $\phi$ -probes and the case features to be assigned to subject and object all reside on a single head, T.

The derivation in figure 3 proceeds as follows: If a stage in the derivation comprises both unvalued  $\phi$ - and case features,  $\phi$ -Agree takes place first due to the ranking in (33). As a consequence, T contains both the (still unassigned) case features and the  $\phi$ -features of the verb's arguments.  $\phi$ -feature driven impoverishment of case features may then apply locally on T, affecting the case feature in certain configurations. Crucially, since both the subject's and the object's  $\phi$ - and case features are present on a single head, nothing prevents impoverishment from affecting the *subject's* case feature in the context of the *object's*  $\phi$ -features (or the reverse). Subsequently, the case features are assigned (via Agree) to the verbal arguments and realized by marker insertion. Since vocabulary insertion is sensitive to whether impoverishment has taken place or not, impoverishment on the verb may indirectly influence marker insertion into the verbal argument. The result is an apparently global case split. Under the present account, however, the mechanisms leading to this type of split are as local as the 'standard' splits observed in Hindi above.

To illustrate this abstract derivation for the Yurok examples in (4), suppose the system in (35). Based on the scales in (34), harmonic alignment and local conjunction along the lines illustrated for Hindi above give rise to the ranking in (35c), the complex constraints of which make reference to subject and object properties. Into this ranking the markedness constraint  $*[-\text{subj}]$  is inserted, yielding deletion in all configurations associated with a lower ranked faithfulness constraint.





that case features and  $\phi$ -probes are contained on T, which is merged only after the subject is brought into the structure.<sup>22</sup>

#### 4.2. Umatilla Sahaptin

The global split in Umatilla Sahaptin, exemplified by (5), may be reduced to the general derivation in figure 3 as well. Recall that in Umatilla Sahaptin the subject is marked with *-nim* if it is 3<sup>rd</sup> person singular and the object is 1<sup>st</sup> or 2<sup>nd</sup> person. In all other cases, the subject is zero marked. This distribution is schematized in (36).

(36) *Conditions for subject case markers*

a. *-nim/*  $\leftrightarrow$  [SUBJ=3SG]  $\wedge$  [OBJ=1/2]

b. *-∅/*  $\leftrightarrow$   $\left\{ \begin{array}{l} \text{[OBJ=3]} \\ \vee \\ \text{[SUBJ=1/2]} \\ \vee \\ \text{[SUBJ=PL]} \end{array} \right.$

As in Yurok, in transitive clauses the verb agrees with both the subject and the object: 3<sup>rd</sup> person subject and object agreement is instantiated as a verbal prefix, 1<sup>st</sup>/2<sup>nd</sup> person agreement as a second position enclitic. I assume that both instances of agreement are local enough to trigger case feature impoverishment on the verb, i.e. that both are within the verbal domain.

As in Yurok, it is verbal  $\phi$ -agreement rather than the DP's  $\phi$ -features themselves that lead to impoverishment. Ditransitives in Umatilla Sahaptin provide evidence that this is correct, cf. (37).

(37) *x<sup>w</sup>ʔsaat-nim=naš i-ni-ya ináy k'úsi*  
 old.man-INV.ERG=1SG 3NOM-give-PST me horse  
 'The old man gave me a horse.'

(Rigsby & Rude 1996:674)

There are two objects present in (37). Only one of them – the indirect object *ináy* 'me' – triggers verbal agreement. Strikingly, it is only this object that matters with regard to the case marker on the subject. If *k'úsi* 'horse' was relevant we would expect zero case marking on the subject (this being a [3>3] configuration). The present analysis thus straightforwardly correlates the asymmetry between both objects with respect to the subject's case marking to the independently observed fact that there exists an asymmetry regarding  $\phi$ -agreement. This suggests that it is indeed the  $\phi$ -features on the verb that lead to impoverishment.

Because 3<sup>rd</sup> person subjects and 1<sup>st</sup>/2<sup>nd</sup> person objects are hierarchically marked it is no coincidence that this configuration receives morphological marking. However, there seems to

<sup>22</sup> Other possibilities exist as well: First, if reprojection is assumed within the verbal domain and features are ordered on the verb (Müller 2010), they may only be accessible after V has reached a position c-commanding the subject. Second, one might adhere to the claim that Agree and impoverishment take place post-syntactically (see Bobaljik 2008 for a recent proposal along these lines). Since merging the subject is a syntactic operation, Agree takes then place after the subject has been integrated. Since nothing hinges on a choice, I will stick to the view that T is all-powerful here.

be one inconsistency: the number condition in (36). Plural subjects are generally treated as *more* marked than singular subjects (the number scale being ‘SINGULAR > PLURAL’). It is therefore unclear why 3SG subjects can be marked with *-nim* but (hierarchically more marked) 3PL subjects cannot. To solve this apparent paradox, I assume that the number property is not derived by scales but that the marker *-nim* is idiosyncratically specified for [NUM: -PL] and can thus never be attached to plural subjects. This is supported by the observation that only number of the *subject* is relevant (which can be locally determined), thus contrasting with the person features (where both subject and object matter).

Having thus excluded the relevance of the number property, the final ranking for Umatilla Sahaptin is basically identical to that of Yurok above. The only difference between the two languages is that in Yurok the case to be assigned to the object is impoverished while in Umatilla Sahaptin it is the subject’s case feature.

(38) a. *Case subfeatures*

$$\text{ERGATIVE: } \left[ \begin{array}{c} -\text{obl} \\ +\text{subj} \end{array} \right]$$

b. *Markers*

$$/\text{-nim}/ \leftrightarrow \left[ \begin{array}{c} \left[ \begin{array}{c} -\text{obl} \\ +\text{subj} \end{array} \right] \\ [\text{NUM: -PL}] \end{array} \right] \quad /-\emptyset/ \leftrightarrow [ \quad ]$$

c. *Ranking for Umatilla Sahaptin*

$$*\text{Subj/NLoc} \ \& \ * \text{Obj/Loc} \ \& \ \text{MAX-CASE} \gg \ * [+ \text{subj}]$$

$$\gg \left\{ \begin{array}{l} * \text{Subj/NLoc} \ \& \ * \text{Obj/NLoc} \ \& \ \text{MAX-CASE} \\ * \text{Subj/Loc} \ \& \ * \text{Obj/Loc} \ \& \ \text{MAX-CASE} \\ * \text{Subj/Loc} \ \& \ * \text{Obj/NLoc} \ \& \ \text{MAX-CASE} \end{array} \right\}$$

$$\left( \begin{array}{l} \sim \text{ Rule-based notation:} \\ [+ \text{subj}] \rightarrow \emptyset / [\text{Subj: Loc}] \vee [\text{Obj: NLoc}] \end{array} \right)$$

The relevant derivations for Umatilla Sahaptin are identical to Yurok above: As a first step,  $\phi$ -Agree applies. As a result, the context of case feature impoverishment (38c) is met on T. Next, the case features are assigned to the respective arguments and vocabulary insertion takes place. Depending on whether impoverishment has applied on the verb, a different case marker is attached. Again, the crucial point of the analysis is that Agree and impoverishment may interact. Due to subsequent Agree, local impoverishment may manifest itself in an apparently global way. In summary, I have proposed a local account for an apparently global case alternation in Yurok and Umatilla Sahaptin. Under this analysis, impoverishment applies strictly locally; the only operation involving more than one head is Agree, which is independently needed.

For reasons of space, I will refrain from an implementation of the more complex data found in Kolyma Yukaghir although they are fully in line with the principles and mechanisms adopted here. For an application of the present theory to Kolyma Yukaghir, see Keine (to appear).

## 5. A prediction: Case splits and verbal agreement

The system presented here – unequivocally reducing case splits to impoverishment and asserting that only the order of operations may differ – makes an interesting prediction concerning the sensitivity of verbal  $\phi$ -agreement to the two types of splits.

Consider local splits first. As shown in figure 1,  $\phi$ -Agree takes place *after* case feature impoverishment on the object. This order of operations gives rise to the possibility that impoverishment indirectly affects  $\phi$ -agreement: Since the information whether impoverishment has applied or not is represented on the object at this point of the derivation,  $\phi$ -Agree might in principle be sensitive to it. This prediction is indeed borne out for Hindi. As illustrated in (39a), zero marked objects may trigger verbal agreement. Objects bearing *-ko*, on the other hand, never control verbal agreement. This is shown in (39b).

(39) a. *ravii-ne roṭii k<sup>h</sup>aayii*  
 Ravi.MASC-ERG bread.FEM.NOM eat.PERF.FEM.SG  
 ‘Ravi ate bread.’

b. *ila-ne roṭii-ko ut<sup>h</sup>aayaa*  
 Ila.FEM-ERG bread.FEM-ACC lift.PERF.MASC  
 ‘Ila picked up the bread.’

(Mohan 1994:90,103)

To see how the present analysis might account for the sensitivity of  $\phi$ -agreement to impoverishment, suppose that, at least in Hindi, [+obj] constitutes a case shell, thereby insulating the DP’s  $\phi$ -features from a  $\phi$ -probe. This is formulated in (40).

(40) CASE OPACITY<sup>23</sup>

The case subfeature [+obj] shields a DP from triggering verbal agreement.

If impoverishment has not applied the DP contains the case feature  $\left[ \begin{smallmatrix} -obl \\ +obj \end{smallmatrix} \right]$ . Due to the presence of [+obj] this DP is inaccessible to a verbal  $\phi$ -probe. By contrast, objects to which impoverishment has applied bear the case feature [–obl]. Since [+obj] has been deleted, the DP is transparent for verbal  $\phi$ -probing and may hence control agreement.<sup>24</sup>

Turning to global splits, it is an essential property of the derivation depicted in figure 3 that impoverishment takes place *after*  $\phi$ -Agree. Consequently,  $\phi$ -agreement may not be sensitive to a distinction that is only later introduced into the structure. The present account therefore makes the falsifiable prediction that for global splits verbal agreement may *not* distinguish between zero and non-zero marked arguments.

The prediction cannot be tested for Yurok, which has portmanteau agreement morphemes. It is borne out for Umatilla Sahaptin and Kolyma Yukaghir: In Sahaptin, the subject *iwínš(-nim)* ‘man(-INV.ERG)’ in (5) triggers the same type of agreement (*i-* ‘3NOM’), regardless of its case

<sup>23</sup> Schütze (1997) and Rezac (2006, 2008) make similar proposals for  $\theta$ -related case. Also see Sigurðsson (2004).

<sup>24</sup> Under this perspective, it is neither abstract case nor m-case (i.e. the morphological exponent) that is relevant for verbal agreement (cf. Bobaljik 2008 for a proposal based on m-case and Legate 2008 for a recent defense of the abstract case view). Rather, (potentially) impoverished feature matrices constitute the input for agreement. This view is of course only tenable under the present assumption that Agree and impoverishment may interact.

marking. Likewise, in (7) from Yukaghir, the object never controls verbal agreement, regardless of the case marker it bears.

It is an interesting state of affairs that, although the present analysis attributes local and global case splits to the same mechanism, it nevertheless predicts these splits to systematically differ with regard to verbal agreement.

## 6. Conclusion and outlook

In this article I have proposed a unified account of local and global case splits. The crucial ingredient of the analysis is that impoverishment and Agree are not extrinsically ordered but may affect each other. Specifically, Agree may percolate the effects of strictly local case-impoverishment operations, leading to a seemingly global operation. Differences between local and global case splits reduce to differences in constraint ranking. A second component of this analysis is that impoverishment is systematically constrained by harmonic alignment of markedness scales. These principled restrictions on impoverishment capture the striking similarity of both local and global splits: morphological marking corresponds to hierarchical markedness. Impoverishment is thus considered functionally motivated and constrained.

The present account is empirically motivated by the observation that, cross-linguistically, local and global case splits exhibit the same properties, which are fairly straightforwardly captured in an impoverishment-based system. The two hallmark features of case splits are that, first, the morphological markedness of the exponents taking part in the alternation correlates with hierarchical markedness and, second, alternations may involve more than one overt marker and a principally arbitrary number of exponents.

It is worth bearing in mind that the two main claims advocated here – (i) impoverishment and Agree interact, and (ii) impoverishment results from harmonic alignment of scales – are not logically connected and thus largely independent. The matter is an empirical one, with case splits serving as a rather direct argument for this particular combination of assumptions.

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## A-chains, arguments, and maturation in child passives

Susannah Kirby

In this paper I evaluate the claim that semantic difficulties underlie young English-acquiring children's poor performance on the passive construction. I propose an account of children's comprehension of the passive that rests on the prototypicality of subjects being agents and objects being patients, arguing against the view that difficulty with passives results from an inability to form A-chains.

### 1. Background

The passive has been extensively studied in first language acquisition. This construction is a marked one in many adult languages; Keenan (1985) notes that passives play a more essential role in some grammars than in others, and that some languages lack passives entirely. Furthermore, given that a language does have the passive, there appears to be a markedness hierarchy of what kind of passives appear. Basic passives — those with no agent (i.e., *by*-) phrase and formed from a transitive main verb which (in the active) takes an agent subject and patient object (e.g., *The steak was eaten*) — are the most widespread.

Perhaps unsurprisingly, then, much research has indicated that the passive is late to be acquired by children. Naturalistic data from English (Horgan 1978), French (Sinclair, Sinclair, and Marcellue, 1971; cited in Suzman 1985), German (Mills 1985), and Hebrew (Berman 1985), among other languages, have shown that spontaneous full passives (that is, those with an agent *by*-phrase; e.g., (1)) are quite rare in child language until age 4;0 at the very earliest (English, German), if not much later (e.g., 8;0 in Hebrew).

(1) Neil was kissed by Louise

To explain this relatively late acquisition, Borer & Wexler (1987, 1992) proposed the *linguistic maturation hypothesis*, according to which certain linguistic constructions in UG (including those involving A-chains, such as the passive) are not immediately available to the child, but rather mature over time, just as do secondary sex characteristics. Structures relevant to the passive are assumed to mature around the age of 4;0; before this age, the *A-Chain Deficit Hypothesis* (ACDH; e.g., Babyonyshev et al. 2001) suggests that A-chains — specifically 'non-trivial,'

or subject-object, A-chains — are ungrammatical for the child, and predicts that passives will therefore not appear in spontaneous speech. Data from a number of other studies on the passive, both naturalistic and experimental, have been interpreted by various researchers as support for the maturation hypothesis (e.g., Horgan 1978; Mills 1985; Pierce 1992).

However, several researchers have suggested alternative accounts for the data which do not appeal to linguistic maturation. First, a number of authors have proposed that naturalistic data may have been incorrectly interpreted. For instance, several authors (e.g., Crain & Fodor 1993; Pinker et al. 1987) have pointed out that the corresponding scarcity of full passives in naturalistic adult speech is never interpreted as a lack of grammatical knowledge, but instead as evidence that the passive is a marked construction. Furthermore, some experimental studies indicating late access to passives have suffered from methodological flaws. Crain & Fodor (1993) suggest that in many cases, the actual cause for children's errors on experimental tasks is not a lack of linguistic maturity but rather the result of nonlinguistic cognitive demands, including sentence parsing, the planning of responses, and pragmatic presuppositions. This *nonlinguistic maturation hypothesis* proposes that experimental linguistic performance improves over time due to the maturation of these other cognitive abilities.

Yet another account of the passive in child speech takes a different tack, assuming that children initially depend heavily on semantics to support their syntactic interpretations. Proponents of the *semantic bootstrapping* hypothesis (e.g., Bowerman 1973; Grimshaw 1981; Pinker 1984) have suggested that children come to the language learning task with inborn knowledge and/or biases about semantics which allow them to 'bootstrap' into the syntax of their language. The details of the account vary from researcher to researcher, but the two major components hypothesized to be part of this task — namely, the categorization of words into syntactic classes, and the identification of syntactic functions within an utterance — are both connected inextricably to the (linguistic and cognitive) semantics of an utterance.

Important to the current discussion is the hypothesis that children initially rely very strongly on canonical notions of subject and object in their interpretation of all sentences. Specifically, children are biased to associate particular syntactic positions (e.g., 'subject' and 'object') with particular thematic roles (e.g., 'agent' and 'patient/theme,' respectively) in a consistent way. This default assumption should aid children when they are presented with a linguistic description of an observable event. For instance, upon hearing *The boy is kissing the girl* in the context of a kissing event, the child will be able to determine the word order of the ambient language — in the case of English, SVO, since the agent, mentioned first, should map onto the subject, and the patient, mentioned second, should map onto the object. As with syntactic categorization, child-learners must initially depend on utterances that display the canonical semantics (e.g., active-voice clauses with agentive-transitive verbs) as they build up a basic level of knowledge; only after building this foundation can they go on to identify subjects and objects that do not conform to this general scheme.

Such a strategy generally works well, considering that syntactic roles and semantic/thematic roles often line up in adult language, resulting in prototypical agent-subjects and patient/theme-objects, crosslinguistically (Fillmore 1968; Keenan 1976; Dowty 1991; Baker 1997).

However, the passive construction results in a syntactic-semantic 'mismatch,' in which themes — and not agents — surface as subjects. As would be predicted by the semantic bootstrapping approach, children who have not yet acquired the relevant syntax interpret



‘reversible’ passives (that is, those with two animate arguments; (2a)) as if they were active sentences, taking the first NP to be the agent (2b) (Bever 1970; Maratsos 1974; Mills 1985).

- (2) a. *Utterance*: Neil was kissed by Louise  
 b. *Child’s interpretation*: Neil kissed Louise

Taking such evidence into account, Hyams et al. (2006) have suggested that children’s trouble with the passive stems from a noncanonical arrangement of theta-roles. They propose the *Canonical Alignment Hypothesis* (CAH), which states that in the earliest grammar, any external argument (for instance, typically an agent) must map onto subject position (Spec, IP). Thus, it is not A-chains *per se* that give children trouble, but rather, only A-chains that result in a violation of the CAH.

If children’s failure on passive comprehension is due to problems overcoming a syntax-semantics mismatch, rather than trouble with A-chains, children should perform better on passives when prototypical theta-role mapping is not violated. Passives embedded under raising-to-object verbs (RO-EPs; shown in (3)) circumvent (surface, and perhaps deeper) violations of canonical roles in argument mapping, thereby providing an utterance type with which to test this prediction.

It should be noted at this juncture that the specific syntax underlying such constructions has been much debated in the literature (for an overview, see Davies & Dubinsky 2003); the major question has been whether the embedded subject actually “raises” to matrix object position, or whether it remains *in situ*, and is “exceptionally case-marked” (ECM) across the clause boundary by the matrix verb. For the purposes of this discussion, I will assume a raising structure of these utterances (e.g. Postal 1974), but see Kirby (2009) for more discussion on the notion that an ECM analysis may actually be more appropriate, given the entirety of the child data.

In RO constructions, semantic objects surface as syntactic objects — for instance, in (3), *Neil* is both the semantic patient of *kiss* and the syntactic object of *wanted/needed*.

- (3) Suki wanted/needed Neil<sub>*i*</sub> [*t<sub>i</sub>* to be kissed *t<sub>i</sub>* by Louise]

On the other hand, if trouble with the passive indeed results from a deficiency regarding non-trivial A-chains, children should perform equally poorly on RO-EPs, which involve movement from the object to the subject of the embedded clause, and secondarily to the object of the matrix clause.

Note that this ‘mismatch’ account of passives does not necessarily preclude an account in which the effects of *nonlinguistic* maturation are also at play; it may prove the case that in a situation of high cognitive load — such as the passive, which involves complex syntactic transformations — a syntax-semantics match provides a ‘protective’ function for children’s interpretations of passive utterances.

The research presented in the current paper served to test the contrasting predictions of the maturation account and the mismatch account.

## 2. *Experimental data*

The research described below tested the working hypothesis that children's difficulty with the passive construction is a result not of a pre-mature grammar which is unable to construct or interpret nontrivial A-chains, as has previously been claimed, but rather of the syntax-semantics mismatch posed by this construction. I tested this hypothesis with English-speaking children (ages 4–5) using two truth-value judgment tasks. Experiment 1 explored children's comprehension of full matrix passives; Experiment 2 then tested the same children's comprehension of passives embedded under RO verbs.

### 2.1. *Participants*

32 monolingual Standard American English-speaking children (ages 4;1.15–5;11.15) were recruited from the Chapel Hill, NC (USA), area to take part in the study. The 4-year-old group contained 8 boys and 8 girls and had a mean age of 4;6 (range: 4;1.15–4;11.12); the 5-year-old group contained 7 boys and 9 girls, and had a mean age of 5;5 (range: 5;0.18–5;11.15). Participants had no known linguistic (speech or hearing) impairment or other cognitive or developmental delays. Children received a small token gift for their participation.

To facilitate analysis of how the acquisition of RO and OC verbs proceeds within a single mental grammar, each child took part in both of the experimental tasks detailed below, so that a within-subjects examination of the data could yield a robust picture of the child's grammar.

### 2.2. *Materials*

Both experiments made use of the truth-value judgment task (TVJ; Crain & McKee 1985; Gordon 1996). In this task, children hear stories and see them acted out with small figurines. After each story, the child listens to a puppet make a comment about the story. The child's task is then to reward the puppet for his correct comments, and to punish him (i.e., provide him with a less attractive reward) for his incorrect comments. The child rewards the puppet by 'feeding' him a plastic orange, and punishes him by 'feeding' him a plastic piece of lettuce. Children receive test items separated by filler items, which are slightly easier, and are included to check for answer biases and/or inattention to the task.

The TVJ task has been used in a number of studies on child language and is highly regarded as an experimental technique, for several reasons. First, because it is a test of comprehension rather than production, it allows researchers to document grammatical knowledge at earlier points than they might otherwise be able to do. Second, because the task requires children to perform a simple judgment of truth-value, it requires absolutely no metalinguistic awareness whatsoever, and thus poses a much lower processing load on children. Moreover, the kind of variability seen with linguistic judgments (even with adults!) is not an issue. Finally, because the children indicate their answers with physical objects as well as verbal responses, this task can convincingly be used even with those children with shy temperaments, who may be reticent to speak around an unfamiliar experimenter.

### 2.3. Experiment 1: Matrix passives

In Experiment 1, children ages 4–5 were tested on their ability to interpret full matrix (non-embedded) passives. Experiment 1 comprised a truth-value judgment task, which has not previously been widely used (if at all) in tests of children’s comprehension of the passive construction. However, as Crain (1991) has shown, using the TVJ often allows researchers to create a pragmatically felicitous situation for the test item in question — an especially important issue for constructions like the passive, which are marked/rare in English. As a result of the pragmatic felicity afforded by the TVJ task, we might discover precocious linguistic competence on the passive — importantly, competence which is apparent at ages younger than previously claimed.

All test items in Experiment 1 involved full, reversible passives — that is, they included an agent *by*-phrase, and both subjects and objects were animate. All arguments were full lexical NPs (i.e., not pronouns). Test items with their target answers appear below.

- (4) The farmer had to send someone to the store to get him some bread. The goat couldn’t go because she was doing homework and the sheep was busy playing a game, so the farmer sent the pig to the store.

*The pig was sent by the farmer (T)*

- (5) This woman wanted to draw a picture of someone. The nurse was busy, and she had already drawn a picture of farmer, so she decided to draw the policeman.

*The woman was drawn by the policeman (F)*

- (6) The farmer had to choose one animal to take with him on his trip. The cow was too old, and the horse was too big to fit in the car, so he picked the sheep.

*The farmer was picked by the sheep (F)*

The outcome of Experiment 1 was that only 5-year-olds performed significantly above chance in their interpretations of basic (non-embedded) passive utterances. That is, the 5-year-olds, but not the 4-year-olds, had mastered matrix passives. The results of Experiment 1 are given in Table 1.

Age	Percent Correct
4	64.6
5	79.2*

\* $p < 0.01$

Table 1: Performance on Matrix Passives

The data was analyzed by age group (4, 5). A series of logistic regressions (with the standard error adjusted for multiple observations within subjects) were performed to compare the num-

ber of correct (adultlike) responses per age group to a chance level of performance (namely, 50%, since answers were binary yes/no). The test of the hypothesis that children performed at a chance level on matrix passive constructions was rejected for 5-year-olds ( $z = 3.08$ ,  $p = 0.0020$ ), but not for 4-year-olds, who did not perform above chance ( $z = 1.83$ ,  $p = 0.0667$ ).

These results are unsurprising, when we consider the existing literature on acquisition of the passive. In short, the children in Experiment 1 performed exactly as the extant literature predicted they would, given their age and native language. Thus, these results are in line with the current received wisdom on matrix passive acquisition: namely, that children do not seem to have adultlike knowledge on this construction until about 5 years of age.

Recall that until now, many researchers (especially Wexler and colleagues) have considered this phenomenon to be related in some way to some syntactic difficulty with A-movement. However, given the predictions of the semantic bootstrapping hypothesis, it is possible that children who failed on the task in Experiment 1 may still succeed on interpreting embedded passives (RO-EPs), in which syntactic and semantic roles align. Evaluating children who fail to comprehend matrix passives on their comprehension of RO-EPs will serve as a test for Wexler's hypothesis that it is specifically the non-trivial (subject-object) A-chain in the passive construction that causes children trouble. The A-chains in RO-EPs (e.g., *Suki wanted Neil<sub>i</sub> [t<sub>i</sub> to be kissed t<sub>i</sub> by Louise]*) are exceedingly non-trivial, in that they constitute object-subject-object A-chains. As a result, if Wexler is right, RO-EPs should prove as problematic for linguistically pre-mature children as do matrix passives. On the other hand, if children have less trouble with embedded than with matrix passives, the mismatch account would be supported over the maturation account.

#### 2.4. Experiment 2: Embedded passives

In Experiment 2, children were tested on their ability to interpret passives embedded under raising-to-object verbs. The participants and methods in Experiment 2 were identical to those in Experiment 1.

In Experiment 2, all test items had a pronoun matrix subject (to reduce processing load) and a lexical NP embedded subject and object. Each child received either *want* items, or *need* items. All vignettes and test items appear below.

##### (7) *Want* items

- a. The goat was playing with the tiger and the bear, and she said, 'Let's play a tickling game. I'll tickle myself, and the bear can tickle the tiger.' Everyone agreed, so the goat tickled herself, and the bear tickled the tiger.  
What did the goat do?  
*She wanted the tiger to be tickled by the bear* (T)
- b. Patrick said to Dora, 'Today I'm drawing pictures of all my friends. Can I draw a picture of you?' Dora said, 'Oh, I don't feel like having my picture drawn today.'

Why don't you go draw Clifford instead?

What did Dora do?

*She wanted Clifford to be drawn by Patrick (T)*

- c. The policeman had a horse that was so big! He loved his horse, but he was worried that if anyone else saw it, they would want to take it away and put it in a zoo. One day, while the policeman was riding the horse, he saw the farmer walking nearby. But the policeman rode away as quick as he could, so that the farmer wouldn't see them.

What did the policeman do?

*He wanted the horse to be seen by the farmer (F)*

(8) *Need* items

- a. Kermit said to Shrek, 'I was supposed to make dinner for Clifford, since he can't make it for himself, but I'm too tired. I need some help. Can you make dinner for him?' Shrek said, 'Sure, I can do that,' and went and made dinner for Clifford.

What did Kermit do?

*He needed Clifford to be fed by Shrek (T)*

- b. Winnie the Pooh said to Tigger, 'Somebody should call Elmo and invite him over to play with us. Do you have his telephone number? Can you call him up?' Tigger said, 'Yes, I can call Elmo,' and he went to call him and invite him over.

What did Winnie the Pooh do?

*He needed Tigger to be called by Elmo (F)*

- c. One day, Dora wanted to take pictures of her friends. She photographed Elmo, and then she wanted to photograph Cookie Monster, but she realized her camera wasn't working anymore. So she said to Elmo, 'Can you use your camera and photograph Cookie Monster? That way I will have all the photos I needed to take.'

What did Dora do?

*She needed Cookie Monster to be photographed by Elmo (T)*

It should be explicitly noted that if children were to interpret the embedded passives as if they were actives, they would fail on the task.

(9) He needed [Tigger to be called by Elmo]  $\neq$  He needed [Tigger to call Elmo]

The outcome of Experiment 2 was that *both* 4- and 5-year-olds behaved significantly above chance in their interpretations of passives embedded under RO verbs. The results of Experiment 2 are given in Table 2.

Age	Percent Correct
4	75.0*
5	79.2*

\* $p < 0.01$

Table 2: Performance on Embedded Passives

As in Experiment 1, the data was analyzed by age group, with logistic regressions performed to compare the number of correct responses per age group to a chance level of performance. The test of the hypothesis that children performed at a chance level on RO-EP utterances was rejected for both age groups: both 4- and 5-year-olds' performance was significantly above chance (4:  $z = 3.30$ ,  $p = 0.0010$ ; 5:  $z = 4.41$ ,  $p < 0.0001$ ).

As noted above, the results from the matrix passive test are unsurprising given previously existing evidence about development of the passive in speakers of Indo-European languages. However, it is precisely this context which makes the current findings on RO-EP performance so interesting: namely, that 4-year-olds, who fail to comprehend matrix passives at above-chance rates, do perform at above-chance levels on their comprehension of RO-EPs. Thus, contrary to the maturation account, but as predicted by the mismatch account, both groups — even 4-year-olds, who had failed to comprehend matrix passives — performed above chance in their interpretation of RO-EPs.

For ease of reference, children's comparative performance on matrix passives and embedded passives are summarized in Table 3.

Age	Matrix	Embedded
4	64.6	75.0*
5	79.2*	79.2*

\* $p < 0.01$

Table 3: Comparative Performance on Passives (Percent Correct)

Importantly, success on two tasks was not significantly correlated ( $r^2 = 0.076$ ,  $p = 0.1273$ ); that is, it was not simply the case that those children who succeeded on the RO-EP task were those who also succeeded on the matrix passive task.

It should be noted here that the fact that children perform better on RO-EPs than on matrix passives suggests that processing load issues (e.g., utterance length and/or transformational complexity) do not necessarily play a more significant role in performance here than grammatical (e.g., the syntax-semantics interface) issues do.

The comparative evidence from the matrix and embedded passive tasks allows us to evaluate Wexler's claims that it is the non-trivial (i.e., subject-object) A-chain which results in linguistically non-mature children's poor performance on passives. If Wexler's claim is right, we would expect children to perform just as poorly — if not worse — on embedded passives (which in-

clude a double non-trivial A-chain) as they do on matrix passives. However, the results on children's comprehension of RO-EPs clearly refute the maturation approach; non-trivial A-chains do not appear to be the determining factor in children's poor performance on the passive.

The data presented here dovetails with independent evidence that children have no trouble with A-chain formation (e.g., in subject raising, Köppe 1994; *get*-passives, Fox & Grodzinsky 1998; and unaccusatives, Guasti 2002). These findings also make sense in light of adult aphasia studies indicating that processing load increases with non-canonical theta-role ordering (Caplan & Hildebrandt 1988).

### 3. Naturalistic data

Although the TVJ task allows researchers to probe comprehension of grammatical constructions, linguistic knowledge naturally also comprises productive competence. Thus, the opposing hypotheses about passive acquisition considered here make predictions about the appearance of passives in spontaneous child speech: specifically, the mismatch/CAH account predicts that embedded passives should arise earlier in spontaneous speech than do matrix passives.

As a final piece of evidence bearing on the issues of maturation versus thematic alignment mismatches, all files in the English (USA) corpora in the CHILDES database (MacWhinney & Snow 1985; MacWhinney 2000) were searched for spontaneous uses of full matrix passives and passives embedded under RO verbs. The CLAN program was used to isolate instances of these constructions.

For matrix passives, the search string 'BE \* by' was used, where \* is a 'wild card' search term which returns any number of lexical items, including zero. Each conjugated form of *be* was put into this string separately. The results were examined by hand to select only the relevant utterances. Children's spontaneous full matrix passives in all corpora appear in (10).

(10) Children's matrix passives (filename, age)

- a. *no a monarch butterfly was killed by a bird* (abe118, 3;7.21)
- b. *I heard that man say he was never bitten by that snake* (abe128, 3;8.23)
- c. *the dinosaur was eaten by the alligator* (abe130, 3;9.0)
- d. *Elsa could have been shotten by the hunter right?* (abe190, 4;8.0)
- e. *no but a lot of the buildings are surrounded by mice* (abe193, 4;8.14)
- f. *your father was killed by a trecher of mine* (boys47, 3;11.30)

- g. *Artoo and Threepee<sub>O</sub> were soon picked up by Jawas* (boys47, 3;11.30)
- h. *I'm putting my bike in here so it won't be taken by the burglars* (boys53, 4;6.01)
- i. *the candy was eaten by the ewok* (boys77, 4;8.02)
- j. *the cat was chased by the dog* (boys77, 4;8.02)
- k. *silly putty is made by eggs* (ded, 4;6-5;0)
- l. *his horse is killed by men* (ethmt3, 5;7.02)

For embedded passives the search string 'WANT/NEED \* to be' was used, with all possible forms of the verbs entered separately. (*Want* and *need* were put into this string separately; however, only passives embedded under *want* were found.) Again, the results returned by these searches were examined by hand to select only the relevant utterances. Children's spontaneous embedded passives for all corpora appear in (11).

(11) Children's embedded passives (filename, age)

- a. *I didn't want my hand to be holded* (abe060, 2;11.30)
- b. *I don't want this to be cut* (abe105; 3;5.29)
- c. *I don't want this to be thrown away* (abe112, 3;6.22)
- d. *I don't want the bird to get eated* (adam34, 3;7.07)
- e. *I don't want my stick to be hit* (boys31, 2;11)
- f. *I don't want Marky to get spanked* (boys55, 4;9.20)
- g. *Mark do you ever want us to get this things to get done* (boys69, 6;0.02)
- h. *I don't want you to get hurt either* (chj, 4;6-5;0)
- i. *I want uhm [:them] to be pulled [!]* (gas, 4;6-5;0)
- j. *I want the story to get listened to* (mig, 4;6-5;0)



- k. *hey do you want her to get fired from her job* (third, 8;0)

The results of these searches are not overwhelming, considering that both constructions are fairly rare in spontaneous speech. However, given the small quantity of data, the results tend to support the mismatch account: children appear to produce embedded passives much earlier (as young as 2;11) than they produce full matrix passives (3;7) in spontaneous speech. Even if Abe — clearly a precocious child speaker — is excluded from the analysis, embedded passives are still attested at younger ages than are matrix passives (3;7 versus 3;11, respectively).

Thus, data from spontaneous production, like that from comprehension, supports the mismatch account over the maturation account. It appears that thematic alignment (and not trouble with A-chains) is the primary driving force constraining children's acquisition of the passive.

#### 4. Conclusion

In the experiments presented here, English-speaking 4- and 5-year-olds were tested on their comprehension of matrix and embedded passives. Although 4-year-olds failed to comprehend matrix passives in an adultlike way, instead interpreting them as active-voice utterances, both age groups performed significantly above chance in interpreting passives embedded under raising-to-object verbs. These results are especially striking considering the greater syntactic complexity and length (both traditionally considered to be correlated with processing load) of RO-EPs, in comparison to matrix passives.

I propose that children's pattern of performance in these two experiments are part of a larger cluster of strategies that I will refer to as *semantic scaffolding* (for more detail, see Kirby 2009). Specifically, I submit that children rely strongly on the semantics of the utterances — including, but not limited to, canonical syntax-semantics matches, as claimed by the CAH — to aid in interpretation, until syntactic representations and processing power reach adult levels.<sup>1</sup>

The semantic scaffolding hypothesis unites a number of previously unconnected proposals about children's exploitation of semantics in syntactic acquisition. For instance, both the semantic bootstrapping hypothesis (Bowerman 1973; Grimshaw 1981; Pinker 1984) and the Canonical Alignment Hypothesis (Hyams et al. 2006) propose that children have default expectations about how the assignment of theta roles will proceed, and as a result, about the shape that sentential frames will take. These expectations are actually interrelated; first, the child is hypothesized to assume that agents map onto subjects and patients/themes map onto objects. However, this hypothesis also entails default assumptions about what an 'agent' or 'patient/theme' looks like, and thus what makes a good *subject* or *object*. Both of these expectations are encompassed by the notion of semantic scaffolding. Importantly, children appear to expect that semantic agents

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<sup>1</sup>Semantic scaffolding also incorporates the notion that children take the clause (that is, the verb plus its arguments) to be the smallest semantically complete proposition, and that single clauses form the default processing unit when children's processing power is overtaxed. This effect was robust in two other tasks, not reported here, in which children were required to perform grammaticality and semantic felicity judgments on biclausal RO and object control utterances. The hypothesis also suggests that subject-first word order may be cognitively primitive, arising from domain-general processes. See Kirby (2009) for details.

will appear in sentential subject position, and that semantic patients will map onto syntactic objects. Thus, for a language like English, the default shape of an utterance, and the shape which a child is biased to expect, includes preverbal agent subjects and postverbal patient/theme objects. Importantly, this predicts that sentences *not* conforming to this template — for instance, matrix passive utterances — will prove problematic for children’s grammars. Matrix passives violate this expectation by swapping the canonical alignment of the agent and patient theta roles: patients now appear as subjects, and agents (if they appear at all) surface as obliques/objects of prepositions. In contrast, though, in embedded passives — in particular, in those passives appearing under RO verbs — underlying objects appear as surface objects, thereby circumventing violations of the CAH.

Taken together, children’s performance on comprehension of matrix passives and RO-EPs, as well as spontaneous data attested in the CHILDES corpora, provide strong support for the semantic scaffolding hypothesis, and specifically for the proposal made by the CAH that interpretation of passives by young children hinges on the match between syntactic and thematic roles, rather than on the ability to form A-chains. In RO-EPs like those tested in Experiment 2, D-structure objects surface as S-structure objects, even though they pass through an external argument position between the two levels of representation. The results of Experiment 2 would then indicate that the CAH acts as a kind of filter, only looking at the head and the tail of the chain, and not at the intermediate traces, which — in the case of RO-EPs — *would* result in a violation of canonical alignment.

The semantic scaffolding analysis of children’s performance here, which relates to the prototypicality of agent-subjects, is moreover attractive in that it may explain a puzzling distinction seen in children’s patterns of competence on *get*-passives versus *be*-passives. It has long been acknowledged that children produce *get*-passives before *be*-passives; experimental data (Crain, Thornton, and Murasagi, 1987; cited in Crain & Fodor 1993) has elicited production of full *get*-passives by children as young as age 3. Although Fox & Grodzinsky (1998) present a cogent argument that the syntax of *get*- and *be*-passives is identical, it could be contended that the comparison is not an entirely fair one, since there is a subtle semantic difference between the two. Specifically, the patient-subjects in *get*-passives are more ‘active’ in terms of the semantic role they play. To illustrate, compare the following *be*- and *get*-passives.

(12) BE: Louise was kissed

GET: Louise got kissed

In the *be*-version, it is possible that all that the patient-subject ‘Louise’ did was to hold still. However, in the *get*-version, Louise may have played a much more active role in bringing the kiss about: namely, she may have flirted, tried to look cute, or even asked someone to kiss her.

Now compare *Louise got kissed* with *Louise got herself kissed*. Although the latter utterance is not completely synonymous with the non-reflexive *get*-passive, it is certainly congruent with the more active interpretation.

The distinction between the two passive types may also have to do with the fact that *be* is stative and *get* is eventive; thus, a *get*-passive will more closely match children’s default

expectation that the verb in a NVN sequence represents an action, as claimed by Grimshaw's CSR principle. In sum, there is just something much less 'passive-y' about *get*-passives, and it may be exactly this semantic distinction that allows children to master them before they master the corresponding *be*-passives.

Finally, the semantic scaffolding analysis is desirable insofar as it provides a better, more refined definition for the concept of 'processing load' than has previously existed — and furthermore, that it makes use of the concept of cognitive load without giving up on a grammatical account of acquisition. As seen in the results on matrix and embedded passives detailed above, processing load does not appear to be monotonically correlated with sentence length or transformational complexity, considering that some children who succeeded on interpretation of the longer RO-EPs (with their longer A-chains) failed to interpret the shorter matrix passives. Instead, the semantic scaffolding approach equates processing load with 'novelty of construction,' including the noncanonical assignment or appearance of theta-roles, and predicts that utterances which violate children's default assumptions about the syntax-semantics interface will result in a greater cognitive load (and poorer performance) than utterances which match those assumptions.

Hyams et al. (2006:1064) suggest that their own *Canonical Alignment Hypothesis* might be

part of a broader hypothesis about the architecture of the language faculty along the lines proposed in Williams' (2003) representation theory. Williams proposes that syntax economizes on 'shape-distortion', rather than 'distance minimalization'... In certain cases, as for example in the formation of passives, structural shape may be distorted at one level in order to satisfy mapping at a different level. Thus, the mapping between thematic and case levels in passives results in non-isomorphic structures because the mapping between the case domain and surface structure needs to be isomorphic. The absence of verbal passives in early language (and their occurrence in adult language) would suggest that children adhere more strictly to 'shape preservation' than adults, who have greater pragmatic and processing resources.

This formulation is very close to what is suggested by the semantic scaffolding hypothesis discussed here.

In short, not only passives but *any* such syntax-semantics mismatch results in the performance patterns seen in the experiments detailed above: children deal with various types of syntactic 'strangeness' in ways that are non-adultlike, but still governed by the grammar. The semantic scaffolding analysis has the appeal of explaining how UG may still constrain and guide non-adultlike performance, even in the face of processing limitations.

#### *Appendix: Wider application of the theory*

Semantic scaffolding relates a number of previously observed phenomena under one rubric: children's earliest grammars do rely strongly on semantics. The effects of semantic bootstrapping have been seen in many arenas, as discussed above. Furthermore, children's earliest productions seem to be stripped down to the barest of lexical categories: first words tend to be

nouns (Gentner 1982), and the earliest multi-word utterances also generally exclude functional categories. A number of researchers (e.g., Braine 1963; Klima & Bellugi 1966; Radford 1988, 1995) have put forth variations on the hypothetical theme that children's earliest grammars lack some (or all) of the adult functional projects — in essence rendering their linguistic representations primarily semantic, rather than syntactic. While the current proposal instead assumes full competence (à la Poeppel & Wexler 1993), it bears noting that the observations made by the researchers just mentioned, and the data to be explained here, do indeed dovetail. Identifying such disparate processes (for instance, the shape of children's earliest productions, their interpretations of passives, their performance on biclausal sentence judgment tasks, and so on) as belonging to (or stemming from) one larger cognitive phenomenon is always attractive to researchers seeking parsimony in their mechanistic explanations. Furthermore, the ability to see multiple processes as related in this way gives us a window into how language, itself a vertical faculty, may interface with other horizontal faculties, like attention and memory (Fodor 1983), as well as with other non-linguistic cognitive modules, such as the conceptual-semantic module.

The passive has long been generally accepted as a late acquisition, but children's performance on interpreting and producing passives does seem to be strongly influenced (or even directed) by the semantics of the construction, in various ways. Maratsos et al. (1985) used *who did it?* questions and a picture identification task to examine children's comprehension of physical action and mental state passives and found that children performed uniformly better on action passives (*Donald Duck is kicked by Batman*) than on mental passives (*Donald Duck is remembered by Batman*). Maratsos et al. suggest that difficulty with mental verb passives stems from a general semantic constraint on children's analysis of the passive, and the authors hypothesize that children may be using a gradient of semantic transitivity to determine which verbs may grammatically passivize.

Pinker et al. (1987) likewise found in an open-ended spontaneous production task that children robustly passivized canonical action verbs (those with agent subjects and patient objects), but were less willing to passivize 'anticononical' action verbs: those with agent objects and theme subjects. (None of these exist in English, nor do the authors mention any language in which they *do* exist. Following Marantz (1982; cited therein), Pinker et al. created and taught children artificial verbs for this category (e.g., *The dog floosed the giraffe*, meaning 'the giraffe leapfrogged over the dog'). Furthermore, children passivized nonactional verbs (*see*) less than actional verbs, and verbs of spatial relations (*suspend*, *contain*) were split, such that passivization of verbs with location subjects occurred more often than of those with theme subjects. (Both of these types of spatial relation verbs — location subject and theme subject — were, like the anticononical verbs, created by the researchers.) The authors propose that English verbs comprise three distinct groups: *canonical action* (universally passivizable), *anticononical action* (universally nonpassivizable), and *nonactional* (passivizable by individual language, on a class by class basis). Children are sensitive to the group of 'affectedness' constraints which define whether or not a nonactional can be passivized, and once they have picked up the thematic core of the passive, then canonical action verbs should be trouble-free; meanwhile, nonactionals must be learned — class by class — based on positive evidence.

The important point for our focus here, though, is that English-speaking children's readiness to passivize verbs appears to be related to how 'canonical' those verbs are, as viewed through the lens of semantic bootstrapping. In particular, Grimshaw's (1981) formulation of the *Canonical*

*Structural Representation* (CSR) principle suggests that children initially assume that verbs map onto actions, and only later expand the class of verbs to include less canonical instances.

Returning for a moment to children's performance on *be-* and *get-*passives, Fox & Grodzinsky (1998) likewise suggest a semantic explanation for the phenomenon, albeit a different one. Namely, they argue that children have trouble with 'theta transmission,' which is thematically unlimited and involves the transmission of a theta role that is otherwise realized by an implicit argument. The non-passive preposition *by* can only assign the theta role 'affecter'; children who attempt to interpret all passives by assuming that the *by*-phrase assigns an 'affecter' theta role will only succeed in actional passives (including *get*-passives). For other semantic roles, however, theta transmission is necessary. Children thus fail in interpreting passives only when the process of theta transmission is necessary: when they try to allow *by* to assign an affecter theta role to a nonactional predicate, the resulting interpretation is incoherent, and semantic interpretation is impossible. In short, *get*-passives are actional, and therefore 'interpretable' for children who have not yet mastered this process of theta transmission. However, if children are interpreting them through a process which does not in fact utilize theta transmission, their interpretation at this stage may be argued to be non-adultlike.

Regardless of the exact details of the account, both analyses of the *get*-passive crucially assume a distinction in semantics which hinges on agency and action, and which allows children to master this construction earlier than the related *be*-passives; this order of acquisition is exactly what semantic scaffolding would predict.

Interpretation of the passive is not the only domain in which the effects of semantic scaffolding can be seen. A number of other researchers (Chapman & Kohn 1977; Maratsos 1974; Lempert 1989; Hyams et al. 2006) have observed the effects of children's initial assumptions about theta-structuring in numerous arenas, and have linked children's non-adultlike behaviors to these default assumptions about the appearance of agents and themes. For instance, Chapman & Kohn (1977) presented evidence that children performed better on interpreting sentences with animate (agent) subjects and inanimate (theme) objects than on sentences with inanimate subjects and animate themes — that is, they performed best with Dowty-esque (1991) 'proto-agent' subjects and 'proto-theme' objects. As a related phenomenon, it is also known that the passive construction is late to be acquired in spontaneous speech, and that young children also perform poorly on interpretations of passive utterances before they begin to produce the passive themselves. As an example, Maratsos (1974) found that children appear to use a default 'behavioral' strategy when confronted with NVN sequences; this processing shortcut leads them to interpret such sequences (regardless of syntax) as actor-action-object utterances. In related work, Lempert (1989) found that children (ages 2;6–5;3) produced more novel passive sentences when they had been trained on items with animate (rather than inanimate) patients — which, in passive utterances, surface as subjects. All these results are consistent with the hypothesis that for children, the 'concept' of *subject* is a category that includes animacy as a component (Lempert 1989).

Note that semantic scaffolding predicts that *all* instances of noncanonical alignment of theta roles (and not just passive constructions which violate canonical alignment) will prove problematic for children. Given the literature on both spontaneous production and the experimental data, this prediction seems to be on the right track. In naturalistic production, Pinker (1984) notes that verbs which violate these canonical 'agent-subject, patient-object' mappings in En-

lish (e.g., *receive*, *please*, *strike as*, *undergo*) are almost nonexistent in children's spontaneous productions. Likewise, in the experimental literature, Marantz (1982) taught 3-, 4-, and 5-year-old English-speaking children to describe observed scenes with novel (made-up) verbs which differed in terms of their structural expressions of agents and patients: AP (agent-patient) verbs had preverbal agents and postverbal patients, while PA verbs had the opposite alignment. For instance, given a scene in which a man was pounding a book with his elbow, *Larry is moaking the book* would utilize an AP verb, while *The book is puming Larry* would make use of a PA verb. Marantz found that 3- and 4-year-olds had much less trouble learning the AP verbs than the PA verbs, since AP verbs display what he refers to as 'a dependence on grammatical rules which connect semantic relations directly to structural positions' (p. 32). In other words, AP verbs appear in utterances which display a non-deformed (canonical) alignment of thematic roles.

Marantz (1982) also tested AL (agent-location) and LA verbs (e.g., *Larry is dabing the chair* or *The chair is dabing Larry*, respectively, to describe a scene in which Larry is rolling his head while standing on a chair). He found that children performed equally well or better on verbs that expressed a patient (i.e., AP and PA verbs) than on those that expressed a location (AL and LA verbs), and that the most difficult type of verb to learn appeared to be the LA verbs. While the AL/LA verbs are not as transparently relevant to our discussion here as the AP/PA verbs, children's performance on the former may in fact indicate default expectations not only about the *alignment* of theta roles appearing in a particular utterance, but also simply about the *particular theta roles* that will appear in an utterance to begin with. That is, the child may well be biased to expect an agent and a patient, rather than any other constellation of thematic elements.

Although the verbs used in the experiment were nonce forms, both AP and PA verbs exist in English, although AP verbs are by far more common (Marantz 1982). For instance, *give* is an AP verb (*Louise gave Neil an espresso machine*), while *receive* is a PA verb (*Neil received an espresso machine from Louise*). Note that the same giving event could be described using a passive, as in *Neil was given an espresso machine by Louise*; this would result in an identical alignment of arguments as in the PA (*receive*) utterance.

It is hard to determine direction of causality when children have difficulty with a verb type which utilizes a particular thematic alignment, but that alignment is confounded with the relative frequency of that verb type in the adult language. MacWhinney (1982) notes that in languages such as Japanese and Tagalog, in which no such tendency of preverbal agents is present in the adult language, there is only weak evidence (at best) for such an AP preference in child language. Meanwhile, in Hungarian and Turkish — which both allow the PA ordering in the adult language — there is no such tendency in the child language at all. MacWhinney notes that English is therefore not a useful test case, and that data on comprehension and production from VOS languages like Tagalog will be more helpful in teasing apart the relative contributions of input frequency and UG biases.

What we have, then, is an instantiation of a chicken-and-egg scenario regarding effects of input frequency versus effects of UG. Are AP verbs more common in English because they are easier to understand, perhaps as a result of semantic scaffolding and canonical alignment (i.e., UG/acquisition drives the shape of adult language)? Or are these verbs easier to understand because they are more common in English (i.e., the shape of adult language input influences

acquisition)? The answer to this dichotomous question may prove to be ‘yes’ — that is, both phenomena may be at work. Even if not, the two possible causalities will certainly prove difficult (if not impossible) to disentangle. As suggested by MacWhinney, data from the acquisition of languages whose adult lexica — unlike English — do *not* show a preference for AP verbs will be vital in this attempt.

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## Internal DP heads in restrictive relative clauses

Jorie Koster-Moeller

In this paper, I show that restrictive relative clauses can be internally headed by a DP, not, as is standardly assumed, an NP. Syntactically, the internal copy of a relative clause head licenses constructions that I show can only be licensed by a full DP: movement out of weak islands and parasitic gapping. Semantically, relative clause heads hosting ACD sites show scopal sensitivity to elements inside the relative clause, which requires an analysis with a full copy of the DP head originating inside the relative clause. Based on both theoretical considerations of licensing ellipsis and empirical diagnostics, I argue that relative clauses require a modified matching analysis, such that a full copy of the DP hosting the relative clause originates inside the relative clause itself, with second copy of the DP in the matrix clause.

### *1. Overview*

It is widely assumed that restrictive relative clauses can have two potential structures: a raising structure, where the NP of the DP hosting the relative clause originates inside the relative clause and a matching structure, where the NP is generated outside of the relative clause, which in turn contains an identical (matched) but elided version of the same NP (Vergnaud 1974, Carlson 1977, Bhatt 2002, Sauerland 1998, Sauerland&Hulsey 2006).

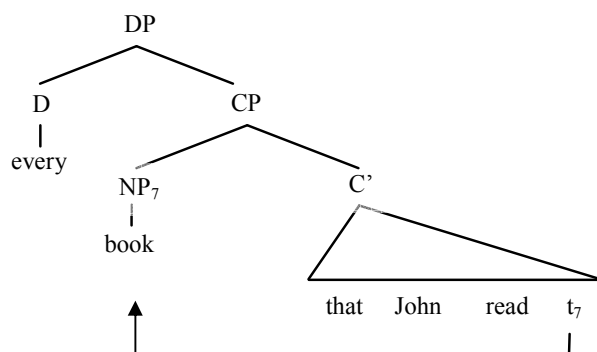
Though a third option is available, in which the internal component of a matching relative clause is a null operator, rather than a copy of the external NP, I follow among others, Safir (1999) and Sauerland (2002), in assuming that both forms of relative clause have a complex internal-head, rather than a phonetically null operator which binds the trace position. Many arguments have been laid out to support this analysis, including reconstruction effects, Double Headed ACD, pronoun and variable binding, and weak-crossover violations (Sauerland 2002).

The standard assumption that this complex internal-head takes the form of a NP (Kayne 1994, Sauerland 2002) gives rise to the raising structure, (1) (1), and the matching structure, (2)(2).

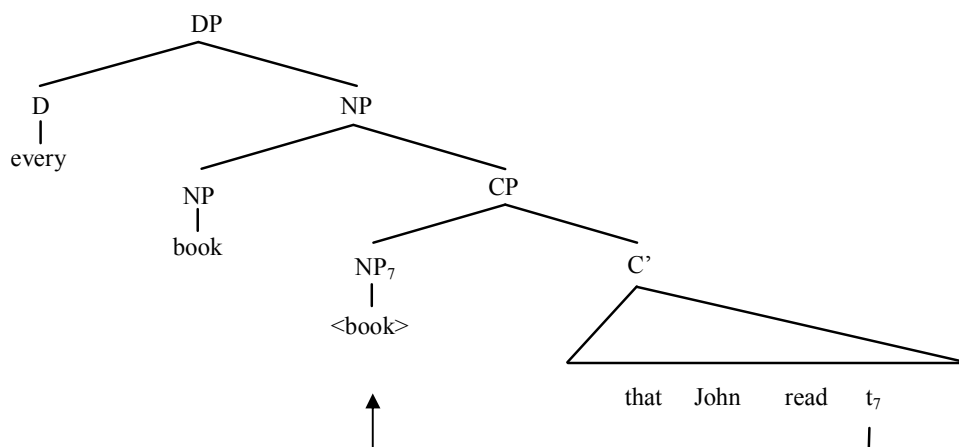
(1) Raising:                    ... [DP every [CP book<sub>i</sub> that Mary read t<sub>i</sub>]].

(2) Matching:                ... [DP every [NP book [CP book<sub>i</sub> that Mary read t<sub>i</sub>]]].

(1(1)')



(2(2)')

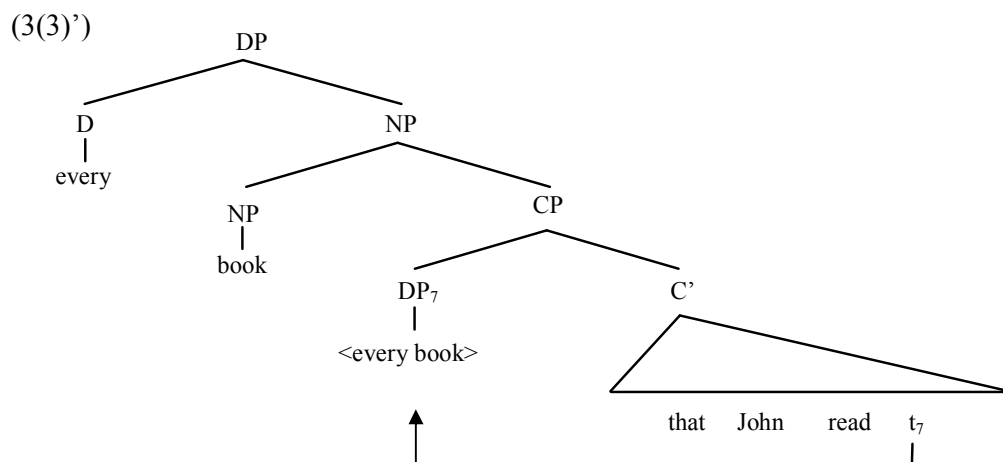


In this paper I will argue that, at least for some relative clauses, these complex internal-heads are not correctly characterized as NPs, but behave syntactically and semantically like full DPs.

In Section 2, I argue that syntactically, relative clause heads license constructions (movement out of weak islands and parasitic gaps) that I show only DPs can license. This argues for an analysis of relative clause heads not as NPs, but as full DPs. In Section 3, I argue that these DPs must be full copies of the external DP (rather than a DP-like operator or indefinite) in order to account for scope interactions in relative clauses hosting ACD sites. In Section 4, I present an ellipsis licensing algorithm that accounts for the data in Section 3, and consider the possible structures for these three constructions. I argue that only a modified matching analysis of relative clauses is theoretically sufficient to account for the data. Finally, in Section 5, I test the empirical predictions of this conclusion, showing that relative clauses with weak islands, parasitic gap sites, and ACD sites are necessarily matching. I draw two conclusions: first some relative clauses must have an internal copy of the DP head, and second that these relative clauses must have a matching structure, (3).

(3) Modified Matching     ... [DP every [NP book [CP ~~every book~~<sub>1</sub> that Mary read t<sub>1</sub>]]].<sup>1</sup>

<sup>1</sup> Note that I will use ~~every book~~ to indicate phonetically unpronounced material, and either t<sub>1</sub> or <every book><sub>1</sub> to indicate the bottom of a movement chain.



## 2. Relative clauses and DP movement

It has been previously argued that relative clause heads can behave like DPs (De Vries, 2002, Borsley, 1997). However, these arguments fail to show that minimally different NPs *cannot* share this behavior. In this section, I show that there are specific types of constructions that DPs, but not NPs, license, specifically extraction out of weak islands and parasitic gapping. NPs are non-referring predicates and pattern with adjunct extraction in that they are sensitive to weak islands, and cannot license parasitic gaps. DPs, on the other hand, carry a “referential” index (Cinque 1990, Rizzi 1991) which allows them to escape weak islands and license parasitic gaps. When tested in English and German, I show that the internal copy of the relative clause head licenses both these constructions, which, if analyzed as a bare NP, should not be possible. This is a strong argument in favor of treating relative clause internal heads as DPs, not NPs.

### 2.1. Weak island effects

Weak islands, such as those created by embedded *wh*-words, show sensitivity to A' movement: Specifically, A' movement out of a *wh*-island is easiest for argument DPs, (4(4)a), and hardest for temporal/manner adjuncts, (4(4)b). Following Rizzi (1991, 2001), I assume that only A' chains involving DPs can cross weak island boundaries (marked by [ ]).

- (4) a. Which problem<sub>1</sub> do you wonder [how to solve <which problem><sub>1</sub>]?  
 b. \*How<sub>1</sub> do you wonder [which problem to solve <how><sub>1</sub>]?

This generalization extends to include a constraint on the extraction of predicates. Baltin (1992) observed that predicates do not readily extract from *wh*-islands, in contrast with full DP extraction. Though both the subject and predicate of a small clause can undergo *wh*-movement, (5)(5), asymmetry arises when the movement is out of a *wh*-island, (6)(6).

- (5) a. How many people<sub>1</sub> do you consider t<sub>1</sub> intelligent? (Baltin 1992)  
 b. How intelligent<sub>1</sub> do you consider John t<sub>1</sub>?
- (6) a. ?How many people<sub>1</sub> do you wonder [whether I consider t<sub>1</sub> intelligent]?  
 b. \*How intelligent<sub>1</sub> do you wonder [whether I consider John t<sub>1</sub>]?

Assuming a standard analysis of NPs as predicates (Heim & Kratzer, 1998), these island effects predict that bare NPs cannot extract out of weak islands.

I look to German to examine cases of bare NP, rather than full DP, movement. In German, both DPs and NPs can be topicalized to the front of the root (SpecCP), as in (7)(7).

- (7) a. (Nur) ein Haus<sub>1</sub> hat er t<sub>1</sub> verkauft. *DP topicalization*  
 (only) one house<sub>1</sub> has he t<sub>1</sub> sold.  
 ‘He sold (only) one house.’
- b. Haus<sub>1</sub> hat er eines t<sub>1</sub> verkauft. *NP topicalization*  
 house<sub>1</sub> has he one t<sub>1</sub> sold.  
 ‘He sold one house.’

However, this topicalization is asymmetrically subject to island constraints – while *wh*-phrases, (8)(8), and DPs, (9)(9), can undergo topicalization out of a (negated) factive infinitival complement clause (a weak island), NPs cannot, (10)(10). Moreover, the data in (11)(11) shows that Split-NP topicalization is grammatical if the splitting occurs after the full DP is extracted out of the island, indicating that it is specifically the NP-movement out of the island that causes the ungrammaticality of (10)(10).

- (8) Was<sub>1</sub> hat er (nicht) bedauert [t<sub>1</sub> verkaufen zu muessen]? *Wh movement*  
 What<sub>1</sub> has he (not) regretted [t<sub>1</sub> sell to must]?  
 ‘What did he (not) regret having to sell?’
- (9) (Nur) ein Haus<sub>1</sub> hat er (nicht) bedauert [t<sub>1</sub> verkaufen zu muessen] *DP Movement*  
 (only) one house<sub>1</sub> has he (not) regretted [t<sub>1</sub> sell to must]  
 ‘Only one house is such that he has (not) regretted having to sell it.’
- (10) \*Haus<sub>1</sub> hat er (nicht) bedauert eines [t<sub>1</sub> verkaufen zu muessen] *\*NP Movement*  
 house<sub>1</sub> has he (not) regretted one [t<sub>1</sub> sell to must]  
 ‘house is such that he has (not) regretted having to sell one’
- (11) Haus<sub>2</sub> hat er [(k)eines t<sub>2</sub>]<sub>1</sub> bedauert [dass er t<sub>1</sub> verkaufen muss] *DP, NP Movement*  
 house<sub>2</sub> has he [(n)one t<sub>2</sub>]<sub>1</sub> regretted [that he t<sub>1</sub> sell must]  
 ‘No house is such that he has regretted having to sell it.’

The sensitivity of Split-NP topicalization extends to (weak) *wh*-islands, as well factive clauses, as shown by the contrast between (12)(12) and (13)(13).

- (12) (Nur) ein Haus<sub>1</sub> hat er sich gefragt [ob er t<sub>1</sub> kaufen soll]      *DP Movement*  
 (only) a house<sub>1</sub> has he self asked [if he t<sub>1</sub> buy should]  
 ‘Only a house is such that he wondered whether he should buy it.’
- (13) \*Haus<sub>1</sub> hat ersich gefragt [ob er eines t<sub>1</sub> kaufen soll]      *\*NP Movement*  
 house<sub>1</sub> has he self asked [if he a t<sub>1</sub> buy should]  
 ‘house is such that he has wondered whether to buy it’

Together, these data strongly support the prediction that DPs, but not NPs, can undergo extraction out of weak islands.

This makes a fairly strong prediction for relative clauses: if relative clause heads are NPs, they should not be able to extract out of weak islands. However, relative clause heads can extract out of infinitival *wh*-complements and factive complements, in both English, (14)(14), and German, (15)b. This is quite unexpected under a standard analysis - these islands are inside of the relative clause, and the internal RC head (which must move to the top of the relative clause) must extract out of these islands before being combined with the external determiner. Treating these heads as NPs predicts that the internal head should never be able to escape the weak island, and so the derivation should crash. The fact that relative clauses hosting weak islands are grammatical, combined with the previous extraction data, seems to require an analysis of relative clause heads not as NPs, but as DPs.

- (14) a. The book<sub>1</sub> that we regretted [that John read t<sub>1</sub>]  
 b. The book<sub>1</sub> that we wondered [how to read t<sub>1</sub>]
- (15) a. Das Haus<sub>1</sub> dass er (nicht) bedauert hat [t<sub>1</sub> verkaufen zu muessen]...  
 the house that he (not) regretted has [t<sub>1</sub> sell to must]...  
 ‘The house that he didn’t regret having to sell.’
- b. Das Haus<sub>1</sub> dass er sich gefragt hat [ob er t<sub>1</sub> kaufen soll]...  
 the house that he self asked has [if he t<sub>1</sub> buy should]...  
 ‘the house that he wondered whether to buy’

## 2.2. Parasitic gaps

Borsley (1997) argues that, like *wh*-DP traces, relative clause heads can license parasitic gaps, as in (16)(16). However, he provides no evidence that NP-traces cannot also license parasitic gaps.

- (16) a. Which form<sub>1</sub> did Bill fill out t<sub>1</sub> without reading \_\_?  
 b. The form<sub>1</sub> that Bill filled out t<sub>1</sub> without reading \_\_ was...

Again, data from German can be used to shed light on the behavior of bare NPs. We see that that, unlike DPs, NPs cannot license parasitic gaps. In topicalized DP constructions, the DP-trace can license a parasitic gap, (17)(17); however, a topicalized NP-trace cannot, (18)(18).

- (17) Dieses Formular<sub>1</sub> hat er ohne durchzulesen \_\_\_ t<sub>1</sub> ausgefüllt ...  
 that form<sub>1</sub> has he without read through \_\_\_ t<sub>1</sub> filled out ...  
 ‘that form he has filled out without reading...’
- (18) \*Formular<sub>2</sub> hat er keines t<sub>12</sub> ohne durchzulesen \_\_\_ t<sub>1</sub> ausgefüllt ...  
 form<sub>2</sub> has he none t<sub>12</sub> without to read through \_\_\_ t<sub>1</sub> filled out ...  
 ‘He has not filled out a form without reading...’

Relative clauses in German, like English, do license parasitic gaps, strongly suggesting that the head must have the properties of a DP, not an NP, (19)(19).

- (19) Das Formular<sub>1</sub> dass er ohne durchzulesen \_\_\_ t<sub>1</sub> ausgefüllt hat ...  
 the form<sub>1</sub> that he without read through \_\_\_ t<sub>1</sub> filled out has ...  
 ‘the form that he filled out without reading ...’

This data shows that relative clause heads do license parasitic gaps, which is again unexpected under an analysis of relative clauses that treats the internal head as a bare NP. Together with the data in the previous section, it seems that relative clause heads are syntactically DPs and not NPs.

### 3. Scope restrictions in relative clauses with ACD

In this section, I argue, following Koster-Moeller & Hackl (2008) that the syntactic data pointing to an analysis of a relative clause head as a DP is strongly supported by the observation that the host-DP of the relative clause seems to be semantically (scopally) active inside the relative clause itself. Moreover, this evidence argues that the DP inside the relative clause cannot be any item that has the semantic status of a DP (e.g. *that* (De Vries, 2002), a null operator with DP-like semantics, an indefinite), but that the external determiner is present inside of the relative clause.

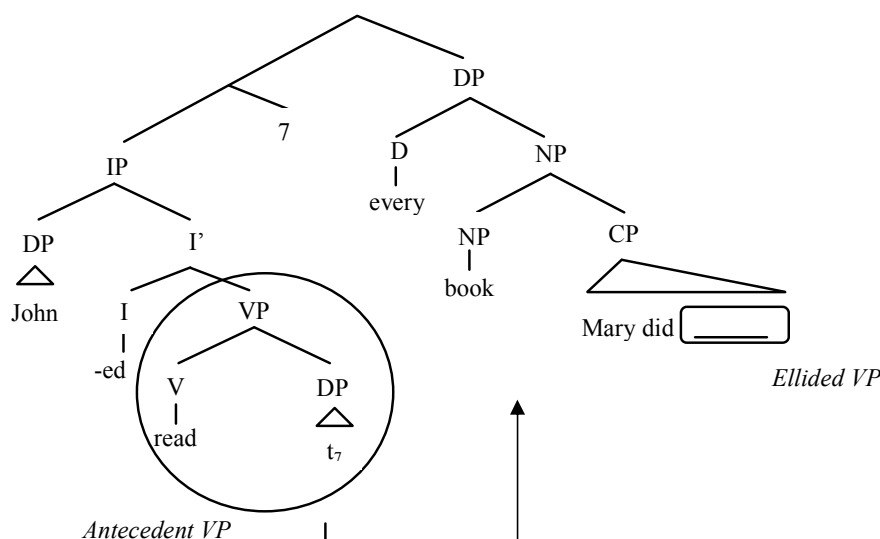
#### 3.1. Antecedent contained deletion (ACD)

ACD occurs when elided material,  , is properly contained within the expression that serves as its antecedent, (20)(20) (cf. Sag 1976 etc.).

- (20)

This seeming paradox can be resolved by raising the object DP hosting the relative clause and ellipsis site outside of the antecedent VP.

(21)



### 3.2. Quantifier scope restrictions in ACD structures

In standard relative clauses (RCs), both surface and inverse scope seem to be available between the matrix subject (*a professor*) and the host object-DP (*every book...*), not unexpectedly regardless of the semantic properties of the RC internal subject (*Mary/a student*).

(22) a. A professor read every book that Mary wrote.

Surface: A single professor read every book that Mary wrote.

Inverse: Every book that Mary wrote is such that some (different) professor read it.

b. A professor read every book that a student wrote.

Surface: A single professor read every book that a student wrote

Inverse: Every book that a student wrote is such that some professor read it

However, in relative clauses that contain an ACD site, there seems to be additional restrictions on scope of the host DP. Specifically, in (23(23)a), we observe that inverse scope of the DP *every book that Mary did* over the matrix subject *a professor* is difficult compared to (22(22)a). In (23(23)b), however, which differs only in that the relative clause subject is the indefinite *a student*, inverse scope seems to be as easy as in the relative clause without ACD, (22(22)a,b).<sup>2</sup>

<sup>2</sup> This contrast is rather reminiscent of Fox's (1995) Scope Economy generalization exemplified below.

(i) a. A boy read every book and Mary did too.

\*Inverse Scope

- (23) a. A professor read every book that Mary did. \*Inverse Scope ( $\forall > \exists$ )  
 b. A professor read every book that a student did. Inverse Scope ( $\forall > \exists$ )

Following Koster-Moeller & Hackl (2008), the contrast in (23)(23) is an instantiation of a larger generalization, (24)(24):

- (24) *ACD-Scope Generalization* (Koster-Moeller & Hackl 2008):  
 In a sentence of the form [... Op<sub>1</sub> ... [DP ... Op<sub>2</sub> ... <VP> ]], where Op<sub>1</sub> is a matrix operator, the DP is the host DP containing a relative clause with an ACD site, and Op<sub>2</sub> an operator inside the relative clause, the DP can have inverse scope over Op<sub>1</sub> only if the DP and Op<sub>2</sub> are scopally non-commutative.

When considering the standard RC analysis in which the relative clause determiner merges outside of the relative clause, the generalization that the scopal relationship between the determiner and an operator inside the relative clause determines properties of the larger sentence is very surprising – the internal operator and the external determiner should not be able to interact dynamically.

The data in (25)(25) confirms that scopal non-commutativity between the host DP and the relative clause subject is the driving force behind the inverse scope restriction observed in (23)(23), as *every book* and *every boy* are scopally commutative, predicting correctly that inverse scope should not be available.

- (25) a. A girl read every book that every boy did. \*Inverse Scope ( $\forall > \exists$ )  
 b. A girl read every book that a boy did. Inverse Scope ( $\forall > \exists$ )

The data below show that the generalization extends to scope-taking operators besides DPs. In (26)(26), the inverse scope reading of the host DP over the matrix negation is only available when the RC also contains a scope taking element, in this case another negative operator. In fact, as (26)(26b) shows, when the RC does contain negation, surface scope is not allowed. Similarly, in (27)(27), inverse scope of the host DP over the modal operator *can* is only possible when RC also contains a modal operator.

- (26) a. Mary didn't read every book that John did. \* $\forall > \text{not}$   
 b. Mary didn't read every book that John didn't. \* $\text{not} > \forall$
- (27) Sue kissed two boys at the party last night. Mary can kiss at most one boy, but ...  
 a. ... she is allowed to kiss every boy that Sue kissed/#did. \* $\forall > \text{allow}$   
 b. ... she is allowed to kiss every boy that Sue was allowed to.  $\forall > \text{allow}$

The data in (22)(22)-(27)(27) shows that the relative clause determiner interacts scopally with relative clause-internal operators. Due to standard movement constraints regarding movement in and out of relative clauses, it is very difficult to treat this interaction as a genuine semantic interaction between the internal operator and the external determiner. Rather, it seems that

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b. A boy read every book and a girl did too.

Inverse Scope

However, because scope inside the relative clause is always fixed (the host DP *every book* is necessarily above the relative-clause subject *Mary/a student*), the conditions that govern Scope Economy do not seem to apply.



the internal relative clause head itself has systematic scopal force, a semantic property of quantificational DPs, not NPs. These data support the conclusion drawn from the previous section – we need an analysis of RC heads as an object with DP-like syntax and semantics. Moreover, this data suggests how that analysis should be implemented: the internal head seems to be identical to the external DP head of the relative clause.

#### 4. A focus semantic account

##### 4.1. Licensing ellipsis

Following Heim (1997), Merchant (2001), and specifically Rooth (2006), I assume that ellipsis licensing relies on a focus semantic notion of contrast rather than simple identity between ellipsis and antecedent constituent, as follows:

Ellipsis of a  $VP_2$  is possible only if there is an antecedent  $VP_1$ , such that there is a constituent (EC) that contains  $VP_2$  and that appropriately contrasts with an antecedent constituent (AC) containing  $VP_1$ ,  
 where  $\alpha$  contrasts appropriately with  $\beta$  iff the ordinary semantic value of  $\square$  entails the grand union of the focus semantic value of  $\alpha$ :  $[[\beta]]^o \Rightarrow \cup([[ \alpha ]]^f)$ .

Thus to satisfy ellipsis of  $VP_2$ , we need a structure as in (28)(28), which gives rise to the entailment relationship in (29)(29).

(28) [<sub>AC</sub> ...  $VP_1$  ...][<sub>EC</sub> ... < $VP_2$ > ...]

(29)  $[[AC]]^o \Rightarrow \cup([[EC]]^f)$ .

Following this algorithm, I give a calculation in (30)(30), where (a) represents the sentence (capital letters indicate F-marking), (b) the focus semantic value of EC (the constituent containing the ellipsis), (c) the grand union of the focus semantic value of EC, and (d) the relationship between the ordinary semantic value of AC and the grand union of the focus semantic value of EC, where entailment licenses ellipsis.

- (30) a. Mary likes John and SUE does, too.  
 b.  $[[SUE \text{ does } \langle \text{like John} \rangle]]^f =$   
     {that Sue likes John, that Mary like John, that Bill likes John...}  
 c.  $\cup[[SUE \text{ does } \langle \text{like John} \rangle]]^f = \exists x[x \in \text{Alt}(\text{Sue}) \ \& \ x \text{ likes John}]$   
 d.  $\text{Mary likes John} \Rightarrow \exists x[x \in \text{Alt}(\text{Sue}) \ \& \ x \text{ likes John}]$

##### 4.2. Licensing ACD

Following Koster-Moeller & Hackl (2008), this theory of focus-based ellipsis licensing can be extended to ACD structures, with AC as the matrix clause and EC as the relative clause. Crucially, however, such an extension requires that there is a point in the derivation where there are two constituents that can strand in the required entailment relationship, both with access to the DP hosting the ACD site. Without the host DP as part of the semantic calculation of both  $[[EC]]^o$  and  $[[AC]]^f$ , the basic entailment relationships found in ACD cannot be calculated.

Treating the AC as the matrix clause with the host DP (without the RC) and the EC as the full relative clause, again with a copy of the host DP, achieves this configuration. The host DP is part of the semantic calculation of both  $[[EC]]^0$  and  $[[AC]]^f$ . (31)(31) illustrates how the system works for basic ACD sentences, where (a) represents the sentence, (b) a sketch of the assumed LF, (c) the grand union of the focus semantic value of EC, and (d) the relation between  $[[AC]]^0$  and  $\cup([[EC]]^f)$ . If the entailment relationship in (d) holds, ellipsis is licensed.

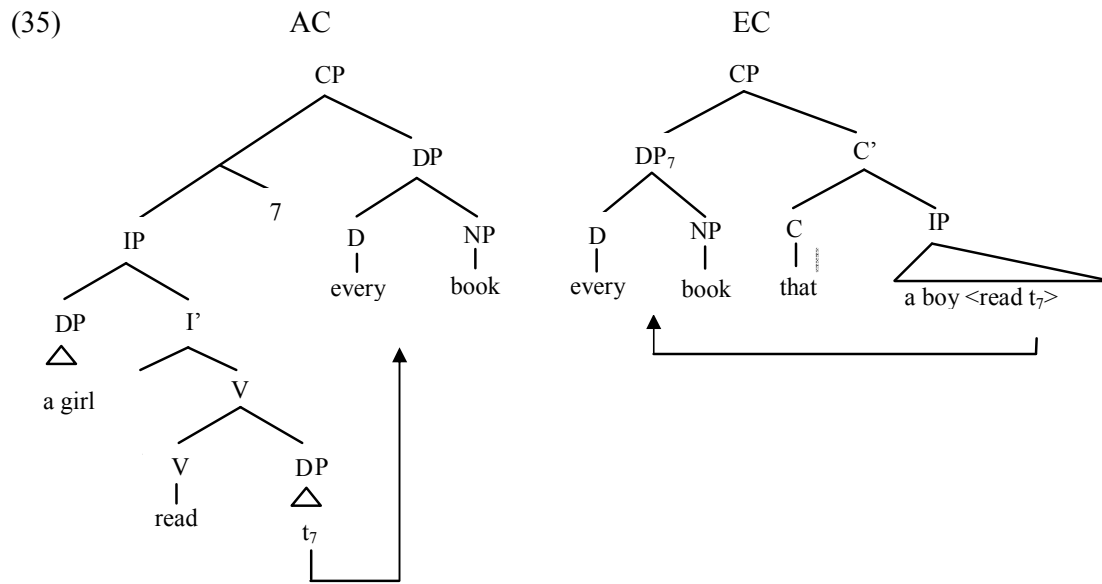
- (31) a. Mary read every book that John did.  
 b. [Every [book<sub>x</sub> [John<sub>y</sub> <y read x>]] [Mary<sub>z</sub> z read x]]  
 c. ( $[[\text{every book}_x [\text{John}_y \text{ y read x}]]]^f = (\{\text{that every book}_x [y \text{ read } x]: y \in D_e\})$   
 $= \exists y[y \in \text{Alt}(\text{John}) \ \& \ [\text{every book}_x [y \text{ read } x]]]$ )  
 d.  $[\text{every book}_x [\text{Mary}_z \text{ z read } x]] \Rightarrow \exists y[y \in \text{Alt}(\text{John}) \ \& \ [\text{every book}_x [y \text{ read } x]]]$

From this system, the ACD-Scope generalization, (24)(24), follows. When the operator embedded in the relative clause is non-scopal, the entailment relationships required to license ellipsis for the inverse scope reading do not hold. Calculations are given in (32) (32)-(34).(34)<sup>3</sup>

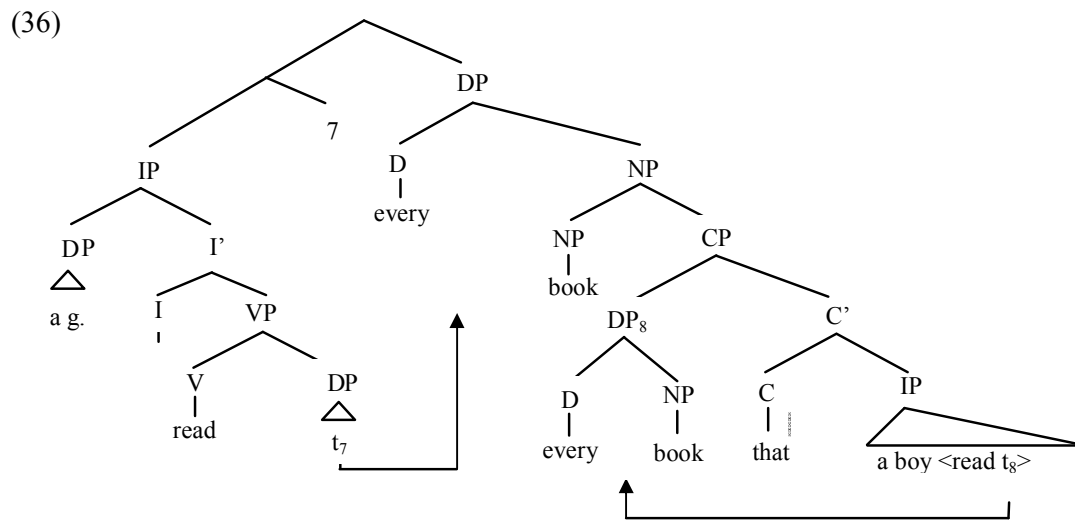
- (32) a. A girl read every book that a BOY did. *every book > a girl*  
 b. [Every [book<sub>x</sub> [a boy<sub>y</sub> <y read x>]] [a girl<sub>z</sub> z read x]]  
 c.  $\cup([[ \text{every book}_x [\text{a BOY}_y \text{ y read } x]]]^f = \exists P[P \in \text{Alt}(\text{boy}) \ \& \ [\text{every book}_x [\text{a } P_z \text{ z read } x]]]$   
 d.  $[\text{every book}_x [\text{a girl}_y \text{ y read } x]] \Rightarrow \exists P[P \in \text{Alt}(\text{boy}) \ \& \ [\text{every book}_x [\text{a } P_z \text{ z read } x]]]$
- (33) a. A girl read every book that JOHN did. *\*every book > a girl*  
 b. [Every [book<sub>x</sub> [John<sub>y</sub> <y read x>]] [a girl<sub>z</sub> z read x]]  
 c.  $\cup([[ \text{every book}_x [\text{John}_y \text{ y read } x]]]^f = \exists y[y \in \text{Alt}(\text{John}) \ \& \ [\text{every book}_x [y \text{ read } x]]]$   
 d.  $[\text{every book}_x [\text{a girl}_z \text{ z read } x]] \not\Rightarrow \exists y[y \in \text{Alt}(\text{John}) \ \& \ [\text{every book}_x [y \text{ read } x]]]$
- (34) a. A girl read every book that every BOY did. *\*every book > a girl*  
 b. [Every [book<sub>x</sub> [every boy<sub>y</sub> y read x]] [a girl<sub>z</sub> z read x]]  
 c.  $\cup([[ \text{every book}_x [\text{every BOY}_y \text{ y read } x]]]^f = \exists P[P \in \text{Alt}(\text{boy}) \ \& \ [\text{every book}_x [\text{every } P_y \text{ y read } x]]]$   
 d.  $[\text{every book}_x [\text{a girl}_z \text{ z read } x]] \not\Rightarrow \exists P[P \in \text{Alt}(\text{boy}) \ \& \ [\text{every book}_x [\text{every } P_y \text{ y read } x]]]$

A critical aspect of this analysis is that to license ACD, the determiner of the host DP needs to be active in both AC and EC. Derivationally, there needs to be separate domains, each with access to the DP, that are able to properly contrast. (35)(35) shows the two cyclic domains (phases) that need enter into the ellipsis licensing algorithm.

<sup>3</sup> Following Heim (1997), the quantified object is always above the matrix subject in AC, because quantified objects must QR to at least VP, and AC cannot contain any unbound variables.



However, it is also clear that there are not two active copies of the DP at the final point in the derivation.



Therefore, we are faced with a theoretical puzzle. We need to reconcile the demands of the ellipsis licensing algorithm, which requires access to two full DPs, (36)(35), with the final relative clause structure, which has only one, (36).

#### 4.3. Implications for the structure of relative clauses

From the previous sections, there seem to be two types of constraints on the possible analyses of relative clauses. Syntactically, we see that relative clause heads license constructions that seem to be licensed only by DPs. Semantically, we see that relative clause heads have robust scopal force, which DPs, but not NPs, have, and that these heads are semantically active both inside and outside the relative clause.

Thus, any successful analysis of these relative clauses needs to provide:

- an internal relative clause head that has the syntactic and semantic status of the external DP
- two properly contrasting constituents (AC and EC), each with access to this host DP
- a way to merge these constituents into a single tree, with correct meaning and spell out

No currently endorsed analysis of relative clauses can accommodate the first of these requirements, which would include the determiner of the host DP inside the relative clause. However, there are two candidate analyses of RCs, raising and matching, that might be modified to accommodate what is needed, (37)(37), (38)(38).

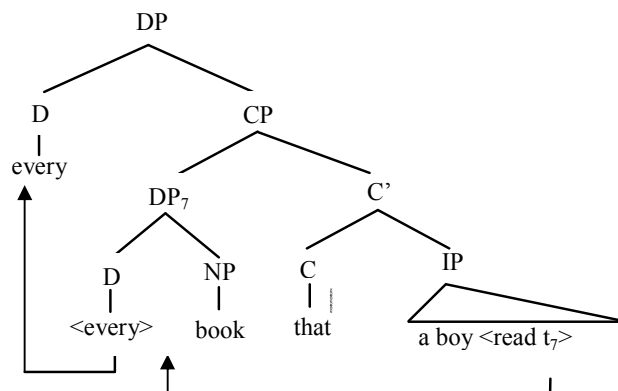
(37) ... [DP every [CP ~~every~~ book<sub>i</sub> that Mary read t<sub>i</sub>]].

(38) ... [DP every [NP book [CP ~~every book~~ that Mary read t<sub>i</sub>]]].

#### Amended Raising: "D-Raising"

An amended version of the raising analysis assumes that the entire host DP raises from the RC internal trace position to SpecCP, which is then followed by the determiner alone raising and projecting (e.g. Donati 2006)

(37(37)')

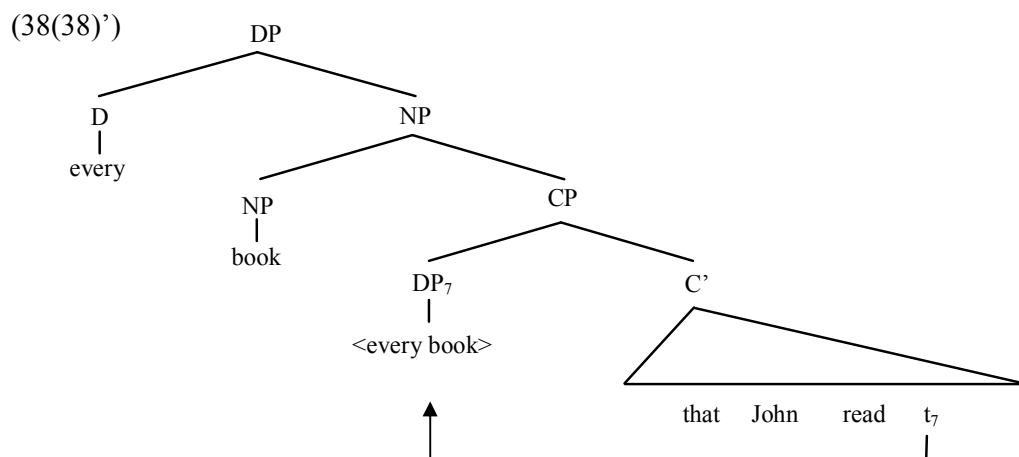


However, in D-Raising structures, all copies of the host DP are part of a single chain (moved from inside the RC object position to SpecCP). This creates a fundamental problem for the ellipsis licensing, as there is no point in the derivational history when both AC and EC can

contain the DP simultaneously, and so ellipsis would never be licensed. This suggests that a D-Raising analysis of relative clauses with ACD sites is not viable.

### *Amended Matching*

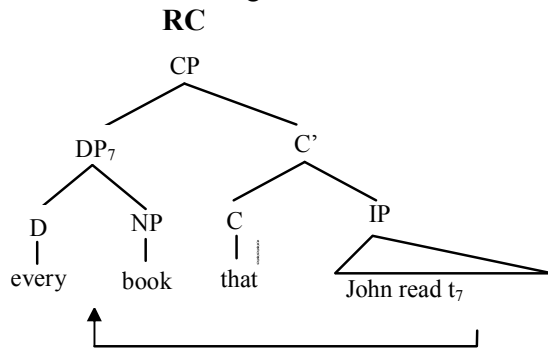
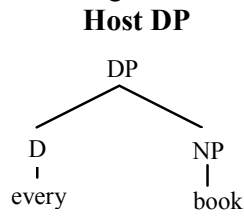
An amended version of the matching analysis assumes that there is a full copy of the host DP inside the relative clause. It raises to SpecCP and undergoes deletion under identity as the relative clause is merged with the external copy of the host DP.



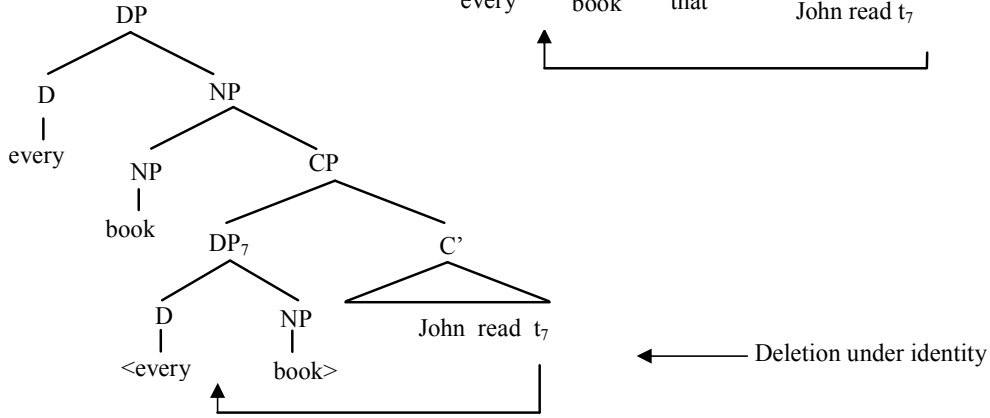
In amending-matching structures, there are two separate copies of the host DP – one in the matrix clause, and one in the relative clause (cf. Chomsky 1965). This provides the conditions needed to satisfy the ellipsis licensing algorithm, as both the AC and the EC will have access to identical copies of the host DP. The RC can then be late (counter-cyclically) merged into host DP (Fox & Nissenbaum 1999, Hulsey & Sauerland 2006), giving rise to a single tree with one spelled-out copy of DP. A sketch of a solution is to assume that (Partee 1975), late merge needs to trigger type-shifting of the RC-internal DP, in addition to deletion of RC internal copy, (39)(39). The specifics of this implementation are beyond the scope of the paper. Rather, the conclusion most relevant to the goal of the paper is that, in order to accommodate the internal copy of the DP head in a way that allows it to affect ellipsis licensing requires a modified matching analysis, with two (fully separate) copies of the relative clause head DP.

(39) Late Merge of Relative Clause, given Amended Matching

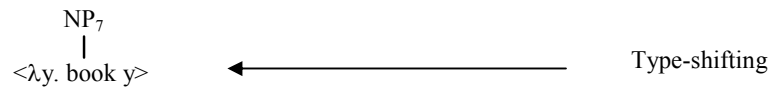
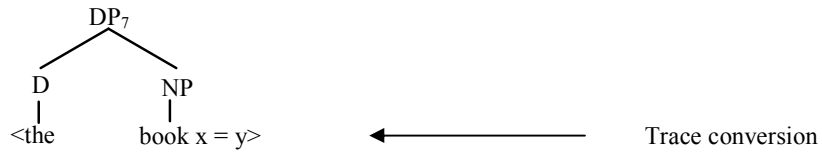
a



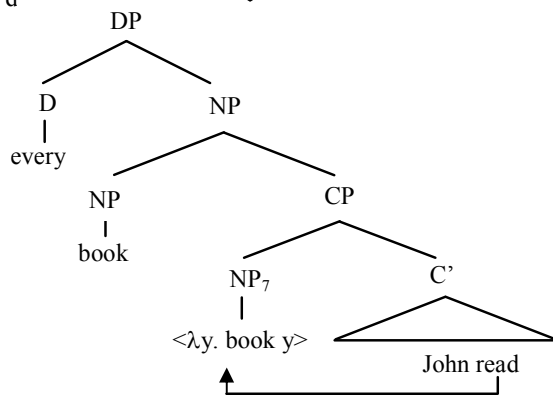
b



c



d



## 5. Empirical support: tests for modified matching and modified raising

I have so far argued that, for theoretical reasons, the most feasible structure for a relative clause with an internal DP head is an amended version of the matching analysis of relative clauses, in which there are two full copies of the DP. In this section, I consider the empirical implications of this analysis.

Using a series of diagnostics following Hulsey and Sauerland (2006), I argue the relative clauses with movement of weak islands, relative clauses with parasitic gap sites, and relative clauses hosting ACD sites all have an underlying matching structure. I will observe that minimally different relative clauses without constructions requiring a full copy of the DP are not necessarily matching, indicating that it is the presence of these constructions is the driving force behind the structural restriction. I apply the same tests to basic *wh*-questions to provide a baseline. These tests, in line with the theoretical arguments, point conclusively towards the fact relative clauses that have DP internal heads have a matching, not raising, structure.

## 5.1. Variable Binding

We can use variable binding as a diagnostic for a raising structure – if a pronoun in the host DP can be bound by an operator inside the relative clause, the relative clause must have a raising analysis, as operators can only bind pronouns whose chains they c-command. In a matching structure, the higher copy of the pronoun will never be within in the domain of the embedded operator, (40)(40). Thus if a pronoun in the host DP is bound by an element inside the relative clause, a raising structure must be assumed.

(40) The picture of his<sub>i</sub> father that every boy<sub>i</sub> saw...

a. Raising

[<sub>DP</sub> the [<sub>CP</sub> the picture of his<sub>i</sub> father<sub>1</sub> [that every boy<sub>i</sub> saw < the picture of his<sub>i</sub> father ><sub>1</sub>]]]

b. \*Matching

[<sub>DP</sub> the [<sub>NP</sub> picture of his<sub>i</sub> father [<sub>CP</sub> the picture of his<sub>i</sub> father<sub>1</sub> [that every boy<sub>i</sub> saw < the picture of his<sub>i</sub> father ><sub>1</sub>]]]]]

*Parasitic Gaps:*

In (41)(41), *wh*-extraction over a binding variable licenses parasitic gaps while retaining the bound reading. In (42)(42)a), when a basic relative clause head moves over its binder, *him* remains bound by *every boy*. However, when a parasitic gap is introduced, (42)(42)b), binding of *him* by *every boy* is no longer available. (43)(43) shows that parasitic gaps are generally acceptable in relative clauses. This suggests an incompatibility between raising structures (required to get variable binding) and internal DP headedness (required to license parasitic gaps).

(41) a. Which picture of his<sub>i</sub> father did every boy<sub>i</sub> purchase?

b. Which picture of his<sub>i</sub> father did every boy<sub>i</sub> purchase without seeing?

(42) a. The picture of his<sub>i</sub> father that every boy<sub>i</sub> purchased was flattering.

b. ??The picture of his<sub>i</sub> father that every boy<sub>i</sub> purchased without seeing was flattering.

- (43) a. The picture of Joe's father that every boy purchased was flattering.  
 b. The picture of Joe's father that every boy purchased without seeing was flattering.

*Extraction out of Weak Islands:*

Similarly, (44)(44) shows that *wh*-phrases that contain a bound pronoun can extract out of weak *wh*-islands and retain the bound reading. However, the contrast between (45)(45)a and (45)(45)b, such that binding is noticeably harder for (45)(45)b, where the head is extracted out of a weak island, indicates that the extraction is not compatible with raising structure required for a bound-variable reading. The lack of contrast in (46)(46) shows that relative clause heads can, in general, extract out of weak islands. This suggests that relative clause heads that undergo movement out of weak islands are necessarily matching, and so cannot support variable binding from inside the relative clause.

- (44) a. Which picture of his<sub>i</sub> father did every boy<sub>i</sub> purchase?  
 b. Which picture of his<sub>i</sub> father did every boy<sub>i</sub> wonder if Sue purchased?
- (45) a. The picture of his<sub>i</sub> father that every boy<sub>i</sub> purchased was flattering.  
 b. ??The picture of his<sub>i</sub> father that every boy<sub>i</sub> wondered if Sue purchased was flattering.
- (46) a. The picture of Joe's father that every boy purchased was flattering.  
 b. The picture of Joe's father that every boy wondered if Sue purchased was flattering.

*ACD-Scope:*

For relative clauses that contain an ACD site, we see that an inverse scope reading is incompatible with a bound variable reading. In (47)(47), the lack of inverse scope of *a boy* over *every picture of his father* (forcing us to consider at most one boy) indicates that *every picture of his father* is not bound by *a boy*, but rather receives co-reference through standard co-indexation (Heim & Kratzer 1998). (48)(48) shows inverse scope is again available when variable binding is not required. This indicates that ACD scope constructions, which require an internal DP head, are not compatible with a raising analysis, which license variable binding.

- (47) a. A girl saw every picture of his<sub>i</sub> father that John<sub>1</sub> did \*Inverse  
 b. A girl saw every picture of his<sub>i</sub> father that a boy<sub>1</sub> did Inverse
- (48) a. A girl saw every picture of Bill's father that John did \*Inverse  
 b. A girl saw every picture of Bill's father that a boy did Inverse

The variable binding test indicates that structures requiring internal DP heads are not compatible with structures that also require a raising analysis, suggesting that constructions with internal DP heads must have a matching structure.



5.2. Idioms<sup>4</sup>

Hulsey and Sauerland (2006) argue (following Brame 1968, Schachter 1973, Marantz 1984, and Chomsky 1993) that if an idiomatic interpretation of the host DP is available, the relative clause must have a raising structure, as the entire head needs to be reconstructable into the idiom constituent, (49)(49). Thus the availability of idiomatic readings can be used to test for a raising structure in various relative clause constructions: if an idiomatic reading of the host DP is not available, it is evidence that the relative clause does not have a raising structure.

- (49) The cat that John let out of the bag
- a. Raising: [DP the [CP ~~the~~ cat<sub>i</sub> [that John let out of the bag <the cat<sub>i</sub>>]]
- b. \*Matching: \*[DP the [NP cat [CP ~~the~~ cat<sub>i</sub> [that John let out of the bag <the cat<sub>i</sub>>]]]

*Parasitic Gaps:*

The relative lack of contrast between (50(50)a) and (50(50)b) shows that, though a bit odd, *wh*-extraction out of an idiomatic expression can license a parasitic gap. However, while the head of a non-idiomatic relative clause can license a parasitic gap, (51(51)a), the host DP of the relative clause cannot both license a parasitic gap and retain an idiomatic reading, (51(51)b). This suggests that a relative clause cannot have an internal DP head and a raising structure.

- (50) a. Which secret did John reveal without meaning to?  
 b. ?Which cat did John let out of the bag without meaning to?
- (51) a. The secret that John revealed without meaning to...  
 b. ??The cat that John let out of the bag without meaning to...

*Extraction out of Weak Islands:*

The data in (52)(52) suggests that *wh*-phrases (DPs) with idiomatic readings are not sensitive to weak islands. However, the contrast between (53(53)a) and (53(53)b) indicates that relative clause heads with idiomatic readings cannot extract out of weak *wh*-islands, while (54)(54) shows that non-idiomatic relative clause heads can. This data also suggests that relative clauses that require DP-internal heads cannot have a covert raising structure.

- (52) a. How much headway did John make last year?  
 b. How much headway did Sue wonder whether John made last year?
- (53) a. I was impressed by the headway that John made last year.  
 b. \*I was impressed by the headway that Sue wondered if John made last year.
- (54) a. I was impressed by the soufflé that John made last year.  
 b. I was impressed by the soufflé that Sue wondered if John made last year.

<sup>4</sup> Note that I do not test the ACD-scope data for idioms, as I could not find any idioms that allow for a quantificational determiner (*\*every cat that John let out of the bag*).


These data together indicate that a raising structure (which is required to retain an idiomatic reading) does not allow an internal DP head, and strongly suggest that both parasitic gap constructions and weak island relative clauses must have a matching structure.

### 5.3. Condition A


To avoid a condition A violation, an anaphor must be bound at some point in its derivational history. In a raising structure, the anaphor chain can be bound by an element inside of the relative clause. However, in a matching structure, the high copy of the anaphor will never be in the c-command domain of a binder inside the relative clause, (55)(55). Thus condition-A violations in relative clauses suggest a matching structure.

(55) The pictures of each other<sub>i</sub> that John and Mary<sub>i</sub> saw

a. Raising:

[<sub>DP</sub> the [<sub>CP</sub> ~~the~~ pictures of each other<sub>i</sub> [<sub>i</sub> [that John and Mary<sub>i</sub> saw  


b. \*Matching:

\*[<sub>DP</sub> the [<sub>NP</sub> pictures of each other<sub>i</sub> [<sub>i</sub> [<sub>CP</sub> ~~the pictures of each other<sub>i</sub>~~ [<sub>i</sub> [that J&M<sub>i</sub> saw  


#### *Parasitic Gaps:*

In (56)(56), movement of a *wh*-phrase containing an anaphor over its binder licenses parasitic gaps while without incurring a condition-A violation. The same is true in (57)(57), in which a relative clause head containing an anaphor moves over its binder, suggesting that parasitic gap constructions are consistent with a raising analysis. This could indicate that a raising analysis is possible for internal DP heads, that parasitic gap licensors are not always DPs, or that the condition A test is not fully straightforward. (58)(58) shows that, generally, condition A violations are not incurred in relative clauses with parasitic gaps.

(56) a. Which pictures of each other<sub>1</sub> did John and Mary<sub>1</sub> purchase?

b. Which pictures of each other<sub>1</sub> did John and Mary<sub>1</sub> purchase without seeing?

(57) a. The pictures of each other<sub>1</sub> that John and Mary<sub>1</sub> purchased were flattering

b. The pictures of each other<sub>1</sub> that J and M<sub>1</sub> purchased without seeing were flattering

(58) a. The pictures of Bill that John and Mary purchased were flattering

b. The pictures of Bill that John and Mary purchased without seeing were flattering

#### *Extraction out of Weak Islands:*

(59) shows that *wh*-phrases that contain an anaphor can extract out of weak *wh*-islands, both when the anaphor binder is outside of the island, (59)(59)b) and inside the island, (59)(59)c). However, the contrast between (60)(60)a), without a weak island and (60)(60)b,c), with weak

island extraction, indicates that relative clauses with DP internal heads incur condition-A violations<sup>5</sup>. This strongly suggests that relative clauses with DP internal heads cannot have a raising structure, but instead must be analyzed as matching.

- (59) a. Which pictures of each other<sub>1</sub> did John and Mary<sub>1</sub> see?  
 b. Which pictures of each other<sub>1</sub> did John and Mary<sub>1</sub> wondered if Sue saw?  
 c. Which pictures of each other<sub>1</sub> did Sue wonder if John and Mary<sub>1</sub> saw?
- (60) a. The pictures of each other<sub>1</sub> that John and Mary<sub>1</sub> saw were flattering.  
 b. <sup>?</sup>The pictures of each other<sub>1</sub> that John and Mary<sub>1</sub> wondered if Sue saw were flattering.  
 c. \*The pictures of each other<sub>1</sub> that Sue wondered if John and Mary<sub>1</sub> saw were flattering.
- (61) a. The pictures of Bill that John and Mary saw were flattering.  
 b. The pictures of Bill that John and Mary wondered if Sue saw were flattering.  
 c. The pictures of Bill that Sue wondered if John and Mary saw were flattering.

#### *ACD-Scope*

In (62)(62), ACD constructions show condition-A violations, suggesting that these constructions must be given a matching, not raising, analysis. This contrasts strongly with (63)(63), in which there is no ACD, (and thus an internal DP head is not necessitated) and no condition-A violation. This suggests that relative clauses with internal DP heads must have a matching structure.

- (62) a. A girl saw every picture that John did  
 b. \*A girl saw every picture of himself that John did
- (63) a. A girl saw every picture that John sent  
 b. A girl saw every picture of himself that John sent

Though the condition-A violation test was not entirely conclusive, for two of the three relevant structures, it, like the other tests, suggests that those structures requiring internal DP heads are not compatible with structures that also require a raising analysis. This furthers the argument that constructions with internal DP heads must have a matching structure.

#### *5.4. Extraposition*

Temporal adjuncts (marking edge of VP) split RC and host DP, indicating late merge of the RC. Late merge requires two self-contained components, the host head and the complete relative clause, which is only available in a matching structure (cf. Hulsey & Sauerland

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<sup>5</sup> There is an additional contrast between (60(60)b) and (60(60)c), when the binder is inside the island, a contrast which deserves further consideration. It is possible that in (60(60)b), the DP can first extract out of the *wh*-island without incurring any sort of violation (not needing to pass over the binder). However, in (60(60)c), the DP cannot even move out of *wh*-island, causing the sentences to be more seriously unacceptable.

2006)<sup>6</sup>: If extraposition is allowed in a relative clause, the RC therefore has a matching structure.

(64) Mary read the book yesterday that John read

a. \*Raising:

[DP every [CP ~~every~~ book<sub>i</sub>]] yesterday [that John read <every book><sub>i</sub>].

b. Matching

[DP every [NP book]] yesterday [CP ~~every~~ book<sub>i</sub> that John read <every book><sub>i</sub>].

*Parasitic Gaps:*

Like the base-line *wh*-questions, (65)(65), relative clauses with parasitic gaps can undergo extraposition, (66)(66), as can standard relative clauses, (67)(67), indicating that they have a matching, not raising, structure.

- (65) a. Which form did Mary find that Bill filled out without reading?  
b. Which form did Mary find yesterday that Bill filled out without reading?

- (66) a. Mary found the form that Bill filled out without reading  
b. Mary found the form yesterday that Bill filled out without reading

- (67) a. Mary found the form that Bill filled out.  
b. Mary found the form yesterday that Bill filled out.

*Extraction out of Weak Islands:*

Similarly, extraposition is consistent with relative clauses with factive islands, (68),(68) and *wh*-islands, (69)(69), indicating that they have a matching, and not raising, structure.

- (68) a. Mary purchased a book that Sue regretted that John read  
b. Mary purchased a book last year that Sue regretted that John read

- (69) a. Sue purchased the book that John wondered how to read  
b. Sue purchased the book yesterday that John wondered how to read

*ACD-Scope*

Finally, extraposition does not change the grammaticality or scope judgments of relative clauses with ACD, indicating they too have a matching, not raising structure.

- (70) a. A girl read every book that John did \*Inverse  
b. A girl read every book yesterday that John did \*Inverse

<sup>6</sup> Idiomatic interpretations give rise to a raising structure (all copies of idiomatic NP need to be part of the idiom constituent), and RC idioms do not allow extraposition over temporal adjuncts:

- (i) a. Mary praised the headway that John made (Hulsey & Sauerland 2006)  
b. \*Mary praised the headway last year that John made

- (71) a. A girl read every book that a boy did Inverse  
 b. A girl read every book yesterday that a boy did Inverse

The extraposition test conclusively points to an analysis of internally DP headed relative clauses as matching, and not raising.

These tests together support the theoretical argument that a relative clause with a full copy of the DP in the head requires a matching, not a raising analysis.

### 6. Conclusion

In this paper, I present two arguments that relative clauses must have internal heads that are full DPs, not, as is standardly assumed, NPs.

One, based on Borsley (1997), argues that DPs, and not NPs, can undergo extraction out of weak islands and licence parasitic gaps. The fact that, cross-linguistically, relative clause heads can do both, argues that some relative clause heads must be DPs, not NPs.

The other, based on Koster-Moeller&Hackl (2008), argues that a full copy host DP of a relative clause is semantically active in ACD constructions, and thus must be part of the relative clause's internal derivation. Specifically, the account requires that the host DP is accessible to both the matrix clause and the relative clause for the purpose of ellipsis licensing.

Two possible relative clause structures, both of which are argued to exist in English, could be modified to include a full DP as the internal relative clause head. However, both theoretical considerations and empirical evidence (based on tests presented by Hulsey and Sauerland (2006)), argue that only the modified matching analysis is adequate. To accommodate this requirement, I proposed an amendment to relative clause syntax, putting forth a modified matching structure with a full copy of the host DP inside the relative clause.

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## The internal structure of dative clitics

Txuss Martín

In this paper I look into some problems concerning Catalan third person dative clitics, with a focus on their internal structure. These clitics show a number of structural and distributive irregularities that seem to reveal something fundamental missing from the picture. By looking at their morphological structure, I will show that we deconstruct them into simpler components, which include definiteness and deixis. With this inquiry I hope to extract some generalizations on the structure of dative clitics in Romance as well as the nature of dative case itself.

### 1. Introduction

Catalan dative clitics, as in many other Romance languages, show syncretism with accusative clitics in most of the paradigm.<sup>1</sup> This is clear for 1<sup>st</sup> and 2<sup>nd</sup> person clitics, and also for what I call 3<sup>rd</sup> person *s*-clitics (e.g. reflexive clitics like Spanish or Catalan *se*). The paradigm of clitics for those cases is shown in (1), where I provide the phonetic form, without the epenthetic vowel [ə], and I give examples in (2), where I give the orthographic form:

- (1) **DAT.1S = ACC.1S:** [m]    **DAT.2S = ACC.2S:** [t]    **DAT.3S = ACC.3S:** [s]  
**DAT.1P = ACC.1P:** [ns]    **DAT.2P = ACC.2P:** [ws]    **DAT.3P = ACC.3P:** [s]

- (2) a. **Em** diràs la veritat    a'. **Em** veuràs demà  
DAT.1S say.2S.FU the truth    ACC.1S see.2S.FU tomorrow  
*You'll tell me the truth*    *You'll see me tomorrow*  
b. **Et** diré la veritat    b'. **Et** veuré demà  
DAT.2S say.1S.FU the truth    ACC.1S see.1S.FU tomorrow  
*I'll tell you the truth*    *I'll see you tomorrow*

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<sup>1</sup> Romanian behaves like Standard Italian in that syncretism between accusative and dative 1<sup>st</sup> and 2<sup>nd</sup> person does not hold. See note 11 below.

Syncretism however breaks with *l*-clitics, a terminology I take from Déchaine & Wiltschko (2002), to abbreviate the expression "third person clitics starting by [*l*]". In this paradigm, accusatives and datives are not identical:

- (3) **Accusative singular masculine:** [*l*]  
**Accusative singular feminine:** [*lə*]  
**Accusative plural masculine:** [*ls*]  
**Accusative plural feminine:** [*ləs*]
- vs. **Dative singular:** [*li*]  
**Dative plural:** [*ls*] / [*lzi*]
- (4) a. **Li** dire la veritat      a'. **La** veuré demà  
 DAT.3S say.1S.FU the truth      ACC.3SF see.1S.FU tomorrow  
*I'll tell him/her the truth*      *I'll see her tomorrow*
- b. **Els / els hi** diré la veritat      b'. **Els** veuré demà  
 DAT.3P say.1S.FU the truth      ACC.3PM see.1S.FU tomorrow  
*I'll tell them the truth*      *I'll see them (masc.) tomorrow*

As we see in the examples, a basic fact of many varieties of spoken Catalan is that they do not use the normative *els* (pronounced [*əls*]) as the 3<sup>rd</sup> person plural dative clitic. Rather, speakers add a vowel [*i*] to the normative form, probably in parallel to the dative singular *l*-clitic [*li*]. Thus, the dative plural *l*-clitic ends up pronounced as [*əlzi*] (cf. Bonet 1991, 1995, Viaplana 1980):

- (5) [*əls*] / [*əlzi*] donaré el llibre<sup>2</sup>  
 DAT.3P DAT.3P give.1S.FU the book  
*I'll give them the book*

Catalan speakers write the colloquial form of this sentence, using the complex ‘*els hi*’, formed by what seem to be two separate clitics: the normative dative clitic *els* (which is also the accusative plural *l*-clitic) plus the so-called locative clitic *hi*. This spelling is nowadays so common that you can even find it written in a variety of formal and informal environments.

Putting aside for the moment this question of why the dative plural *l*-clitic is written as *els hi*, the typical morphemic decomposition of Catalan *l*-clitics is as follows (Bonet 1995):

- (6) **ACC.3PM / DAT.3P**      **ACC.3SM**      **ACC.3SF (PF)**      **DAT.3S (3P)**
- |                                  |                        |  |  |
|----------------------------------|------------------------|--|--|
| [ <i>l</i> + <i>s</i> ]          | [ <i>l</i> ]           | [ <i>l</i> + <i>ə</i> + ( <i>s</i> ) ]       | [ <i>l</i> + ( <i>z</i> ) + <i>i</i> ]     |
| ↓                  ↓             | ↓                      | ↓                  ↓                  ↓      | ↓                  ↓                  ↓    |
| 3 <sup>rd</sup> person    plural | 3 <sup>rd</sup> person | 3 <sup>rd</sup> person    plural<br>feminine | 3 <sup>rd</sup> person    dative<br>plural |

The morpheme *l*- is usually considered to be 3<sup>rd</sup> person (for example in Bonet 1991, 1995). The initial vowel, not reflected in this morphemic decomposition, is a schwa [*ə*] that is con-

<sup>2</sup> In line with Bonet (1991, 1995), I am going to use for the moment the phonetic transcription of the dative clitics [*lzi*] in order to avoid the orthographic question.



sidered epenthetic (Viaplana 1980). The feminine or plural morphemes are considered agreement  $\phi$ -features (Kayne 2000).<sup>3</sup> The feminine morpheme in Catalan is also a schwa [ə], but it is clearly not epenthetic, as it is used as the mark of feminine gender. It always drops in front of words starting with a vowel (including auxiliary verbs), but this is due to general phonological properties of the language, something Catalan shares with other Romance languages like Italian:

(7) *Catalan*

La Maria, l' he vist-a avui i la re-veuré demà  
 the Mary, ACC.3SF have.1S seen.SF today and ACC.3SF re-see.1S.FU tomorrow

(8) *Italian*

Maria, l' ho vista oggi, e la ri-vedrò domani  
 Mary ACC.3SF have.1S seen.SF today and ACC.3SF re-see.1S.FU tomorrow  
 LITERALLY (BOTH): *Mary, I've seen her today, and I'll see her again tomorrow*

As we saw in (3) above, dative *l*-clitics, unlike accusative, do not show gender distinctions in Catalan (or most Romance languages). Now, since masculine is not represented by overt morphemes, it is not surprising that the normative dative plural *l*-clitic, which lacks gender features, is syncretic with respect to the accusative plural masculine, i.e. with the accusative form without an overt gender morpheme. This way, the dative singular *li* remains as the only form of the normative paradigm which has no syncretism with an accusative form, and also as the only one including the morpheme [i] (Bonet 2002, Viaplana 1980). Indeed, if we take into account the non-standard form of the plural dative [lzi], then both forms of dative *l*-clitics share this morpheme [i], and both forms are different from the accusative.

Now, is this morpheme [i] really a dative morpheme as stated in the decomposition in (6) above? The question is far from settled in the Catalan literature (Viaplana 1980). According to some authors, it would be an epenthetic vowel characteristic of 3<sup>rd</sup> person *l*-clitics (López del Castillo 1976). This is a really implausible option, as this would be the only case where that vowel would be an epenthesis in the whole Catalan system. The epenthetic vowel in Catalan is always the schwa [ə]. More plausible is the proposal, adopted by Bonet (1991, 1995), Harris (1997), or Solà-Pujols (1998), that the vowel [i] is a dative morpheme, just as the one that appears in the dative singular *l*-clitic [li]. The problem for this proposal is that if it is so, why would it only appear with *l*-clitics, but neither with 1<sup>st</sup> or 2<sup>nd</sup> person?

- (9) a. \*[mi] compraràs el llibre a mi<sup>4</sup>  
 DAT.1S buy.2S.FU the book to me  
*You'll buy me the book*  
 b. \*[ti] compraré el llibre a tu  
 DAT.2S buy.1S.FU the book to you  
*I'll buy you the book*

<sup>3</sup> This decomposition of the Catalan clitics in smaller subpieces relies heavily on a morphological theory that adopts underspecification and subanalysis. Hence, the analysis I develop should be compatible with any realizational theory of morphology, following the terminology in Stump (2001), like Distributed Morphology (Halle & Marantz 1993), or Network Morphology (Corbett & Fraser 1993).

<sup>4</sup> It might be potentially interesting that the strong pronouns *mi*, *ti*, *si* (just like Spanish *mi*, *ti*, *si*) both incorporate a segment [i] and have to be obligatorily preceded by the preposition *a*.

Another characteristic of Catalan dative *l*-clitics is the irregularity in the formation of the plural. The morphological realization of number features of Romance clitics constitutes a problem. In most cases, it does not seem that singular and plural clitics are related by the usual rules of nominal pluralization. Catalan forms regular nominal plurals by adding an [s] (or allophonic variant) to the corresponding singular form. Thus, the plural of *home* (man) is *homes* and the plural of *cap* (head) is *caps*.

Now, this does not hold for the dative *l*-clitics. The accusative *l*-clitics do form the plural in a regular way, as we saw in (3). The plural of masculine singular [l] is [ls] and the plural of feminine singular [lə] is [ləs]. However, the plural of dative singular [li] is either [əls] in the normative form, or [lzi] in the colloquial version, instead of the expected [\*lis].<sup>5</sup>

This irregularity on dative *l*-clitics is not only characteristic of Catalan, though. Other Romance languages also display it. In French, for instance, the plural of the dative singular *l*-clitic *lui* is not \**luis*, but *leur*. In Italian, the plural dative in high registers of the language is *loro*, for both masculine and feminine. Italian has gender marking in dative singular *l*-clitics, at least in those high registers. The dative singular masculine is *gli*, and the feminine is *le*. In colloquial Italian, the dative singular is *gli* for both masculine and feminine.

Notice that the appearance of the [i] in the spoken Catalan plural dative *l*-clitic does not solve, in principle, the problem. If the dative singular *l*-clitic is [li] and the plural dative *l*-clitic is [lzi], then what seems to happen is that in the dative plural the plural morpheme [z] is interpolated between the [l] and the [i], in a case of *distributed exponence*, a term taken from Solà-Pujols (1998). This solution was proposed by Bonet (1991, 1995), but Catalan does not productively use this kind of infixation, except for cases where the dative is involved. Another example, discussed in Bonet (1991), is the infixation of a partitive clitic [n] within the dative plural *l*-clitic:

- (10) De llibres, a ells, [əlz-ən-i]            donaré    tres  
       of books, to them, DAT.3P-PART-LOC    give.1S.FU three  
       *I will give them three books*

This kind of infixation with dative plurals might make us think that there is something special at stake with dative *l*-clitics. This is the main topic of this paper, but before address it I need to give some details on the other component of the complex *els hi*, namely the so-called locative clitic *hi*.

Summing up sofar: there are three irregularities with the Catalan dative *l*-clitics: they have a special morpheme [i], they are not syncretic with the correspondent accusative clitics, unlike the rest of the other dative clitics, and the plural form is irregular with respect to the singular. The questions I want to answer in this paper are (i) why are Catalan dative *l*-clitics irregular? and (ii) why would a locative clitic appear with dative *l*-clitics? Before attempting an answer, I need to say some things about the locative clitic in Catalan.

*Hi* is usually considered a locative clitic in Catalan, equivalent to French *y*, Italian *ci*, or Medieval Spanish *y*. This clitic pronominalizes all kinds of locative complements, regardless of whether they are stative or directional. That is to say, as its equivalent in other Romance languages, *hi* is an impersonal clitic that represents a circumstance concerning place or direction:

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<sup>5</sup> The form *lis* is attested in Valencian Catalan, and some other varieties strongly influenced by Spanish. It is also attested in other Romance languages such as Sardinian (Jones 1993: 220), quoted in Kayne (2008). Similarly, in Spanish, the dative plural *l*-clitic *les* is the regular plural of the singular *le*.

- (11) a. Si vas a casa, *hi* trobaràs en Joan.<sup>6</sup>  
 If go.2S to home LOC find.2S.FU the John  
*If you go home, you'll find John there.*
- b. Quan jo tornava de la plaça, ella *hi* anava.  
 When I return.1S.PST from the market, she LOC went  
*When I was coming back from the market, she was going there.*

In these examples, we see how *hi* can stand for argument locative PPs. Sometimes those PPs may be present in the antecedent discourse, and sometimes may not. For example, in (11a), the verb *trobar* 'find' selects a theme DO, and a location introduced by the preposition *a* 'at'. As a result, *hi* acts as complement of the verb *trobar*, and its denotation is *a casa* 'at home', recovered from the precedent clause. In (11b), the PP in the precedent clause *de la plaça* 'from the market' cannot be the form *hi* stands for, because in Catalan PPs introduced by the preposition *de* 'of', like in French, require a clitic *en*. As a result, since the verb *anar* selects a PP headed by the preposition *a* (*anar a Barcelona*, 'to go to Barcelona') the clitic *hi* has to be the one playing the role of this complement.<sup>7</sup>

However, as in the other Romance languages where a counterpart of *hi* is present, this clitic goes beyond the mere locative function (Kayne, 2008). It also pronominalizes non-locative PPs (12), as well as non-locative adjectives and non-locative adverbs (13), or sentences with perception verbs (see, hear) when used intransitively (14):

- (12) a. Ningú no defensava aquella opinió, però ell s' **hi** entossudí  
 Nobody NEG advocated that opinion, but he SE LOC got-stubborn  
*Nobody else advocated that opinion, but he stubbornly kept doing it*
- b. En Pere pensa en son xicot? No **hi** pensa gaire  
 the Peter thinks in his boyfriend? NEG LOC thinks little  
*Does Peter think of his boyfriend? No, he doesn't think much*
- (13) a. No era ric, però **hi** he esdevingut com a lingüista  
 NEG was.1S rich but LOC have.1S become as a linguist  
*I wasn't rich, but I've become so as a linguist*
- b. Havia de parlar lentament, però no **hi** he parlat gaire  
 had.1S of speak.INF slowly but NEG LOC have.1S spoken much  
*I was supposed to speak slowly, but I haven't (spoken slowly)*
- (14) En Joan no **hi** veu gaire  
 the John NEG LOC sees molt  
*John doesn't see very well*

The obvious question is why, if *hi* is a locative clitic, it can be used in these other non-locative contexts. Maybe we should avoid the terminology *locative clitic* because it is misleading, as

<sup>6</sup> For now, I gloss the clitic *hi* as LOCATIVE (LOC), but I revise this notation later.

<sup>7</sup> As pointed out by a reviewer, both *hi* and *en* seem to have some uses as locative morphemes, where *en* seems to express a movement away from a specified center, and *y* seems more neutral with respect to directionality. The absolutely pertinent question the reviewer raises is then this: Why would *hi* but not *en* attach to the *l*-morpheme in the dative? I do not have an elaborated answer to this question right now. As a first speculation, I would like to consider that it is precisely its neutrality with respect to directionality, as well as its uses as a general deictic clitic (see below in the text for elaboration on this), what allows *y* to attach to the dative clitic and form the complex forms I am arguing for in this paper.

claimed in Kayne (2008). These other behaviors of the locative clitic seem to suggest that to be a *locative clitic* is only part of a bigger story, i.e. maybe locative *hi* is just a sub-case of a more general phenomenon.<sup>8</sup> Following Kayne (2008), I am to propose a deictic status for this kind of element in Romance and their counterpart in English (*there*). Observe the following uses of these elements, both used as locative, i.e. referring to a location (15) or as non-locative (16):

- (15) a. I want to go there  
 b. Je veux y aller (French)  
     I want LOC go.INF  
 c. Ci voglio andare (Italian)  
     LOC want.1S go.INF  
 d. Hi vull anar (Catalan)  
     LOC want.1S go.INF
- (16) a. There is a book  
 b. Il y a un livre (French)  
     EXPL LOC has a book  
 c. C'è un libro (Italian)  
     LOC is a book  
 d. Hi ha un llibre (Catalan)  
     LOC has a book

For the sentences in (15) to be felicitous it has to be contextually clear what the locative clitic (*y*, *ci*, *hi*) or English *there* refers to. For example if I see a picture of Prince Edward Island in the subway, I will say to my boyfriend: *I'd like to go there next summer*, or if we speak in Catalan to each other I would say: *Hi vull anar l'estiu vinent*. Since by definition (Levinson 1983, for example), deictics are grammatical elements used to point more or less directly to contextual information, the examples in (15) seem to fit neatly to this: the meaning of these elements is deictic in the sense that it does not refer or name any particular entity on all occasions of use; rather it is a variable or place-holder for some particular entity given by the context.<sup>9</sup> Now, the reader may consider in fairness that the fact alone that these elements retrieve their referent from the environment is not enough to consider them deictic. Anaphoric pronouns also behave in this way, and we would not call them deictic, at least if we stick to the common use of deixis, which necessarily incorporates the speaker as the center of the indexical field (Bühler 1934), i.e. as the element with respect to which deictics gain reference. Thus, whereas demonstratives like *this* and *that* (and morphemes like *here* and *there*) do indeed make reference to the speaker (near the speaker vs. away from the speaker), the Romance elements (*hi*, *ci*, *y*) are completely neutral with respect to the speaker (i.e. when used as locatives, they are neutral with respect to the speaker, near or away). Although I agree in part with

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<sup>8</sup> The fact that this morpheme *hi* has a more abstract use than mere locativity is another instance of underspecification, where the morpheme realizes little information and hence fits in a larger number of contexts.

<sup>9</sup> According to Levinson (1983: 54), typical examples of deictic expressions are demonstratives, first and second person pronouns, tense, certain place and time adverbials like *here* and *now*, and some verbs such as *come* and *go*. According to the theory developed by the psychologist Karl Bühler in 1934, deictic expressions refer into what he calls the indexical field, whose zero (or *origo*) is fix by the person who speaks (the 1<sup>st</sup> person), the place of utterance (the *here*), and the time of utterance (the *now*) (Weissenborn & Klein 1982). This entails that there are three types of deixis: *personal*, *spatial*, and *temporal* deixis.

this idea, I think the problem is much more complex, and the reference to the speaker is not always so crucial in some uses of clearer deictics. Let me examine some examples: French *ici* et *là* (equivalent to English *here* and *there*), the indefinite use of the English demonstrative *this*, and the use of so-called deictic verbs like *come*.

With respect to French demonstratives *ici* et *là*, they generally express the same opposition that *here* and *there* express in English: *ici* express proximity to the speaker, and *là* express distality from the speaker:

- (17) a. Arrêtons - nous ici  
 stop.IMP.1PL us here  
*Let's stop here*
- b. Ne restez pas ici, allez là  
 NEG stay.IMP.2PL NEG here go.IMP.2PL there  
*Don't stay here, go there*

However, facts are not so clear. *Là* can sometimes mean *here* too:

- (18) a. Je reste là  
 I stay here
- b. Marie est là  
 Mary is here

In these cases, we see how a clear demonstrative (and therefore deictic element) as *là* can also be rather neutral with respect to the speaker, in terms of proximality and distality. The meaning of these uses can only be distinguished by clues provided by the context of utterance, exactly as what happens with *hi*.

It is also of interest in this respect, the indefinite use of the English demonstrative *this*, used colloquially to refer to a person or thing previously unspecified. In that same context, *that* is not allowed, because it presupposes that the person or thing is contextually clear:

- (19) My brother went to a party yesterday, and he met this / \*that very interesting guy.

If we understand contextual knowledge of the person mentioned as proximity to the speaker, as intuitively one may think, then the behavior of *this* and *that* in (20) is exactly the opposite from what we would expect. If that were the case, *this* should be used when the reference was clear from the context, and *that* when it was not. The fact that is exactly the opposite case seems to suggest that *this* and *that* might not be inherently specified as proximal or distal, but may gain this kind of meaning from contextual tips.

Finally, it is usual to consider that verbs like 'to come' or 'to go' are deictic, in the sense that they incorporate the speaker as the point of reference with respect to which the action of coming is interpreted. For example, according to the *Oxford English Dictionary*, 'come' is defined as "move or travel toward or into a place thought of as nearer familiar to the speaker", whereas 'go' is conceptualized either neutrally with respect to the speaker, or as moving away from the speaker. However, in many languages, a telephonic interchange like the following would be absolutely standard:

- (20) A: Could you please come to my office for a moment?  
 B: I'm coming right away (cf. #I'm going right away)

Catalan makes a really pervasive use of the verb ‘come’ as ‘going to where you are’:

- (21) A: Podries venir un moment?  
 Could.2S come.INF a moment  
*Could you come for a moment?*  
 B: Ara vinc (cf. #Ara vaig )  
 Now come.1S Now go.1S  
*I'll come right away*

In any case, these facts seem to suggest that even clear deictics like *là*, *this* or the verb ‘to come’ are not always clearly defined with respect to the speaker, and therefore, the fact that *hi* is not so specified either should not count as a proof against its deictic character. What makes it a deictic is the fact that it is used to point more or less directly to contextual information, that it does not refer or name any particular entity on all occasions of use; rather it is a variable or place-holder for some particular entity given by the context. From this point of view, maybe what we consider as two separate phenomena, namely deixis and anaphora, might turn out to be just two subcases of the same thing (von Heusinger 2002).

Let’s now turn to (16)? Can we consider deictic the presentational uses of the so-called locative clitics in Romance and English *there*? In this respect, as I said above, I am going to assume here the proposal in Kayne (2008), that considers these elements as deictic expressions. According to Kayne, the Romance elements in (16) (and in my examples in (12) and (13)), as well as their English counterpart *there* are the same deictic element as the locative version in (15). These deictic elements behave like demonstratives in that they modify a noun, in structures like the following.

- (22) a. We spoke thereof → We spoke there THING of  
 b. Jean pense à quelque chose → Jean y THING pense (French)  
 John thinks to some thing John LOC thinks  
 c. Anirem a Barcelona → Hi PLACE anirem (Catalan)  
 we.go.FU to Barcelona LOC go.1P.FU

As we see, the silent noun is PLACE in the case of locatives, but other nouns in the other cases. As in Kayne (2008), I will use the term *locative clitic*, as abbreviation for ‘*deictic clitic modifying the silent noun PLACE*’. This means, then, that all the other uses of the clitics *hi*, *y*, etc are [DEICTIC + N]. Support for this idea comes from the overt character of this structure in some other languages, where we find an overt demonstrative as complement of a noun, in addition to the demonstrative or (in)definite article:

- (23) a. That there car  
 Cette voiture -là (French)  
 this car there  
 b. El coche este (Spanish)  
 the car this  
 c. Un tipo aquí (Spanish)  
 a guy here  
 d. det herre huset (Colloquial Norwegian; Leu 2008)  
 the here.INF house

An also interesting support for the idea that *hi* can be a deictic accompanying a (silent) noun comes from sign languages (Emmorey 2002). In many SLs, there is a marked distinction between first and second person, which are signed by pointing to the participants in the discourse, and third person, which can be produced by pointing (deictic gesture *per se*) to specific locations within the signing space, without necessity of pointing to any person. Interestingly, the association between a person and a spatial location is typically made by producing a nominal sign at a location in space, or by pointing to a location while producing a nominal sign. That pointing (indexing) sign has been analyzed as a determiner when it is produced prior to the noun and a locative adverbial when produced in isolation after the noun (like a clitic?). The important idea is that the pointing gesture (the deictic) occurs with a noun first, and as a pure deictic (pointing gesture) later. We could consider that in both cases, the pointing gesture is a deictic clitic with a status similar to the one of *hi* and its Romance and Germanic counterparts.

Notice that this underlying phrasal structure of *hi* may explain why in Catalan, verbs of perception like *see* or *hear*, when used intransitively need the deictic clitic *hi*, as we saw in examples (8e), repeated as (24a), with the underlying structure in (24b):

- (24) a. En Joan no hi veu gaire  
           the John NEG LOC sees little  
           *John doesn't see very well*  
       b. En Joan no [hi THING] veu gaire

This indeed entails that all the other non-locative uses of *hi* behave in a similar way, with the structure [DEICTIC+N]. This is an interesting result, because it means that *hi* can never go isolated, but has to always be related to some element that it modifies somehow. This result is going to be important in the proposal I going to develop in the next section to explain the behavior of the dative *l*-clitics in Catalan and other languages.

## 2. The [DEF + DEIX] proposal.

The questions I want to answer in this paper are two: (i) Why are Catalan dative *l*-clitics irregular in so many respects?, and (ii) Why does a deictic clitic (updating the original question with the result of the last section) appear with dative *l*-clitics? The answers to these two questions are related. My proposal is that whereas 1<sup>st</sup> and 2<sup>nd</sup> person dative clitics can appear on their own, because they internally incorporate deixis features (person deixis), dative *l*-clitics require the presence of the extra deictic clitic *hi*, in order to incorporate deixis features (locative deixis in this case; more on this below).<sup>10</sup> This entails that I am going to take the form '*els hi*' in Colloquial Catalan to be transparent with respect to the structure of the dative *l*-clitics. I refer to that complex structure as [DAT+DEIX]. This proposal, as I show in the next subsections, not only solves the apparent irregularities of the Catalan dative *l*-clitics, but also allows us to explain other phenomena related to this grammatical case in Catalan and Romance: in section 2.1, I justify the proposal for Catalan, and in section 2.2, for other Romance

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<sup>10</sup> This proposal needs refinement to include the *s*-clitics, which also are 3<sup>rd</sup> person clitics. As such, my theory would predict that when used as datives, they should also incorporate a deictic clitic *hi*, which they do not. A tentative idea is that these kind of clitics are mainly anaphoric, and as such are linked to the person features of their antecedents, and this is why do not need deixis of their own. Further research would be necessary.

languages. Whether or not we can extend this proposal to all languages, by the *uniformity principle* in Chomsky (2000), following the logic in Hale & Keyser (2002), is left as a topic for future work.

### 2.1. The proposal for Catalan

Several ideas support the proposal of the underlying structure [DAT+DEIX] for Catalan. The first and more immediate advantage of this proposal is that it offers a unified solution to the apparent irregularities of the dative *l*-clitics that we reviewed in section 1. In the first place, it entails that the morpheme [i] is not part of the dative, i.e. it is not a dative morpheme, as suggested by Bonet (1991, 1995), Harris (1997), or Solà-Pujols (1998). This solves the question on why it does not show up in the other person forms of the paradigm of the dative clitics. Indeed, we still have to explain why this extra element appears with the dative *l*-clitics. I come back to this issue, with a proposal, in section 3.

The second irregularity of dative *l*-clitics is also solved by my proposal, because it provides a simple explanation for the plural form. According to my proposal, dative *l*-clitics are part of a structure [DAT+DEIX]. This means, that the singular dative *l*-clitic, written according to the normative as *li*, could also be written as *l'hi*, making thus transparent its internal structure: the morpheme *l* would be, properly, the singular dative *l*-clitic, and *hi* would be the deictic clitic. Then, if the actual dative singular *l*-clitic is *l*, its now regular plural is *ls*, i.e. *l+s*. The deictic clitic, since it lacks  $\phi$ -features remains invariable in both cases, and therefore it makes absolute sense that the Catalan dative *l*-clitics are *l'hi* and *els hi*.

A direct consequence of the previous discussion of the plural, and therefore the third advantage of my proposal, is that it also solves the problem of the syncretism of the dative *l*-clitics with respect to the accusatives. If the actual dative *l*-clitics are in fact the bare *l* and *ls* (both pronounced in most cases with the epenthetic vowel [ə]), then we see how they are syncretic to their accusative counterparts, exactly as the rest of their paradigms. That is to say, accusative and dative have the same form across the board.

However, this is not strictly true for two reasons. Firstly, there is an important difference between dative and accusative *l*-clitics, namely the gender morphemes of the accusative are not present in their dative counterparts. Notice that if dative *l*-clitics lack gender features, then it is expected that the syncretism of the dative *l*-clitics be with the masculine forms of the accusative *l*-clitics. These are the accusative forms that also lack overt gender marking. Indeed, it remains to be clarified why dative clitics in Catalan, and most other Romance languages, do not have gender features.<sup>11</sup> Secondly, the dative clitics require deictic information, either internal (1<sup>st</sup> / 2<sup>nd</sup> person) or external (3<sup>rd</sup> person). The presence of the deictic features is what ends up establishing the difference between accusative and dative clitics across the board. So if anything, my proposal has the advantage that it offers a unified explanation to the irregularities we have talked about in the first section. However, my proposal has other benefits that need to be taken into consideration.

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<sup>11</sup> Actually, dative clitics do have gender in (varieties of) some Romance languages like Italian, Romanian or Spanish. For example, some varieties of Spanish do show gender in datives through the phenomenon called *laísmo*. This phenomenon involves the use of feminine morphemes in dative *l*-clitics:

(i) A Maria la dije la verdad  
 to Mary DAT.SF I-said the truth  
*I said the truth to Mary*



A first one that comes to mind is that this proposal solves an apparent incompatibility between dative and locative clitics that we find in Catalan and some other Romance languages. This problem arises when a locative and a dative *l*-clitic should co-occur, for example in cases of multiple clitic left-dislocation (CLLD, in the sense of Cinque 1990). In (27) we see how none of the dative *l*-clitics allows the presence of a locative clitic, even if the dislocation of the dative PP *a casa seva* 'to his place' would also require a locative clitic:

- (25) a. Als nois, no els hi portis res  
 to-the boys, NEG DAT DEIX bring.2s.SJV nothing  
*Don't bring anything to the boys*
- b. Als nois, a casa seva, no els hi (\*hi) portis res  
 to-the boys, to house POSS.3SF, NEG DAT.3P DEIX LOC bring.2s.SJV nothing  
*Don't bring anything to the boys to their place*
- c. Al Miquel, a casa seva, no li (\*hi) portis res  
 to-the Michael, to house POSS.3SF, NEG DAT.3S LOC bring.2s.SJV nothing  
*Don't bring anything to Miquel to his place*

In (25a) the complex clitic *els hi* resumes the left-dislocated dative phrase 'als nois' (to the boys). In (25b), since we have the dative and a locative phrase dislocated, we should add another resumptive *hi* to the sentence, and we would expect two consecutive instances of that clitic, the first one as part of the complex [DAT+DEIX], the second one as the resumption of the left-dislocated locative phrase. But as we see in (20b), this is ungrammatical, maybe because of a kind of *Repeated Morph Constraint* (Menn & McWhinney 1984) understood as the avoidance of adjacent identical morphs. Interestingly, the same happens with the dative singular *l*-clitic in (25c). This is easily explainable if we assume that the dative singular *l*-clitic also has the structure [DAT+DEIX].

This phenomenon is indeed not only phonological. It is perfectly possible to have an [*i*] after a dative:

- (26) Als altres professors també els hi igualaré els salaris  
 to-the other professors too DAT DEIX equalize the salaries  
*I'll equalize the salaries to the other professors too*

A quite similar phenomenon happens in French, where the co-occurrence of a dative and the locative clitic is normally reduced to the occurrence of the dative. The co-occurrence of the singular dative *lui* with the expletive *y* is clearly ungrammatical in the singular. In the plural is not so strongly rejected, but some speakers report redundancy:

- (27) A la fête, je { lui (\*y) / leur (y) } ai parlé  
 to the party I DAT.3S LOC DAT.3P LOC have.1s talked  
*I talked to him / them at the party*

Another fact in support of my proposal would be the following. As we saw above, according to the Catalan normative, the dative singular *l*-clitic has to be written *li*. However, when this clitic appears in combination with other clitics, it is reduced to the deictic clitic *hi*. In those cases, *hi* can apparently assume the function otherwise reserved for the dative. This is shown in the interaction of dative with accusative (28a), with partitive (28b), or it is used to avoid the Person Case Constraint (28c):

- (28) a. Li donaràs el llibre o no **l'** **hi** donaràs?<sup>12</sup>  
 DAT.3S give.2S.FU the book or NEG ACC.SM DEIX give.2S.FU  
*Will you give him the book, or will you not?*
- b. Si me' n demana deu, **n'** **hi** donaré dotze.  
 If DAT.1S PART asks ten PART DEIX give.1S.FU twelve  
*If he asks me for ten, I will give him twelve.*
- c. Demà t' **hi** presentaran al(s) president(s)  
 Tomorrow ACC.2S DEIX introduce.3P.FU to-the(PL) president(s)  
*Tomorrow you will be introduced to the president(s)*

Some scholars (e.g. Wanner 1977, quoted in Bonet 1995) have considered this phenomenon to be a case of phonological constraint on identical sequences like *el li*. However, in Valencian or Occitan such combinations are possible, as we see in (29) and therefore it does not seem to be a case of pure phonology:<sup>13</sup>

- (29) El llibre al xiquet li' l compraré (Valencian)  
 The book to-the boy DAT.3S ACC.3SM buy.1S.FU  
*I'll buy the book to the boy*

## 2.2. Extension of the proposal

Remember that the question I am trying to answer is about the structure of dative clitics in Romance. I have concluded that the [DAT+DEIX] hypothesis can be posited for Catalan, and it has advantages, because it offers an answer to some of the problems of the dative in that language, and explains some other facts. However, I said in the introduction that this proposal can be applied to (at least some) other Romance languages. This is what I explore in this section.

In order to empirically support the relationship between dative and the deictic clitic, we need to find languages where either they co-occur as in Catalan, or where the presence of the deictic clitic can be used to express the function of the dative. Although it is rather common to acknowledge that there is an intuitive relationship between locative (or deictic clitic, in our terms) and dative, the common fact is that most Romance languages either do distinguish them (French, Catalan, or Italian) or do not have a locative (deictic) clitics (Spanish, Portuguese).

However, there do exist languages where the property we are looking for holds. In some of these languages, datives and locatives cannot be distinguished. For example, Lassiter (2004) analyzes the case of Hittite, an extinct language of the Indo-European family. According to Lassiter, this language had a case used for both locative and dative markings, called dative-locative, with endings *-i/-ya* in the singular. Since dative case is more common cross-linguistically, Lassiter concludes that the dative-locative case of Hittite can be reduced to dative.

A related fact occurs in colloquial French, where dative singular *lui* is often reduced to the

<sup>12</sup> This is written in standard orthography, as we see in the difference between *li* and *l'hi* I am denying here.

<sup>13</sup> Although I do not go into this topic further here, let me suggest that this might be a case of polydefiniteness of the definiteness marker *l-* similar to the one we find in Greek definite determiners. Further research may clarify this.

locative *y* (Dominique Sportiche, pc):

- (30) J' *y* parlerai (cf. Je lui parlerai) (French)  
 NOM.1S DEIX speak.1S.FU  
*I'll speak to him/her*

Another instance of the phenomenon is provided by Rigau (1982). Rigau documents Catalan dialects where the locative clitic sometimes substitutes the standard dative clitic:

- (31) a. En Joan *hi* donà cops (Catalan)  
 the John DEIX gave knocks  
*John knocked the door*  
 b. En Joan *li* donà cops  
 the John DAT gave knocks  
*John knocked the door*

Another case along the same lines is provided by Northern Italian dialects, especially those of the Veneto area (Kayne, 2008). In these languages, the clitic *ghe* (pronounced [ge]) can cover both dative and locative cases. Paduan provides an example. In this language, there are not separate clitics for dative and locative. And as we have seen, Kayne defends in that paper the status of *ghe* as a deictic clitic. This explains why *ghe* can cover a pure locative and a dative function:

- (32) a. *Ghe* dago el libro  
 DEIX I-give the book  
*I'm giving him/her/them the book*  
 b. *Ghe* meto el libro  
 DEIX I-put the book  
*I put the book there*

According to Kayne, the use of *ghe* is systematic for Paduan. For him, the underlying form of the dative sentence in (32a) involves the presence of a silent third person dative clitic. This means, that (32a) should be thought as (33):

- (33) DAT.3 *ghe* dago el libro

Also colloquial Italian, at least for some speakers, accepts the locative clitic *ci* in the place of the dative clitic in the equivalent to (30) (Nicola Lampitelli, pc):

- (34) a. *Ci* daró il libro  
 DEIX give.1S.FU the book  
*I'll give him/her/them the book*

Finally, this co-occurrence of dative and locative clitics is overt in other languages apart from Catalan. Kayne (2008) mentions the following case from Sardinian (Jones 1993: 220):

- (35) Narra– bí – lis  
 Tell – DEIX – DAT.3P

*Tell them (about it)*

These examples provide empirical support to the co-occurrence of datives and deictic clitics. Such co-occurrence must always happen, regardless of whether one of the two elements remains silent. The silent one is normally the locative, although as we have seen in this section, it can sometimes be the dative. If this is true, and some of the two elements of the complex [DAT+DEIX] can be silent, as this happens in Paduan, according to Kayne, we could apply the same logic to other languages. Thus, we have: (i) languages where both participants of the [DAT+DEIX] are overt, like Catalan or Sardinian, or even the French *lui*, (ii) languages where only the deictic element is overt, like Paduan, the cases of Catalan where the dative combines with other clitics, and the cases of French in (30). Following the same logic, we could consider that languages like Spanish or Portuguese do present the same structure [DAT+DEIX], but since they lack an overt deictic clitic, they only show the dative clitic overt. I do not consider here the question of whether both elements of the complex [DAT+DEIX] can be silent at the same time.

### 3. *The special character of l-clitics*

Now we come to the last part of the paper. The proposal is that dative *l*-clitics, unlike all the other dative clitics have to co-occur with an overt deictic clitic. That proposal makes good predictions for Catalan and other Romance languages. Now, indeed, the question is why would only dative *l*-clitics enforce the structure [DAT+DEIX]? Why would no other clitic require the presence of a deictic clitic? The thesis I am going to pursue in this last section is that this is so because dative *l*-clitics are different from 1<sup>st</sup>/2<sup>nd</sup> person dative clitics. The latter are provided with internal deictic features (person deixis), and hence do not need to get deixis externally. However, I am going to argue that *l*-clitics lack person features, and hence need the complementation of an external deictic in order to get those features. This is the reason why they appear within the structure [DAT+DEIX].

In order to argue in favor of this hypothesis, in this section I need to examine two questions: (i) Are we justified in considering that *l*-clitics lack person features?, and (ii) why do dative clitics need this deictic feature, but the accusative clitics do not? Let me start by the first question.

#### 3.1. *The special character of l-clitics*

Clitics can be defined as bundles of  $\phi$ -features without substantive or encyclopedic content whatsoever (i.e. as functional words). If we consider this idea, then we should expect that clitics can be decomposed in morphemes expressing those features, ideally with each feature being expressed by a single morpheme (Bonet 1991, 1995; Kayne 2000, or Viaplana 1980, among many others). An example of this approach is offered in Kayne (2000:131-ff), where the author elaborates on the decomposition of French and Italian possessives and other pronouns. According to Kayne, clitic pronouns such as *mes*, *tes*, *ses*, and *les* (the latter homophonous to the definite article plural) would be composed of the morphemes *m*, *t*, *s*, and *l*, plus morphemes expressing agreement in number *es*. Kayne notices that it is quite uncontroversial that *m* and *t* are person morphemes, 1<sup>st</sup> and 2<sup>nd</sup> person respectively. However, the status of *s* and *l* is not so clear. According to some authors, like Bernstein (in press) the *l*-morpheme, like

the *th-/d-* morpheme in Germanic languages, is 3<sup>rd</sup> person. Other scholars, though, consider *th-/d-* and *l-* morphemes to be definiteness morphemes (Déchaine & Wiltschko 2002, Leonetti 1999, or Leu 2005).

Kayne (2000) notices the asymmetry between *mes*, *tes*, *ses*, which can be possessive determiners and reflexive pronouns, and *les*, which can be neither possessive nor reflexive. It is also interesting that the reflexive 3<sup>rd</sup> person pronoun in most Romance languages (French, Spanish, Catalan, etc.) uses the *s* rather than the *l*:

- (36) Se ha comprado su propio coche (Spanish)  
 RFL.3S has bought his own car  
*He's bought himself his own car (= He has bought a car for himself)*

In addition to the reflexive, the *s*-morpheme is present in the paradigm of possessive determiners in most Romance languages, going back to Latin which also used that morpheme in the possessive.<sup>14</sup> Example from French:

- (37) Mon / Ton / Son / \*Lon pere  
 my your his / her father

Finally, that the *l-* morpheme is not a person morpheme is also suggested by the following fact from Catalan (it would also work in Spanish). If the *l-* in the definite determiner is essentially the same as the one in the accusative *l*-clitics, and if that morpheme is a person morpheme, then (40) would be rather surprising:

- (38) a. Les professores vole**m** el nostre propi horari  
 the.PF professors.PF want.1P the our own schedule  
*We (female) professors want our own schedule*  
 b. Les professores vole**u** el vostre propi horari  
 the.PF professors.PF want.2P the your own schedule  
*You (female) professors want your own schedule*  
 c. Les professores volen el seu propi horari  
 the professors.PF want.3P the their own schedule  
*The (female) professors want their own schedule*

(38) shows two things: First, the definite DP *les professores*, which includes the *l* morpheme, can interact with different person inflections of the verb *voler* (to want), either 1<sup>st</sup>, 2<sup>nd</sup> or 3<sup>rd</sup> person plural, unlike what happens for example with pronouns:

- (39) a. Nosaltres vole**m** / \*vole**u** / \*volen una millora  
 We want.1P want.2P want an improvement  
 b. Elles \*vole**m** / \*vole**u** / \*volen una millora  
 They.F want.1P want.2P want.3P an improvement

<sup>14</sup> Strictly speaking, this generalization applies to 3<sup>rd</sup> person singular in all Romance languages. However, the *l-* is present in the possessive paradigm of some Romance languages, but only for the 3<sup>rd</sup> person plural. French: *leur*, Italian: *loro*. It is interesting that these forms can be both dative and possessive, a topic not unknown in the literature: Szabolcsi (1981, 1983), Freeze (1992), Kayne (1975, 2000).

If the DP *les professores* were 3<sup>rd</sup> person, we should expect a behavior similar to that observed in (39), but we do not see it. Second, in (38) we also see the complex form of the possessive in Catalan, consisting in the definite determiner, plus a possessive element. Notice that the number of the possessive depends on the number of the possessive element, and not on the definite determiner, even though the definite determiner is the head of the whole DP [*el nostre propi horari*] "our own schedule". This seems to indicate that no person morpheme is associated with the definite determiner, or the *l-* morpheme in general.

Now, if the *l-* morpheme does not have person features, why is the belief so common that it is 3<sup>rd</sup> person? In my view, this is related to what the 3<sup>rd</sup> person actually is. As stated in Benveniste (1966: 197), the notion of person only makes sense when applied to the 1<sup>st</sup> and the 2<sup>nd</sup> person. Person, which is a kind of deictic mark, as we see below, can be defined (informally) as the grammatical category that indicates discourse roles, i.e. the speaker (1<sup>st</sup> person) and the addressee (2<sup>nd</sup> person). That is to say, these have to be obligatorily people (or entities metaphorically humanized). However, the form that is called the 3<sup>rd</sup> person does not contain a specification about any particular participant in the discourse. According to Benveniste (1966), the 3<sup>rd</sup> person is a merely negative concept: it is used to indicate the 'non-person', i.e. any other thing different from the speaker and the addressee. In that complement set we find not only humans, but all kind of things for which the notion of person does not apply or it is only *metaphorical*.<sup>15</sup> That this split between person and non-person is true is suggested by two things: (i) by the fact that the *l*-clitics need the association with the deictic clitic, that the 1<sup>st</sup>/2<sup>nd</sup> person clitics do not, and (ii) by the different behavior shown by 1<sup>st</sup>/2<sup>nd</sup> person clitics, with respect to the *l*-clitics.

With respect to (i), it is interesting that the syncretism between dative and accusative in the 1<sup>st</sup> and 2<sup>nd</sup> person breaks with the *l*-clitics. The latter, in effect, have more overt morphological material: the accusative *l*-clitics have gender marking, and the dative *l*-clitics need the deictic clitic. It seems, then, that for the *l*-clitics to do the same work of 1<sup>st</sup> and 2<sup>nd</sup> person clitics, they require some more overt material.

With respect to (ii), it is interesting how some *l*-clitics can be sometimes omitted, keeping their meaning, whereas 1<sup>st</sup> and 2<sup>nd</sup> cannot.<sup>16</sup> For example, when dative and accusative *l*-clitics co-occur, the accusative can sometimes be omitted:

- (40) [əłzi] donaràs el llibre o no [əłzi] (\*el) donaràs?  
 DAT.3P give.2S.FU the book or NEG DAT.3P ACC.3SM give.2S.FU  
*Will you give them the book, or will you not?*

It seems that the presence of the dative clitic is enough to cover both the accusative and the dative arguments, and as a result the accusative clitic has to be omitted in the second disjunct in (40). This omission is not a unique property of Catalan. A similar phenomenon occurs in French:

- (41) Le livre, à l' enfant, je (le) lui ai donné  
 the book, to the boy, I ACC.3SM DAT.3S have.1S given  
*I've given the book to the boy*

In (41), the presence of the accusative clitic *le* is optional to the extent that for some speakers its absence is in fact the preferred option. This phenomenon, though, seems to be limited to *l-*

<sup>15</sup> Although I assume Benveniste's idea about this in this paper, I keep using the term '3<sup>rd</sup> person' for simplicity.

<sup>16</sup> On these differences, see the insightful discussion in Savescu Ciucivara (2004).

clitics. The optionality disappears when one of the clitics is 1<sup>st</sup> or 2<sup>nd</sup> person:

- (42) Le livre, Jean me \*(l') a donné  
 the book, John DAT.1S ACC.3SM has given  
*John has given me the book*

Catalan though takes the optionality to a more extreme point than French. In Catalan, the opacity occurs without taking into account the number features of the clitics involved. If both clitics are singular, the clitic is the dative singular [li] (43a); in any other case the dative is [əlzi]:

- (43) a. El llibre, a el nen, [li] donaré demà.  
 The book to the boy DAT.3S give.1S.FU tomorrow  
*I'll give the book to the boy tomorrow*  
 b. El llibre(s), a el(s) nen(s), [əlzi] donaré demà.  
 The book(s) to the boy(s) DAT.3P give.1S.FU tomorrow  
*I'll give the book(s) to the boy(s) tomorrow*

In any case, the reason why the *l*-clitics and *l*-DPs are considered to be person features seem related to the fact that verbs take a special form (the 3<sup>rd</sup> person form, probably a default form of agreement) when they agree with *l*-constituents. But this is like a negative marking: it is assigned, when required by the verb, to whatever is not 1<sup>st</sup> or 2<sup>nd</sup> person. The *l*-, then, is not a person morpheme, but rather the same definiteness morpheme we find in definite determiners. This is not surprising if we consider that accusative clitics and definite determiners are strongly related cross-linguistically (a fact that in Romance is particularly clear), in line with proposals in Postal (1969), Abney (1987), Kayne (2000), and Elbourne (2001). I do not go into this any further here.

In any case, since the *l*- does not seem to be a person morpheme, but rather a definiteness morpheme, the class of *l*-clitics is different from the, strictly personal clitics. This difference might be responsible for the fact that only dative *l*-clitics have to make explicit deixis when expressing datives. This might be due to the fact that 1<sup>st</sup> or 2<sup>nd</sup> person contain internal deixis, precisely because they contain person morphemes. That internal deixis allows to directly relate them to the immediate context of utterance, and then they do not need further specification to be identified as participants in the discourse (Benveniste 1966). With the 3<sup>rd</sup> person, we need to add something else to identify the referent, and this extra information is provided by the deictic.

As extra support for that idea, I would like to bring up the following fact. In Catalan or French, the verb *venir* 'to come', does not always allow the presence of the locative clitic *hi*. Crucially, the verb *venir* incorporates the information of 1<sup>st</sup> person in its meaning, and therefore disfavors the presence of another element indicating location. The opposite happens with the verb *anar* 'to go', and as a result *anar* has a stronger requirement for the deictic (locative) clitic:

- (44) a. Vine a Barcelona. Vine (\*-hi) demà.  
 come.2S.IMP to Barcelona come.2S.IMP DEIX tomorrow  
 INTENDED: *Come to Barcelona. Come here tomorrow*  
 b. Ves a Barcelona. Ves (\*-hi) demà.  
 go.2S.IMP to Barcelona come.2S.IMP DEIX tomorrow  
*Go to Barcelona. Go there tomorrow.*

Strictly speaking, the locative is not always ungrammatical with the verb 'to come', although here it clearly is. It is not so clearly ungrammatical in a sentence like (45):

- (45) Demà, fem una festa. Si (hi) véns, truca 'm.  
 Tomorrow do.1P a party. If LOC come.2S.SJV call.2S.IMP me  
*We do a party tomorrow. Call me if you want to come.*

Now, although the locative clitic is not strictly ungrammatical, it is optional. A possible explanation might be that the speaker is not in the party at the moment of utterance. I leave the topic as subject for further research.

All this seems to point in the direction we need, that is to say, that *l*-clitics require an overt association with deictic features that 1<sup>st</sup>/2<sup>nd</sup> do not need because they internally incorporate personal features, and these are indeed already deictic. This additional material is provided by the deictic clitic, or the (locative) preposition in overt DPs.<sup>17</sup>

Summing up this section, I have said that the special character of the *l*-clitics is due to their lack of internal deixis, which they have to add by means of association with an explicit deictic clitic. I have argued that the *l*- is actually not a person morpheme, and that therefore, the *l*-clitics lack person features, and then deixis. This indeed entails that the deictic clitics are somehow linked to the dative case. The 1<sup>st</sup> and 2<sup>nd</sup> person gets them internally, as part of the person morphemes they have got. The internal lack of this feature for the *l*-datives is what motivates the [DAT+DEIX] structure, in order for them to get that feature externally.

### 3.2. Why do datives require deictic features?

One of the main claims in this paper is that accusative and dative clitics are actually syncretic across the whole paradigm, and that the only difference between accusative and dative *l*-clitics in general (at least in Romance) is the requirement of deictic features by dative clitics. Now, although this seems to apply to *l*-clitics neatly, one question arises about 1<sup>st</sup> and 2<sup>nd</sup> person clitics. In these cases, since both accusative and dative carry person features, my proposal would require that accusatives also incorporate deictic features, and that lead us to eliminate the difference between the personal clitics. Now, look at the following sentences:

- (46) a. \*(A) mi, em va-veure ahir  
 to me ACC.1S see.3S.PST yesterday  
*He saw me yesterday*  
 b. \*(A) mi, em va-dir la veritat  
 to me, DAT.1S tell.3S.PST the truth  
*He told me the truth*

---

<sup>17</sup> Actually, if we think about accusative and dative DPs in most Romance languages, we see that they are identical too, except for the fact that all dative DPs in most (or all) Romance languages require to be the complement of a preposition or preposition-like element, normally *a*, whereas only some accusative DPs, in some languages require that element, and most of them are direct complements of the verb. Whether the *a* with dative DPs is the same as the *a* with accusative DPs still remains a mystery, as it does the question on which accusative DPs, and why, require the *a*. I do not wish to say anything about these interesting topics here. I only ask why dative clitics and DPs require the extra (overt) element.



In cases of CLLD, first and second person can only be doubled by strong pronouns, and in those cases, the double of the accusative has to be marked by the dative preposition (a phenomenon called Distinctive Object Marking, DOM). This, however, is not necessary with the 3<sup>rd</sup> person, which can be doubled for pronouns or DPs, and the latter do not necessarily have to be marked by the dative preposition:

- (47) a. (\*A) *la taula, la va-veure ahir*  
to the table ACC.3SF see.3S.PST yesterday  
*He saw the table yesterday*
- b. \*(A) *la noia, li va-dir la veritat*  
to the girl DAT.1S tell.3S.PST the truth  
*He told the truth to the girl*

So the fact that accusative and dative refer to people (participants in the speech event) may be responsible for the fact that they are syncretic in both cases. However, in the 3<sup>rd</sup> person clitics, the absence of person features makes it necessary to add the extra overt material. However, this does not explain what makes it obligatory for datives to have deictic features, while accusative have gender features. Two possible lines of investigation into this topic might be that (1) gender somehow plays the same kind of identifying role that deictics play in the case of the dative, i.e. gender would have some deictic function itself, in the sense that it helps to narrow down the reference of a DP; or (2) accusative clitics are actually intransitive definite articles with an elided NP (Baltin & van Craenenbroeck 2008, going back to Postal 1969, or Abney 1987), and as such they have to agree in gender and number with the head of the noun phrase they select. In the latter case, the gender features in the accusative case may actually block deictic features. As for the dative, if one assumes a localist theory of case (as in Anderson 1971, for example), then it makes sense that the dative is related to locations, and as such to locative deixis. This is specially suggestive in the prototypical use of the dative as goal, source, or possessor (Freeze 1992, Kayne 2000, Szabolcsi 1983). Of course, this leaves unexplained other uses of the dative like ethical, benefactive, etc., for which I do not want to say anything about here, as these other uses may not even be a unified phenomenon, but rather a myriad of different things (Susana Huidobro, pc). As I say, this is pure speculation, and the topic would require extensive further research to even consider these as real possibilities.

#### 4. Conclusions

Catalan, and probably Romance, dative and accusative *l*-clitics are syncretic across the board. They are distinguished because dative clitics add deictic features to the accusative form unmarked for gender. Those features are provided internally by their person morphemes, for the 1<sup>st</sup> and 2<sup>nd</sup> person clitics, and externally for the *l*-clitics, by means of their association with deictic clitics, which motivates the structure [DAT+DEIX]. The reason for this association was that the *l*-clitics actually lack person features, the *l*- being probably a definiteness morpheme, not a person morpheme. In Catalan, this lead us to consider that dative clitics are actually *l'hi* and *els hi*, against the standard proposals of the Catalan academy, and this solved the complete set of problems that have traditionally plagued the dative *l*-clitics in this language.

I think that the theory may very well provide the basis for a new theory of datives, based on the fact that they require deixis, at least in the case of the prototypical datives (goal, source, possessor). Such a theory, may also help to understand that the great heterogeneity of

the dative case may be due to the fact that they are just a cover term for a multiplicity of different grammatical phenomena.

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## Coordinated vs. matching questions in Romanian

Dafina Rațiu

In this paper I discuss and compare two types of questions in Romanian, namely multiple questions, where all interrogatives (WHs) appear in clause initial position and coordinated questions, where WHs appear coordinated in clause initial position. I show that coordinated questions and multiple questions in Romanian have different syntactic and semantic properties. Following Hornstein (1995), Dayal (1996), Comorowski (1997) and extending proposals in Chierchia (1993) I adopt an analysis of multiple questions in terms of skolem functions. I further argue that contrary to multiple questions, which are monoclausal, coordinated questions in Romanian are biclausal – that is, they are derived from the conjunction of two CPs.

### 1. Introduction

Romanian is a multiple WH-movement language, where all interrogatives (WHs) must surface in a clause initial position (see Rudin 1991, Comorowski 1997). Fronted WHs in Romanian can appear with or without coordination as illustrated respectively in (1-2). I will refer to non coordinated multiple questions in (1) as *matching questions* and to coordinated questions in (2) as *WH&WH questions*:

Matching questions:

- (1) Cine ce a cumpărat ?  
who what aux bought  
'Who bought what?'

WH&WH questions:

- (2) Cine și ce a cumpărat ?  
who and what aux bought  
'Who bought what?'

In this paper I first show that the properties of matching questions are both syntactically and semantically different from those of WH&WH questions (Section 2.). Syntactically, the order of multiply fronted WHs in matching questions are subject to strict ordering constraints

whereas in WH&WH questions the order of WHs is free. Semantically, matching questions only allow pair list readings, whereas WH&WH questions do allow single pair readings.

Following Hornstein (1995), Dayal (1996), Comorowski (1997) and extending proposals in Chierchia (1993), I adopt an analysis of matching questions in terms of skolem functions (Section 3). This analysis will account for both ordering restrictions and the lack of single-pair readings in Romanian matching questions. I then propose an analysis of WH&WH questions in Romanian in terms of sluicing (Section 4.). The sluicing analysis will automatically derive both the syntactic properties of WH&WH questions in Romanian (the lack of ordering restrictions) and their semantic proprieties (the single pair readings interpretations). Finally, extending the sluicing analysis to a third type of questions, namely questions with one coordinated WH in sentence final position as in (3), it will be shown that the sluicing analysis also correctly predicts the behavior of this type of questions in Romanian.

- (3) Cine a venit și când?  
who aux came and when  
'Who came and when?'

## 2. Matching questions vs. WH&WH questions

### 2.1. Syntactic properties

In matching questions in Romanian, WHs are subject to strict ordering constraints :

- (4) a. Cine ce a cumpărat?  
who what aux bought  
'Who bought what?'  
b. \*Ce cine a cumpărat?  
what who aux bought  
'Who bought what?'

Contrary to matching questions, the order of WHs in coordinated questions is free, as illustrated in (5) below:

- (5) a. Cine și ce a cumpărat ?  
who and what aux bought  
'Who bought what?'  
b. Ce și cine a cumpărat ?  
what and who aux bought  
'Who bought what?'

### 2.2. Semantic Properties

Matching questions in Romanian only allow pair-list (PL) readings whereas in WH&WH questions single-pair readings are available. Consider the context in (6) (Adapted from Bošković) which rules out pair-list readings (enforcing a single-pair reading):

### *Coordinated questions vs. matching questions in Romanian*

(6) *John is in a store and off in the distance sees somebody buying an article of clothing, but he doesn't see who it is, and neither does he see exactly what is being bought. He goes to the shop assistant and asks:*

- |   |  |
|---|--|
| (7) #Cine ce a cumpărat?<br>who what aux bought<br>'Who bought what?' | (8) Cine și ce a cumpărat ?<br>who and what aux bought<br>'Who bought what?' |
|---|--|

The Romanian matching question in (7) is not felicitous in the (single-pair) context provided in (6) whereas the WH&WH question in (8) is felicitous in the context in (6). Consider now the context in (9), where (as opposed to (6)) only a pair-list reading is felicitous.

(9) *John sees Jim finishing off his daily sales. He sees a bunch of people that he knows walk away and Jim stacking his leftover merchandise, so he asks:*

- |  |  |
|--|--|
| (7) Cine ce a cumpărat?<br>who what aux bought<br>'Who bought what?' | (8) Cine și ce a cumpărat ?<br>who and what aux bought<br>'Who bought what?' |
|--|--|

The matching question in (7) and the WH&WH question in (8) repeated above are felicitous in the (pair-list) context provided in (9)<sup>1</sup>. Any analysis of multiple questions vs. coordinated questions in Romanian should account for their differences with respect to both their ordering constraints (illustrated in section 2.1) and their interpretation.

### *3. Matching Questions and the skolem functional analysis*

Assuming an analysis of multiple questions in terms of skolem functions (as argued for English by Hornstein (1995), Dayal (1996), Comorowski (1997) extending proposals in Chierchia (1993), I analyze ordering restrictions in matching questions in Romanian as being constrained by the principle (governing the bound variable interpretation of a pronoun) responsible for WCO. In order to uniformly derive both Anti-Superiority effects in Romanian and Superiority effect in English and assuming that covert movement is counter-cyclic we merely need to modify the denotation Dayal gives for functional  $C^\circ$ . Finally, I argue that in order to uniformly account for the semantics of multiple questions - be it in languages with overt multiple *wh*-movement like Romanian or languages with covert multiple *wh*-movement like English - we must assume that covert movement is counter-cyclic – ‘tucks in’ - in the sense of Richards (2002).

Under Dayal's skolem functional analysis, multiple questions with pair-list readings such as (10a) involve a functional dependency where every member in the subject term (which sets

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<sup>1</sup> Despite the fact that both matching questions and WH&WH questions are felicitous in the Pair-List context in (9), they have different interpretations ; in the interpretation of the matching question in (7) the members of the subject term must be exhaustively paired with one member of the object term, whereas in the coordinated question in (8) the pairing between the members of the subject term and the members of the object term is not subject to such the constraint.

the domain of the function) must be paired with some member in the object term (which sets the range of the function).

- (10) a. Which man<sub>i</sub> likes which woman<sub>j</sub>?  
 b. [<sub>CP</sub> which woman<sub>j</sub> [<sub>CP</sub> which man<sub>i</sub> [<sub>IP</sub> t<sub>i</sub> likes t<sub>j</sub><sup>i</sup>]]]  
 c.  $\lambda p \exists f_{\langle e, e \rangle} [\text{dom } f = \text{man}' \ \& \ \forall y (\text{woman}(f(y))) \ \& \ p = \cap \lambda p' \exists y [\text{man}(y) \ \& \ p' = \wedge \text{like}'(y \text{ likes } f(y))]]$   
 d. {a likes b and c likes d, a and c both like b, a and c both like d, a likes d and c likes b}  
 e.  $\text{Ans}(Q) = \uparrow p [\forall p \ \& \ p \in Q \ \& \ \forall p' \in Q [\forall p' \rightarrow p \subseteq p']]$

Covert movement of the object *which woman<sub>j</sub>* in (10) leaves a complex functional trace, represented as a doubly indexed trace in (10b): the subscript j-index represents the functional variable and the superscript i-index represents an implicit individual variable bound by the c-commanding subject argument. I represent henceforth the individual argument of the functional trace as (*pro*<sub>i</sub>). (10b) is assigned the logical form in (10c). Each propositions in the denotation of the question is a conjunction of atomic propositions obtained by varying the values of *y* and its dependent function *f(y)*. To illustrate, if we assume that a and c are the two men in the situation and b and d the two women, then the question denotes the set of the four propositions in (10d). The answerhood operator in (1e) picks out the unique maximally true proposition from the question denotation.

Hornstein (1995), Dayal (1996, 2000) (extending proposals in Chierchia (1993) argue that if multiple questions involve functional dependencies then Superiority<sup>2</sup> effects reduce to Weak Crossover (WCO) violations. On these accounts, Superiority violations are ruled out by the principle(s) governing the bound variable interpretation of a pronoun<sup>3</sup>. Consider the English grammatical question in (11a) and its derivation in (11b-c):

- (11) a. Who likes what?  
 b. PF: [<sub>CP</sub> who<sub>i</sub> [<sub>IP</sub> t<sub>i</sub> likes what?]  
 c. LF: [<sub>CP</sub> what<sub>j</sub> [<sub>CP</sub> who [<sub>IP</sub> t<sub>i</sub> likes [t<sub>j</sub> [*pro*<sub>i</sub>]]]]]

Covert movement of the object *what* in (11c) leaves a complex functional trace. *Pro*<sub>i</sub> is a licit bound variable since it is A-bound by the trace t<sub>i</sub> (the trace left by *who*<sub>i</sub>). Note that the derivation in (11) is cyclic in the sense of Chomsky (1993)<sup>4</sup> (that is, it expands the tree) since covert movement of the object *what* expands the tree. The Superiority Condition is also obeyed since the highest WH (the subject *who*) has first/overtly moved to Spec CP. Consider now the ungrammatical example in (12a) and its derivation in (12b-c):

- (12) a. \*What does who like?  
 b. PF: [<sub>CP</sub> What<sub>j</sub> [<sub>IP</sub> does who like t<sub>j</sub>]] ?

<sup>2</sup> The Superiority Condition (Chomsky 1973)

a. No rule can involve X, Y in the structure: ...X... [α...Z... W Y V] ..., where the rule applies ambiguously to Z and to Y and Z is superior to Y.

b. A category A is superior to B if every major category that dominates A dominates B, with N, V, A the major categories.

<sup>3</sup> For concreteness I state the principle governing the bound variable as in (1), from Reinhart (1998):

(1) "A pronoun is a licit bound variable iff it is A-bound by a QP."

<sup>4</sup> Chomsky's (1993) definition of cyclicity: 'All operations necessarily expand the tree'.

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c. LF: [CP  $who_i$  [CP  $what_j$  [IP [ $t_i$  [ $pro_j$ ]] like  $t_j$ ]]]

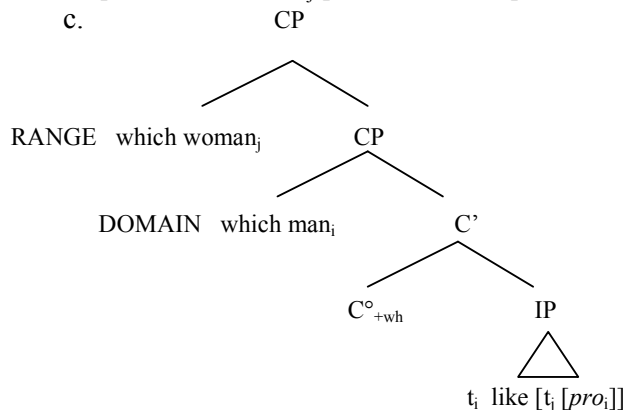
Note that in (12c) the pronominal variable  $pro_j$  is not a licit bound variable since it is not A-bound by the trace of  $what_j$ . The question in (12) is thus filtered out as a WCO violation. Note again that the derivation is cyclic (that is, covert movement of the subject  $who$  expands the tree) but that this time, the Superiority Condition is violated since it is the lowest WH (the object  $what$ ) which has first / overtly moved to Spec CP.

In sum, on a functional analysis of multiple questions, superiority effects follow from whatever constraint governs bound variable interpretation of a pronoun. However, for this account of superiority effects to hold, we have to ensure that the functional trace is introduced by the WH *in situ* (that is, by the object term  $what$  in (11) and by the subject term  $who$  in (12)). Hornstein (1995) stipulates that the WH which introduces the functional trace is indeed the one which is *in situ* in the overt syntax. For Dayal, this follows automatically from the denotation she gives for functional  $C^\circ$  in (13).

(13)  $[[C_0]] = \lambda Q \lambda Y \lambda X \exists f [\text{dom } f = Y \ \& \ \forall y (X(f(y))) \ \& \ p = \cap \lambda p' \exists y [Y(y) \ \& \ p' = \wedge Q(y)(f)]]$

Under a functional interpretation the meaning of  $C^\circ$  introduces three variables; two of them are property variables standing for the domain and the range of the function and their values are fixed by the WH expressions (which denote properties). The third variable allows  $C^\circ$  to combine with IPs with free variables as well as functional variables and its semantic type is a relation between individuals and functions of type  $\langle e, e \rangle$  (one place skolem functions)<sup>5</sup>. Now notice that the denotation for  $C^\circ$  given in (13) requires that the first individual argument to combine with (the function in the denotation of)  $C^\circ$  sets the domain of the function. To illustrate, consider the English multiple question repeated in (14) below and its derivation:

(14) a. Which  $man_i$  likes which  $woman_j$ ?  
 b. [CP which  $woman_j$  [CP which  $man_i$  [IP  $t_i$  likes  $t_j^i$ ]]]  
 c.



Note crucially that covert movement of *which woman<sub>j</sub>* in (14c) proceeds cyclically, that is, it expands the tree. The denotation for  $C^\circ$  given in (13) ensures that the subject term *which man<sub>i</sub>*

<sup>5</sup> A one place skolem function  $f$  is a function that takes an individual  $x$  as its arguments and returns an individual  $f(x)$ .



sets the domain of the function, since it is the closest/first argument to combine with  $C^\circ$ . It follows then automatically that *which woman<sub>j</sub>*, which is *in situ* at SS, sets the range of the function. This proposal ensures that the multiple question in (11) does not violate WCO whereas the multiple question in (12) is ruled out as a WCO violation.

We now turn to Romanian where multiple WH movement takes place in the overt syntax. Assuming that movement is cyclic, that is, it expands the tree) we must conclude that in (15) the structurally lowest WH (the object *ce/what*) is the one which has moved first to Spec CP. But this derivation entails a violation of the Superiority Condition:

- (15) Cine<sub>i</sub> ce<sub>j</sub> t<sub>i</sub> a cumpărat t<sub>j</sub>?  
 who what aux bought  
 ‘Who bought what?’

Richards (2002) concludes that languages (like Romanian) where multiply fronted WHs are subject to strict ordering constraints, exhibit an ‘Anti-Superiority’ effect. To account for Anti-Superiority, he assumes that movement obeys the Superiority Condition but is counter-cyclic (it does not expand the tree). Under this analysis, the highest WH (the subject *cine/who*) in (15) raises first to Spec CP and the lowest one (the object *ce/what*) ‘tucks in’ underneath it then. Irrespective of whether movement is cyclic (expands the tree) in the sense of Chomsky (1993) or ‘tucks in’ in the sense of Richards (2002), Dayal’s semantics for functional  $C^\circ$  given in (13) applied to Romanian multiple questions makes the wrong predictions. Consider again the Romanian contrast below:

- (16) a. Cine ce a cumpărat ?  
 who what aux bought  
 b. CP  
 RANGE cine<sub>i</sub> /who CP  
 DOMAIN ce<sub>j</sub> /what C'  
 C<sup>°</sup><sub>+wh</sub> IP  
 [t<sub>i</sub> [pro<sub>j</sub>]] a cumpărat t<sub>j</sub>  
 aux bought
- (16') a. \*Ce cine a cumpărat ?  
 what who aux bought  
 b. CP  
 RANGE ce<sub>j</sub> /what CP  
 DOMAIN cine<sub>i</sub> /who C'  
 C<sup>°</sup><sub>+wh</sub> IP  
 t<sub>i</sub> a cumpărat [t<sub>j</sub> [pro<sub>i</sub>]]  
 aux bought

The denotation for the functional  $C^\circ$  given in (13) applied to the grammatical question in (16a) entails that the object term *ce/what* sets the domain of the function, since it is the first argument to combine with (the closest to)  $C^\circ$  whereas the subject term *cine/who* sets the range of the function - hence introduces the functional trace -. The grammatical multiple question in (16a) is thus predicted to be ill-formed since it violates WCO. As shown in (16b), *pro<sub>j</sub>* is not a licit bound variable since it is not A-bound. Conversely, the denotation for  $C^\circ$  in (13) applied to the ungrammatical question in (16'a) entails that the subject term *cine/who* sets the domain of the function, since it is the first argument to combine with  $C^\circ$  and the object term *ce/what* set its range (introduces the functional trace). The ungrammatical multiple question in (16') is thus predicted to be well formed since it doesn't violate WCO; *pro<sub>i</sub>* is a licit bound variable since it is A-bound by *t<sub>i</sub>*, as shown in (16'b). We conclude that

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Dayal's account for Superiority effects in English, where the scopal hierarchy of the domain/range terms is established via covert movement, makes incorrect predictions for languages with Anti-Superiority effects like Romanian, where the scopal hierarchy of the domain/range terms is established via overt movement. These generalisations are recapitulated in *Table 1* below:

Superiority in English	Filtered as WCO	Anti-Superiority in Romanian	Filtered as WCO
PF: $\sqrt{\text{Who } t_i \text{ bought what?}}$ LF: $\text{What}_j \text{ who}_i t_i \text{ bought } t_j(\text{pro}_i)$ .	No	PF: $\sqrt{\text{Who}_i \text{ what}_j t_i \text{ bought } t_j?}$ LF: $\text{Who}_i \text{ what}_j t_i (\text{pro}_j) \text{ bought } t_j$ .	Yes
PF: * $\text{What}_j \text{ did who bought } t_j?$ LF: $\text{Who}_i \text{ what}_j t_i (\text{pro}_j) \text{ buy } t_j$ .	Yes	PF: * $\text{What}_j \text{ who}_i t_i \text{ bought } t_j?$ LF: $\text{What}_j \text{ who}_i t_i \text{ bought } (\text{pro}_i) t_j$ .	No

*Table 1*

In order to account for Anti-Superiority effects in languages like Romanian, where the scope of WHs is fixed overtly, we need to modify Dayal's  $C^\circ$  denotation, so that the first argument/the closest to combine with the function in the denotation of  $C^\circ$  sets its range and the second argument to combine with the function sets its domain as in (17) (compare with (13) above:

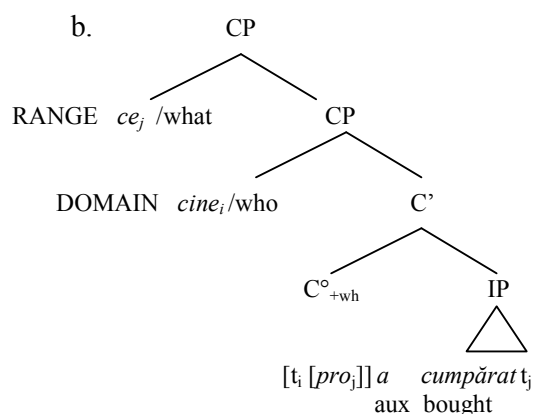
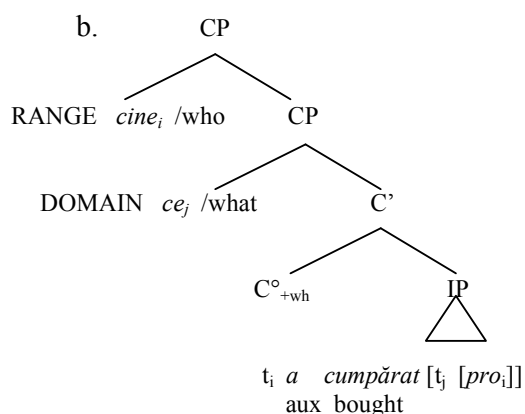
(17) Modified meaning of  $C^\circ$  :

$$[[C^\circ]] = \lambda Q \lambda X \lambda Y \exists f [\text{dom } f = Y \ \& \ \forall y (X (f(y)))] \ \& \ p = \cap \lambda p' \exists y [Y(y) \ \& \ p' = \wedge Q(y) (f)]$$

Given the denotation provided for  $C^\circ$  in (17), it doesn't matter whether overt multiple movement is cyclic (expands the tree) or counter-cyclic ('tucks in' in the sense of Richards). The modified meaning for  $C^\circ$  in (17) ensures that in either case, the first argument to combine with (the function in the denotation of)  $C^\circ$  - that is, the structurally lowest WH - will fix the range of the function and the second argument to combine with  $C^\circ$  - that is, the structurally highest WH - will fix the domain of the function. The new meaning for  $C^\circ$  in (17) thus correctly predicts the grammaticality *versus* ungrammaticality of Romanian multiple questions as shown in (18-18') below:

(18) a. Cine ce a cumpărat?  
who what aux bought

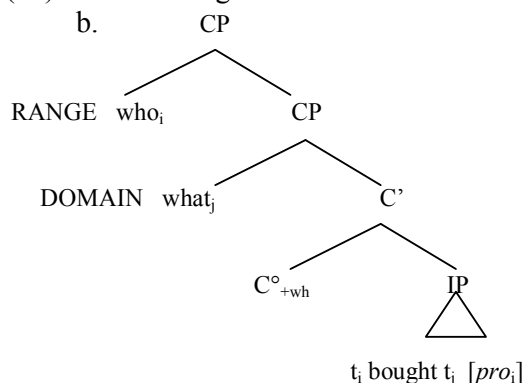
(18') a. \*Ce cine a cumpărat?  
what who aux bought



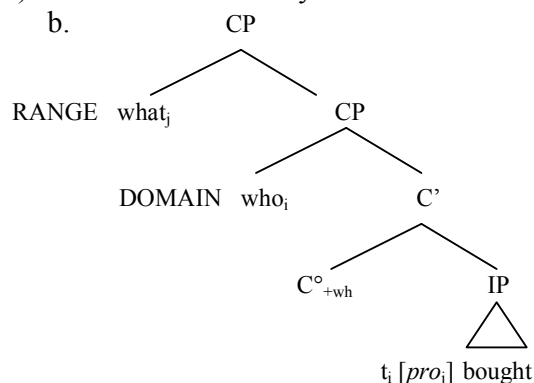
In (18b)  $pro_i$  is a licit bound variable since it is A-bound by  $t_i$ . In (18'b) on the other hand  $pro_j$  is not a licit bound variable since it is not A-bound; the multiple question in (18') is thus filtered as a WCO violation.

Assuming that the denotation for  $C^{\circ}$  is uniform across languages (is not subject to parametric variation) then the null hypothesis must be that the overt syntax of multiple questions in Romanian tells us what the covert syntax of multiple questions is in English. In other words, the output of covert movement in English must yield the scopal hierarchy that overt movement yields in Romanian. This in turn entails that covert movement is counter-cyclic, that is, it targets the WH *in situ* and it tucks in the WH *in situ* under the overtly fronted one. Let's now see how our new meaning for  $C^{\circ}$  together with the assumption that covert movement is counter-cyclic accounts for the grammaticality *versus* ungrammaticality of English multiple questions. Consider again the English multiple questions in (19-19'):

(19) a. Who bought what?



(19') a. \*What did who buy?



The object term *what* in (19b), by (17), sets the range of the function, since it is the first/closest argument to combine with (the function in the denotation of)  $C^{\circ}$ . The trace left by covert counter-cyclic movement of WH “*what*” is thus interpreted as a complex functional trace containing a bound pronoun.  $Pro_i$  is a licit bound variable in (19b) since it is A-bound by the trace of the subject term *who*. In (19b) on the other hand, the first argument to combine with (the function in the denotation of)  $C^{\circ}$  and set the range of the function in the subject term *who*. The trace left by covert (counter-cyclic) movement of the WH *who* is interpreted as a complex functional trace;  $pro_j$  in (19b) is not a licit bound variable since it is not A-bound.

To summarize, on the basis of the syntax of overt multiple movement in Romanian I have concluded that the denotation proposed for functional  $C^\circ$  in (13) must be modified as in (17). On the assumption that covert movement tucks in, this proposal uniformly derive Superiority & Anti-Superiority violations as WCO violations.

Superiority in English	Filtered as WCO	Anti-Superiority in Romanian	Filtered as WCO
PF: $\checkmark$ Who $t_i$ bought what? LF: What $_j$ who $_i$ $t_i$ bought $t_j$ ( $pro_i$ ).	No	PF: $\checkmark$ Who $_i$ what $_j$ $t_i$ bought $t_j$ ? LF: Who $_i$ what $_j$ $t_i$ bought $t_j$ ( $pro_i$ ).	No
PF: * What $_j$ did who bought $t_j$ ? LF: Who $_i$ what $_j$ $t_i$ ( $pro_j$ ) buy $t_j$ .	Yes	PF: * What $_j$ who $_i$ $t_i$ bought $t_j$ ? LF: What $_j$ who $_i$ $t_i$ ( $pro_j$ ) bought $t_j$ .	Yes

Table 2

Note crucially that our analysis of Superiority effects in English hinges on the assumption that multiple covert movement tucks in, in the sense of Richards. What about overt multiple *wh*-movement in Romanian? On the analysis of Superiority effects developed in *Section 2*, it doesn't matter whether movement proceeds cyclically - expands the tree - (e.g. first move *what* (in 19), then move *who* over *what*) or counter-cyclically - tucks in - (e.g. first move *who* in (19), then tuck in *what* underneath it. In either case the object *ce/what* will set the range of (the function in the denotation of)  $C^\circ$  since it is the first/closest to combine with  $C^\circ$  and the subject *who/cine* will set the domain. We thus leave open the question as to whether overt movement tucks in or not.

The ordering restrictions in Romanian follow from the assumption that multiple questions involve a functional trace. Crucially, on this account, if one WH did not introduce a functional trace, there should be no ordering restrictions on the distribution of WHs in matching questions in Romanian. Since this is not the case, we must conclude that matching questions in Romanian always involve complex functional traces. This, in turn, entails that Romanian matching questions will not allow single-pair readings. This conclusion is correct. To illustrate, consider again the context in (21) rules out a pair-list reading:

(20) *John is in a store and off in the distance sees somebody buying an article of clothing, but he doesn't see who it is, and neither does he see exactly what is being bought. He goes to the shop assistant and asks:*

(21) #Cine ce a cumpărat? (Romanian)  
 who what aux bought  
 'Who bought what?'

Recall that the Romanian matching question in (21) is not a felicitous question in the context provided in (20). Grebenyova (2003) argues that the Bulgarian multiple question in (22) is not a grammatical question either, on a single-pair reading:

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- (22) #Koj kakvo e kupil? (Bulgarian, Grebenyova, 2003)  
who what aux bought  
'Who bought what?'

Consider now the context in (23), in which, contrary to the context in (20), only a pair-list reading is felicitous:

- (23) *John sees Jim finishing off his daily sales. He sees a bunch of people that he knows walk away and Jim stacking his leftover merchandise, so he asks:*

- (24) Cine ce a cumpărat? (Romanian)  
who what aux bought  
'Who bought what?'

- (25) Koj kakvo e kupil? (Bulgarian, Grebenyova, 2003)  
who what aux bought  
'Who bought what?'

The matching questions in (24-25) repeated above are appropriate questions for the context in (23). The absence of single-pair readings in multiple questions in Romanian follow from the assumption that multiple questions involve functional traces and hence show ordering restrictions.

Assuming a functional analysis of multiple questions (following in essence Dayal (1996) and Hornstein (1995), I have uniformly analyzed both Superiority violations in languages like English and Anti-Superiority violations in languages like Romanian, as WCO violations. The proposal developed here correctly predicts the absence of single-pair readings in multiple questions in Romanian. On the basis of syntax and semantics of multiple questions in Romanian, I have provided an argument that covert movement is counter-cyclic - tucks-in- in the sense of Richards (2002). Though we have left opened here the question of whether there are any arguments that overt multiple movement also tucks in, the default assumption is plausibly that overt multiple *wh*-movement is counter-cyclic - tucks-in-.

*4. Coordinated questions and the sluicing analysis*

*4.1. WH&WH questions*

Recall that Romanian allow the coordination of two fronted WHs arguments, hence WH&WH questions, as shown in (26) below:

- (26) Cine și ce a cumpărat?  
who and what aux bought  
'Who bought what?'

Recall also that contrary to matching questions, discussed in the previous section, the order of WHs in a WH&WH question in Romanian is free:

- (27) Ce și cine a cumpărat?  
 what and who aux bought  
 ‘Who bought what?’

Moreover, contrary to matching questions, which only allow pair-list readings, WH&WH questions freely allow pair-list and single pair readings. In order to account for the distribution of WHs in coordinated questions on the one hand and for their interpretation on the other, I propose to analyse WH&WH questions in Romanian as involving a coordination of two CPs, where the IP of the first conjunct is sluiced. The intuition I will pursue here is that the coordinated question in (28a) is derived from the conjunction of two questions:

- (28) a. Ce și cine a cumpărat?  
 what and who aux bought  
 ‘Who bought what?’  
 b. Interpretation: What did someone buy and who bought it.  
 c. [&P [CP<sub>1</sub> Ce<sub>x</sub> [IP<sub>1</sub> ] ] [ și [CP<sub>2</sub> cine [IP<sub>2</sub> a cumpărat x]] ]?  
 what and who aux bought

One piece of evidence that WH&WH questions in Romanian involve a coordination of two questions/CPs comes from the distribution of the question word *oare* in Romanian. The question word *oare* in Romanian can appear in Yes/No questions as in (29):

- (29) Oare va ploua azi?  
*oare* aux.fut rain today  
 ‘Is it going to rain today?’

*Oare* can either be used in single constituent questions (30) or in matching questions (31):

- (30) Oare cine bate la ușă?  
*oare* who knock at door  
 ‘Who's knocking at the door?’

- (31) Oare cine ce a comandat?  
*oare* who what aux ordered  
 ‘Who ordered what?’

When the question word *oare* is used in a matching question, it can only appear once, as shown by the ungrammaticality of (32) where *oare* appears twice:

- (32) \*Oare cine oare ce a comandat?  
*oare* who *oare* what aux ordered  
 ‘Who ordered what?’

I conclude from the data illustrated above that the question word *oare* in Romanian can appear only one per clause/question and for the time being I assume that *oare* is a interrogative head (C°). Crucially, in a question with two coordinated WH, the question word *oare* can appear twice, as shown in (33) below:

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- (33) Oare cine și oare ce a comandat?  
*oare* who and *oare* what aux ordered?  
 ‘Who ordered what?’

I take the grammaticality of (33) as an indication that coordinated questions with two coordinated WHs in Romanian are derived from the conjunction of two questions (two CPs). In the next section I will show how an analysis of WH&WH questions in terms of coordination of two CPs explains both the similarities between matching questions and coordinated questions and the differences between these two types of questions.

Under the sluicing hypothesis the question in (34a) below is derived from the conjunction of two CPs where the first CP is sluiced (39b) (see Kliashchuk (2005) for similar analysis in Russian):

- (34) a. Ce și cine a descoperit ?  
 what and who aux discovered  
 ‘Who discovered what?’  
 b. Interpretation: What did someone buy and who bought it.  
 c. [ $\&P$  [ $CP_1$   $Ce_i$  [ $IP_1$  ] ] [ și [ $CP_2$   $cine_j$  [ $IP_2$   $t_j$  a descoperit]]] ?  
 what and who aux discovered

Following Chung, Ladusaw and McCloskey (1995) I adopt an LF approach for sluicing – copying of the antecedent IP at LF. At LF in (34c), the semantic content of the antecedent (IP2) is copied/recycled in the (IP1). The problem with the structure in (34c) is that the second conjunct/question (CP2) is ill-formed because it violates the theta criterion: the internal argument of the verb *discover* in the antecedent (second conjunct) is missing. I crucially assume an empty category standing for that the internal argument of the verb *discover* in the antecedent (IP2) which is licensed via Specifier-Binding (Reinhard, 1983) by the operator *What/Ce<sub>i</sub>* before Spell-Out, as illustrated in (35a):

- (35) a. Spell-Out: [ $\&P$  [ $CP_1$   $Ce_i$  [ $IP_1$  ] ] [ și [ $CP_2$   $cine_j$  [ $IP_2$   $t_j$  a descoperit  $e_i$  ]]] ?  
 what and who aux discovered  $e_i$   
 ‘Who discovered what?’  
 b. LF: [ $\&P$  [ $CP_1$   $ce$   $\lambda x$  [ $IP_1$   $y$  a descoperit  $x$  ] ] [ și [ $CP_2$   $cine$   $\lambda y$  [ $IP_2$   $y$  a descoperit  $x$  ]]] ?  
 what  $\lambda x$   $y$  aux discovered  $x$  and who  $\lambda y$   $y$  aux discovered  $x$

If this analysis is on the right track then the interpretation of the question in (34) does not come as a surprise: the dependency we observe between the two conjuncts in the interpretation of the question in (34b), namely the dependency between the interrogative *what* in the first conjunct and the internal argument of the verb in the antecedent (the object *it*, in the second conjunct), is established Specifier-Binding of the empty category by the WH operator (before Spell-Out).

On the assumption that the empty category in the antecedent must be licensed via Specifier-Binding before Spell-Out, sluicing in the second conjunct is predicted to be illicit as illustrated with the configuration below:

- (36) [ $\&P$  [ $CP_1$   $Ce_j$  [ $IP_1$   $e_i$  a descoperit  $t_j$ ] [ $\&P$  [ $CP_2$  cine [ $IP_2$  ] ]]]]?  
 what aux discovered and who

Assuming an empty category standing for the external argument of the verb *descoperit* discover/ in the first conjunct (the antecedent  $IP_1$ ), the configuration in (36) is illicit since the empty category  $e_i$  cannot be Specifier-Bound by *cine*/'who' and thus fails to be licensed. This prediction is correct as shows the ungrammaticality of the data in (37) below:

- (37) \*Ce a descoperit și cine?  
 what aux discover and who  
 'Who discovered what?'

Note that contrary to WH&WH questions discussed in this section, where two fronted WH appeared coordinated, in (38) below, one WH appears preceded by the coordination in a clause final position. I will discuss this third type of questions in the next sub-section.

The analysis in terms of sluicing of WH&WH questions in Romanian, together with the assumption of the presence of an empty category in the antecedent IP (second conjunct) which is licensed via Specifier-Binding before Spell-Out, automatically generates their interpretation. Given that WH&WH questions are derived from the conjunction of two questions, the availability of SP reading follows automatically since no functional  $C^\circ$  is selected in these structures. This in turn correctly predicts the lack of ordering constraint in WH&WH structure. Recall that we derived ordering constraints in matching questions (section 2) from the skolem functional analysis and WCO.

#### 4.2. &FinalWH questions

The third type of question which will be addressed in this article are questions with one fronted WH and a second WH appearing in a sentence clause final position preceded by the conjunction. I will refer to these questions as &FinalWH. Crucially, and contrary to WH&WH questions discussed in the previous sub-section (*sub-section 4.1*), &FinalWH are only licit if the final interrogative is an adjunct:

- (38) Cine și ce a descoperit ?  
 who and what aux discovered  
 'Who danced and where?'
- (39) \*Cine a descoperit și ce ?  
 who aux discovered and what  
 'Who danced and where?'
- (40) Cine a dansat și unde?  
 who aux danced and where  
 'Who danced and where?'

I propose that &FinalWH questions as (38) are derived exactly in the same way as WH&WH questions, namely, from the conjunction of two questions (two CPs). Recall that in WH&WH questions, sluicing took place in the first conjunct as repeated in (41) below:



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- (41) a. Ce și cine a descoperit ?  
 what and who aux discover?  
 ‘Who discovered what?’  
 b. Interpretation: What did someone discover and who discover it.  
 c. Spell-Out: [ $\&P$  [ $_{CP}$   $Ce_i$  [ $_{IP1}$  ] ] [ $\&P$  [ $_{CP}$   $cine_j$  [ $_{IP2}$   $t_j$  a descoperit  $e_i$  ]]]]?  
 what and who aux discover  
 d. LF: [ $\&P$  [ $_{CP}$   $ce$   $\lambda x$  [ $_{IP1}$   $y$  a descoperit  $x$  ] ] [ $\&P$  [ $_{CP}$   $cine$   $\lambda y$  [ $_{IP2}$   $y$  a descoperit  $x$  ]]]]  
 what  $\lambda x$   $y$  aux discover  $x$  and who  $\lambda y$   $y$  aux discover  $x$

Extending the sluicing analysis to  $\&FinalWH$  questions, we must assume that in (42) sluicing takes place in the second conjunct:

- (42) a. Cine a râs și când?  
 who aux laughed and when  
 ‘Who laughed and when?’  
 b. Spell-Out: [ $\&P$  [ $_{CP}$   $Cine_i$  [ $_{IP2}$   $t_i$  a râs ] ] [ $\&P$  [ $_{CP}$   $cînd$  [ $_{IP1}$  ] ]]]]?  
 who aux laugh and when  
 c. LF: [ $\&P$  [ $_{CP}$   $cine$   $\lambda x$  [ $_{IP2}$   $x$  a râs ] ] [ $\&P$  [ $_{CP}$   $cînd$   $\lambda y$  [ $_{IP1}$   $x$  a râs ] ]]]]?  
 who  $x$  aux laugh and when  $x$  aux laugh

Note that the LF in (42b) should be illicit since *cînd* ‘when’ (or rather the  $\lambda$ operator that takes when as an argument) doesn't bind anything (vacuous quantification). I adopt the mechanism of *sprouting* proposed by CML (1995) which allows us to create/ to add a trace in the recycled IP:

- (43) a. John ate a dinner but I don't know with whom. CML (1995)

Once the LF of the antecedent IP is recycled into the IP of the sluice in (43) there will be no appropriate syntactic position for the displaced constituent to bind. This will be remedied by sprouting an extra PP-position in the recycled IP:

- (43) b. [ $_{IP1}$  John ate a dinner] but I don't know [ $_{CP}$  [with whom] $_i$  [ $_{IP1}$  John ate a dinner  $t_i$  ]]]?

CML argue that sprouting is a freely available process for building structure at LF, subject only to constraints of X-bar theory. Moreover, the verb of the recycled IP must respect the argument structure of the verb in the antecedent.

Sprouting in our case corresponds to adding a variable for the adjunct *cînd* ‘when’ to bind in the recycled IP as shown in the LF in (43b):

- (44) a. Cine a râs și când?  
 who aux laughed and when  
 ‘Who laughed and when?’  
 b. LF: [ $_{CP}$   $cine$   $\lambda x$  [ $_{IP1}$   $x$  a ris ] ] [ $\&P$  [ $_{CP}$   $cînd$   $\lambda y$  [ $_{IP2}$   $x$  a ris at/in  $y$  ] ]]]]?  
 who  $\lambda x$   $x$  aux laugh and when  $\lambda y$   $x$  aux laugh

The sluicing analysis for &FinalWH questions in Romanian thus correctly predicts the grammaticality of the &FinalWH question in (44). With this in mind, let's go back to the &FinalWH question in (45), where the WH appearing in clause final position is a selected argument:

- (45) a. \*Cine a descoperit și ce?  
 who aux discovered and what  
 'Who discovered what?'  
 b. [<sub>CP</sub> Cine<sub>i</sub> [<sub>IP1</sub> t<sub>i</sub> a descoperit e<sub>j</sub>]] [și [<sub>CP</sub> what [<sub>IP2</sub> ] ] ]?

The licensing of the empty category the antecedent IP is crucial. Recall that in WH&WH questions, the empty category in the antecedent is licensed via Specifier-Binding in the overt syntax by the operator in the first conjunct. In (45b) however the empty category (standing for the internal argument of the verb *descoperit/discover*) in the antecedent IP1 cannot be bound by *ce/'what'* and hence fails to be licensed. The sluicing analysis correctly predict the ungrammaticality of (45). Note that, as expected, pronouncing both conjuncts in does not save the derivation, as shows the ungrammaticality of (46).

- (46) \*Cine<sub>i</sub> t<sub>i</sub> a descoperit e<sub>j</sub> și ce<sub>j</sub> t<sub>i</sub> a descoperit e<sub>j</sub>?  
 who aux discovered and what aux discover

On the assumption that the empty category has to be licensed via Specifier-Binding by an operator the ungrammaticality of (46) does not come as a surprise since the variable "e<sub>j</sub>" in the first conjunct is still not licensed.

### 5. Conclusion

Assuming a functional analysis of multiple questions (following in essence Dayal (1996) and Hornstein (1995)), I have uniformly analysed both Superiority violations in languages like English and Anti-Superiority violations in languages like Romanian, as WCO violations. The proposal developed here correctly predicts the absence of single-pair readings in multiple questions in Romanian. The sluicing analysis I propose for WH&WH questions in Romanian automatically derives both the syntactic properties of WH&WH questions in Romanian, namely the lack of ordering restrictions, and their semantic proprieties, the availability of single pair readings. Finally, extending the sluicing analysis to &FinalWH questions in Romanian it correctly predicts the behavior of this type of questions in Romanian.

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**Some people are repeaters<sup>1</sup>**  
Medial copy spell-out in long-distance wh-dependencies

Ankelien Schippers

This article addresses a number of unresolved issues regarding the spell-out of wh-phrases in intermediate SpecCPs in partial wh-movement and wh-copy constructions. I argue that intermediate SpecCPs are potential terminal landing sites and as such count as the head of a chain. Due to the general requirement for spelling out heads of chains, a wh-phrase may and in some cases must be spelled out in this position. I argue that this analysis satisfactorily explains a range of problematic facts regarding the spell-out of intermediate wh-phrases.

*1. Introduction*

Over the past few decades, medial wh-movement constructions like partial wh-movement and wh-copying have drawn considerable attention (see for example the collection of papers in Lutz et al. 2000). The constructions can be attested in a number of languages, including German, Hindi, Romani, Frisian, Albanian, Hungarian, Passamaquoddy, Warlpiri, Finnish, Iraqi Arabic, Russian and Polish.<sup>2</sup> Partial wh-movement is exemplified in the German example in (1) below, and wh-copying in (2).

- (1) [CP<sub>1</sub> Was   meinst   du [CP<sub>2</sub> wen   Marie geküsst    hat?]]  
      what think   you     who   Marie kissed    has  
      ‘Who do you think Mary kissed?’
- (2) [CP<sub>1</sub> Wen   meinst    du [CP<sub>2</sub> wen   Marie geküsst    hat?]]  
      who think     you    who   Marie kissed    has  
      ‘Who do you think Mary kissed?’

In case of partial wh-movement, the lowest CP contains the true wh-phrase, while higher CPs contain what is called the scope marker (*was* ‘what’). In wh-copy constructions, all CPs are

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<sup>1</sup> This title is inspired by a quotation in Bruening (2006), where the use of wh-copying by some speakers of Passamaquoddy is commented on by an informant saying ‘Some people are repeaters’ (Bruening 2006: 37, fn.6).

<sup>2</sup> For an extensive overview of languages that have partial wh-movement, see Fanselow (2006). In this article, I focus on partial wh-movement and wh-copying in Germanic. In the remainder of this article, I therefore limit myself to examples in German unless noted otherwise.

filled by copies of the true *wh*-phrase. These constructions contrast with ‘standard’ long-distance *wh*-movement as in (3), where only one *wh*-phrase is spelled out, namely the one in the matrix interrogative CP.

- (3) [<sub>CP1</sub> Wen    meinst    du [<sub>CP2</sub> dass    Marie geküsst    hat?]]  
       who    think    you    that    Marie kissed    has  
       ‘Who do you think Mary has kissed?’

A returning question regarding the constructions in (1) and (2) has been why intermediate copies are spelled out. Most analyses of partial *wh*-movement and *wh*-copying assume that the *wh*-phrases in intermediate SpecCPs are some sort of a reflex of successive-cyclic movement. This would in fact follow quite naturally under the copy theory of movement (cf. Chomsky 1993). Here, it is assumed that movement doesn’t leave behind traces, but full copies of the moved element. However, intermediate copies are usually not spelled out and as such, it is surprising that copies of the *wh*-phrase may overtly surface. In fact, multiple copy spell-out seems to violate interface requirements, since these copies do not contribute to the interpretation and arguably cause a failure to linearize the structure. I discuss these and related problems in full detail in section 3. Before that, I briefly discuss some of the main analyses that have been proposed regarding partial *wh*-movement and *wh*-copying in section 2. In section 4, I point out how the problems discussed in section 3 can be solved. Finally, the article closes off with a conclusion.

## *2. Analyses of partial wh-movement and wh-copying*

Regarding partial *wh*-movement, two main types of analyses can be distinguished: the Direct Dependency Approach (cf. Tappe 1980; van Riemsdijk 1983; McDaniel 1989 and many others) and the Indirect Dependency Approach (cf. Dayal 1994, 2000; Fanselow & Mahajan, 2000; Horvath 2000; Mahajan 2000; Felser 2001; Sternefeld 2001). Within the Direct Dependency Approach, it is assumed that there is a direct link between the scope marker and the true *wh*-phrase. How this link is established exactly differs somewhat across authors. Some assume that the scope marker and the true *wh*-phrase are coindexed, and that the true *wh*-phrase overwrites the scope marker at LF (cf. Van Riemsdijk 1983; McDaniel 1989 and many others). Others assume that the scope marker is a partial spell-out of some part of the true *wh*-phrase itself (cf. Hiemstra 1986; Sabel 1998; Cheng 2000; Barbiers et al., 2008).

The Indirect Dependency Approach, on the other hand, does not posit such a direct link between the scope marker and the true *wh*-phrase. This approach further differs from the Direct Dependency Approach in that it is assumed that the scope marker originates in a low position within the matrix clause. In Dayal’s analysis, which is the canonical one, the scope marker is viewed as a true argument of the main verb that originates in object position of the matrix clause. The scope marker is seen as a true quantifier, whose restriction is formed by the embedded clause. Other variants of the Indirect Dependency Approach essentially assert the same: namely that the scope marker originates in a low position within the matrix clause, and that the scope marker forms a link with the embedded clause as a whole, not just with the *wh*-phrase it contains. The technicalities may sometimes differ: some argue that the scope marker is a *wh*-expletive that is replaced by the embedded clause at LF (cf. Herburger 1994; Fanselow & Mahajan 2000; Horvath 2000; Mahajan 2000; Sternefeld 2001), while others (cf.

Felser 2001; Den Dikken 2009) argue that the embedded clause together with the scope marker forms a complex predicate.<sup>3</sup> In general, the Indirect Dependency Approach differs from the Direct Dependency Approach in that it is assumed that there is a link between the scope marker and the entire embedded clause, and that this link is established semantically, not syntactically. Within the Direct Dependency Approach, on the other hand, the scope marker forms a link only with the embedded wh-phrase, and this link is syntactical in nature.

There has been an ongoing debate as to which analysis (Direct or Indirect Dependency Approach, or any of their variants) is the correct one for partial wh-movement. It goes beyond the scope of this paper to discuss all the arguments for and against specific analyses. For reasons that will become clear, I adopt an Indirect Dependency Approach to partial wh-movement. In particular, I adopt a type of analysis along the lines of Felser (2001), Den Dikken (2009) and Koster (2009).

Finally, a note regarding the analysis of wh-copying is in order. This construction appears to be (almost) invariably analyzed as a surface variant of long-distance wh-movement, in which the wh-phrase in the embedded SpecCP is analyzed as a spelled out copy of the moved wh-phrase. As such, wh-copying is sometimes analyzed as a variant of partial wh-movement in certain Direct Dependency Approaches (cf. Hiemstra 1986; Bayer 1996; Brandner 2000; Höhle 2000; Barbiers et al. 2008). This is possible since the Direct Dependency Approach analyzes partial wh-movement as a surface variant of long-distance wh-movement, too. Indirect Dependency Approaches, on the other hand, generally do not analyze wh-copying as a variant of partial wh-movement. This is due to the fact that in this approach, the highest wh-phrase in medial wh-movement constructions necessarily has to be analyzed as a quantifier or a wh-expletive. Clearly, full copies of the wh-phrase do not fit this description. Therefore, wh-copying is usually not treated as a variant of partial wh-movement within the Indirect Dependency Approach, but rather as a variant of long-distance wh-movement.<sup>4</sup>

### *3. Problems regarding multiple spell-out in medial wh-movement constructions*

While the analyses discussed in the previous paragraph provide both syntactic and semantic analyses that cover much of the particularities of partial wh-movement and wh-copy constructions, there are a number of unresolved issues these analyses leave open, all centered around the problem of what permits the spell-out of wh-phrases in intermediate SpecCPs. This is particularly true for the wh-copy construction. In this section, I discuss five main problems that are (to various degrees) in need of further explanation. Subsequently, in section 4, I point out how these problems may be resolved.

#### *3.1. Problem 1: Wh-phrases show up in positions where they do not seem licensed*

One of the main problems regarding medial wh-movement constructions has been the fact that wh-phrases are spelled out in positions where they do not seem licensed. The matrix predicates in partial wh-movement and wh-copy constructions overlap to a great degree with those allowed in long-distance movement contexts (i.e. so-called bridge verbs). However,

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<sup>3</sup> An analysis along similar lines is proposed in Koster (2009).

<sup>4</sup> However, one exception is Koster (2009), who analyzes copy constructions as Indirect Dependencies as well. I will come back to his analysis in section 4.1.

these predicates normally may only combine with declarative complements. In fact, they generally may not select for an interrogative *wh*-clause, as can be learned from (4):

- (4) \*Meinst du    wen    Marie geküsst    hat?  
       think you    who    Marie kissed    has?  
       ‘Think you who Mary has kissed?’

Partial *wh*-movement and *wh*-copy constructions are therefore in apparent violation of Lasnik & Saito’s (1984) *Wh*-Criterion, which states that [–*wh*] COMPS may not contain *wh*-phrases (cf. McDaniel 1989).

Indirect Dependency Approaches are quite successful in accounting for this violation when it comes to partial *wh*-movement, since it is assumed in this approach that the embedded clause is semantically a question and may therefore be introduced by a *wh*-phrase. However, since the Indirect Dependency Approach cannot be extended to *wh*-copying, it fails to explain why *wh*-phrases are spelled out in intermediate SpecCPs in these cases as well.

Within Direct Dependency Approaches, it has been proposed to modify the *Wh*-Criterion in order to solve the problem. For example, McDaniel (1989) has argued to restate the *Wh*-Criterion in that every *wh*-phrase in SpecCP must be part of a *wh*-chain (headed by a scope marker), while others propose that the *Wh*-Criterion only holds at LF (cf. von Stechow & Sternefeld 1988; von Stechow 2000). While such proposals in principle work, they require resort to abstract notions such as LF-movement and chain formation.

### 3.2. Problem 2: *Wh*-phrases are only spelled out in intermediate SpecCPs

In the previous section I discussed the peculiar fact that in medial *wh*-movement constructions, *wh*-phrases show up in CPs from which they are normally banned. These intermediate CPs also constitute the only positions in which copies are allowed to surface. That is, copies in medial *wh*-movement constructions may neither surface in their base position, nor in any intermediate landing site other than SpecCP.<sup>5</sup> This is illustrated below in examples (5) and (6): (5) shows that copies may not stay in situ, and (6) shows that they may also not show up in intermediate landing sites other than SpecCP, such as SpecvP:

- (5) \*<sub>[CP1]</sub> Was    meinst    du <sub>[CP2]</sub> Marie hat    wen    geküsst?]  
       what    think    you    Marie has    who    kissed?
- (6) \*<sub>[CP1]</sub> Wen/Was meinst <sub>[VP1]</sub> (\*wen) du <sub>[CP2]</sub> (wen) (dass) Marie <sub>[VP2]</sub> (\*wen) geküsst hat?]  
       who/what think            who you    who    that Marie    who kissed has

The fact that in *wh*-movement languages, the true *wh*-phrase in medial *wh*-movement constructions may not stay in situ is rather surprising in view of multiple *wh*-questions. Multiple questions and medial *wh*-movement constructions have in common that they both contain more than one (overt) *wh*-phrase. In case of multiple questions, only one *wh*-phrase moves to the interrogative CP, while all other *wh*-phrases must stay in situ. This is illustrated in example (7a). In fact, movement of the second *wh*-phrase is not allowed, as (7b) shows.

<sup>5</sup> The requirement that *wh*-phrases in partial *wh*-movement constructions must be in SpecCP only holds in *wh*-movement languages. In a *wh* in situ language like Hindi, both the true *wh*-phrase and the scope marker remain in situ. *Wh*-copy constructions are (to my knowledge) only attested in *wh*-movement languages.

This may be explained by assuming that the highest *wh*-phrase has already checked the uninterpretable Q-feature and EPP feature of the interrogative C, so that overt movement of a second *wh*-phrase is superfluous (cf. Chomsky 2000). One would then expect the same to hold in partial *wh*-movement and *wh*-copy constructions. That is, the highest *wh*-phrase (i.e. the scope marker or *wh*-copy) should be able to check the uninterpretable features of the interrogative C, obliterating the need for further movement. Clearly, this is not the case, as examples (7c) and (7d) show.

- (7) a. [<sub>CP1</sub> Wer glaubst du [<sub>CP2</sub> dass wen besucht hat?]]  
       who believe you that who visited has  
       ‘Who do you believe visited whom?’
- b. \* [<sub>CP1</sub> Wer glaubst du [<sub>CP2</sub> wen (dass) t<sub>wen</sub> besucht hat?]]  
       who believe you who (that) visited has  
       ‘Who do you believe visited whom?’
- c. \* [<sub>CP1</sub> Was/wen glaubst du [<sub>CP2</sub> dass Fritz wen besucht hat?]]  
       what/who believe you that Fritz who visited has  
       ‘Who do believe that Fritz visited?’
- d. [<sub>CP1</sub> Was/wen glaubst du [<sub>CP2</sub> wen Fritz t<sub>wen</sub> besucht hat?]]  
       what/who believe you who Fritz visited has  
       ‘Who do believe that Fritz visited?’

Next to the *wh*-phrase not being allowed in situ in medial *wh*-movement constructions, it is also not allowed in any position other than SpecCP, as (6) showed. This forms a problem for analyses which assume intermediate *wh*-phrases in medial *wh*-movement constructions are a reflex of successive-cyclic movement. In that case, *wh*-phrases should be able to show up in other intermediate landing sites, notably *vP*, since this is one of the positions through which successive-cyclic movement is hypothesized to proceed (cf. Chomsky 2000, 2001).

The ban on spell-out of *wh*-phrases in *vP*-edges is specifically problematic for the analysis of *wh*-copying. As pointed out earlier, *wh*-copying is (almost) invariably analyzed as a surface alternative to long-distance *wh*-movement, and as such should proceed through phase edges (specifically *vP* and CP). Under the assumption that *wh*-copying is possible because multiple spell-out of *wh*-copies is allowed, it should then also be possible to spell out copies of the *wh*-phrase at *vP* phase edges. Clearly, this is not the case.

The same problem arises for the Direct Dependency analysis of partial *wh*-movement: again it is unclear why partially moved *wh*-phrases may only occupy SpecCP positions. Under the Indirect Dependency Approach, on the other hand, this prohibition follows more naturally, since the Indirect Dependency Approach assumes that intermediate CPs are interrogative and for this reason attract a *wh*-phrase to their specifier, resulting in subsequent spell out of the *wh*-phrase.

The prohibition against spelling out copies in positions other than SpecCP may be explained by assuming *vP* is not a phase or that successive-cyclic *wh*-movement does not have to pass through *vP* edges. This, however, would be in violation of the Phase Impenetrability Condition (Chomsky 2000, 2001). Either way, it remains to be explained why *wh*-phrases may not occur in base position in medial *wh*-movement constructions.



### 3.3. Problem 3: Multiple copies are in violation of the Linear Correspondence Axiom

This third problem concerns wh-copying specifically. Namely, in wh-copy constructions, the occurrence of two or more non-distinct copies should result in a failure to linearize the structure (cf. Nunes 1995, 1999, 2004; Felser 2004). Specifically, wh-copy constructions appear to violate the Linear Correspondence Axiom (LCA) (Kayne 1994). The LCA requires anti-symmetric c-command relations between terminals so that they can be linearized. However, in case of wh-copying, two non-distinct copies both (asymmetrically) c-command and are c-commanded by intervening material. As a result, linearization is not possible, which should cause the derivation to crash. Normally, the spelling out of more than one copy indeed results in a non-converging derivation, as illustrated in (8):

(8) \*Mary<sub>i</sub> was hit Mary<sub>i</sub>.

Multiple copy spell-out is actually a rare phenomenon and the LCA is able to account for this quite naturally. However, it is also clear that in cases where multiple copy spell-out does take place, it results in converging derivations. This is also the case for wh-copying: these constructions are grammatical in many languages and for many speakers, suggesting these constructions somehow circumvent an LCA violation.

There are a number of proposals explaining why the LCA is not violated by wh-copy constructions. Nunes (1995) suggests that the copies may actually count as distinct, and as such do not form a problem for the LCA. However, he abandons this hypothesis in Nunes (1999) and (2004) and argues instead that the LCA is not able to ‘see’ the intermediate copies. This, he claims, is due to the fact that the wh-phrase and C undergo fusion, turning C and the wh-phrase into one phonological word. This way, the wh-phrase and its copy become distinct and may (in fact must) be both spelled out. Felser (2004), on the other hand, suggests that PF spell-out takes place automatically at the CP level. Assuming that the LCA is only operative at PF, she argues that intermediate copies do not violate the LCA, since phase-internal c-command relations disappear once a structure has been linearized. However, as she notes herself, this analysis is rather speculative and requires the assumption of two independent spell-out mechanisms (one at LF and one at PF), which obviously complicates the system.

### 3.4. Problem 4: Prohibition against copying complex wh-phrases

One of the main differences between partial wh-movement and wh-copying is that the first, but not the latter, allow complex wh-phrases in intermediate SpecCPs (cf. McDaniel 1986; Fanselow & Mahajan 2000; Höhle 2000).<sup>6,7</sup> This is illustrated in (9a,b) and (10a,b) below, where (9a,b) involve movement of a wh-phrase modifying a NP, while (10a,b) involve movement of a prepositional wh-phrase:

<sup>6</sup> Long-distance wh-movement patterns with partial wh-movement in this respect: complex wh-phrases may readily be moved long-distance.

<sup>7</sup> It must be noted that complex wh-phrases are not banned from surfacing in the wh-copy construction itself; they are only prohibited from being copied. Hence, complex wh-phrases may show up in wh-copy constructions when it involves a multiple questions, and only the simple wh-phrase is copied. This is illustrated in (i) below:

(i) [<sub>CP1</sub> Wer glaubst du [<sub>CP2</sub> wer welche Bücher gekauft hat?]]  
 who believe you who which books bought has  
 ‘Who do you believe bought which books?’

- (9) a. [<sub>CP1</sub> Was glaubst du [<sub>CP2</sub> welche Bücher sie gekauft hat?]]  
       what believe you which books she bought has  
       ‘Which books do you believe she has bought?’  
    b. \* [<sub>CP1</sub> Welche Bücher glaubst du [<sub>CP2</sub> welche Bücher sie gekauft hat?]]  
       which books believe you which books she bought has?  
       ‘Which books do you believe she has bought?’
- (10) a. [<sub>CP1</sub> Was hat er gesagt [<sub>CP2</sub> auf wen er warten soll?]]  
       what has he said for who he wait would?  
       ‘For whom has he said he would wait?’  
    b. % [<sub>CP1</sub> Auf wen hat er gesagt [<sub>CP2</sub> auf wen er warten soll?]]  
       for who has he said for who he wait would?  
       ‘For whom has he said he would wait?’

Wh-phrases of the type wh-NP seem invariably out for wh-copying, while wh-phrases that combine with a preposition show variable behavior. Regarding German, judgments on sentences like (10b) are variable (cf. Fanselow & Mahajan 2000; Felser 2004). In general, as long as the wh-phrase is contained in one phonological word, copying is possible. However, in Afrikaans, copying of more complex prepositional wh-phrase is fully grammatical, as illustrated in (11) (example from Du Plessis 1977):

- (11) Met wie het jy gesê met wie gaan Jan trou?  
       with whom have you said with whom go John marry  
       ‘With whom did you say John will marry?’

Hence, copying of complex wh-phrases does not only show variability within a language, but also across languages. As a general rule, one may say that wh-phrases modifying a noun do not allow copying, while wh-phrases contained in a PP allow copying to various degrees.

The fact that complex wh-phrases cannot be (overtly) copied has been used as evidence in favor of the fusion account. Nunes (2004) argues that morphological fusion deals with heads, not maximal projections. This, according to him, explains why pronominal wh-phrases may undergo fusion, whereas complex ones, e.g. those of the type ‘wh-NP’, may not.<sup>8</sup> However, such an analysis incorrectly rules out copying of prepositional wh-phrases as in (10b) and (11) above. To handle these cases, Nunes suggests that speakers may differ in the degree to which they allow morphological reanalysis. This points towards a more problematic aspect of his analysis, since it is not clear why and when morphological reanalysis takes place. Specifically, why does it take place in some languages (e.g. German), but not in others (e.g. English)? And why is there between-speakers variation in this respect? I will turn to this issue in more detail in section 4.

<sup>8</sup> Fanselow & Mahajan (2000) offer a similar solution to the problem that complex wh-phrases may not be copied. They point out that in German, the CP must always be phonologically visible, and that this requirement can be satisfied by either spelling out a copy of the wh-phrase or by spelling out the complementizer. They argue that in case the copy is spelled out, it must cliticize onto C. According to them, this explains why only monomorphemic wh-phrases can be copied (since more complex wh-phrase cannot cliticize). However, this again incorrectly rules out copying of more complex prepositional wh-phrases. Their proposal is further complicated by the fact that there are German varieties that allow Doubly Filled Comps in copying contexts, which refutes the claim that copies are spelled out to salvage an otherwise phonologically empty CP.

### 3.5. Problem 5: What prevents multiple spell-out?

A final problem regarding the spell-out of medial wh-copies is the fact that many of the current analyses of partial wh-movement and wh-copying fail to explain why multiple copies are not always spelled out. This is the case for the analysis of wh-copying in general and for Direct Dependency Approaches to partial wh-movement as well.

Wh-copying is invariably analyzed as a surface alternative of long-distance wh-movement. The spelling out of the intermediate copy is something that is usually attributed to the phonological component, assuming intermediate spell-out is freely available. This suggests that wh-copying may freely occur in long-distance wh-movement languages, but this does not seem to be the case. For example, English is a long-distance movement language, but is generally reported not to have wh-copying. The same is true for Scandinavian languages.

Direct Dependency Approaches to partial wh-movement also generally predict partial wh-movement to be possible in long-distance movement languages. This is due to the fact that partial wh-movement is analyzed as a surface alternative to long-distance wh-movement as well within this approach. Analyses that assume that the scope marker is a wh-expletive which is coindexed with the true wh-phrase (e.g. van Riemsdijk 1983, McDaniel 1989) predict partial wh-movement to be possible in any language that has a wh-expletive. This does not seem to be the case: for example, Dutch has a wh-expletive *wat* ‘what’, but generally does not allow partial wh-movement (cf. Müller 1997). Others have proposed that the scope marker is a partial spell-out of the true wh-phrase (cf. Hiemstra 1986; Sabel 1998; Cheng 2000 and Barbiers et al. 2008). These analyses also fail to account why this is apparently possible in some long-distance movement languages (e.g. German), but not in others (e.g. English).

In sum, Direct Dependency Approaches predict free variation of long-distance movement and partial wh-movement, but the empirical evidence argues against this. In fact, there is evidence that these constructions are usually in complementary distribution (cf. Stepanov & Stateva 2006).

The Indirect Dependency Approach, on the other hand, does not analyze partial wh-movement as a surface variant of long-distance wh-movement. Therefore, this approach does not predict partial wh-movement to generally show up in long-distance wh-movement languages. However, as pointed out in section 2, it is difficult to extend the Indirect Dependency Approach to wh-copying. So while it may account for the fact that partial wh-movement is not generally possible in long-distance wh-movement languages, it has nothing to say about the (un)availability of wh-copying in a language.

## 4. The proposal

In this section, I give an account of what licenses intermediate copies in medial wh-movement constructions and discuss how this account resolves the problems discussed in the previous sections. The proposal revolves around the hypothesis that intermediate SpecCPs are heads of a chain, and that therefore a wh-phrase may and in some cases must be spelled out there. In section 4.1, I present an Indirect Dependency Analysis of partial wh-movement along the lines of Felser (2001), Den Dikken (2009) and Koster (2009) and discuss to what degree this analysis can be extended to wh-copy and long-distance wh-movement constructions. In line with Stepanov & Stateva (2006), I argue that in all the wh-dependencies under consideration, movement to the intermediate SpecCP is triggered independently of higher feature checking

requirements. In section 4.2, I argue that it follows quite naturally from this analysis to regard intermediate SpecCPs as heads of a movement chain. I claim that as such, they are candidates for spell-out, contrary to other position within the movement chain, which can never be the head of a chain. Section 4.3 discusses how this proposal accounts for the fact that wh-copy constructions do not violate the LCA, and how the restriction against copying complex wh-phrases can be accounted for. Next, in section 4.4, I demonstrate how the analysis proposed here accounts for the crosslinguistic distribution of long-distance versus medial wh-movement constructions. Finally, section 4.5 gives a short summary.

#### 4.1. *Partial wh-movement as an indirect dependency*

As I pointed out in section 2, there has been an ongoing discussion as to which approach to partial wh-movement is the correct one. Here, I adopt an Indirect Dependency Approach to partial wh-movement. There are two main reasons why I believe an Indirect Dependency Approach is essentially correct. The first argument has to do with the types of matrix predicates that are allowed in partial wh-movement constructions. It is well-known that the set of matrix predicates in partial wh-movement constructions and long-distance wh-movement constructions are not the same. Of particular interest is the fact that matrix predicates that do not tolerate an extra nominal argument are not allowed in partial wh-movement constructions. That is, complex object-verb predicates such as ‘have an idea’ or ‘have the feeling’ are out in partial wh-movement constructions (cf. Reis, 2000: 380). Furthermore, predicates that may only combine with a sentential (but not a pronominal) object are out as well (cf. Reis, 2000 for German and Den Dikken, 2009 for Hungarian). This follows naturally from the assumption that *was* actually derives from an object position in the matrix clause. A second argument in favor of the Indirect Dependency Approach has to do with the crosslinguistic distribution of long-distance wh-movement versus partial wh-movement. It appears that these constructions are often in complementary distribution (cf. Stepanov & Stateva, 2006). This is something which is unexpected under the Direct Dependency Approach. In this approach, partial wh-movement is contingent upon long-distance wh-movement, hence there is no reason why a language that allows long-distance wh-movement would not allow partial wh-movement and vice versa. Under an Indirect Dependency Approach, however, this follows much more straightforwardly: partial wh-movement is fundamentally different from long-distance wh-movement; hence we do not expect these constructions to co-occur in a language (while also not excluding that they might, viz. in German).

Over the years, various types of Indirect Dependency Analyses have been proposed. In Dayal’s original approach (cf. Dayal 1994, 2000), the embedded clause is analyzed as a question in its own right, which is only semantically linked to the subordinate clause. More recently, however, it has been proposed to analyze the embedded clause as a type of free relative (cf. Felser, 2001; Den Dikken, 2009 and Koster, 2009). I will here shortly sketch Felser’s analysis. Felser proposes that the embedded clause is a predicate, of which the scope marker is the semantic subject. The scope marker is assumed to derive from a low position within the matrix clause (i.e. SpecVP), and moves to the matrix SpecCP in languages that require wh-movement. The wh-phrase in the embedded clause also moves to its own local SpecCP, but not to check a Q-feature, but rather to check a relative/operator like feature.<sup>9</sup> A structural sketch of this analysis is given in (12) below:

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<sup>9</sup> This may seem unlikely given that the intermediate copy surfaces as a wh-phrase. However, Felser suggests this is due to a form of interrogative concord, resulting in spelling out the operator phrase as a wh-form.

(12) [<sub>CP1</sub> What<sub>i</sub> think<sub>j</sub> you [<sub>VP</sub> t<sub>i</sub> t<sub>j</sub> [<sub>CP2</sub> who<sub>k</sub> Mary kissed t<sub>k</sub> has]]]]

One of the main arguments in favor of such an analysis is that it readily explains why partial wh-movement constructions are not interpreted as involving more than one question (cf. Felser, 2001). Second, it also explains the subcategorization properties of the matrix verbs attested in partial wh-movement constructions. That is, partial wh-movement is only possible with matrix verbs that combine with a declarative complement, and not an interrogative. This follows naturally under the assumption that the embedded clause is a predicate, not an interrogative. It goes beyond the scope of this paper to address all the points that speak in favor of a particular analysis. For further details, I would like to address the reader to the literature mentioned earlier, specifically Lutz et al. (2000) and Felser (2001).

Of particular relevance to the current discussion is the fact that the Indirect Dependency Approach readily explains why wh-phrases are spelled out in intermediate SpecCPs, and in these positions only. Namely, under the Indirect Dependency Approach, intermediate SpecCPs are terminal landing sites for the embedded wh-phrase. In other words; they constitute the head of a chain. Hence, a wh-phrase can, and in fact must be spelled out in this position, and in this position only.

The next question is whether we can extend this analysis to the wh-copy construction. This is indeed what is proposed in Koster (2009). He argues that the wh-copy construction is an indirect dependency, too, and subsequently analyzes the wh-copy construction as a type of free relative clause as well, whereby the highest wh-phrase is the (questioned) focus of a cleft sentence, and the lower wh-phrase simply a relative pronoun introducing the relative clause. I am not convinced though that this is the right way to go, since the wh-copy construction generally patterns with long-distance wh-movement, and not with partial wh-movement. Most importantly, it appears that wh-copying patterns with long-distance wh-movement in terms of the predicate restrictions mentioned earlier. That is, unlike partial wh-movement constructions, the wh-copy construction is possible with complex object-verb predicates and predicates that do not allow pronominal objects, like ‘es scheint’ / ‘es heist’ (it seems, it is said) (cf. Reis 2000 and Felser 2004). However, if the wh-copy construction is essentially an indirect dependency, with the highest wh-phrase deriving from an object position in the matrix clause, we would expect it to pattern with the partial wh-movement construction in this respect. Since it does not, I believe the wh-copy construction should be analyzed as a direct dependency, that is, as involving long-distance wh-movement. This also happens to be the leading view on this construction.

In sum, an Indirect Dependency Approach to partial wh-movement readily explains why in this construction intermediate wh-phrases are spelled out in SpecCP positions and in these positions only. However, this leaves open the question of what licenses intermediate spell-out in the wh-copy constructions. I treat this issue in more detail in the next subsection.

#### 4.2. Intermediate SpecCPs as heads of chains

In the previous section, I argued that the wh-copy construction should not be analyzed as a type of indirect dependency, mainly because there is no reason to assume that the highest wh-phrase in this construction derives from an object position, and because the construction behaves by all means and purposes like a long-distance movement construction (cf. Rett, 2006). But if the wh-copy construction is essentially a long-distance movement construction, then why is the intermediate copy spelled out?

Here I would like to follow (in part) a line of reasoning that is put forward in Stepanov & Stateva (2006). They propose that partial *wh*-movement and long-distance *wh*-movement constructions are derivationally related in that in both cases, a scope marker is present and movement to the intermediate SpecCP is triggered irrespective of the feature checking requirements of higher positions. While I essentially adopt this latter assumption, I do not adopt the first. Stepanov & Stateva assume that the scope marker in long-distance movement constructions is silent, and that because of this, it may incorporate into the matrix verb. This allegedly opens up the way for the lowest *wh*-phrase to move all the way up to the matrix SpecCP. The difference between languages that have partial *wh*-movement and those that lack it boils down to a lexical issue under this analysis: it depends on whether the language in question has an overt or a silent scope marker. There are various problems, however, with the assumption that the matrix verb in long-distance *wh*-movement constructions has incorporated the scope marker. First of all, it requires the postulation of a silent scope marker and second, the assumption that this scope marker is able to incorporate into the matrix verb. A further assumption that has to be made is that there is some kind of tree pruning mechanism at work that alleviates the complex NP configuration merger of the scope marker has induced. The difficulty is that it is hard to verify whether these operations have really taken place, since they all take place covertly. Another problematic issue is that Stepanov & Stateva attempt to equate the class of bridge predicates with the class of predicates that allow incorporation. They argue that canonical non-bridge predicates of the whisper/shout type do not allow incorporation because they are derived from nominals, and for this reason do not allow affixation c.q. incorporation. However, the etymology of these manner-of-speaking verbs (at least for Germanic) suggests otherwise, namely that the nominal counterparts of these verbs are derived from the verbal forms, not the other way around (cf. Klein 1966). A final problem with Stepanov & Stateva's analysis is that they assume the embedded clause in long-distance movement constructions is interrogative, which raises the question of why long-distance movement constructions (and *wh*-copy constructions) may not have a matrix predicate that selects for an interrogative complement (i.e. verbs of the ask/wonder type).<sup>10</sup>

In sum, I do not believe that Stepanov & Stateva's analysis in terms of the overtness/incorporation of the scope marker makes the right cut. Rather, I believe the relevant difference between direct dependencies (long-distance *wh*-movement and *wh*-copying) and indirect dependencies (partial *wh*-movement) is determined by whether the matrix predicate allows for an extra argument (i.e. the scope maker). If it does, partial *wh*-movement is possible, if not, long-distance movement of the lower *wh*-phrase results.

However, while I do not adopt the assumption that both partial *wh*-movement and long-distance *wh*-movement constructions are scope marking constructions, I do follow Stepanov & Stateva (2006) in assuming that movement to the intermediate SpecCP in all cases is triggered irrespective of successive-cyclicity.<sup>11</sup> That is, I assume every intermediate CP in long-distance *wh*-dependencies has a feature checking requirement of its own which triggers movement, rather than assuming it is an optionally assigned edge feature or some other derivational rule that forces these intermediate movement steps. But instead of assuming the relevant feature is a Q-feature (as Stepanov & Stateva propose), I assume it is essentially a non-interrogative feature, specifically an operator (OP) feature. In effect, I propose that it is

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<sup>10</sup> This problem was pointed out to me by Dafina Ratiu.

<sup>11</sup> By this I mean: triggered irrespective of some derivational rule whose only job it is to derive successive-cyclicity. Technically speaking, under the current analysis, the *wh*-phrase still moves successive-cyclically through CP edges.

not just partial wh-movement constructions that have an operator feature in the embedded SpecCP, but also long-distance wh-movement constructions. Furthermore, I propose it is exactly this feature checking requirement which gives rise to the wh-copy construction.<sup>12</sup>

Hence, I depart here from the general conception of long-distance movement. That is, movement to intermediate positions in long-distance movement constructions has always been analyzed as being solely contingent upon movement to a higher position, e.g. by assuming chain formation must take place in a strictly local manner (Form Chain, Chomsky 1993), or by assuming intermediate landing sites may have optional edge features assigned to them (cf. Chomsky 2000, 2001). Instead, I propose that movement to intermediate CPs is triggered independently of requirements of positions higher up the tree, because intermediate CPs have a feature checking requirement of their own. Moreover, I argue that intermediate SpecCPs are potential terminal landing sites and in this capacity constitute the head of a chain. I regard long-distance movement as such to be simply the result of a wh-phrase being able to enter into multiple feature checking relations. In essence, what I thus propose is that long-distance movement constructions are built up through of a succession of local movement chains, instead of consisting of one (long) movement chain with intermediate stops. This may just seem like playing words, but as will become clear from the remainder of this discussion, the idea that long-distance movement constructions consist of a succession of movement chains instead of consisting of one single chain plays a paramount role in the explanation of the wh-copy phenomenon. Namely, if intermediate SpecCPs in long-distance movement constructions are potential terminal landing sites, and form the head of a chain, it follows that a wh-phrase may get spelled out in this position, since heads of a chain are usually spelled out, and tails deleted. If, on the other hand, intermediate SpecCPs are essentially non-terminal landing sites, as the traditional analyses of long-distance movement hold, the spell -out of an intermediate wh-phrase would by no means be licensed.

Now, as mentioned before, in case of partial wh-movement, intermediate SpecCPs are unambiguously the head of a chain under an Indirect Dependency Approach: movement in fact stops at the intermediate SpecCP, and hence the wh-phrase must be spelled out there. From this, it follows naturally that partial wh-movement is never allowed without spelling out the wh-phrase in the intermediate SpecCP. This is indeed the case.<sup>13</sup> In case of long-distance wh-movement, the picture is different. In that case, intermediate SpecCPs are not the final landing site of the wh-phrase, since it has to move further up. Contrary to the partial wh-movement construction, there is no checker available in the matrix clause itself. Hence, the only element that can fulfill the requirements of the matrix interrogative CP is the wh-phrase in the embedded clause. Therefore, the wh-phrase in the embedded SpecCP enters into a

<sup>12</sup> The current analysis is thus very much in accordance with Felser (2004), who argues that the intermediate wh-phrase in wh-copy constructions is the spelled-out operator part of the wh-phrase, while the highest copy constitutes the interrogative part of the wh-phrase.

<sup>13</sup> It appears that some speakers of German allow the intermediate SpecCP to not be overtly filled by a wh-phrase in case of more than one embedding (although there is some disagreement as to whether this is really grammatical (cf. Müller 1997: 258 fn.8; Beck & Berman 2000: 20). An example of this is in (i) below:

(i) [<sub>CP1</sub> Was meint Hans [<sub>CP2</sub> dass Peter glaubt [<sub>CP3</sub> wen Marie geküsst hat?]]

What thinks Hans that Peter believes who Marie kissed has?

‘Who does Hans think Peter believes kissed Mary?’

Müller (1997) suggests such constructions may involve a combination of partial wh-movement of the lower wh-phrase and long-distance wh-movement of *was* ‘what’. This is not so surprising since German allows both partial wh-movement and long-distance wh-movement to some degree. Hindi, on the other hand, does not allow long-distance movement at all. Consequently, every SpecCP between the scope marker and the true wh-phrase must be filled by a scope marker.

second feature checking operation with the matrix CP. Intermediate SpecCPs in long-distance wh-movement constructions may thus be viewed as being both the head of one chain, and the tail of the next. Because of this, there are two conflicting requirements: deletion of the tail of one chain also results in deletion of the head of another chain. If the requirement to delete the tail of a chain is met, long-distance wh-movement is the result. If, on the other hand, the requirement to retain the head of a chain is fulfilled, wh-copying ensues. Hence, there is a certain degree of optionality regarding the spell-out of intermediate copies in SpecCP. Examples (13) – (15) (repeated from examples (1) to (3)) illustrate this principle for partial wh-movement, wh-copying and long-distance wh-movement, respectively.

(13) [<sub>CP1</sub> Was meinst du [<sub>VP</sub> was V [<sub>CP2</sub> wen Maria wen geküsst hat]]]

(14) [<sub>CP1</sub> Wen meinst du [<sub>CP2</sub> wen Marie wen geküsst hat]]]

(15) [<sub>CP1</sub> Wen meinst du [<sub>CP2</sub> wen Marie wen geküsst hat]]]

I return to the issue of what allows multiple copy spell-out in long-distance movement contexts in more detail in section 4.4. But let's first see how the current analysis may solve the problems addressed in section 3. The first problem concerned the fact that intermediate wh-phrases in medial wh-movement constructions appear in positions where they do not seem licensed. Under the current account, this problem is solved since it is assumed that OP-feature checking takes place in every intermediate CP, which licenses a wh-phrase in these positions.

The second problem that was discussed in section 3 concerned the fact that wh-phrases in medial wh-movement constructions may only surface in SpecCP positions, and not in base position or other intermediate landing sites (i.e. vP). Under the current analysis, this follows naturally from the fact that these positions are never the head of a chain and as such, may never be spelled out (but see note 12 for an exception).

The third and the fourth problem that were discussed in section 3 concerned the fact that wh-copies appear to violate the LCA, and that complex wh-phrases are not allowed to copy. Since these issues are related, I address them in more detail in section 4.3 below. The final problem addressed in section 3 was the issue of what prevents multiple copy spell-out, or, to put it differently: what the source of parametric variation in medial wh-movement constructions is. This problem is discussed in full detail in section 4.4, where the cross-linguistic distribution of long-distance wh-movement, partial wh-movement and wh-copying is treated.

#### 4.3. Wh-copying and the Linear Correspondence Axiom

As I mentioned in section 3.3, wh-copy constructions appear to violate the LCA. This may be circumvented by assuming the LCA operates on each CP level separately, a la Felser (2004), or by assuming that copies of the wh-phrase undergo fusion with the embedded CP, rendering them distinct from the higher copy, along the lines of Nunes (1999, 2004). However, as I already pointed out in section 3.3, both types of analyses are problematic. Felser's analysis is problematic since it requires two separate spell-out mechanisms, and Nunes' analysis fails to explain satisfactorily the circumstances under which fusion is possible.



Note that the analysis of wh-copying presented here is in fact able to account for the fact that wh-copy constructions do not violate the LCA. Recall that the main problem these constructions pose is the fact that copies of the wh-phrase are non-distinct, and as such cause a failure to linearize the structure. However, under the current analysis, multiple (overt) copies of the wh-phrase are in fact distinct: each copy represents the head of a different movement chain. As such, they are distinct and form no problem for the LCA.<sup>14</sup>

However, this leaves open the question of what prevents complex wh-phrases from being copied. Remember that under Nunes analysis, this is explained by assuming these wh-phrases cannot undergo fusion and hence remain non-distinct from the other copies. But since the current proposal does not analyze wh-copy constructions in terms of fusion, it is not clear why complex wh-phrases may not be copied. In fact, there is nothing about the current analysis which would circumvent this. This means we need an independent explanation for the ban on copying complex wh-phrases.

First of all, it is important to note that complex wh-phrases have a deviant behavior in other contexts as well. For one, it is well known that complex wh-phrases of the wh-NP type are not sensitive to superiority (cf. Pesetsky, 1987). But they differ in a range of other respects from pronominal wh-phrases as well, as pointed out in Van Craenenbroeck (to appear). This suggests that the reason why these phrases cannot be copied is not simply due to the inability of these wh-phrases to undergo fusion with C, as Nunes suggests. Rather, it is more likely that it has to do with the semantics of complex versus simple wh-phrases. This is what is proposed in Rett (2006) and Van Craenenbroeck (to appear).

Van Craenenbroeck argues that complex wh-phrases (modifying an NP) can be distinguished from simple wh-phrases (e.g. pronominal ones) in that the latter are operators, while the first are not. He argues that because complex wh-phrases are not operators themselves, they are base-generated in the position where they overtly surface. From this, it should follow that complex wh-phrases cannot be copied: if they don't move, they cannot leave copies behind.

Rett (2006) also subscribes the aberrant behavior of complex wh-phrases to semantic differences between complex and simple wh-phrases. In particular, she suggests that wh-phrases without an NP complement can be copied because they are non-quantificational (introducing only a free variable into the derivation), while wh-phrases with an NP complement cannot be copied because they are quantificational, and interpreting them twice leads to vacuous quantification.

Of interest to the current discussion is the fact that Rett points out that whenever an intermediate wh-copy is spelled out, it is interpreted in the same way as the head of a chain, while if it is deleted, it is interpreted as the tail of a chain. This is entirely in line with what I

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<sup>14</sup> Mark de Vries has pointed out to me that this would only follow under the assumption that the LCA is able to differentiate between identical elements based on chain membership, which indeed seems to be the case, cf. Nunes, 2004:22. An immediate question that arises is why the head of the A-chain is not spelled out (i.e. case checking positions for argument wh-phrases). I do not have a definitive answer to this, but as I will argue below, only the final landing site of long-distance wh-movement constructions (i.e. matrix SpecCP) is unambiguously the head of a chain, while intermediate positions may be ambiguous between being the head and the tail of a chain. This also holds for the head of A-chain in wh-movement construction: it is simultaneously the head of the A-chain and the tail of the A'-chain. Hence, even if spell-out is possible in this position, it should be optional, not obligatory. Interestingly, from this, it follows that wh-phrases in multiple questions do in fact get spelled out in A-position. Namely, in this case, wh-phrases are unambiguously in head position, because the lower wh-phrase in multiple questions does not A'-move to SpecCP (at least not in non-multiple fronting languages like German).

proposed earlier: intermediate SpecCPs are ambiguous between being the head of a chain and the tail of a chain. If the intermediate SpecCP is interpreted as the head of a chain, the wh-phrase is spelled out, if it is considered the tail of a chain, it is not. I am therefore inclined to adopt Rett's analysis in this matter.

Either way, it is clear that the fact that complex wh-phrases cannot copy has to do with their semantics, rather than with the structural complexity of the wh-phrase. Both Van Craenenbroeck and Rett are successful in making the right distinction between wh-phrases that may be copied, and those that cannot, while Nunes' distinction (i.e. the one between heads and phrases) does not.<sup>15</sup>

#### 4.4. Crosslinguistic variation in medial wh-movement constructions

In section 3.5, I pointed out that many analyses of partial wh-movement and wh-copying fail to explain which parametric property makes partial wh-movement and wh-copying possible. This is specifically true for Direct Dependency Approaches, which view partial wh-movement (and wh-copying) as surface alternatives to long-distance wh-movement. Such analyses generally fall short of explaining why long-distance wh-dependencies are sometimes formed by means of a scope marker or wh-copy, and in other cases by means of 'standard' long-distance wh-movement. In fact, most Direct Dependency Approaches suggest that partial wh-movement may generally surface in long-distance wh-movement languages, since partial wh-movement is contingent upon long-distance wh-movement. The same is true for wh-copying: because it is generally assumed that wh-copying is a surface alternative to long-distance wh-movement, it should be possible to employ wh-copying in long-distance wh-movement languages in general.

In section 3.5, I already pointed out that Indirect Dependency Approaches, contrary to Direct Dependency Approaches, do not have trouble in explaining why partial wh-movement and long-distance wh-movement constructions do not generally coexist in a language, since they are fundamentally different structures. This is also corroborated by the cross-linguistic distribution of long-distance wh-movement versus partial wh-movement constructions. Namely, these constructions are usually in complementary distribution: languages that employ long-distance wh-movement generally do not allow partial wh-movement, and vice versa. An apparent exception to this pattern is formed by German and Hungarian, which allow both partial wh-movement and long-distance wh-movement. However, the availability of long-distance wh-movement and partial wh-movement in German and Hungarian appears to be tied to particular dialects: while all speakers accept partial wh-movement, only some also accept long-distance wh-movement (cf. Reis 2000; Fanselow et al. 2005; Stepanov & Stateva 2006). Furthermore, in German, the availability of long-distance wh-movement versus partial wh-movement appears to have a historical dimension: long-distance wh-movement constructions have been rapidly declining from the 18<sup>th</sup> century onward (cf. Behaghel 1928; Andersson & Kvam 1984). Instead, alternatives such as partial wh-movement appear to be used and are preferred by many speakers over long-distance wh-movement. Therefore, it is not so surprising that German still allows both long-distance wh-movement and partial wh-movement to some degree, since historical changes are never abrupt, and usually preceded by a period in which two systems coexist.

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<sup>15</sup> This does not mean that Nunes' analysis in terms of fusion may not be the correct analysis for other copying phenomena. However, it is questionable whether his analysis can be extended to account for all types of copying; specially, the question is whether it can satisfactorily account for wh-copying.

Interestingly, the pattern for the *wh*-copy construction is different. I argued that *wh*-copying, unlike partial *wh*-movement, is actually a direct dependency, i.e. a surface alternative to long-distance movement. Hence, we expect this construction to show up in languages that allow long-distance movement, but not in languages that don't (i.e. languages that only have partial *wh*-movement). This indeed seems to be the case. Languages that do not allow long-distance *wh*-movement (e.g. Hindi, Russian) also do not allow *wh*-copying. In fact, all the languages for which it has been argued that they employ *wh*-copying, employ long-distance *wh*-movement as well. This corroborates the hypothesis that *wh*-copying is a surface alternative of long-distance *wh*-movement, and not of partial *wh*-movement.

One interesting question is whether there are any languages that solely use *wh*-copying. As far as I can tell, there is no such language: all the languages that employ *wh*-copying also employ long-distance *wh*-movement. This suggests that *wh*-copying is a secondary strategy to long-distance *wh*-movement.

Such a hypothesis is in fact corroborated by language acquisition data and grammaticality judgment data. It is well-known that *wh*-copying shows up in children's speech in a variety of languages that do not have this construction in the adult grammar. These languages include Dutch (van Kampen 1997; Strik & Jacobowicz 2008; Strik 2009), French (Oiry 2002; Oiry & Demirdache 2006; Strik & Jacobowicz 2008; Strik 2009), Spanish (Gutierrez 2006) and Basque (Gutierrez 2004). Furthermore, a number of studies have shown that *wh*-copy constructions show up in second language acquisition data as well. This has been reported for Japanese speakers of English (cf. Okawara 2000; Wakabayashi & Okawara 2003; Yamane 2003; Schulz 2006), for Spanish and Basque bilinguals learning English (Gutierrez 2005) and for French speakers of English (Slavkov 2007). In all these cases, *wh*-copy constructions were unavailable in both the first and the second language. Nonetheless, the constructions did surface in the L2 of the speakers under consideration.<sup>16</sup>

Under the current analysis, it is not surprising that *wh*-copying shows up in these populations in particular. As I have argued, the dual nature of intermediate SpecCPs makes spell-out of intermediate *wh*-copies possible. However, while it is possible to spell out an intermediate copy, it is not necessary, since intermediate copies generally have no effect on the interpretation. This may explain why adult and L1 speakers lean towards not spelling out the intermediate copy in the languages under consideration, and also, why *wh*-copying does not generally show up in long-distance *wh*-movement languages.

Another argument in favor of the hypothesis that *wh*-copying is a secondary strategy to form a long-distance *wh*-dependency comes from Dutch grammaticality judgment data (Schippers 2009; Strik 2009). Schippers conducted a magnitude estimation task where subjects were asked to rate long-distance *wh*-movement constructions, partial *wh*-movement constructions and *wh*-copy constructions relatively to each other. The results showed that long-distance *wh*-movement was rated the highest, followed by *wh*-copying and finally partial *wh*-movement. It turned out that the differences in ratings between these three types of constructions were all significant.

These findings were mirrored in Strik (2008). In this study, subjects were asked to judge whether they considered long-distance *wh*-movement, partial *wh*-movement and *wh*-copy constructions grammatical. If they answered yes, they were asked to rate the sentence from 1 – 5, 1 indicating the sentence was very uncommon to them, 5 that it was very common. It turned out that subjects most frequently accepted long-distance *wh*-movement sentences,

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<sup>16</sup> The acquisition data discussed here also shows that children and second language speakers produce partial *wh*-movement questions, and that this construction was also unavailable in both the L1 and the target language.

followed by wh-copy sentences and finally partial wh-movement sentences. This pattern was also reflected by the ratings subjects gave to the constructions under consideration: long-distance wh-movement constructions were rated the highest, followed by wh-copy constructions, while partial wh-movement constructions were rated the lowest.

These findings are in accordance with the hypotheses entertained in this article: Dutch is a long-distance wh-movement language, and as such, it is predicted to allow wh-copying, but not (necessarily) partial wh-movement. The grammaticality judgment data from Schippers and Strik confirm this hypothesis to a large extent. While Strik's and Schippers' studies do not prove that partial wh-movement is impossible in Dutch, they do show that partial wh-movement is marked in this language. The fact that some speakers do accept partial wh-movement suggests that Dutch is also a mixed language, allowing both partial and long-distance wh-movement to some degree. In that respect, Dutch is the mirror image of languages like Hungarian and German, where partial wh-movement is widely accepted, in contrast to long-distance wh-movement, which is accepted on a much smaller scale.

Interestingly, the data in Schippers and Strik show that wh-copying was judged much more favorable than partial wh-movement. However, overall the construction was still judged less acceptable than long-distance wh-movement. This actually follows quite naturally under the assumption that wh-copying is a secondary strategy to long-distance wh-movement.<sup>17</sup>

### 5. Conclusion

I have argued that the spelling out of intermediate copies in partial wh-movement and wh-copy constructions is due to the fact that the SpecCPs in these constructions are heads of a chain. In partial wh-movement constructions, the wh-phrase must therefore be spelled out in intermediate SpecCPs, because these positions are unambiguously the head of a chain. In case of long-distance wh-movement, the head of a chain falls together with the tail of the chain. Hence, a wh-phrase may but must not be spelled out there. If the intermediate wh-phrase is spelled out, satisfying the requirement to retain the head of a chain, wh-copying is the result. However, it is also possible to regard the intermediate copy as the tail of a chain, resulting in subsequent deletion of the intermediate wh-phrase. In that case, only the highest copy is retained, which is the only copy that is unambiguously the head of a chain.

I have argued that this analysis provides a solution for a number of problematic issues regarding medial wh-movement constructions. First of all, under the assumption that intermediate CPs attract wh-phrase for their own independent requirements (i.e. OP-feature checking), we have a possible explanation for the fact that wh-phrases may show up there. This is not only a welcome result for the analysis of medial wh-movement constructions in particular, but also for the analysis of long-distance wh-movement in general. While it has always been clear that long-distance movement must proceed in strictly local steps, it has been less clear how these intermediate steps are triggered. The current analysis actually gives a possible solution to this puzzle, by analyzing long-distance wh-movement as derivationally

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<sup>17</sup> It would be interesting to see to what degree wh-copying is judged less acceptable than long-distance wh-movement in other wh-copy languages as well. At the moment, I do not have much information on this. Pankau (to appear) notes that wh-copying is optional for some speakers, in accordance with the claims put forward here. Furthermore, Bruening (2006) gives some anecdotal evidence for the secondary status of wh-copying in Passamaquoddy: one of two informants he consulted rejected wh-copying, while the other indicated this construction was indeed used by some speakers. This indicates that wh-copying is not a primary strategy for forming long-distance wh-dependencies in Passamaquoddy as well.

related to partial wh-movement in that movement to intermediate SpecCPs is triggered independently of requirements of higher CPs in both types of constructions.

Since the current analysis only views intermediate landing sites in SpecCP as the head of a chain, it was possible to explain why this is also the only position in which medial wh-phrases may get spelled out. Furthermore, the analysis also gives an explanation for the fact that wh-copy constructions do not violate the LCA. Since the spelled-out copies are the heads of different movement chains, the LCA is able to differentiate between them.

Finally, I pointed out how the crosslinguistic distribution of partial wh-movement and wh-copying follows from the current analysis. Partial wh-movement and long-distance wh-movement are predicted not to co-occur under an Indirect Dependency Approach. Since wh-copying is a surface alternative to long-distance wh-movement, it follows that partial wh-movement and wh-copying should also not co-occur. This hypothesis indeed seems to hold to in general. Moreover, empirical data in fact suggests that wh-copying is a secondary strategy to form a long-distance wh-dependency. This, I argued, might be due to the fact that long-distance wh-movement constructions induce a spell-out conflict: deletion of the tail of a chain simultaneously results in deletion of the head of a chain. The safest option may therefore be not to delete anything, resulting in wh-copying.

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## Root vs. *n*

A study of Japanese light verb construction and its implications for nominal architecture

Mina Sugimura

Noun Incorporation (NI) in Japanese has been treated as an instance of head movement in the previous literature. In this paper, I argue that NI is not in fact an instance of syntactic movement, but rather, it is a direct *Merge* of a root (Pesetsky 1995) and the light verb. I claim that NI and its non-NI counterpart have quite different structures: in NI, a root merging with the light verb *su* ‘do’ constitutes a verb as a whole, whereas in non-NI, a root first merges with an *n* and then the root-*n* complex is combined with the light verb. This structural difference brings about quite interesting consequences both from syntactic and phonological point of views.

### 1. Facts: noun incorporation (NI) in Japanese

It has been assumed that in Japanese, the nominal structures for noun-incorporation (NI) (1a) and for non-NI (1b) are underlyingly the same (Kageyama 1982, Tsujimura 1990, Terada 1990 among others):

- (1) a. Matto-ga (Nihongo-o) benkyoo-sita. (NI)  
Matt-NOM (Japanese-ACC) study-did  
b. Matto-ga (Nihongo-no) benkyoo-o sita. (Non-NI)  
Matt-NOM (Japanese-GEN) study-ACC did  
‘Matt studied (Japanese).’

In (1), the *sino*-Japanese nominal *benkyoo* ‘study’ appears with the light verb *sita* (*su* + past tense *ta*) in either an incorporated form (1a) or a non-incorporated form (1b):

- (2) a. Matto-ga [VP [NP t<sub>i</sub>] benkyoo<sub>i</sub>-sita] (NI)  
b. Matto-ga [VP [NP benkyoo-o] sita] (Non-NI)

Thus, *benkyoo* ‘study’ in its NI form is underlyingly an N under the general assumption, and incorporation seems quite optional. In fact, other nominals such as *ryokoo* ‘travel’, *kekkon* ‘marriage’, and *kaiwa* ‘conversation’ pattern the same way, and can appear either in the NI forms or non-NI forms.

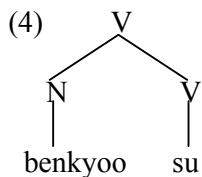


Throughout the discussions that follow, I will set aside the case with meaningful ‘*su*’, the heavy verb, as in (3):

- (3) Émile-ga shukudai-o sita.  
 Émile-NOM homework-ACC did  
 ‘Émile did homework.’

What has been agreed upon among linguists about the constructions like (1a) and (1b) is: (i) that the verb *su* is ‘void’ of meaning; and (ii) that it is the noun that bears  $\Theta$ -roles (Grimshaw & Mester 1988; Saito & Hoshi 2000). Thus, in (1) *Matto* is the argument of the noun *benkyoo* ‘study’, and is not the argument of *sita* ‘did’.

However, it has been controversial whether the incorporation is syntactic (Kageyama 1982; Terada 1990; Tsujimura 1990) or lexical (Miyagawa 1987, 1989; Grimshaw & Mester 1988). Grimshaw & Mester (1988), for example, claim that it is lexical, and assume that a noun yields all its  $\Theta$ -role-assigning capacities to *su* in the lexicon. On this view, NI and non-NI are not underlyingly the same, and NI constitutes a category of a verb in the lexicon. Miyagawa (1987, 1989) posits the following structure:



In (4), *su* is the head of a word, and its verbal feature percolates up to the upper node, creating a single verb. The argument structure of this complex predicate then projects the argument structure of the noun, just in the same way that *su* in the light verb constructions reflects it. On this view, *benkyoo* ‘study’ is not derived from an NP structure, and it is part of the verb from the beginning.

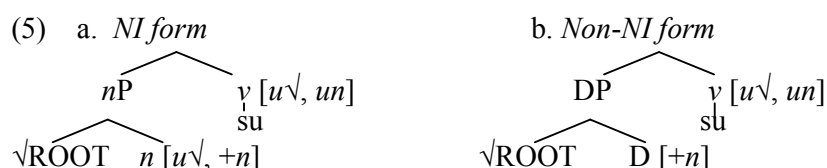
The goal of this paper is to provide a hybrid account of the syntactic and lexical analyses. I will argue that NI in Japanese is in fact not an instance of incorporation, but is simply an instance of *Merge* of a category-less element called ‘root’ (Pesetsky 1995) and the light verb *su*. Thus, on one hand, I argue for the lexical analysis since I do not assume any sort of movement for NI, and I will also do not assume that an NI-form starts off in the same structure as its non-NI counterpart. But on the other hand, I argue for the syntactic analysis in that I assume that the direct *Merge* is implemented in the syntax, and there is no operation involved in the lexicon. This is what I describe as a hybrid account.

The organization of this paper is as follows: In section 2, I will briefly introduce my previous account (Sugimura 2008) on NI that is based on syntactic *root-movement* (Johns 2007). I will then raise some empirical issues with stranded modifiers of incorporated nominals that are incompatible with my previous analysis. Since Sugimura (2008) assumes that NI originally constitutes an NP, it predicts that the NP is able to host noun-modifiers such as adjectives or numeral quantifiers, which is in fact not the case. As a solution to this problem, in section 3, I will alternatively seek a new account that has advantages of both syntactic and lexical sides in the previous literature. I will abandon lexical operations, but at the same time maintain the idea that NI does not involve a nominal head at any point of derivations. More specifically, NI is not derived from movement but is rather simply created

by merge of a root and a category-defining head *v* that contains the light verb *su*. However, instead of the *v* containing  $\Theta$ -grids, I will assume that roots have their own  $\Theta$ -grids and that their  $\Theta$ -roles are discharged by functional category heads such as *n* or *v*. I will then discuss what consequences this assumption brings about both from syntactic and phonological point of views. Section 4 concludes this paper.

2. Issues: stranded modifiers of incorporated nouns

In Sugimura (2008), I argued that NI and non-NI both involve nominalizing syntactic heads, an *n* and a D, respectively, and that they have a different degree of opacity. The posited structures are as follows:



I argued that what triggers incorporation was the uninterpretable root feature [u√] (Johns 2007) on *n* and *v*. I attribute this to the fact that only nominals undergo incorporation in Japanese: thus, the light verb only attracts a root-*n* complex, but not a root-*adj* complex, for example. Along the line of Distributed Morphology framework (Halle & Marantz 1993), I assume that all the words start off as category-neutral elements called *roots* √ that are assigned categories by category-defining elements like *n*, *a*, *v*. Assuming that D can also put roots into a nominal context (Marantz 1997), I argued that *benkyoo* ‘study’ can be realized either as a DP or an *nP*. But when the root appears as a DP, the incorporation is blocked due to the phasal status of DPs (Svenonius 2004). The uninterpretable features on the *v* are thus checked via *AGREE* instead of *MOVE* (Chomsky 2000, 2001), under the assumption that only *MOVE*, not *AGREE* is subject to Chomsky’s Phase Impenetrability Condition (PIC) (Bošković 2007).

However, the fact that only a non-NI form can co-occur with noun-modifying elements like (complex) numeral classifiers, or adjectives (Poser 1990) suggests that an NI form does not seem to be derived either from an NP or from a DP:

- (6) a. \* Nino-ga hutu-ka-bun-no syokuji-sita.  
       Nino-NOM two-days-amount-GEN dining-did  
       b. Nino-ga hutu-ka-bun-no syokuji-o sita  
       Nino-NOM two-days-amount-GEN dining-ACC did  
       ‘Nino had two days worth of meals.’
- (7) a. \* Nino-ga kantanna syokuji-sita  
       Nino-NOM simple dining-did  
       b. Nino-ga kantanna syokuji-o sita  
       Nino-NOM simple dining-ACC did  
       ‘Nino had a simple meal’

Sugimura's (2008) DP/*n*P structural analysis can arguably accommodate the fact in (6) by assuming that genitive Case in Japanese is assigned by a D head (Ochi 2005), which renders a non-NI form constituting an *n*P incompatible with genitive elements. However, there is no concrete evidence that a D head assigns genitive Case in Japanese (Bošković 2008); and in fact, the genitive marker *no* could be a dissociated morpheme (Embick 1997) that is post-syntactically inserted, as Watanabe (2007) suggests. Moreover, the DP/*n*P distinction cannot explain why an *n*P cannot host an adjective in (7). If an NI form is derived from an *n*P, it should be able to host these noun-modifying elements in (6) and (7).

In fact, in other NI languages like Mohawk, the NI form can host these elements (Baker 1996):<sup>1</sup>

- (8) a. **Akwe'ku**  $\Lambda$ -ye-**nakt**-a-n'uhwe'-ne'.  
 All FUT-FsS-bed- $\emptyset$ -like-PUNC  
 'She will like **all the beds**'
- b. **Aséhtsi**  $\Lambda$ -ye-**nakt**-a-n'uhwe'-ne'.  
 new FUT-FsS-bed- $\emptyset$ -like-PUNC  
 'She will like the **new bed**'

Thus, the data in (8) seem to suggest that unlike Mohawk, in Japanese there is no N-head involved in NI. In the following section, I propose alternative structures for NI and non-NI.

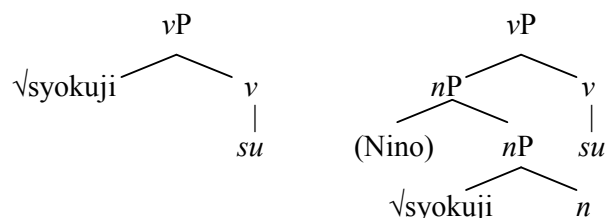
### 3. Noun-Incorporation as Root-*v* Merger

I do not posit a similar structure for NI and non-NI configurations. Instead, I propose that NI in Japanese is not an instance of incorporation, but is rather an instance of direct Merge of a category-less element *root* (Pesetsky 1995) and the light verb. In contrast, a non-NI form consists of a root and at least a category-defining head *n* (Halle & Marantz 1993) that necessarily discharges the external argument of the root, which eventually moves to a (sentential) subject position:

<sup>1</sup> Interestingly, however, another NI language like Mapudungun behaves the same way as Japanese in this respect. The grammatical contrast between a Mohawk example in (ia) and a Mapudungun example in (ib) shows that Mapudungun does not allow N-modifying elements such as a demonstrative (Baker et al. 2004):

- (i) a. Wa'-k-**ather**-a-hninu-' **thikv**  
 FACT-1sS-basket- $\emptyset$ -buy-PUNC that  
 'I bought **that basket**.'
- b. \* **Pedro** ngilla-waka-y **tufachi**.  
 Pedro buy-cow-IND.3sS this  
 'Pedro bought **this cow**.'

What this fact seems to suggest is that in Japanese, and possibly in Mapudungun, there is no N-head involved.

(9) a. *NI-form*b. *Non-NI form*

Thus, in (9a),  $\sqrt{\text{syokuji}}$  ‘dining’ is never a noun at any derivational stage. The root combines with *su* ‘do’, and thereby *syokuji-sita* ‘dining-did’ becomes a verb as a whole. In contrast, in (9b),  $\sqrt{\text{syokuji}}$  is assigned the category of noun, before it merges with *su*. The ungrammaticality of (6a) and (7a) then follows since noun-modifying elements are incompatible with the verb [ $vP$  *syokuji-sita*], while they can attach to the nominal *syokuji* for [ $vP$  [ $nP$  *syokuji-o*] *sita*].

The first implication of the structures in (9) and of the theoretical assumptions behind them is that unaccusative nouns can only have a bare-root option since they do not have an external argument that *n* can discharge. Thus, it follows that unaccusative nouns can only have an NI form, and the prediction is actually born out:<sup>2</sup>

#### Obligatory Noun Incorporation

- (10) a. John-ga butyoo-ni syoosin-sita. (NI)  
       John-NOM section chief-to promotion-did.  
       b. \*John-ga butyoo-ni syoosin-o sita. (Non-NI)  
       John-NOM section chief-to promotion-ACC did.  
       ‘John obtained a promotion to section chief.’  
       (Tsujimura 1990)

In (10) the unaccusative nominal *syoosin* ‘promotion’ (Tsujimura 1990) obligatorily undergoes NI.<sup>3</sup>

The second implication of the current approach is that the subject in an NI sentence is base-generated above the  $vP$ , while that in a non-NI sentence, the subject is derived from inside the nominal projection. This is so since in NI, there is no subject slot below the relevant  $vP$ , while there is one inside the  $nP$  for non-NI (see (9a) and (9b)).

To see if this implication is actually the case, let me introduce the following diagnostic for derived subjects:

<sup>2</sup> However, Terada (1990) observes that not only unaccusative nominals, but other nominals such as *shisa* ‘suggestion’ which take Source as their external arguments are also incompatible with their non-NI forms:

- (i) a. Kono deeta-ga atarashii mondai-o shisa site-iru  
       this data-NOM new problem-ACC suggestion doing  
       b. \* Kono deeta-ga atarashii mondai-no shisa-o site-iru  
       this data-NOM new problem-ACC suggestion-ACC doing  
       ‘This data suggests a new problem’

If we were to accommodate this data, then in (9b) not only does an *n* head discharge an external argument, it also imposes a thematic restriction on the external argument: namely, *n* only discharges an Agent theta-role.

<sup>3</sup> However, when they appear in full DPs in non-light verb contexts (e.g. *John-no syoosin* ‘John’s promotion’), the  $nP$  structure should be available even for these unaccusative nominals. But this is only because the *n* head does not play a role of assigning theta-roles in this case: i.e., the unaccusative root does not contain thematic-roles since it does not appear in light verb contexts by realizing as a full DP.

(11) *Numeral Quantifier (NQ)*

The associated NP, or its trace, and the NQ must mutually c-command each other.  
(Miyagawa 1989)

The requirement for NQ in Japanese forces an NQ and its associated NP to be in a local relationship. Thus, in the following examples, (12b) is ungrammatical because the NQ is not adjacent to the associated NP:

- (12) a. Gakusei-ga san-nin nomiya-de sake-o nonda.  
 student-NOM three-CL bar-at sake-ACC drank  
 b. \*Gakusei-ga nomiya-de san-nin sake-o nonda  
 student-NOM bar-at three-CL sake-ACC drank  
 ‘Three students drank sake at a bar.’

The trace of an NQ can also satisfy this requirement. In (13b), the trace of the scrambled object is adjacent to the NQ, and thereby it rescues the sentence:

- (13) a. Gakusei-ga sake-o san-bon mottekita.  
 student-NOM sake-ACC three-CL brought  
 b. Sake<sub>1</sub>-o gakusei-ga t<sub>1</sub> san-bon mottekita.  
 sake-ACC student-NOM three-CL brought  
 ‘Students brought three bottles of sake.’

Now that we know that the NQ test is a good diagnostic for tracing the underlying position of a moved element, let us apply this test to our current cases. In (14) the VP adjunct *restoran-de* ‘at a restaurant’ is situated in between the subject and the NQ<sup>4</sup>:

- (14) a. \*?Gakusei-ga restoran-de san-nin syokuji-sita.  
 students-NOM restaurant-at three-CL dining-did  
 b. Gakusei<sub>1</sub>-ga restoran-de san-nin t<sub>1</sub> syokuji-o sita.  
 student-NOM restaurant-at three-CL dining-ACC did  
 ‘Three students dined at a restaurant’

As expected, the NI sentence is ungrammatical since the NQ and its associated NP are not adjacent to each other on the surface, and furthermore, there is no trace of the subject that can potentially c-command the NQ. On the contrary, in (14b) since the subject is (predicted to be) derived, there is a trace left behind by movement of the subject which can establish the mutual c-command relationship with the NQ.

<sup>4</sup> It has been pointed out by Yuko Asada (p.c.) that the manner adverbs like *tanosiku* ‘happily’, which unambiguously modifies the lower vP, contributes to more obvious grammatical contrast:

- (i) a. ?? Gakusei-ga tanosiku san-nin syokuji-sita.  
 students-NOM happily three-CL dining-did  
 b. Gakusei-ga tanosiku san-nin t<sub>1</sub> syokuji-o sita.  
 students-NOM happily three-CL dining-did  
 ‘Three students dined happily.’

One might worry that the incorporated structure would license the subject in *vP* spec position, given the following structure where the subject raises to TP Spec, with the option of stranding an NQ:<sup>5</sup>

- (15) [TP SUBJ [<sub>vP</sub> ADV [<sub>vP</sub> <SUBJ> NQ [ N su ] ] ] ]  
 └──────────────────────────┘

This suggests, then, that there seems to be a projection above *vP* for subjects such as VoiceP (Kratzer 1994) to block such cases.<sup>6</sup> Assuming that adverbs continue to adjoin to the *vP*, there would be no word order problem:

- (16) [TP SUBJ [VoiceP <SUBJ > [<sub>vP</sub> ADV [<sub>vP</sub> NQ [ N su ] ] ] ] ]  
 └──────────────────────────┘

The structure in (16) brings an interesting prediction. If the adverbs are to attach as high as the subject (eg. sentential adverbs such as *probably*), we would expect the NI-form to license an NQ because there will be no intervening adverb between the subject and the NQ to block the adjacency requirement between the two in that case (see (11)). The prediction is in fact born out:

- (17) Gakusei-ga tabun 3-nin shokuji-sita.  
 student-NOM probably three-CL dining-did  
 ‘Three students probably dined.’

- (18) [TP SUBJ [VoiceP ADV [VoiceP <SUBJ > [<sub>vP</sub> NQ [ N su ] ] ] ] ]  
 └──────────────────────────┘

Thus, the current approach consequently contributes to Kratzer’s (1994) VoiceP system.<sup>7</sup>

The third and the last implication is that interestingly, NI forms and non-NI forms also show a phonological difference in terms of pitch accents. Although the light verb *su* itself is unaccented, when it is followed by the politeness suffix *masu* that carries an accent, the NI

<sup>5</sup> Thanks to Richard Larson for raising this question.

<sup>6</sup> Thanks to Tatiana Marvin for pointing out this possibility.

<sup>7</sup> One might worry that the fact (17) is grammatical is due to the ability of sentential adverbs to cancel the adjacency requirement between the NQ and its associated NP. A sentence like (i) is surprisingly grammatical, according to Miyagawa & Arikawa (2004):

(i) Gakusei-ga sake-o imamadeni san-nin nonda.  
 student-NOM sake-ACC so far three-CL drank  
 ‘Three students drank sake so far’

If the sentential adverb was left out, the sentence becomes ungrammatical:

(ii) \*Gakusei-ga sake-o san-nin nonda.  
 student-NOM sake-ACC three-CL drank  
 ‘Three students drank sake’

Thus, it could be the case that in (17) the NQ is not in fact licensed under the adjacency by the subject trace inside the *nP*, but is simply allowed to be separated from the subject trace when there is an adjunct. However, I still claim that the adverb is attached to the VoiceP since even in English, the adverb also appears in a post-subject position:

(iii) John will probably leave

form must have the accent of a single word, whereas the non-NI form can have that of two words:

*NI-Form*

- (19) *dókusyo* + *si-másu* → *dókusyosimasu* \* *dókusyosimásu*  
 reading + do-politeness-suf (I) read  
 (Poser 1989)

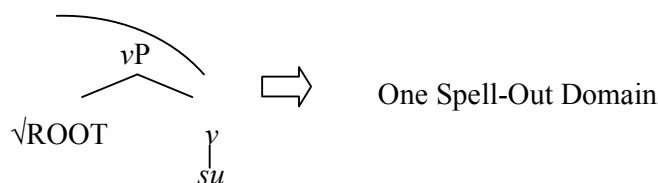
*Non-NI Form*

- (20) *dókusyo* + *si-másu* → *dókusyo-o simásu*, *dókusyo-o simasu*  
 reading + do-politeness-suf (I) read

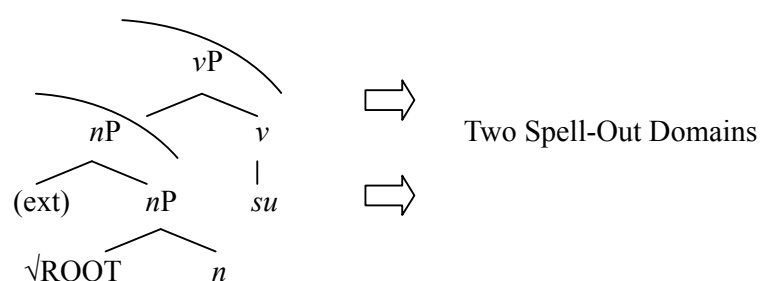
In (19), when an accented verbal noun and the polite form of *su* that carries an pitch-accent are combined in an NI form, the leftmost accent is preserved, and the one on *si-másu* deletes itself. In contrast, in (20) when the same nominal and *su* appear in a non-NI form, accents can appear on both.

The pattern can be easily captured with the posited structures in (9a) and (9b). On the assumption that  $\checkmark$  is not a phase, but *n* and *v* are phases at a word level (Marantz 2001), then the NI form obligatorily has the stress of a single word, whereas (26) will have that of two words. This is so since in (9a), there is one Spell-out domain (i.e. *vP*), while in (9b) there are two such domains, namely, *nP* and *vP*.

- (21) a. *NI*



- b. *Non-NI*



#### 4. Conclusions

Focusing on the structural difference between NI and non-NI, I have claimed that NI is not an instance of incorporation, but rather is a direct *Merge* of a root and the light verb. Thus an NI form does not involve an N-head at any point of its derivations. This is a radical departure from a syntactic point of view in that I do not assume any syntactic movement in NI. But at the same time it is quite different from a lexical analysis in that I do not assume any lexical operation in that everything is implemented in the syntax.

Most importantly, I have assumed that roots have their own  $\Theta$ -grids, and thus they contain grammatical information, which is contrary to what has been assumed in general (Embick & Noyer 2005).

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## Possible theoretical relevance of subphonemic vowel reduction in Hungarian

Dániel Szeredi

This paper deals with the question whether subphonemic phenomena like phonetically measurable consistent reduction processes have any theoretical, or phonological relevance. The answer may be yes if there is a theoretical background that sees phonological patterns in languages as emergent from the nature of diachronic processes that have been so far explained as consequences of universal innate rules or constraints. The subphonemic process used in the discussion is vowel reduction in Hungarian, for which experimental evidence will be shown.

### *1. Introduction*

This paper will present experiments to show the existence of vowel reduction in Hungarian, and to prove that vowel harmony affects that the pattern of this process. It will argue that the examination of this phenomenon, which has been neglected because it has been regarded to be non-phonemic and therefore non-linguistic, is important as some recent theoretical frameworks argue for the importance of the phonetic facts in their phonological analysis.

A common assumption made on Hungarian is that centralization or reduction of (unstressed) vowels is rare, as it is usual in syllable-timed languages (Roach 1982). Whereas this seems to be true phonemically, the extent of phonetic centralization has been scarcely researched. A commonplace claim is that there is no such in Hungarian either. The situation, however, seems to be more complicated. Section 2 will present the vowel inventory of Hungarian, and section 3 will describe an acoustic experiment conducted to see if vowel reduction exists in Hungarian. Section 4 will present a perceptual study to check if the attested reduction pattern has an effect on listeners' perception of centralized vowels.

Section 5 will argue for the importance of examination of subphonemic phenomena like vowel reduction in Hungarian, and the importance of theoretical background to discuss the source and the effects of these processes. Section 6 will finally show the process of finding an adequate phonetically grounded theoretical background to describe this process.

## 2. Hungarian vowel system

Descriptions of Hungarian vowels in the standard dialect elicit a system that might first look symmetrical as it contains 7 short and 7 long vowels, with the symmetry also supported by the orthography that clearly sets up pairs of short and long vowels: <a>:<á>, <o>:<ó>, <u>:<ú>, <e>:<é>, <i>:<í>, <ö>:<ő> and <ü>:<ű>. Looking at the standard description of the phonemic system looking at the table of Siptár & Törkenczy (2000: 51) shown here in Table 1, we can see that this symmetry doesn't seem to be apparent even in the phonological level. (Siptár & Törkenczy 2000: 51) use /ɔ/ for the short low back vowel, however experimental data as it will be shown below makes it clear that its phonetic value is closer to [ɒ] while the realization of /o/ can be quite close to [ɔ] thus /ɒ/ will be used for this phoneme throughout this paper.

	[-back]		[+back]	
	[-round]	[+round]	[-round]	[+round]
high	/i/	/i:/	/y/	/y:/
mid		/e:/	/ø/	/ø:/
low	/ɛ/		/a:/	/ɒ/

Table 1: Vowel inventory of Standard Hungarian

There are several phonological alternations that are used in morphological processes. The most important ones are:

- vowel harmony: /ɒ/:/ɛ/, /a:/:/e:/, /o/:/ɛ/:/ø/, /o:/:/ø:/, /u(:):/y(:)/
- length alternation: /ɒ/:/a:/, /ɛ/:/e:/, /o/:/o:/, /ø/:/ø:/, /u/:/u:/, /i/:/i:/, /y/:/y:/
- some other lexical alternations: /ov/~/ɒv/~/ɒj/:/o:/, /ɛv/~/øv/~/ɛj/:/ø:/, /o/:/ɒ/ and /ø/:/ɛ/ (in suffixes), etc.

It can be seen that these phonological alternations are in their true sense 'lexical' processes as they are structure-preserving and occur under certain morphological conditions. There is no alternation pattern that needs to be defined with a reference to (the lack of) stress, or any kind of reduction or centralization. Therefore if some kind of phonetic reduction shows up in spoken Hungarian, it must be described as a subphonemic process.

### 2.1. Previous research on Hungarian vowel reduction

Textbooks for foreigners usually contain the assertion that there is no vowel reduction in Hungarian, moreover, they usually claim that every Hungarian vowel is pronounced distinctly and clearly, which should mean they are not to be pronounced in a reduced or centralized form, e.g. *'Die Betonung des Wortes liegt immer auf der ersten Silbe. Die Vokale werden aber auch*

*in allen folgenden Silben voll und klar ausgesprochen.* [The stress of the word always falls on the first syllable. But vowels are pronounced full and clear in every following syllable.] (Ginter & Tarnói 1974/1993: 11). Prescriptivist tradition also holds that distinct clear pronunciation of sounds of speech is typical for Hungarian, contrary to other languages, and ‘blurring’ segments is a maleficent effect of modern life and these other languages (e.g. Benkő 1992).

However, reduced or centralized vowels do occur on the surface, but their distribution and frequency is still an issue that has been lightly researched (Ács & Siptár 2001). de Graaf (1987) confirmed in an auditory experiment with two speakers that vowels do centralize towards a target of a [ə] in Hungarian, and the extent of centralization is dependent on its context: free vowels are less reduced than vowels in isolated words and much less reduced than vowels in context.

Gósy (1997) has made an experiment where continuous speech of a male speaker with average voice parameters and no speech defects was recorded. Then several vowels were cut from the signal and these short isolated sound segments were played back to 10 participants, who were asked to transcribe the sound they had heard in Hungarian orthography. If they categorized any vowel that is not underlying /ø/ as <ö>, which was supposed to be the closest sound acoustically to schwa, it was posited that the sound they heard was actually schwa, or at least a largely centralized vowel. It was found that 49.3% of non-/ø/ phonemes had been transcribed as <ö>, implying that there might be a large proportion of centralized vowels in the surface in non-formal Hungarian speech.

Gósy (2006) merely elicits the types of schwa-like sounds occurring in any type of phonetic environment in Hungarian. She lists the reduced [ə], but without citing any further research on the reduction process, and besides, takes schwa-like sounds into account like the carrier signal occurring after the burst of utterance-final plosives and inbetween taps in trilled [r] sounds.

### 3. *The acoustic experiment*

#### 3.1. *Set-up*

The acoustic experiment (first described in Szeredi 2008) was designed to test the following hypotheses:

- centralization is to be found in casual natural speech, affecting both the F1 and F2 formants
- the rate of the centralization to correlate with stress: the more stressed the vowel, the less centralized it is
- the pattern of the centralization resembles the way this kind of process works in other languages, so some kind of theoretical model could explain the results, and this model could make some prediction on the behavior of other Hungarian vowels

Seven native speakers of Educated Colloquial Hungarian (Siptár & Törkenczy 2000), all between 21 and 28 years old, took part in the this experiment. 14 sentences were recorded from each participant six times in a row, so for the later sessions the register they used was more

casual. The aim of the experiment was to investigate four environments under three different stress conditions. The four environments were (z) [tɒz], (a) [ʃɒk], (o) [hoj] and (e) [kɛj]; and the vowels in these sequences were either (1) having primary stress – i.e. the first syllable of a content word, (2) unstressed – the last syllable of a content word and (0) unstressed in function words. After these reading sessions 12 words were recorded from each participant, with the instruction to read these words slowly and carefully to attain the formants for slow careful speech vowels (g). The test sentences and words are listed in Appendix A.

The recording was unfortunately not conducted in the best environment, but a silent room was available for the recordings. The recorded audio was sampled at a 44100 Hz, and resampled at 10000 Hz for males and 14000 Hz for females. Five formants have been measured with the top threshold of 5000 Hz for males and 7000 Hz for females. Each vowel was examined at the point of the highest intensity. The measured vowels obviously showed the coarticulatory effect of neighboring consonants so additional tests have to be carried out to check if these had significant effect on the results. The first three formants have been measured, but in the following sections, only F1 and F2 will be discussed as F3 does not show any significant change according to the examined stress environments in any examined environment. This formant will be discussed, however, in the analysis of the different behavior of the (a) and (z) environments as it has a significant role there.

The examined vowels are defined in Hungarian phonology as (cf. Siptár & Törkenczy 2000:51-52) :

- [ɒ] – a low back rounded vowel, however it is often characterized as [ɔ]
- [ɛ] – a low front non-rounded vowel, which is in contrast with [e:] and [a:]
- [o] – a mid back rounded vowel

### 3.2. Results

#### 3.2.1. The phoneme /ɛ/

Figure 1 shows the average position of the careful speech vowels and the average position of the vowels in the (e1), (e2) and (e0) environments in faster, more casual speech.

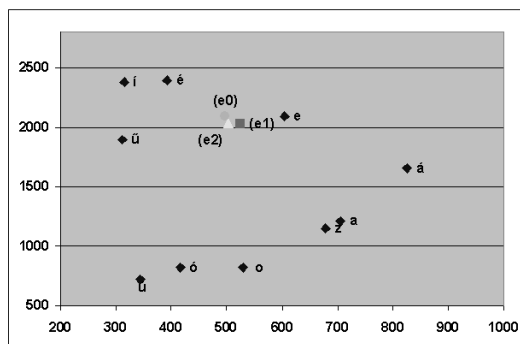


Figure 1: Average positions of vowels in environments with phoneme /ɛ/

It can be seen that the second formant of the /ɛ/ phoneme does not show centralization. The careful speech mean is 2086 Hz, which is not significantly different from casual realizations, where the overall mean is 2052 Hz ( $t = 0.6014$ , d.f.=20 yields  $p > 0.55$ ). The  $t$ -test shows no significant difference for different stress positions:  $p > 0.97$  for (e1)~(e2),  $p > 0.71$  for (e2)~(e0) and  $p > 0.68$  for (e1)~(e0). However, this can be attributed to the effect of the neighboring palatal /j/ consonant, which can hinder the velarization of the preceding vowel. This result is odd considering the analysis of de Graaf (1987) as well, who finds the short [ɛ] to reduce its F2 formant.

The first formant of the vowel in this environment shows centralization: it is almost always higher than the F1 of the vowel in careful speech (careful speech mean is 604 Hz, casual speech mean is 508.24,  $t = 7.7645$  with d.f.=20 yields  $p < 0.001$  significance). The only exception is the fifth speaker, who read the (eg) environment as a mid-high [e], either because of emphatic or dialectal pronunciation, so his (e1), (e2) and (e0) vowels are more open and more centralized. However, these environments were pronounced in the same way as they were by other speakers. Another oddity is that the (e0) variable of one speaker is very open, pronounced as [æ], however this vowel is very short, about 20 ms long. In other recordings of the speech of this speaker, the vowel is mid-high, like the (e0) of other speakers, so this phenomenon can be attributed to the fact that the formants show the impact of neighboring consonants on a very short vowel that is almost elided.

The hypothesis on the graduality of centralization corresponding with (the lack of) stress doesn't seem to be correct in the case of /ɛ/, as the realizations of the vowel in this environment are approximately in the same place in the dimensions of the first three formants, as it can be seen in the boxplot on figure 2. The  $t$ -test shows no significant difference:  $p > 0.45$  for (e1)~(e2),  $p > 0.91$  for (e2)~(e0) and  $p > 0.44$  for (e1)~(e0), so all of them are pronounced in casual speech as [ɛ̞] or [ɪ].

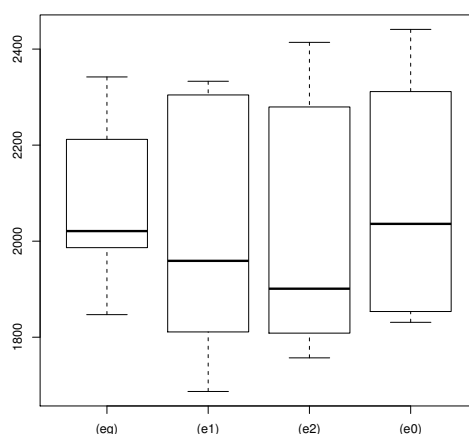


Figure 2: Boxplot for the F2 values for the vowel /ɛ/ aggregated by stress position.

## 3.2.2. The phoneme /o/

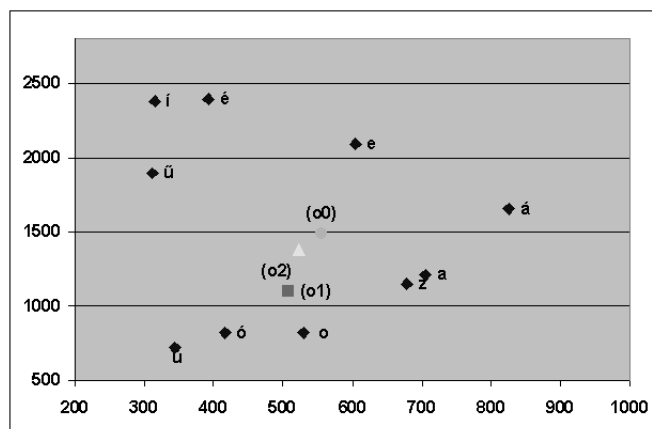


Figure 3: Average positions of vowels in environments with phoneme /o/

As it can be seen in figure 3 the realizations of the phoneme /o/ behave in the opposite way than the realizations of /ɛ/. The first formant does not undergo centralization in casual speech ( $t = 0.0903$ , with d.f.=20 yields  $p > 0.92$ ). This is quite easily explained as the careful speech (og) environment usually has a mid, or centralized F1 itself, so no more centralization is possible. However, the second formant shows significant centralization, and this process is indeed related to the lack of stress as it was supposed in the hypotheses, so the stressed (o1) environment usually has a low F2 but (o2) is more centralized and (o0) in the function word *hogy* 'that (complementizer)' has a quality quite close to [ə] as shown on figure 3 and summarized in table 2 and the boxplot on figure 4.

stress var.	mean F2	st. dev. F2	p value for $\text{mean}_n = \text{mean}_{n-1}$
(og)	821.3	100.4	
(o1)	1102.4	73.3	(o1)~(og) $p < 0.001$
(o2)	1378.8	277.5	(o2)~(o1) $p = 0.039$
(o0)	1491.1	317.5	(o0)~(o2) $p = 0.495$

Table 2: Mean F2 values for the vowel /o/

The environment of this vowel in the experiment is also a palatal /j/, thus it is possible that the fronting seen is affected by this consonant. Then an explanation for the graduality of centralization could be that the less stressed the vowel is, the shorter it is, thus the neighboring palatal consonant has a greater effect on its formants. However, as the environments containing underlying /ɒ/ do not stand beside palatal consonants but still show centralization, it seems that the effect of /j/ cannot be the only factor in this fronting process.

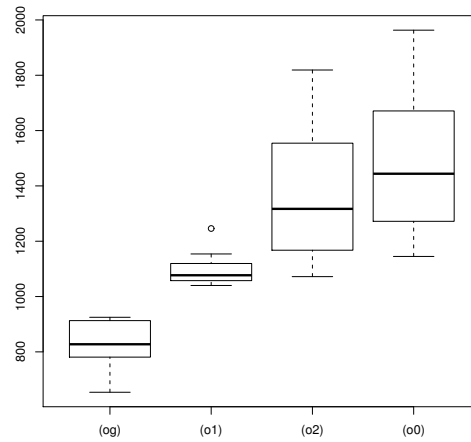


Figure 4: Boxplot for the F2 values for the vowel /o/ aggregated by stress position.

### 3.2.3. The phoneme /ɒ/

The /ɒ/ phoneme shows the most typical schwa-oriented centralization, as both of its formants undergo this process in casual speech. This phoneme was examined in two environments to see the extent of centralization in both versions of the definite article (/ɒ/ and /ɒz/). The F1 of these vowels is lowered with loss of stress as it can be seen on figure 5. The centralization of /ɒ/ in F2 is less gradient as careful speech is significantly more back than casual speech ( $t \approx 7.8$ , d.f.=20 yields  $p < 0.001$  for both environments), but there is no significant difference for stress environments in the casual realization of the vowel.



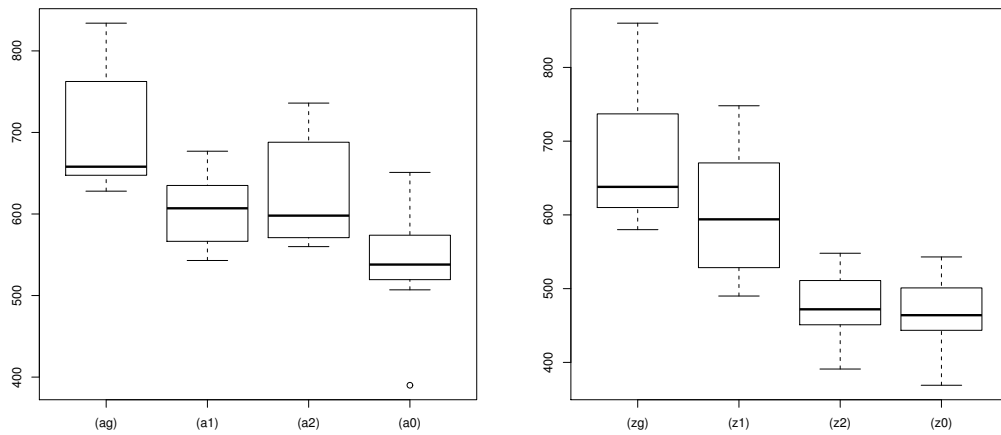


Figure 5: Boxplot for the F1 values for the vowel /ɒ/ in the two environments (a) and (z) aggregated by stress position.

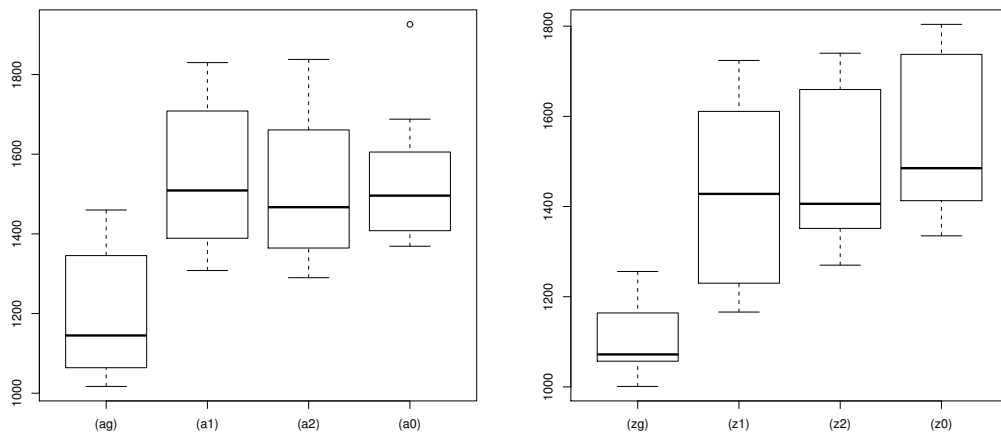


Figure 6: Boxplot for the F2 values for the vowel /ɒ/ in the two environments (a) and (z) aggregated by stress position.

The different behavior of the two environments can be seen in table 3, and can be seen on figures 7 and 8 as well. It can be seen that there is a significant difference between the value of the third formant of these two vowels, as it is much higher in the case of the [tɒz] environment, nearing the value of non-rounded vowels. Whether the effect of the [t<sub>z</sub>] environment is in fact a loss of rounding is an interesting question to be addressed in future research. Lowering of F1 of the vowel (i.e. raising the vowel) in the (z) environment is also significantly more prevalent (paired

*t*-test,  $t = 2.8169$ ,  $d.f.=38.509$  yields  $p \approx 0.008$ ), and reduction in vowel length also seemed to be stronger for the (z) environment. This reduction was so strong that some speakers had only one or two records out of six with an observable vowel segment in the (z0) environment.

variables	F1	F2	F3
ag-zg	<b>67.9 Hz</b>	48.4 Hz	<b>-180.6 Hz</b>
a1-z1	0 Hz	120.3 Hz	-191.5 Hz
a2-z2	<b>153.8 Hz</b>	29.7 Hz	-182.8 Hz
a0-z0	71 Hz	-15.3 Hz	-206.8 Hz
a-z all casual	<b>75.1 Hz</b>	44.9 Hz	<b>-193.8 Hz</b>

Table 3: Differences between formants of vowels in [ʃɒk] (a) and [tɒz] (z) environments. Bold values indicate significant ( $p < 0.05$ ) difference.

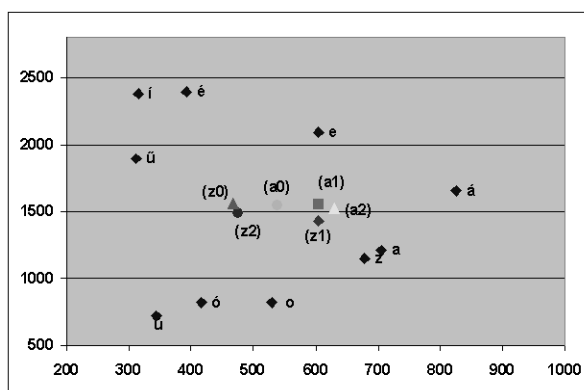


Figure 7: Average positions of vowels in environments with phoneme /ɒ/ in the dimensions of F1 and F2

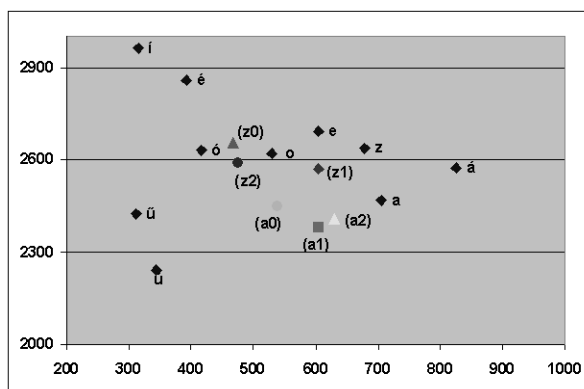


Figure 8: Average positions of vowels in environments with phoneme /ɒ/ in the dimensions of F1 and F3

### 3.2.4. Summary

The results have shown that it is clear that Hungarian speakers tend to centralize vowels in certain positions. It can be seen, that back vowels do centralize their formants towards a target of [ə], and they show the expected graduality of reduction caused by stress position. The examined front vowel [ɛ], however, fails to do so, no stress effect has been found, and there is no significant F2 lowering:

- Environments that centralize F2
  - The environment (o) – gradient fronting occurs as the vowel loses stress
  - The environment (a) and (z) – gradient raising raising and shortening as the vowel loses stress, fronting in casual stress
- Environment that does not centralize F2
  - The environment (e) – no backing, but F1 raising in casual speech under all settings of the stress parameter

The average tendencies of the examined vowels can be summarized in the table 4.

	(a)	(z)	(e)	(o)
(g)	[ɒ]	[ɒ]	[ɛ]	[o]
(1)	[ɔ̃]	[ɔ̃]	[ɪ]	[ɔ̃]
(2)	[ɔ̃]	[ə]	[ɪ]	[ə]
(0)	[ə]	[ə]	[ɪ]	[ə]

Table 4: Average vowel qualities in the tested environments

The result is somewhat problematic as the extent of the effect of neighboring consonants is not clear. However, research that followed up this experiment confirmed and used the above findings in other environments. Rácz & Szeredi (2009) showed that [o] is centralized towards [ə] in non-palatal environments like [tok], [kot], [toʃ], [doʃ] and [dok]. As for the raising and the lack of centralization for [ɛ] Blaho & Szeredi (2009) showed the same effect working in environments like [fɛl], [lɛk], [kɛz], [zɛt], [mɛg], but found some significant centralization for certain speakers towards a target of [ə] in the context [lɛg], [nɛfi], [fiɛz] and [zɛb] in the word *legnehezebb* ‘heavy.SUPERLATIVE’.

## 4. The perceptual experiment

### 4.1. Set-up

The aim of the experiment presented here was to see if the F2-based dichotomy seen above is found in perception as well, because if it is, it might also prove that subphonemic processes

might work on phonological categories, be influenced by the phonological system of a language and therefore be able to possibly influence the phonology of the language as predicted by functional approaches.

The pilot study presented here was conducted with 3 native speakers of Educated Colloquial Hungarian, who had the task to assign an acceptance score to test sentences. These test sentences were built on the same syntactic construction (the test sentences and words are listed in Appendix B):

subject	negation	verb	verbal prefix	article	object
<i>János</i>	<i>nem</i>	<i>védte</i>	<i>ki</i>	<i>az</i>	<i>ütést.</i>
John	not	parry.PAST.3SG.DEF	PFX	the	blow.ACC

‘John did not parry the blow.’

The verbs in every sentence took the PAST.3SG.DEF suffix /tɒ/~/tɛ/. The verbal prefix [ki] always followed the main verb, therefore all tested vowels were in the environment of [t\_k]. Every verb stem was monosyllabic and all distinct vowel qualities in Hungarian were used as stem vowel of a verb in a sentence (quantity distinction was not taken into account). The neutral /i/ vowel was tested as the stem vowel for two verbs: one taking front suffixes and one taking back ones to test the effect of different kind of lexical storage for the two classes argued for by Benus & Gafos (2007). The hypotheses of the experiment described below were that centralized vowels will be accepted as vowel reduction is present in Hungarian and that [ə] will be more accepted in the place of a back vowel than in the place of a front one ([ɒ] and [ɛ] in the experiment respectively).

The test sentences were synthesized using the MBROLA text-to-speech system (Dutoit et al. 1996), thereafter the tested vowel of the suffix was altered using Praat to six affix vowel variables: [ɒ], [ɐ], [ə], [ɜ], [ɛ] and [ɪ], with [ɐ] placed halfway between [ɒ] and [ə], [ɜ] placed halfway between [ɛ] and [ə] and the F1 of [ɪ] halfway between [ɛ] and [e:] and  $F2_{[ɪ]} = \frac{2 * F2_{[ɛ]} + F2_{[e:]}}{3}$ . The actual formant values of these vowels are given in table 5. Vowel duration, pitch and intensity were not altered, so this experiment focused on the effects of reduction on formants as well.

	[ɒ]	[ɐ]	[ə]	[ɜ]	[ɛ]	[ɪ]
F1	491	453	416	425	435	367
F2	941	1173	1405	1637	1870	1997
F3	2336	2406	2476	2546	2616	2717

Table 5: First three formants for test affix vowels of the perceptual study

Each test sentence listed in Appendix B was altered, with each of the six tested affix vowel edited into the past tense morpheme. Ten stem vowels therefore yielded 60 test sentences in a repetition, and every speaker was tested three times in three randomized orders to eliminate any skewing effects of the order of sentences. These 180 sentences were played to the speakers, who had to assign an acceptance score to each sentence they heard in a range of 1 (worst) to 7 (best).

## 4.2. Results

The results of this experiment are summarized statistically in table 6 ( $N = 45$  for front,  $N = 36$  for back vowels in all columns as stems with [i] and back affixes were excluded;  $t$ -test was used for significance analysis).

affix vowel	[ɒ]	[ɛ]	[ə]	[ɜ]	[ɛ]	[ɪ]
front stem	1.89	2.38	2.73	4.88	6.24	5.98
back stem	6.53	6.64	6.42	5.03	2.39	2.17
$p$ value	<0.001	<0.001	<0.001	0.3881	<0.001	<0.001
significance	***	***	***	none	***	***

Table 6: Mean acceptance scores for a given affix vowel after front and back stems

The most striking observations that can be made from the study are:

- scores for [ə] were significantly worse for front stems than for back stems:  $t$ -test shows  $t=12.14$  with d.f.=71.52 meaning highly significant difference of means. The boxplot for this difference is shown on Figure 9.
- there is no significant difference of scores at the quality of [ɜ], which is quite on the front half of the vowel chart ( $F2=1637$ ).
- scores for [ɛ] were non-significantly different from scores for [ɪ] for front stem vowels ( $t=1.135$  with d.f.=82.335 meaning  $p = 0.2596$ ).
- scores for [ɒ], [ɛ] and [ə] were non-significantly different for back stem vowels ( $p = 0.55$  for [ɒ]~[ɛ],  $p = 0.27$  for [ɛ]~[ə] and  $p = 0.61$  for [ɒ]~[ə]).
- stems taking back suffixes with [i] as stem vowel show significantly better scores than other back stems for an [ɪ] affix vowel ( $p \approx 0.01$ ) and significantly worse for an [ɒ] affix vowel ( $p \approx 0.02$ ), thus tolerating more front affixes better.
- stems with front round vowels [y] and [ø] showed a significantly better score for centralized affix vowels than front non-round stems:  $p \approx 0.04$  for [ə] and  $p \approx 0.01$  for [ɛ].

These observations fall in line with the findings of the acoustic experiment: the back vowel phoneme /ɒ/ can be centralized towards [ə] and still be acceptable to listeners, whereas the front /ɛ/ phoneme cannot be centralized, however its raised realization [ɪ] is accepted.

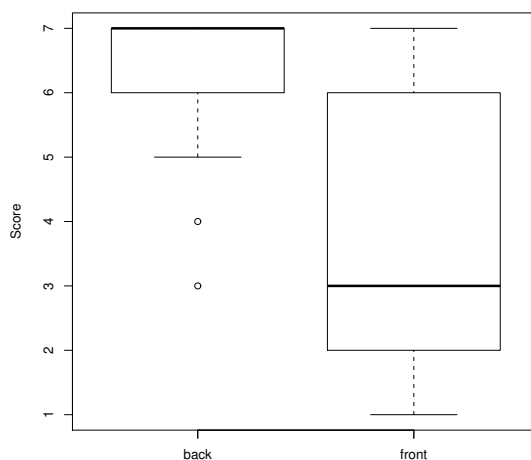


Figure 9: Boxplot for acceptance scores for the [ə] vowel in suffixes attached to back and front harmonic stems

### 5. Theoretical relevance of subphonemic vowel reduction

Subphonemic processes have not been seen as very much relevant to phonology or phonological theory. There are reasons however that could give one the idea to work on subphonological data using phonological accounts developed to explain phonological phenomena.

Subphonemic variation that had been previously treated as irrelevant for language has become more important in theoretical frameworks that do not explain universal features in languages with an innate Language Faculty. Diachronic approaches like Blevins (2004) argue that universal features of languages follow from the diachronic nature of language: they share similar traits because the way languages change is similar, so constraints on synchronic phenomena can be traced back to constraints on diachronic processes. This means that contrary to generative tradition where universals are explained by using the innatist axiom and assuming the existence of a Language Faculty, there is no need for such a (from a certain point of view) extra-linguistic explanation and constraints on synchronic phenomena can be traced back to constraints on diachronic processes, that can, and should be described inside the domain of linguistics.

This claim also means that *synchronic* phonological accounts that are able to account for patterns found in the subphonemic domain like vowel reduction in Hungarian should be preferred over those that are not, because subphonological behavior in a generation is the source of phonological rules/constraints/patterns of a later generation. Ohala (1981) describes the way how the listener, and in an acquisitional point of view, the learner is the source of sound change in different scenarios. These scenarios take subphonemic issues into account, and Ohala describes how these issues can lead to a *phonological* shift in the next generation.

Functional approaches using exemplar-based rich lexicon models like Bybee (2001), Pierrehumbert (2001) also rely on strict phonetic forms that are stored in the lexicon and claim that categories evolve or emerge based on these forms. Subphonemic variation is equally im-

portant to these approaches as well, as various linguistic processes are explained by analogy in these frameworks, which relies on the knowledge of the phonetic forms of a given lexical or item or ‘phonological’ unit.

The analysis of a subphonemic phenomenon like the vowel reduction pattern in Hungarian can therefore be important as claims of several theoretical frameworks can be tested on it, as subphonemic patterns are expected to show similarities to strictly phonemic processed originally described by these frameworks. Furthermore, if a given theory provides a simpler and functionally more grounded explanation for a phenomenon like this, it can be preferred over those frameworks that fail to do so.

## 6. Analyses

Table 7 shows that the Hungarian vowel reduction pattern seen above in the experiments for Hungarian vowel reduction (sections 3 and 4) can be easily compared to *phonological* reduction patterns in other languages frequently cited in the literature (Crosswhite 2004; Harris 2005; de Lacy 2006). It is therefore not hard to compare the treatment of these languages in various descriptive models, and to see their predictions on Hungarian.

	Target of centralization in						
	Hungarian	Catalan	SLPC	Bulgarian	Belarusian	CI	SI
a	?	ə	ə	ə	a	a	a
ɒ/ɔ	ə	-	o	-	-	o	-
ɛ/æ	e/ɪ	ə	e	-	-	e	-
e	?	ə	e	i	a	e	i
o	ə	u	o	u	a	o	u

Table 7: Hungarian vowel reduction compared to other reduction patterns. SLPC = Sri Lanka Portuguese Creole, CI = Central Italian, SI = Southern Italian

### 6.1. Standard OT

The standard OT analysis for vowel reduction will be shown as it is described in Crosswhite (2004). She describes two kinds of reduction patterns found in languages: prominence reduction and contrast enhancement. The Hungarian data above shows prominence reduction as the low back vowel seems to converge towards a central [ə]-like quality rather towards the corner vowel [a].

Prominence reduction is described in Crosswhite (2004) using feature-specified faithfulness constraints in a language-specific order like MAX[+BACK], MAX[+ROUND] and so forth; and using a universally ranked markedness scale of vowel qualities and their occurrence in unstressed situations in the form of the following hierarchy:

UNSTRESSED/a  $\gg$  ...  $\gg$  UNSTRESSED/high vowels  $\gg$  UNSTRESSED/ə

There is no explanation in this theory why [ə] is less prominent than high corner vowels, and in which detail it differs from other mid vowels like [e], [o] or lax [ɪ], [ɛ] or [ɔ]. The above ranking is supposed to be universally bound and language-specific reduction patterns are explained using the faithfulness MAX constraints ranked to different places in the above hierarchy. Register-specific reduction can be explained with the gradual demotion of these faithfulness from an initial high position.

Analyzing Hungarian data using this account will show some more of its weaknesses. First, looking at the behavior of unstressed /ɛ/ shows that two faithfulness constraints have to be used: a MAX[+LOW] constraint that can be demoted in casual speech and a MAX[+FRONT] constraint that cannot. To illustrate this, the following tableau shows the evaluation of candidates in careful speech where both MAX constraints are ranked high (\*U/V being an abbreviation for \*UNSTRESSED/V):

/ɛ/[+stress]	MAX[+FRONT]	MAX[+LOW]	*U/ɛ,ɔ	*U/e,o	*U/i,u	*U/ə
☞ [ɛ]			*			
[e]		*		*		
[ə]	*	*				*

In casual speech the MAX[+LOW] constraint is ranked lower (anywhere under UNSTRESSED/ɛ,ɔ), thus resulting prominence reduction:

/ɛ/[-stress]	MAX[+FRONT]	*U/ɛ,ɔ	MAX[+LOW]	*U/e,o	*U/i,u	*U/ə
[ɛ]		*				
☞ [e]			*	*		
[ə]	*		*			*

A prediction given by this analysis is that faithfulness constraints could be demoted further, thus yielding other centralization targets for /ɛ/ in even more casual speech: [i] or [ɪ] if MAX[+LOW] is demoted under UNSTRESSED/e,o; and even [ə] if MAX[+FRONT] can be demoted as well.

The analysis for the behavior of /o/ needs a MAX[+BACK] faithfulness constraint, as [o] and [ə] are contrasted only in this feature. The demotion of the MAX[+LOW] constraint has no effect on /o/ because centralization or raising would not violate this constraint, as [o] itself is already [-low]. Careful speech evaluation is then as follows:

/o/[+stress]	MAX[+BACK]	*U/ɛ,ɔ	*U/e,o	*U/i,u	*U/ə
☞ [o]			*		
[ə]	*				*

With the demoted faithfulness constraint in casual speech:

/o/[-stress]	*U/ɛ,ɔ	*U/e,o	MAX[+BACK]	*U/i,u	*U/ə
[o]		*			
☞ [ə]			*		*



The constraint selection and their re-ranking patterns above can be tested with the reduction of /d/. This phoneme can be represented as [+back] and [+low]. Its UNSTRESSED constraint should be between UNSTRESSED/a and UNSTRESSED/ε,ɔ. First, careful speech:

/d/ [+stress]	MAX[+BACK]	MAX[+LOW]	*U/d	*U/ε,ɔ	*U/e,o	*U/ə
[d]			*			
[ə]	*	*				*

In casual speech MAX[+BACK] and MAX[+LOW] have to be demoted under \*UNSTRESSED/e,o as seen above, so in this register the evaluation of candidates is as follows:

/d/ [-stress]	*U/d	*U/ε,ɔ	*U/e,o	MAX[+BACK]	MAX[+LOW]	*U/ə
[d]	*					
[ə]				*	*	*

The problem with this analysis is the lack of its explanatory power, lack of functional groundedness and some of its ad hoc nature. One might point out that both MAX[+BACK] and MAX[+FRONT] have to be used in order to get the Hungarian reduction pattern. The actual rank where faithfulness constraints get demoted to seems to be random, and this analysis lacks any type of phonetic or other grounding for this.

The question of graduality is also avoided here: if a certain constraint is demoted to a certain point in casual speech, its quality is set to the target phonetic realization instead of the somewhat gradual and stochastic realization pattern that can be seen in the data. Stochastic OT (Boersma & Hayes 2001) can help here, but it still does not have the functional grounding that is needed if the approach set in section 5 is taken seriously.

## 6.2. Phonetic grounding

An important phonetic notion when talking about vowel reduction is the dispersion of phonemes in a vowel inventory. Dispersion Theory portrays vowel reduction as the lesser need of dispersion in unstressed environment, ie. the lesser need of a wide space for vowels. Flemming (2004) has two phonologically grounded types of constraints based on this theory: a constraint that enforces maximal dispersion of vowels in the space available (like MINDIST=F1:3 which is violated if two vowels are closer to each other than 3 measurements on an arbitrary scale) and constraints that prefer to minimize articulatory effort (like \*SHORT LOW V for F1 centralization or \*HIGH EFFORT for F2 centralization). As it evaluates vowel inventories it also uses a constraint that requires to maximize the numbers of vowel contrasts in a language.

This latter property of this analysis is in fact a disadvantage, as well as the use of auditory effort-based constraints, while the listener (or the learner, the child) is practically the source of sound change (Ohala 1981). Using this theory also needs more research in Hungarian, as the effect of vowel length, and its retention in the standard spoken dialect is not very well understood yet.

Dispersion-Focalization Theory (DFT, Schwartz et al. 1997) adds the focalization factor to the dispersion-based analyses. Focalization is meant to refer to the observed characteristics about the least ‘marked’ vowels in spoken languages. The assertion is that the most easily perceived auditory feature of vowels is the convergence of two formants, i.e. when they are so close to each other that their energy adds up to one salient frequency peak which is easily perceived by the human ear and mind. Dispersion and focalization work slightly against each other, but their balance can explain the distribution of vowel inventory types in the world (cf. Kingston 2007; Becker-Kristal 2007).

### 6.3. Representation of phonetic facts

The question whether representation or derivation is of greater phonological interest is debated since the beginning of generative linguistics. The point of view taken here is definitely not generative because of the support for the diachronic explanation for universal features in languages. It will be argued, however, that representational approaches can help describing subphonemic phenomena and their diachronic behavior.

Diachronicity is sometimes imported to generative analyses either in a form of lexical derivation or in the form of abstract complex representation of underlying forms. Vowel-zero alternations in Slavic for example are still usually described using an underlying abstract set of phonemes called *yers* that are eliminated either through derivation (from Halle 1959 to Rubach 1993) or through running default processes in a representationalist approach (e. g. Scheer 2002 in strict CV phonology). The notion of yers is available from the knowledge of older forms of Slavic, where these (later abstract) phonemes had a phonetic value of a reduced [ɪ] and [i].

The use of diachronicity in this paper is very much different. As described in section 5 the importance of phonological treatment for subphonemic data is explained through diachronicity, and representations should help in understanding language change. A given representational approach in this analysis would not describe phonological, rather *phonetical* facts in this case. This approach has to be well grounded phonetically and has to be linked to phonetic facts and it must provide falsifiable predictions on universal cross-linguistic level and falsifiable predictions on vowels not yet examined.

### 6.4. Element Theory analysis using DFT

Harris (2005, 2007) uses the phonetic grounding of DFT in his Element Theory (ET) framework for vowels. The abstract elements (A), (I) and (U) used in the phonological representation of vowels therefore have a phonetically grounded meaning as well, representing the perceptually most salient effects that carry information over the neutral [ə]-like carrier signal:

- (A) – called a “mAss”, the salient convergence of high F1 and central F2
- (I) – called a “dIp”, the salient convergence of high F2 and F3
- (U) – called a “rUmp”, the salient convergence of F0, low F1 and low F2

The representation of corner vowels is the simple element that characterizes them. The central vowel is represented by the lack of any segmental information: /ə/ = (). Non-corner vowels are treated as complex, therefore their representation is the combination of elements, e.g. /o/ = (A+U), /e/ = (A+I). In more detailed systems one element is called the ‘head’ of the representation having a greater impact on the vowel quality, and the other is a ‘dependent’, e.g. /ɔ/ = (A+U) and /ɔ/ = (A+U).

In this ET-DFT framework vowel reduction is then easily handled as the loss of some elements, and different reduction patterns are explained as the retention or loss of one of them. Harris (2005) shows how this works in a variety of languages, including Catalan (see table 7) which seems to be quite similar to the Hungarian pattern attested above in this analysis: whereas Hungarian reduces (A) and (U) while keeping (I), Catalan reduces (A) and (I) and retains (U), as it can be seen on table 8.

phoneme	vowel	Catalan reduction		Hungarian reduction	
	representation	quality	representation	quality	representation
/a/	(A)	[ə]	()		
/ɒ/~/ɔ/	( <u>A</u> +U)	[u]	(U)	[ə]	()
/o/	(A+ <u>U</u> )	[u]	(U)	[ə]	()
/ɛ/	( <u>A</u> +I)	[ə]	()	[ɪ]	(ə+I)
/e/	(A+ <u>I</u> )	[ə]	()		

Table 8: Comparison of Catalan and Hungarian vowel reduction in ET-DFT following Harris (2005)

The result of centralization of Hungarian /ɛ/ in the representation in this framework would be a segment with an empty head (orthographically represented here as (ə)) with a dependent (I), which renders a lax [ɪ] quality (cf. Harris & Lindsey 1995), the vowel realization seen in the acoustic experiment in section 3. The representation of Catalan /ɔ/ and Hungarian /ɒ/ is the same as the (not reduced) realization of these phonemes are really close to each other and there is no theoretical issue that would necessitate their differentiation.

It is important to underline that the use of representations in this model does not mean that these representations would need to have any psychological or cognitive reality. These “elements” are merely shorthand abbreviations of certain phonetic facts as they had been defined in terms of focalization in DFT. The use of these abbreviations in this framework suggests that focalization is a very important perceptual cue, and manipulation with them only represents the pattern underlying the articulatory undershoot and its perceptual consequences, and points out the importance of the lack of loss of a certain focalized quality: the one represented with the element (I).

### 6.5. Predictions of the ET-DFT framework

The Element Theory analysis has the advantage of providing theoretical background, while having phonological groundedness. It is capable of predictions on the behavior of vowels not examined and is able to reveal tendencies or changes that are possible in the vowel inventory of Hungarian. The main prediction is that the palatality distinction is strongly retained in Hungarian, so one would not expect front vowels like /y/ (U+I), /ø/ (A+U+I) or /i/ (I) to centralize their F2 formants, their target of reduction is predicted by the Element Theory analysis to be [i] (I) or [ɪ] (ə+I). This seems quite odd as the vowel quality of [ø] is the one substituted by Hungarian speakers for a [ə]-like sound (Gósy 1997), therefore its loss of roundedness is a very strong – and falsifiable prediction of this model.

The fact that the (I) element is intact means that F2 vowel harmony seems to be strong, with the power of constraining the reduction of this element. The loss of F2 vowel harmony (like the one that occurred in the genetic relative Estonian, cf. Viitso 1998) is, therefore, not probable. Should vowel reduction in unstressed syllables get stronger by time and its effects enter the phonological domain, at least two reduced phonemes (back [ə] and front [ɪ]) behaving according to vowel harmony are predicted to survive, if such bold predictions can be made by the models used above.

The question can be raised why vowel harmony is so resistant that the (I) element is preserved in Hungarian vowel reduction. Vowel harmony could even possibly strengthen reduction, as the information of the frontness of the word is enough to be carried on one syllable, which is likely the first one with the primary stress. On the other hand, in a language with vowel harmony the persistence of a certain “feature” or articulatory (and therefore auditory) clue might be easier or more straightforward.

However, recent studies and this experiment as well provide evidence for the latter point of view. Pearce (2009) has shown that vowel harmony has in fact an effect on subphonemic vowel reduction in the way of conserving the harmonizing feature. She also shows some results on Hungarian, where she finds quite similar effects to those in the experiment above: back vowels centralize while front vowels do not. She has shown this pattern for more F2-based vowel harmony systems (even with genetic relatives of Hungarian like Finnish) as well as for other auditory cues, like [ $\pm$ ATR].

## 7. Consequences and further research

This article has two main claims besides presenting experimental data from an area previously not well researched. The first claim is that subphonemic phenomena that have been dismissed by phonologic theory on the grounds that they are extra-linguistic or part of the ‘performance’ or being merely some kinds of errors in speech are well enough worth to be examined, as they can be described by several theories.

It has been argued that with the recent re-emergence for diachronic explanations for synchronic patterns these subphonemic phenomena need to be researched and functionally well grounded theories are needed to describe them. These descriptions must account for phonetic facts, the gradient nature of subphonemic processes and must provide enough theoretical power

to provide falsifiable predictions. The second main claim of this paper is that for vowel reduction phenomena, the combination of Dispersion-Focalization Theory and Element Theory is an account that meets these expectations.

Further research would obviously need to check the validity of the predictions of the ET-DFT framework sketched up in section 6.5 and to widen the array of subphonemic phenomena that could prove or falsify claims of this framework. The development of different functional phonological theories also needs to be tracked as newer theories and newer findings may provide more understanding to vowel reduction and other subphonemic patterns.

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