**On children's late acquisition of raising *seem* and control *promise*:
Is a unified account possible?**

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Abstract:
The goal of this study is to address two questions: (i) whether the delays in the acquisition of subject-to-subject raising (StSR) *seem* and subject control (SC) *promise* are related, as would be predicted by various developmental accounts, and (ii) whether delays are due to limited processing capacity or immature grammatical abilities. Our two comprehension tasks reveal two groups of children: (i) below-chance group: they have a non-adult grammar of StSR or SC and processing capacity does not predict performance; and (ii) at-/above-chance group: they have an adult-like grammar of StSR or SC and processing capacity modulates performance. Importantly, we find no correlation between StSR and SC performance – some children have mastered StSR with *seem* but not SC with *promise* and some show the opposite pattern, suggesting a dissociation between the grammatical development of StSR and SC, specifically of the mechanisms required to circumvent intervention.

Keywords: acquisition, raising, control, intervention, processing, smuggling

1. Introduction

Subject-to-Subject Raising (StSR) (1a) and Subject Control (SC) (1b) structures both involve an interpretive dependency between an overt subject DP in the matrix clause, '*John*', and a lower unpronounced subject in the complement clause, represented pretheoretically as <John> in (1).

(1) a. John seems (to Mary) [<John> to be nice.]
b. John promised (Mary) [<John> to be nice.]

Additionally, both Raising verbs such as *seem* and Control verbs such as *promise* constitute exceptions to minimality constraints, such as the Minimal Distance Principle (MDP, Rosenbaum 1967), the Minimal Link Condition (MLC, Chomsky 1995) or Relativized Minimality (RM, Rizzi 1990, 2004), because the dependency must be established between the embedded subject and the matrix subject, as opposed to the more local c-commanding DP, the experiencer or benefactive '*Mary*'. This parallelism has led to the hypothesis that Raising *seem* and Control *promise* structures are derived by A-movement (Hornstein 1999, Boeckx & Hornstein 2004), and also to the proposal that both involve Smuggling (Belletti & Rizzi 2013).

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Perhaps relatedly, both constructions are mastered relatively late in children, at approximately 6 or 7 years old (*seem*: Choe 2012, Hirsch & Wexler 2007, Mateu 2016, Orfitelli 2012; *promise*: C. Chomsky 1969, Hsu, Cairns & Fiengo 1985, Mateu 2016, Sherman & Lust 1993). One prominent explanation holds that these delays are due to the presence of the intervening argument, which creates difficulties for children, either for grammatical or processing reasons. However, to our knowledge, there are no studies that investigate the acquisition of Raising and Control constructions in the *same* group of children to see if they are developmentally related. Moreover, there have been no independent tests of whether the purported intervention effects are grammar- or processing-based. The two studies reported here investigate whether the delays in the acquisition of raising *seem* and control *promise* are due to immature grammatical abilities or processing limitations. If both delays are due to grammatical deficiencies, we further ask: are they both of the same nature? In other words, is the grammatical mechanism necessary to by-pass the experiencer in Raising the same mechanism necessary to by-pass the benefactive in Control?

This chapter is organized as follows: section 2 provides the theoretical and experimental background on Subject-to-Subject Raising (StSR) and then details our experimental study; section 3 focusses on Subject Control (SC) *promise*, first laying out the experimental and theoretical background and then our experimental study. Section 4 compares the results from the two experiments and discusses the theoretical implications. Section 5 offers a summary.

2. Subject-to-Subject Raising *seem*

2.1. Background

Experimental studies show that even though children perform well with unraised *seem*, they exhibit difficulties when the subject is raised (Choe 2012, Hirsch 2011, Hirsch & Wexler 2007, Hirsch, Orfitelli & Wexler 2007, Mateu 2016, Orfitelli 2012). This difficulty cannot be attributed to the raising operation alone, however, because children do not show the same difficulties with other raising constructions, for instance, StSR with verbs that do not select for an experiencer, e.g. *be going*, *be about*, and *tend* (Orfitelli 2012). While experimental results for StSR *seem* with a covert experiencer are inconsistent -- some studies finding a delay (Hirsch 2011, Hirsch et al. 2007, Mateu 2016, Orfitelli 2012) and others not (Becker 2006),[1] results consistently show that when the intervening experiencer is overt children under the age of 6 or 7 perform poorly with StSR *seem* (Choe 2012, Hirsch & Wexler 2007, Hirsch, Orfitelli & Wexler 2007, Mateu 2016). Specifically, many of these studies find that children misinterpret the raising verb either as a copula and ignore the experiencer if it is explicit (i.e., (1a) would be interpreted as 'John is nice'), or they interpret *seem* as 'think' (i.e., 'John thinks Mary is nice'). In this chapter we report on children's performance on StSR sentences that include an overt intervening experiencer argument.

Most current accounts that attempt to explain children's difficulties with StSR *seem* rely on the notion of intervention, either from a grammatical (e.g., Hyams & Snyder 2005, Orfitelli 2012, Snyder & Hyams 2015) or a processing standpoint (e.g., Choe 2012, Choe & Deen 2016). The following two sections will discuss these two approaches.

[1] See Mateu (2016) for potential methodological differences that could account for this discrepancy of results.

2.1.1. Grammar-based accounts

StSR constitutes a well-known instance of a structure in which adults successfully overcome a potential intervention configuration. In StSR the experiencer DP c-commands into the embedded clause, as shown by tests such as Principle C (2a), pronominal binding (2b), and NPI licensing (2c) (Collins 2005a:290, McGinnis 1998:201, Pesetsky 1995:105).

(2) a. *John seems to her$_i$ to like Mary$_i$.
b. Mary seems to every boy$_i$ to like all of his$_i$ jokes.
c. Mary seems to no boy$_i$ to like any of his$_i$ jokes.

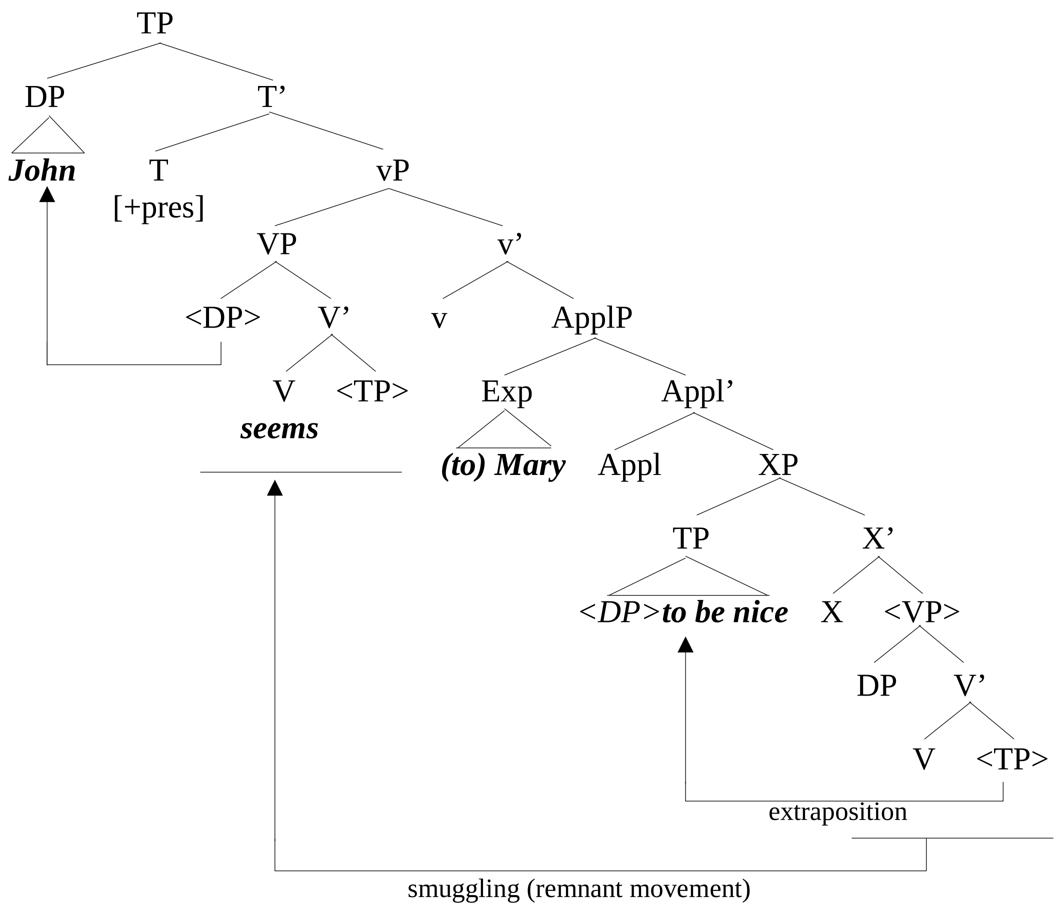
If the experiencer and the subject are both DPs, and the experiencer c-commands the A-trace, then movement of the subject to the matrix subject position would cross over the position of the intervening experiencer argument, a straightforward violation of MLC/RM. In fact, this problem, which Boeckx (1999) labeled the '*experiencer paradox*', is a long-standing puzzle in formal syntax, for which various solutions have been proposed (e.g., Boeckx 1999, 2008; Collins 2005a, Epstein, Groat, Kawashima & Kitahara 1998; Gehrke & Grillo 2008; Grillo 2008; McGinnis 1998).

One such proposal is that intervention effects in adult grammar can be voided through a "smuggling" mechanism. However, it is possible that this mechanism (and subsequent extraction) becomes accessible to children only at later stages in linguistic development (Snyder & Hyams 2015; see also Hyams & Snyder 2005, 2006). Snyder and Hyams adopt Collins' (2005a) smuggling account of StSR, according to which the subject of the embedded clause, XP in (3), moves past the experiencer, ZP, inside a larger piece of verbal structure, YP. This smuggling operation is illustrated in detail in (4) (from Collins 2005a:295). Here, the subject DP '*John*' undergoes a series of movements: starting from its original position in the Spec of AP '*nice*',[2] it moves first to the Spec of the TP '*to be nice*' and then to the Spec of the VP headed by the verb '*seem*'. The embedded TP 't *to be nice*' is then extraposed to the specifier position of a functional XP, and the VP '*John seems t*' undergoes remnant movement to the Spec of vP, smuggling the subject '*John*', and allowing for it to raise to the Spec of TP. In this way a locality (e.g. RM) violation on movement is circumvented.

(3) XP [$_{YP}$ ~~XP~~ …] . . . ZP . . . ~~[$_{YP}$ XP …]~~

[2] For clarity of presentation, the tree in (4) does not include these first steps described in the text. The AP projection is embedded in the bottom TP '*to be nice*' shown in the tree. We assume this TP to have the following representation: [$_{TP}$ John$_i$ to [$_{VP}$ be [$_{AP}$ t_i nice]], and that 'John' moves from Spec-TP to the Spec-VP headed by 'seem' (shown at the bottom of the tree in (4)) before the TP 'to be nice' moves to Spec-XP.

(4) John seems to Mary to be nice.



Snyder and Hyams follow Collins in assuming that smuggling represents an exception to the Freezing Principle (Wexler & Culicover1983) which rules out movement from an already moved phrase. This is precisely the operation involved in smuggling. A recent reformulation of the Freezing Principle is given in (5) (from Müller 1998:124).

(5) *XP [$_{YP}$ … <XP> …] <YP>.

Appealing to these processes of smuggling and freezing, Hyams and Snyder's Universal Freezing Hypothesis (UFH) claims that while adult grammar permits exceptions to the Freezing Principle,[3] the immature child's grammar has 'zero-tolerance' in this regard (6).

[3] On the basis of adult cross-linguistic data, Bošković (2018, this volume) independently shows that adults effectively allow exceptions to the Freezing Principle as traditionally defined. He thus concludes that the relevant problem does not arise when an XP moves from a moved YP but with the movement of YP itself; i.e., moving YP does not freeze the internal structure of YP for movement; it is successive-cyclic movement of XP to the edge of YP that prevents movement of YP.

(6) *Universal Freezing Hypothesis* (UFH): For the immature child (at least until age four), the Freezing Principle *always* applies: No subpart of a moved phrase can *ever* be extracted.[4]

As a consequence, 'immature' children will have difficulties with structures that require smuggling – such as StSR sentences and verbal passives (Collins 2005a,b). Importantly, the UFH predicts that children's comprehension of A-movement structures is problematic only when an intervening argument is structurally present and not otherwise. Snyder and Hyams (2015) (see also Snyder et al. 1995) show that children easily acquire unaccusative structures such as the impersonal *se/si* constructions in French and Italian, demonstrating that A-movement itself is not delayed in acquisition (cf. Borer & Wexler 1987; see also Friedmann 2007; Shimada & Sano 2007, a.o.).

It is important to mention that the morphosyntactic features shared between the moved element and the intervener are also crucial. Under Relativized Minimality (RM, Rizzi 1990, 2004) intervention effects are triggered when the moved element and the intervening argument share some relevant morphosyntactic feature that plays an active syntactic role, e.g., that can trigger movement. Predictably, children do better with free object relatives than headed object relatives in which the subject and moved object are both NPs (Friedmann, Belletti & Rizzi 2009). They also do significantly better when the subject and object are NPs but mismatch with respect to number in Italian (Adani 2010), gender in Hebrew (Belletti, Friedmann, Brunato & Rizzi 2012) or case in German (Arosio, Yatsushiro, Forgiarini & Guasti 2012). In the two experiments discussed here, all relevant arguments are NPs and are thus predicted to trigger a RM violation unless some by-pass mechanism is assumed.

Orfitelli (2012) proposes an account that abstracts away from the specific details associated with smuggling and freezing to explain children's delay both in StSR and in verbal passives. Like the UFH, her *Argument Intervention Hypothesis* (AIH) claims that children have difficulty with structures that require A-movement across a structurally intervening argument, for example, the experiencer argument in *seem*-type raising predicates. While Orfitelli is agnostic with respect to the smuggling hypothesis per se, her experimental results show that the same children who fail with STSR *seem* are adutlike in interpreting raising structures that do not have an intervening experiencer argument (e.g., *tend, be going to, be about to*). Because these structures do not require smuggling even "frozen" children do well. Relatedly, Mateu (2016) tested English-speaking children on StSR with *seem* and Spanish-speaking children on StSR with *parecer* 'seem', a modal-like functional verb (Ausín & Depiante 2000; Ausín 2001; Fernández Leborans 1999; Torrego 1996, 1998, 2002) that, unlike *seem*, does not select an experiencer argument. Using the same materials and procedure, she found that English-speaking children have difficulties interpreting StSR *seem* sentences until age 6, even when the experiencer is *not* overtly realized, while Spanish-speaking children perform at ceiling on StSR with functional *parecer* by age 4. This behavioral difference suggests a more complex syntactic derivation for English *seem*, namely, one in which

[4] The actual age at which children acquire smuggling is a bit of a moving target. Snyder and Hyams (2015), based on a review of several early studies of passive acquisition and raising, give age 4 as the lower bound at which smuggling is acquired (see 6). However, not all children develop smuggling at age 4. For example, various studies have found that many children do not master verbal passives until age 6 or so (see Orfitelli, 2012, for an extensive review), while other studies have found limited, but early production of passives (e.g., Contemori & Belletti, 2014; Manetti & Belletti, 2015). Methodological differences, as well as the thorny issue of deciding what criteria to use in determining "age of acquisition" may account for some differences. We return briefly to the issue of age at the end of Section 2.

the experiencer argument of English *seem* is always structurally present (Orfitelli 2012, Snyder & Hyams 2015) and therefore requires smuggling in cases of StSR.

Additionally, two different studies have linked children's performance on StSR *seem* sentences and verbal/non-actional passives. Hirsch and Wexler (2007) investigated children's comprehension of StSR *seem* with an overt experiencer phrase and non-actional verbal passives with an overt *by*-phrase. They examined 53 English-speaking children aged three to nine and found that there was a very high significant correlation between the scores in the two constructions, $r(51) = 0.799$, $p < 0.0001$. Similarly, Orfitelli (2012), compared children's comprehension of non-actional passives and StSR *seem* (with an implicit experiencer). She found a near perfect correspondence in above chance performance on the two constructions. Both the UFH and the AIH predict this correspondence because both StSR *seem* and verbal passives involve A-movement over an intervening argument (experiencer or *by*-phrase). In particular, the correlation between individual children's performance on *seem* and passives – two constructions directly implicating smuggling (Collins 2005a,b) – seems to provide strong *prima facie* support for the UFH, viz. the claim that children's difficulty with StSR *seem* is an inability to smuggle (see Snyder & Hyams 2015).

2.1.2. Processing-based accounts

Another way to explain children's difficulty with interveners is by appeal to performance-based limitations that impinge on the proper functioning of an adult-like grammar system. Under this type of account, the difficulty with StSR (7a) is not unique to this construction, but reflects a processing difficulty associated with various structures that involve crossing dependencies, or "interveners".

Indeed, it has been reported that children up to the age of six do poorly with various "intervention-type" constructions, including: *(non-actional) passives* (7b) (Gordon & Chafetz 1990, Hirsch & Wexler 2006b, Maratsos et al. 1985, Orfitelli 2012); *subject control* with *promise* (7c) (C. Chomsky 1969, Hsu, Cairns & Fiengo 1985, Sherman & Lust 1993); *tough*-movement (7d) (Anderson 2005, C. Chomsky 1969); *object relatives* (7e) (Friedmann et al. 2009, Friedmann & Novogrodsky 2004); *object wh-questions* (7f) (Avrutin 2000, de Vincenzi, Arduino, Ciccarelli 1999; Friedmann et al. 2009); and *object topicalizations* (7g) (Friedman & Lavi 2006).

(7)
a. StSR *seem*: The boy seems to the girl __ to be nice.
b. Passive: The boy was loved by the girl __.
c. SC *promise*: The boy promised the girl __ to leave.
d. *Tough*-movement: The boy is tough for the girl to please __.
e. Object relative: The boy who the girl kissed __.
f. Object *wh*-question: Which boy did the girl kiss __?
g. Object topicalization: The boy, the girl kissed __.

Choe (2012) (see also Choe & Deen 2016) adopts this kind of approach, which she refers to as the Performance-based Intervention Effects hypothesis (8), based on Gibson's (1998, 2000) Dependency Locality Theory (DLT; see also Grodner & Gibson 2005; Warren & Gibson 1999, 2002, 2005), originally proposed to explain intervention effects in adult language processing.

Importantly, in contrast to grammar-based accounts, which define intervention hierarchically, processing-based accounts generally define it in terms of linear word order.

(8) *Performance-based Intervention Effects* (PIE) *Hypothesis*: The delay in the acquisition of StSR is attributed to the increased processing cost associated with the presence of an *overt* NP between the raised NP and the gap in its original position.

Thus, according to PIE the delay in the acquisition of StSR is due to the general difficulty of computing an interpretative dependency between a DP, e.g. '*the boy*' in (7a), and the position with which it is associated when a DP intervenes between them, e.g. '*the girl*' (7a).

PIE makes several predictions, some of which remain untested. PIE (and DLT) predict processing cost to increase with the number of overt NPs ("discourse referents" in DLT terms) that *linearly* intervene between the filler and the gap. Therefore, we should expect a significant decrease in performance in (9b) with respect to (9a), because it includes an additional DP, '*the ponytail*'.[5]

(9) a. The boy seems to the girl __ to be nice.
b. The boy seems to the girl with the ponytail __ to be nice.

Moreover, if processing limitations are a significant factor in children's difficulty in StSR, correct comprehension of StSR sentences should be positively correlated with an independent measure of processing capacity. Some studies of the acquisition of object relative clauses have in fact found a correlation between children's comprehension of object relative clauses and different measures of working memory or short-term memory capacity (e.g., Arosio, Adani, & Guasti 2009, Felser, Marinis & Clahsen 2003, Roberts, Marinis, Felser & Clahsen 2007). So, there is at least some evidence that memory resources are implicated in the delays associated with intervention-related constructions. PIE attributes children's difficulties with StSR *solely* to processing deficits and sees no role for the grammar. However, it is also possible that both grammatical *and* processing effects influence the development of StSR *seem*. The experimental results presented in this chapter will show that this is indeed the case.

Finally, processing-based accounts like PIE predict a parallel development between StSR *seem* and other constructions that involve crossing dependencies, such as SC *promise*. As we will discuss in greater detail in Section 3, depending on the specific assumptions one makes about how SC *promise* is syntactically derived, some grammar-based accounts also predict a correlation between the acquisition of StSR *seem* and SC *promise*. Hence, our experimental study was designed to directly compare children's development of these two constructions. This last prediction will be discussed and addressed in Sections 3 and 4.

[5] Note that Gibson's memory-based approach to the dependency-length effect is not at odds with other processing approaches. Any theory in which at least one component of dependency construction is length sensitive would predict a contrast between the sentences in (9) as well (see Hawkins, 1994; McElree, 2000; O'Grady, 1997, a.o.).

2.2. Study 1: StSR

The goal of our first experiment is to investigate whether grammatical or processing deficits are behind children's difficulties with StSR. As just outlined, under PIE, we expect increased difficulties with StSR when the intervening experiencer includes an embedded DP, lengthening the distance between the filler and the gap. Additionally, if the difficulties that we observe in StSR are due to processing limitations, we expect to find that children's performance on this construction is positively correlated with an independent measure of verbal processing capacity. The predictions for this experiment are outlined in Table 1.

Table 1. Predictions for StSR *seem* experiment.

Prediction	**Grammatical Accounts (AIH, UFH)**	**Processing Accounts (PIE)**
Performance on StSR with *seem* with longer intervening experiencer arguments (DP[PP]) vs shorter ones (DP).	Same	Worse
Relationship between StSR and independent measure of working memory	No (necessary) correlation	Positive correlation

2.2.1. Methods and procedure

Thirty English-speaking children aged four to six years old (range = 4;2-6;7, *M* = 5.53) and 10 adult controls participated in a Truth-Value Judgment Task (TVJT, Crain & McKee 1985) that investigated comprehension of *seem.* In this procedure, the child observes a story and a puppet then comments on it. The task of the child is to indicate whether the puppet commented truthfully or not, and why. There were 4 main conditions relevant to this study: copula, unraised, raised with a short experiencer DP, and with a long experiencer DP+PP (Table 2). Children were also tested on StSR *seem* with an implicit experiencer; however, for reasons of space, we will focus our attention on the four conditions in Table 2. There were six trials per condition and six unique scenarios were used to keep children engaged, each one consisting of a short story and two pictures. The design and stories resemble those employed in Becker (2006), Hirsch et al. (2007), and Orfitelli (2012). The images were created by the first author using Pixton (Pixton Comics Inc. 2015). An example story (10) and set of pictures (Figure 1) are given below. Moreover, in order to avoid carry-over effects and minimize the number of experimental sessions, we interspersed the StSR and SC trials and divided the task into two balanced sessions. However, for expository purposes, we present the different parts of the experiments and their results separately.

Children needed to correctly answer at least 5/6 items in the copula and unraised conditions in order to be included in the study.[6] The copula condition was used to verify children understood

[6] Data from seven children were excluded because they failed to correctly answer at least 5/6 trials in one of the two control conditions. We report on the results from the remaining 30 children.

the real, permanent state of the main character. [7] Also, inclusion of the copula condition allowed us to determine if children were using a "copula strategy" by comparing their responses in this condition to their responses in the raised *seem* condition. Under a copula strategy their responses in the two conditions would be the same. As discussed above, previous studies found that children often assign a copula interpretation when they fail to understand raising *seem* sentences (e.g., Hirsch 2011, Orfitelli 2012). The unraised condition served to verify that children understood the meaning of the verb *seem*. Finally, in order to ensure that the PP in the 'long experiencer' conditions was felicitous, the two potential experiencers in the story were of the same type (e.g. both were cats), but the experiencer had an additional trait (e.g. stripes).

Table 2. Test conditions and examples for the StSR experiment.

Condition	**True Test Items**	**False Test Items**
Copula	The dog is definitely white.	The dog is definitely gray.
Unraised *seem*	It seems that the dog is gray.	It seems that the dog is white.
StSR *seem*, short exp.	The dog seems to the cat to be gray.	The dog seems to the cat to be white.
StSR *seem*, long exp.	The dog seems to the cat with stripes to be gray.	The dog seems to the cat with stripes to be white.

(10) Narrator: *This is a story about a dog and a parrot. The dog is talking to his parrot friend when he sees a gray light and he mentions that when he stands under it people think he's gray. He decides to walk under it and just then, his cat friend comes by and says "Oh! I thought you were white! Maybe I was wrong, maybe you're gray!" What happens in this story?*
Puppet: *I know what happens in this story! The dog seems to the cat to be gray.*
Target: *True.*

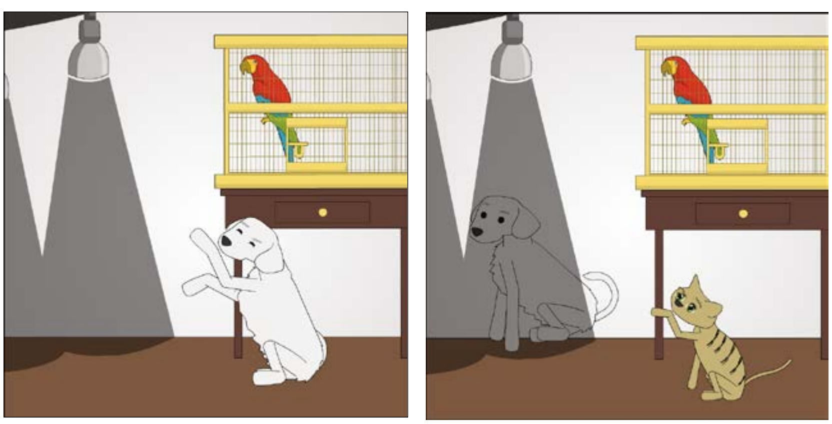


Figure 1. Experiment sample pictures for the 'dog seems to be gray' scenario: The left picture shows reality – the dog is white. The right picture shows appearance – the dog seems to be gray.

[7] The inclusion of *definitely* on the copula condition follows Hirsch, Orfitelli & Wexler (2008), as a way to disambiguate between a stage- versus individual-level predicate reading of the copula, i.e. in order to rule out the interpretation in which adults would accept that the dog *is* gray *when he stands under the light*.

To assess verbal processing capacity, we administered a developmentally appropriate version of the Competing Language Processing Task (CLPT, Gaulin & Campbell 1994), which is a listening span test. In this test, children are presented with a series of sentence sets of increasing number. Children are asked to recall as many sentence-final words as possible. Additionally, to ensure they are processing the sentence, as opposed to focusing only on the last word, they are asked to judge each sentence as True or False. The task included three levels and had three trials per level. The task included a total of 18 sentences (and words to remember), three for Level 1 (one word to remember at the end of each on-sentence trial), six for Level 2 (two words to remember at the end of each two-sentence trial), and nine for Level 3 (three words to remember at the end of each three-sentence trial). Table 2 provides one example trial for each of the three levels. Sentence length, grammatical complexity, and vocabulary level were held constant across the three levels of the task. Participant's CLPT score was the number of sentence-final words he or she could recall with a correct True/False answer.

Table 3. Three example trials (one for each of the three levels) of the CLPT.

Level	**Test Items in English**	**T/F**	**Target words**
1	Cows have wings.	F	wings
2	Mice eat cheese.	T	
	Babies drive cars.	F	cheese, cars
3	Bees make honey.	T	
	Zebras have stripes.	T	
	Fish drink milk.	F	honey, stripes, milk

2.2.2. Results and Discussion

Our results, provided in Figure 2, show first that children did very well with the unraised *seem* and copula trials, scoring 93.89% (*M* = 5.63/6) and 97.22% (5.83/6) respectively. However, we see that they performed rather poorly in both the StSR *seem* trials, scoring 56.11% (*M* = 3.37/6) in the short experiencer condition and 48.33% (*M* = 2.9/6) in the long experiencer condition. The high scores on unraised (and the copula) show that the children understood the meaning of the verb *seem*, and hence that their poor performance in the StSR trials cannot be attributed to a lack of lexical knowledge.[8] Rather, our results are consistent with the hypothesis that children experience difficulties only when there is movement over a structurally intervening argument.[9] The adults in our study did well on all conditions, as expected.

[8] The results also show that they were not adopting a copula strategy to interpret *seem* in unraised contexts (Hirsch et al. 2007, Orfitelli 2012) since the correct answer to the copula trials was the opposite of the unraised *seem* condition and children scored virtually at ceiling on both constructions.

[9] Strictly speaking, these results show that children do badly with raising, as the control is an unraised structure. That their problem is specifically with raising over an intervener comes from other studies such as Orfitelli (2012) and Mateu (2016) discussed above (Section 2.1.1).

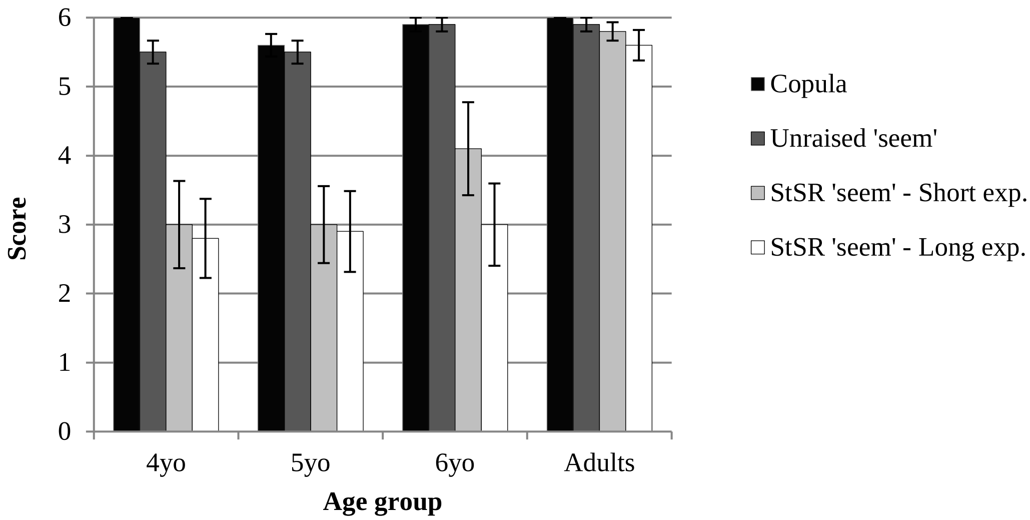


Figure 2. StSR experiment results by age group and condition.

Importantly for processing-based accounts such as PIE, we found no significant difference between children's performance on the short experiencer condition and the long experiencer condition. These results were interpreted using a mixed ANOVA with score as the dependent variable, condition (copula, unraised, StSR short, StSR long) as a within-subjects variable, and age group (4, 5, or 6) as the between-subjects variable. We found a main effect of condition, $F(1.775, 47.918) = 48.166$, $p < 0.001$, $\eta_p^2 = 0.641$, but no main effect of age, $F(2, 27) = .776$, $p = .47$, $\eta_p^2 = 0.054$, or interaction of condition and age, $F(3.549, 47.918) = .581$, $p = 0.658$, $\eta_p^2 = 0.041$. Post-hoc tests with Bonferroni correction for multiple comparisons revealed a significant difference between the copula condition and the two raised conditions, $p < .001$, and between the unraised condition and the two raised conditions, $p < .001$. No difference was found between the copula condition and the unraised condition, $p = 0.62$, or crucially, between the StSR short and long experiencer conditions, $p = .546$. Additionally, we found no significant correlation between children's performance on StSR and the CLPT, $r_s (28) = .312$, $p = .093$. As observed in Figure 3, some children had good verbal processing capacity, obtaining a score of 10 or more out of 18 in the CLP task and yet they performed very poorly with StSR, performing below chance or scoring 2 or less out of 12 (top left corner of Figure 3). This is not predicted under processing accounts that claim that the sole cause of poor performance with StSR (and other 'intervention' constructions) is low verbal processing capacity.

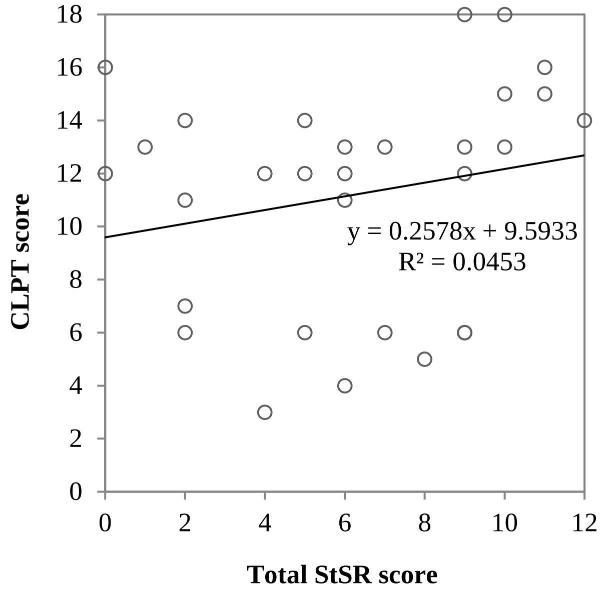


Figure 3. Correlation between CLPT scores and StSR scores.

Therefore, our overall results fail to support the predictions of processing-based accounts such as PIE (Choe 2012; Choe & Deen 2016) and are in line with the UFH (Hyams & Snyder 2005, 2006; Snyder & Hyams, 2015). Specifically, our results are consistent with the idea that young children do not have access to the smuggling and/or subsequent extraction operation that adults use to circumvent the intervener.[10] This results in an inability to reconstitute the lower VP (e.g., *The dog seems gray*) due to the presence of the experiencer (e.g., *(to) the dog*). Children therefore rely on compensatory heuristics such as the copula strategy (e.g. *the dog is gray*) which also involves ignoring the intervening material (i.e., *seem* and its experiencer argument). This strategy would generate consistent below-chance performance in StSR trials because the permanent state of the characters was always the opposite of what they seemed to be. Alternatively, children who have not yet acquired smuggling may fail to parse the sentence all together and may simply guess the answer. This would yield chance performance.

Closer inspection of our data reveals a more complex picture, however. In contrast to some previous studies where children performed either consistently below or above chance (Hirsch et al. 2007, Orfitelli 2012), in our study we found a large proportion of children who obtained a score that was statistically at chance level, i.e., between 3/12 and 9/12 according to a binomial test.

[10] A reviewer points out that our results are also compatible with other difficulties children may have with the smuggling derivation illustrated in (4). The problem could be (i) extraposition, (ii) the identification of the relevant XP, or (iii) movement to Spec-vP. However, verbal passives involve no extraposition or movement to vP (according to Collins 2005b) and yet a significant positive correlation in performance has been found between verbal passives and StSR (Hirsch & Wexler, 2007; Orfitelli, 2012). Difficulties identifying the relevant XP could explain children's low performance with StSR and passives, but the predictions of this hypothesis would be the same as PIE, i.e., a correlated pattern of results between StSR and SC. As we will see below, this is not the case. Nevertheless, while we believe a core part of children's difficulty is related to the smuggling and subsequent extraction operation, we do not exclude the possibility of there being additional steps that are challenging at some level.

Interestingly, however, their response justifications suggested to us that children in this at-chance group were not simply guessing. Rather, they would sometimes interpret StSR *seem* as a copula (and ignoring the experiencer) (11a), and at other times they would correctly interpret the sentences, which would entail A-movement across the experiencer argument (11b). The examples in (11) show that while this ‘chance’ child seems to be using a copula strategy in some trials (e.g.,11a), she is nevertheless able to obtain an adult-like interpretation of StSR in other trials (e.g., 11b).

(11) a. Puppet: *The dog seems to the cat with stripes to be white!* [F]
C Child: *True […] because the dog is really white.* [T]
b. Puppet: *The dog seems to the cat to be white!* [F]
C Child: *Funny […] because the dog is white but he looks like he's gray when he stands under the light.* [F]

We take this type of response justification as potential evidence that the at-chance children have access to the *grammatical machinery* to by-pass the intervener, i.e., smuggling, but do not have the *processing wherewithal* to do so. We therefore hypothesize that we are dealing with a heterogeneous group of children as outlined below:

A. Children that consistently performed below chance do not have the grammatical mechanism to by-pass the experiencer intervener. They are “frozen” in Snyder and Hyams’ terms.
B. Children that performed at chance and above chance have an adult-like grammar but still experience difficulties due to limited processing resources. That is, smuggling is part of their grammar, but they do not have the processing resources to implement this operation or parse complex sentences that involve crossing dependencies.

A child who does not have access to the smuggling operation will likely show consistent poor performance in StSR trials. On the other hand, a child whose problem is processing-based may show more variable performance, insofar as processing “bottlenecks” can be caused by many different experimental, contextual, and psychological factors that may vary throughout one experimental session.

To investigate this ‘heterogeneous group hypothesis’, we separated out the two subgroups – A and B – and recalculated the Spearman rank order correlation coefficient on each sub group. Looking first at sub-group B (chance and above-chance children) we indeed found a significant correlation between their scores on the raising conditions and the CLPT, $r_s(21) = .576$, $p = .004$, as we would expect if the difficulty for these children is based in limited verbal processing capacity. On the other hand, no positive correlation was found for the Group A (below- chance children). These findings support the hypothesis that we are indeed dealing with two groups of children, and that in Figure 3 the data points from the below-chance children were strongly influencing the slope of the regression line, masking this positive correlation in the other sub-group of children. Moreover, if the chance and above-chance children (Group B) have a processing deficit, we also expect a significant difference in their performance on short vs. long experiencer conditions. This prediction is confirmed by a mixed ANOVA with score as the dependent variable, condition (short, long experiencer) as a within-subjects variable, and age group (4, 5, or 6) as a between-subjects variable. Only the main effect of condition was significant, $F(1, 20) = 5.182$, p

= 0.03, η_p^2 = 0.213. The same model (with only two levels for condition) with all children included did not show a main effect of condition, $F(1, 27) = 3.073$, $p = 0.09$, $\eta_p^2 = 0.102$.

A final remark concerns the age at which children acquire the smuggling+extraction operation. As noted above, we did not find a main effect of age. Two 4-year-olds, three 5-year-olds, and two 6-year-olds performed below chance. Their average age was 5.5. Conversely, one 4-year-old, one 5-year-old, and four 6-year-olds performed above chance, average age 6 years old. The majority of children performed at chance (n = 17/30, average = 5.4 years old). This suggests great variability in terms of both grammatical and performance development. Importantly, however, it is entirely in line with the idea that some, if not most children have smuggling in their grammar by age 4 (i.e., 80% of 4-year-olds in our study) (as proposed under the UFH, see (6)), but have difficulties related to immature verbal processing capacity for quite some time after. But by age 6, about half of all the children perform above chance with StSR, i.e., they have the adult grammar ***and*** the processing wherewithal to do well with StSR.

Summarizing, we have found some evidence for processing-based difficulties associated with StSR past an experiencer. Crucially, however, these processing difficulties are only visible in the one group of children, those who by our hypothesis have a grammar that licenses A-movement over an intervener, which entails the acquisition of smuggling. Intervention accounts based uniquely on processing limitations (Choe 2012, Choe & Deen 2016) cannot account for the full range of our data. Let us turn now to children’s development of SC *promise*.

3. Subject Control *promise*

3.1. Background

Since C. Chomsky's (1969) pioneering work, a number of studies have investigated children’s acquisition of control structures in English (Adler 2006; Broihier & Wexler 1995; Cairns, Hsu, McDaniel & Rapp 1994; Goodluck 1981, Hsu, Cairns & Fiengo 1985; Landau & Thornton 2011; McDaniel, Cairns & Hsu 1991; Sherman & Lust 1986, 1993). An established result is that while children generally master subject and object control into verbal complements by age 3;6-4;0, they persist in incorrectly assigning object control with the verb *promise* until they are 6 or 7 years old.

Interestingly, and similar to the case of StSR *seem*, SC *promise* appears to represent a violation of the MDP (Rosenbaum, 1967), now also subsumed under RM (Rizzi 1990, 2004). Evidence showing that the benefactive DP c-commands into the embedded clause is provided by tests such as Principle C effects (12a), pronominal binding (12b), and NPI licensing (12c).

(12) a. *John promised her_i to take $Mary_i$ to the dance.
b. Mary promised every $student_i$ to meet his_i parents.
c. Mary promised no $student_i$ to correct any of his_i unnamed assignments.

A number of syntactic and semantic analyses have attempted to derive the exceptional control behavior of *promise* (Belletti & Rizzi 2013, Boeckx & Hornstein 2003, Bowers 1993, Hornstein 1999, Jackendoff & Culicover 2003, Landau 2015, Larson 1991, Sag & Pollard 1991). Some of these accounts make specific predictions relevant to language acquisition by arguing that the mechanism that is required to circumvent the intervener is not yet available to children. In the following sections we discuss two different grammatical accounts, neither of which has been tested yet, as well as a potential processing-based account.

3.1.1. Grammar-based accounts

Boeckx and Hornstein (2003), Boeckx, Hornstein, and Nunes (2010) and Hornstein and Polinsky (2010) propose a grammatical account for children's difficulties with SC *promise*. According to these authors, the benefactive of *promise*, e.g., '*his mom*' in (13a), is headed by a null preposition, similar to the one observed in other SC verbs such as *swear* (13b) or *commit*, and as observed in other languages, such as Spanish *prometer* 'promise' (13c).

(13) a. The boy$_i$ promised P_{null} his mom$_k$ PRO$_i$ to be loyal and honest.
b. The boy$_i$ swore to his mom$_k$ PRO$_i$ to be loyal and honest.
c. *El chico*$_i$ *prometió/ juró a su madre*$_k$ PRO$_i$ *ser leal y honesto*.
The boy promised/ swore to his mother to.be loyal and honest
'The boy {promised/swore to} his mother to be loyal and honest'

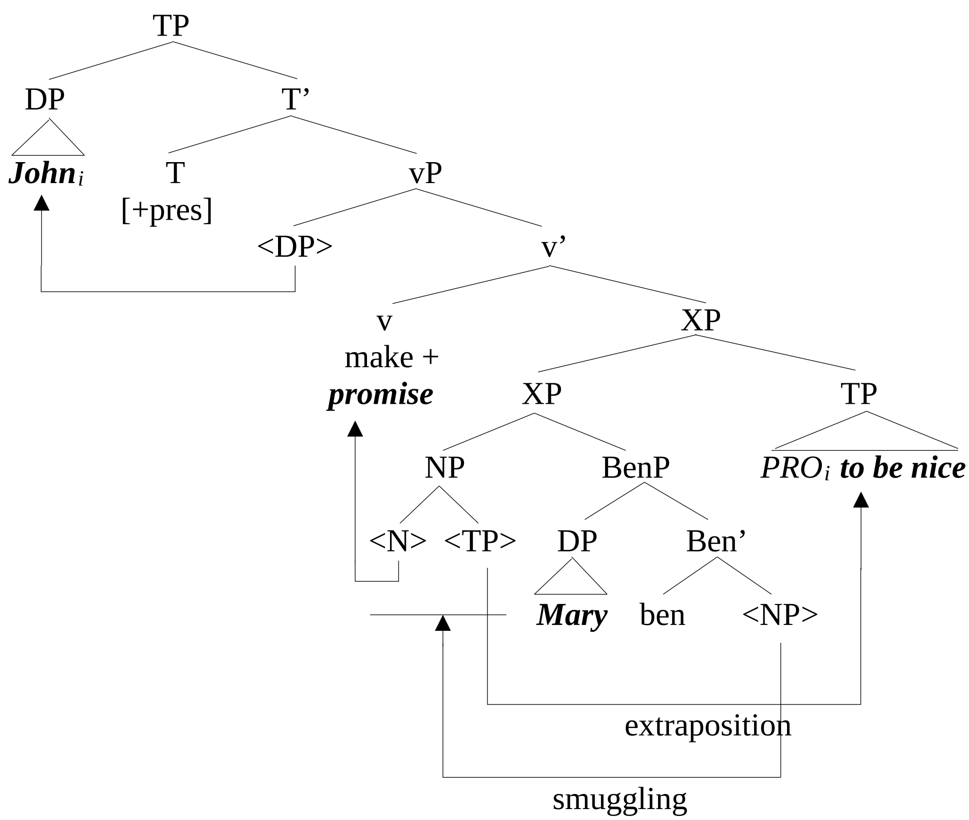
They claim that English-speaking children have difficulty assigning subject control with *promise* because they fail to represent the P_{null}, which would prevent the benefactive DP from being a potential intervener for control of PRO. We label this account the *Null Preposition Hypothesis* (NPH). This hypothesis thus predicts that children will be more likely to incorrectly assign object control with *promise* than with *swear*-type verbs due to the opaqueness of the prepositional complement structure in the former. The extant literature on the acquisition of subject control with *promise*-type verbs is limited to the English verb *promise* (and *ask*) and predates by decades recent syntactic theories of control (C. Chomsky 1969, Hsu, Cairns & Fiengo 1985). We will test this hypothesis experimentally by comparing English-speaking children's comprehension of subject control sentences with *swear,* which has an overt preposition and hence a transparent PP structure, to their performance with analogous *promise* sentences. Additionally, as proponents of NPH also adhere to the Movement Theory of Control (Hornstein 1999, *et seq.*), according to which obligatory control is derived by A-movement, on this account the embedded and matrix subject positions in sentences such as (13) are A-chain connected. We thus expect a parallel development of StSR *seem* and "StSR" *swear*.

Belletti and Rizzi (2013) propose a different grammatical account which builds on the idea that SC *promise,* like StSR *seem*, undergoes a smuggling-type process that renders the subject the closest controller for PRO. This analysis is based on lexical decomposition of the verb-structured meaning à la Hale and Keyser (1993) (see also Folli & Harley 2007, Travis 2000). According to the authors, *promise* and *order* allow paraphrases with light verbs plus nominal elements, as exemplified in (14).

(14) a. John promised Mary to be nice. > John made Mary the promise to be nice.
b. John ordered Mary to be nice. > John made Mary have the order to be nice.

Here *order* would incorporate first into light verb v_{have} and then into v_{make}; the object *Mary* would be the closest potential controller for PRO in the derived representation, hence we have object control in this case. In the case of *promise*, *Mary* is a kind of benefactive, and this relation is mediated by a benefactive (*ben*) particle-like functional head, comparable to the ApplP of raising structures (see tree in 4). Belletti and Rizzi propose that the locality problem can be circumvented by a leftward movement of the NP '*promise PRO to be nice*', which smuggles *promise* to a position suitable for incorporation, as in the tree in (15). At this point the noun *promise* can merge with the light verb *make*. The surface word order is obtained through extraposition of the TP '*PRO to be nice*'.[11] On this representation the benefactive *Mary* does not c-command PRO, hence it does not structurally intervene between the subject and PRO. Subject control thus obtains, as the subject is the closest potential controller.

(15) John v_{make} [Mary *ben* [promise [PRO to be nice]]]



Belletti and Rizzi argue that children are generally delayed with smuggling operations, as similarly proposed by Snyder & Hyams (2015) (see also Hyams & Snyder 2005, 2006). Since under Belletti and Rizzi's control analysis, SC with *promise* involves as much derivational machinery as StSR with *seem*, i.e., smuggling and extraposition (as per Collins' (2005a) analysis,

[11] The authors suggest that extraposition is made mandatory by the need to have *Mary* adjacent to the case assigning v. Note also that although they describe extraposition in traditional rightward terms, an antisymmetric analysis with double movement to the left (Kayne, 1994) would also yield the required configuration.

see (4)), the developmental course of the two constructions is predicted to be roughly the same, all else being equal.

3.1.2. Processing-based accounts

To our knowledge there is no performance-based account that has specifically addressed the protracted development of SC *promise*. However, a processing explanation along the lines of PIE would follow the same line of argumentation that applies to StSR *seem*. That is, the nature of the difficulty with SC would not be grammatical, but a reflection of processing limitations that are observed in all constructions that involve crossing dependencies, including StSR *seem*, verbal passives, and object relative clauses. Therefore, as discussed above, PIE would predict: i) a decline in performance if we increase the number of intervening NPs; ii) a positive correlation between performance on *promise* and an independent measure of verbal working memory; iii) a positive correlation between performance on StSR *seem* and SC *promise*.

3.2. Study 2: SC *promise*

The main focus of this study is to gain a better understanding of what properties of SC *promise* are particularly challenging for children and why. According to the NPH (Boeckx & Hornstein 2003, Boeckx et al. 2010, Hornstein & Polinsky 2010) children should have more difficulties comprehending SC with *promise* than with *swear* due to the difficulty of detecting the null preposition heading the benefactive of *promise* – while the benefactive of *swear* is headed by an overt preposition. Moreover, because these authors analyze obligatory control as raising, we should expect a positive correlation between children's performance on StSR *seem* and SC *swear*. Belletti and Rizzi (2013), on the other hand, claim that SC with *promise*-type verbs is derived via smuggling like StSR *seem*. They therefore predict children should show similar results with SC *promise* and StSR *seem*, all else being equal. Similarly, PIE (Choe 2012, Choe & Deen 2016) would predict a positive correlation between the two constructions. They also predict increased difficulties with SC as a function of the length of the intervening benefactive which increases the distance between the subject and PRO, as well as a positive correlation between their performance on SC and an independent measure of verbal processing capacity. Lastly, recall that the two representational hypotheses postulated for StSR, the UFH and AIH, do not necessarily predict a correlation between children's performance between StSR *seem* and SC *promise*.[12] The predictions relevant to this experiment are outlined in Table 4.

[12] The UFH proposes a smuggling analysis for StSR *seem* but is agnostic with respect to SC *promise.* Orfitelli (2012) defines the AIH as applicable to A-movement structures only. Therefore, the AIH would only predict a correlation with SC *promise* if one adopts the Movement Theory of Control Analysis. However, she makes no mention of this herself.

Table 4. Predictions for SC *promise* study

Prediction	**Grammatical Accounts**		**Processing Accounts (PIE)**
	NPH	**Smuggling (B&R)**	
Performance on SC *promise* vs SC *swear*	Worse	Same	Same
Performance on SC *promise* with longer intervening benefactive arguments (DP[PP]) vs shorter ones (DP)	Same	Same	Worse
Relationship between SC and independent measure of working memory	No (necessary) correlation	No (necessary) correlation	Positive correlation
Relationship between performance on SC and performance on StSR	Same[13] (*seem*, *swear*)	Same	Same

3.2.1. Materials and Procedure

The same 30 children and 10 adults that participated in the StSR experiment participated in the SC study. The same TVJT procedure (Crain & McKee 1985) was used. There were six conditions: object control with *tell*; subject control with *promise* without a benefactive, subject control with *promise* with a short benefactive, subject control with *promise* with a long benefactive, subject control with *swear* without a benefactive, subject control with *swear* with a short benefactive (Table 5), and there were six trials per condition. Six different scenarios were designed to keep children engaged, each one consisting of a short story and two pictures. The pictures were again created by the first author using Pixton (Pixton Comics Inc. 2015). An example story (16) and set of pictures (Figure 4) are given below.

The condition with OC *tell* served to ensure children were capable of deriving a control dependency with two arguments. The *promise* and *swear* conditions without a benefactive were used to verify that children had the appropriate lexical understanding of the verbs *promise* and *swear*. Additionally, because promising was depicted through the entwining of pinky fingers, at the first SC trial the experimenter would ask the child if he or she knew what a "pinky promise" or "pinky swear" was. Although the vast majority of the children said 'yes', the experimenter would then explain the meaning by saying: '*it's when you tell someone you are going to do something and you hook pinkies to show that you really mean it*'. Children were required to answer correctly at least 5/6 control trials, i.e., the OC *tell*, and the no benefactive condition trials. Lastly, in order to ensure the PP in the 'long benefactive' condition was felicitous, the two potential

[13] Proponents of the NPH analyze obligatory control as raising, and therefore predict a correlation between StSR *seem* and SC *swear* (both followed by '*to* DP' + non-finite TP). However, they would also predict these to be in an implicational relationship with respect to *promise*. That is, children that can establish a dependency over 'to_{null} DP' interveners should also be able to so with '*to* DP' interveners. Thus, if they do well on SC *promise* they should also do well on StSR *seem*.

benefactives in the story were of the same type (e.g. both were wizards), but the benefactive had an additional trait (e.g. had a cape).

Table 5. Test conditions and examples for the SC experiment.

Condition	**True Test Items**	**False Test Items**
OC *tell*	The wizard tells the knight to defend the castle.	The knight tells the wizard to defend the castle.
SC *promise*, no ben.	The knight seriously promises to defend the castle.	The wizard seriously promises to defend the castle.
SC *promise*, short ben.	The knight promises the wizard to defend the castle.	The wizard promises the knight to defend the castle.
SC *promise*, long ben.	The knight promises the wizard with the cape to defend the castle.	The wizard promises the knight with the cape to defend the castle.
SC *swear*, no ben.	The knight seriously swears to defend the castle.	The wizard seriously swears to defend the castle.
SC *swear*, short ben.	The knight swears the wizard to defend the castle.	The wizard swears the knight to defend the castle.

(16) Narrator: *This is a story about a wizard, a knight, and a princess. The princess discovers the castle is in danger, so the knight and the wizard have a conversation because one of them needs to defend the castle. They make an agreement with a pinky swear, then [...] the knight goes out to defend it. What happens in this story?*

Puppet: *Oh! I know what happens in this story! The knight promises the wizard to defend the castle.*

Target: *True.*

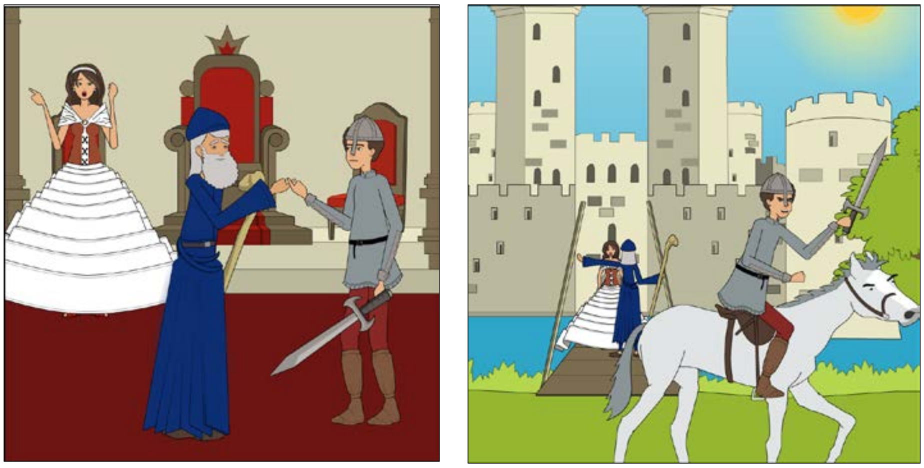


Figure 4. Experiment sample pictures for the 'The knight promises the wizard to defend the castle' scenario. The left picture shows the character making the promise. The right picture shows the character fulfilling his promise.

3.2.2. Results and Discussion

Our results show that, as expected, children have no difficulties correctly assigning OC with *tell*, scoring 92.22% (M = 5.53/6), or SC with *promise* and *swear* when there is no benefactive, scoring 95.5% (M = 5.73/6) and 92.17% (M = 5.53/6), respectively. However, they performed significantly worse in the short benefactive condition, at 46.17% correct (M = 2.77/6) with *promise* and 39.5% (M = 2.37/6) with *swear*.

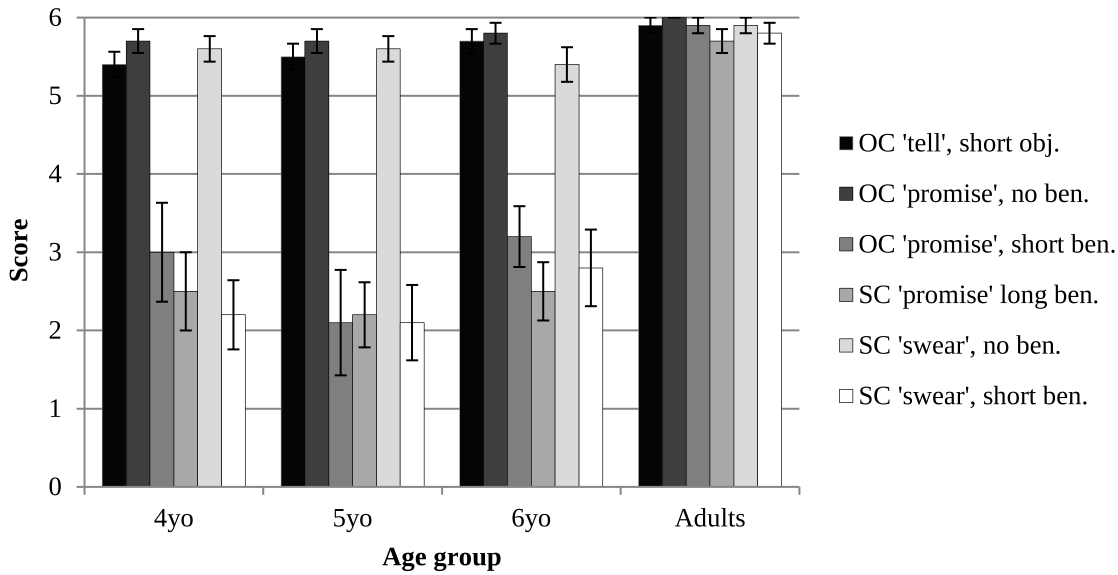


Figure 5. SC experiment results by age group and condition.

In order to test the significance of these results, we used a mixed ANOVA with score as the dependent variable, condition (OC *tell*, SC *promise*, SC *promise* short, SC *promise* long, SC *swear*, SC *swear* short) as a within-subjects variable, and age group (4, 5, or 6) as the between-subjects variable. We found a main effect of condition, $F(1.995, 53.862) = 80.946$, $p < 0.001$, $\eta_p^2 = 0.750$, but no main effect of age, $F(2, 27) = .693$, $p = .509$, $\eta_p^2 = 0.049$, or interaction of condition and age, $F(3.99, 53.862) =.662$, $p = 0.62$, $\eta_p^2 = .047$. Post-hoc tests with Bonferroni correction for multiple comparisons revealed no significant difference between the OC *tell* and the SC *promise* and *swear* conditions that had no benefactive, $p = 1$. Importantly, however, there was a significant difference between each of those three conditions and the ones that included a benefactive, i.e., SC *promise* with a short or long benefactive conditions as well as with SC *swear* with a short benefactive condition, all $p < .001$. That is, children performed significantly worse when there was an intervener. Importantly for the predictions we wanted to test, we found no difference between the SC *promise* and SC *swear* with short benefactive conditions, $p = 1$; that is, children performed equally poorly on *promise* (no overt P heading the benefactive) and on *swear* (overt P heading the benefactive), contrary to the predictions of the NPH.

Lastly, we found no difference in performance between the SC *promise* short and long benefactive conditions, p = 1. Children had comparable difficulties with short and long benefactives, contrary to the predictions of PIE. Additionally, we found no significant correlation between children's performance on SC (with a benefactive) and the CLPT, r_s (28) = .008, p = .964 (Figure 6). Therefore, as a whole, the results fail to support the predictions of the PIE as well, and are more in line with grammar-based accounts that do not predict such correlation with working memory (see Table 4).

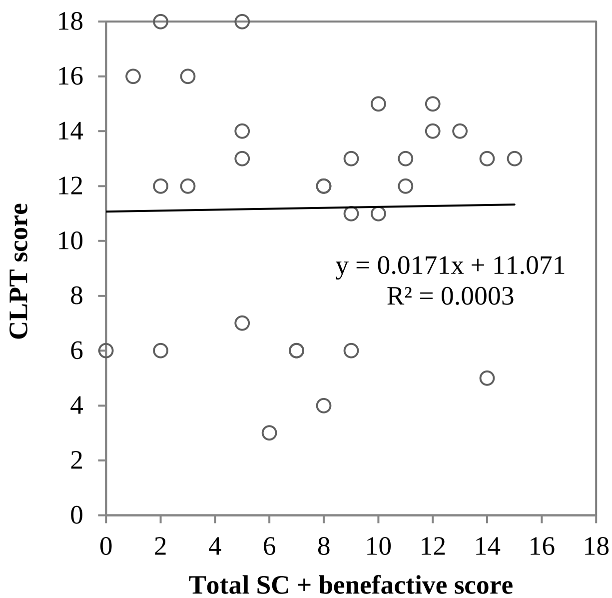


Figure 6. Correlation between CLPT scores and StSR scores.

However, we need to consider the possibility that processing deficiencies are in fact modulating the responses of some of the children, as was found in the StSR study. As observed in Figure 6, a large number of children obtained a score that was statistically within chance levels (between 6 and 12 correct responses out of 18). Also, as in the StSR experiment, the children's response justifications once again suggested that they were not guessing. Rather, they sometimes interpreted SC *promise/swear* as an OC verb like *tell* (17a), and at other times, they correctly interpreted the sentences, which would require the ability to establish a control dependency between the matrix subject and the lower PRO subject (17b).

(17) a. Puppet: *The policeman promises the firefighter to climb the ladder!* [F]
C Child: *So I think this is real […] because the man figured out the cat was in danger and the policeman wanted the fireman to climb the ladder.* [T]
b. Puppet: *The firefighter promises the policeman with the scarf to climb the ladder!* [T]
C Child: *So the guy with the scarf did not climb the ladder, but the firefighter was climbing the ladder. So I think that is real.* [T]

Given this evidence, we again hypothesize that the children do not form a homogenous group. Rather, for the children that performed below chance (<5/18) the problem is grammar-based, i.e., these children lack the adult representation and/or derivation of SC *promise*. On the other hand, for the children that performed at chance and above, the problem is processing-based, i.e. these children have the adult grammar but are limited in the memory resources needed to compute subject control over an intervener successfully. In order to test this hypothesis, we again divided up the two sub-groups and conducted parallel correlation analyses. Looking first at the chance and above-chance sub-group, we found a significant positive correlation between performance on SC *promise/swear* and CLPT score, r_s (17) = .583, p = .009. On the other hand, as expected, no correlation is found in the below-chance group, r_s (9) = .235, p = .486. Moreover, if the children who performed at and above chance with *promise* have a processing deficit, we also expect a significant difference in their performance on short vs. long benefactive conditions. This prediction was confirmed by a mixed ANOVA with score as the dependent variable, condition (short, long benefactive) as a within-subjects variable, and age group (4, 5, or 6) as a between-subjects variable and only including the children that scored at chance and above-chance in the two *promise* conditions (≥3/12). Only the main effect of condition was significant, $F(1, 19) = 11.492$, $p = 0.003$, $\eta_p^2 = 0.377$. The same model (with only two levels for condition) with all children included did not show a main effect of condition, $F(1, 27) = 3.251$, $p = 0.083$, $\eta_p^2 = 0.107$. Thus, as in the StSR experiment, we see support for a processing-based intervention account, but only for one group, the chance and above-chance children.

Summarizing the results so far, the children in our study show comparable difficulties assigning SC with *promise* and SC with *swear* when there is an intervening benefactive, contrary to the predictions of the NPH (Boeckx & Hornstein 2003 *et seq.*). Therefore, an undetectable null preposition heading the benefactive of *promise* is unlikely to be the root of the problem. On the other hand, we have some evidence for processing-based difficulties associated with SC past an argument, as predicted by PIE (Choe, 2012, Choe & Deen 2016). However, these processing difficulties only seem to affect one group of children, those who, based on their variable performance and response justifications, seem to have the adult rule-system that derives SC across an argument. The last question to be addressed is whether the grammatical mechanism used by the children to by-pass this argument in SC is the same as the one used for StSR, e.g., smuggling. The following section addresses this question.

4. Comparison of StSR *seem* and SC *promise*

As discussed in the previous sections, we hypothesize that the children in our two studies do not constitute a homogeneous class. The group of children that performed at chance and above sometimes comprehended these structures in an adult-like manner, performed significantly differently in the ‘short’ and ‘long experiencer/benefactive’ conditions, and crucially, their chances of correctly interpreting these structures is predicted by their verbal processing capacity. Thus, we attribute the poor performance on both StSR and SC of the ‘chance and above-chance’ children to non-grammatical, processing factors.

On the other hand, for the children that performed below chance in the StSR and SC conditions with an intervening argument, the problem seems to be grammatical. They consistently displayed non-adult-like comprehension and their responses were indicative of an inability to by-pass the intervener, that is, they either interpreted *seem* as a copula and ignored the experiencer in the case of StSR *seem* or interpreted the benefactive as the controller in the case of SC *promise*/*swear*. For this group, there are two possibilities: i) the grammatical delay with StSR *seem* has the same underlying source as for SC *promise* (e.g. no access to smuggling, as predicted by Belletti & Rizzi 2013) (see Table 4), in which case the children that performed below chance with StSR should be largely the same ones that performed below chance with SC; or ii) the grammatical delay with StSR *seem* is in fact not due to the same underlying source for SC *promise*, in which case the children that performed below chance with StSR with an intervener may not be the same children that performed below chance with SC with an intervener.

In order to investigate this question, we now compare the results of the StSR and SC TVJTs. We begin by testing the NPH which predicts parallel performance on 'StSR *seem* with a short experiencer' condition and the 'SC *swear* with a benefactive' condition (see table 4). Both these constructions have an intervening argument headed by a preposition. If children's difficulty with these two constructions is due in both cases to the same constraint (NPH) and both are derived via A-movement (per the Movement Theory of Control) then individual children should perform equally with the two constructions. Out of the five children that performed below chance in *either* of these conditions, i.e., those that answered correctly 0/6 trials according to a binomial test, only one child performed below chance in *both* conditions. The other four performed either below chance with StSR and at chance with SC (n = 2) or vice-versa (n = 2). We then conducted a Spearman's rho test comparing all 30 children's performance on these two conditions and found no significant correlation, r_s (28) = .138, p = .468. This strongly suggests that children are performing poorly in the different conditions for different reasons, i.e., different constraints are operating. Recall also that the NPH predicts a significant improvement in performance in the *swear* condition relative to the *promise* condition due to the overt presence of 'to' on the benefactive, and yet that was found not to be the case (see Section 3.2.2). Therefore, the predictions of the NPH are not borne out in our study (Boeckx & Hornstein 2003, Boeckx et al. 2010, Hornstein & Polinsky 2010).

Next, we test the predictions of the smuggling hypothesis (Belletti & Rizzi 2013). To do this we compare children's performance on StSR with *seem* collapsing across the 'short' and 'long experiencer' conditions, and SC with *promise* collapsing across the 'short' and 'long benefactive' conditions.[14] If the below-chance children's poor performance in StSR *and* SC is due in both cases to an inability to smuggle across the intervener they should perform poorly in both the constructions. In other words, if a child does not have Smuggling for the purposes of StSR, he/she should also not have Smuggling for the purposes of SC. Out of the 12 children that performed below chance in *either* of these conditions, i.e. those that answered correctly ≤2/12 trials according to a binomial test, only three children performed below chance in *both* conditions. The other nine either performed below chance with StSR and above chance with SC (n = 1), below chance with StSR and at chance with SC (n = 3), at chance with StSR and below chance with SC (n = 3), or above chance with StSR and below chance with SC (n = 2). That is, there were children who performed well with StSR but poorly with SC, and vice-versa. We also conducted a second Spearman's rho test comparing all 30 children's performance in these two constructions. The results indicate that there is no significant correlation between the two constructions, r_s (28) =

[14] Recall that there was no effect of length of intervener.

.222, p = .237, reinforcing the individual results just discussed. The data is illustrated in Figure 7.

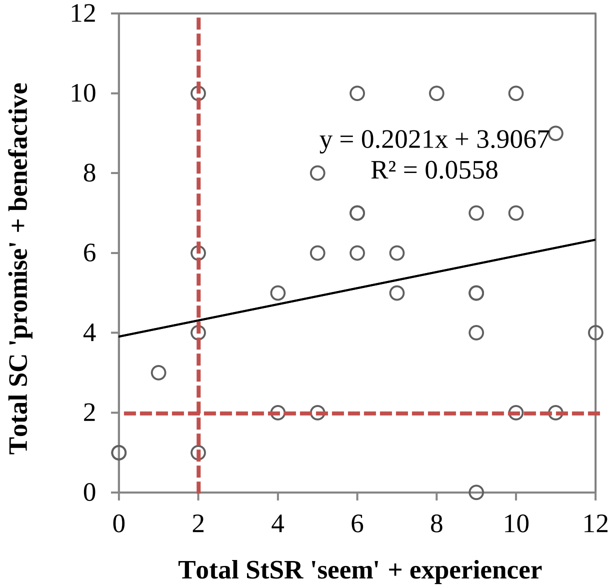


Figure 7. Correlation between the score obtained in the StSR *seem* conditions with an experiencer and the score obtained in the SC *promise* conditions with a benefactive (sample of 30 children). Discontinuous lines define the below-chance sections (≤ 2/12 score).

These results would seem to argue against the hypothesis that children's delays with StSR and SC stem from a common difficulty with smuggling, as per Belletti and Rizzi (2013). If that were the case, we would expect each individual child to perform similarly on the two constructions. This lack of correlation also strengthens our conclusion that processing limitations *alone* cannot be the cause of children's difficulties with StSR *seem* and SC *promise*, contrary to Choe's (2012) PIE hypothesis. The dissociation that we observe in our results leads us to believe that different grammatical mechanisms may be at play to deal with the intervening argument in StSR *seem* and SC *promise* and that these are independently acquired. We will continue to assume that the at-chance and above-chance (Group B) children in the StSR experiment, like adults, circumvent intervention in StSR *seem* sentences through smuggling, and that the below-chance (Group A) children have not yet acquired this grammatical escape hatch. Orfitelli (2012), though she does not adopt a smuggling analysis herself, provides strong prima facie support for this hypothesis. The individual children in her study (and also those in Hirsch and Wexler (2007)) showed a strong correspondence in their performance on StSR *seem* and verbal passives, the two constructions that Collins (2005a,b) ties to smuggling. Despite other structural differences between StSR *seem* and verbal passives, children who passed the raising test passed the passive test and conversely, those who failed on one failed on the other.

So, what about SC *promise* and its dissociation from the StSR *seem* results? One way of explaining the uncorrelated pattern of results is to assume that Smuggling is not involved in deriving SC with *promise* (cf. Belletti & Rizzi 2013). In this case, whether children perform poorly with StSR *seem* would be independent of their performance on SC *promise*. For instance, Jackendoff and Culicover (2003), and more recently, Landau (2015) show that the exceptional control properties of *promise*-type verbs are regulated by various complex semantic and pragmatic considerations that play out at the syntax-semantics interface. Children's difficulty with SC *promise* may reside in these interface properties.

Concretely, Landau proposes that obligatory control is achieved either through *predicative* control or through *logophoric* control, the former in complements to nonattitude verbs such as *manage, avoid, begin, force*, the latter in attitude complements to attitude verbs such as *want*, *believe*, *offer*, and *promise*. Predicative control involves simple syntactic predication and does not require any specific ingredient. Logophoric control, on the other hand, is established via predication *and* variable binding. Crucially, logophoric control the choice of controller is not grammatically encoded as it is in predicative control, but is determined by complex semantic and pragmatic factors.

A priori it seems entirely plausible that children's difficulty in assigning subject control with *promise* is related to the additional syntactic (and semantic) complexity involved in logophoric control. Children might simply default to the simpler syntactic representation of predicative control, and that would result in local (object) control.[15] Alternatively, it may be the case that children syntactically represent logophoric control appropriately, but that young children adopt a simpler pragmatically-based strategy such as choosing the controller who is deemed to be in a higher authority position. These hypotheses are yet to be tested. Importantly, logophoric control only occurs in non-finite complements (Landau 2015), and consequently, children's problems with *promise* would surface only in control complements and not when it takes a finite clause (consistent with our experimental results).

Although these ideas are admittedly speculative and our experimental results do not allow us to conclude that smuggling is not involved in SC *promise* in the adult (or child) grammar (cf. Belletti & Rizzi 2013), we believe that the most parsimonious interpretation of our data (as well as other StSR studies, e.g., Orfitelli 2012) is that children's grammatical difficulties with StSR with *seem*-type predicates have a different source from their difficulties with SC with *promise*-type verbs.

In addition to the constructions discussed in this chapter – StSR *seem*, verbal passives, and SC *promise* – this volume contains a number of potential lines of future research for acquisition. For instance, certain *tough*-movement analyses posit a smuggling derivation (e.g., Hicks 2009, Bošković, this volume). If that is the case, we should expect children to (i) show a protracted development of this construction, which has already been shown experimentally (Anderson 2005, C. Chomsky 1969, but cf. Becker, Estigarribia & Gylfadottir 2012) and (ii) show a correlated pattern of acquisition with other constructions that involve smuggling. Specifically, children who perform below chance with verbal passives and StSR *seem* should also perform below chance with *tough*-movement. This remains to be empirically addressed.[16] Similarly, in this volume Corver argues that *V by + Measure Phrase* patterns such as the one in '*John outweighs Bill* <u>*by two pounds*</u>'

[15] One prediction of this hypothesis is that children who analyze *promise* as predicative control should disallow implicit, partial and split control, and control shift.
[16] Note that C. Chomsky (1969) examined children's performance on *tough*-constructions and SC *promise* on the same 40 children. She includes a table with each of children's performance on each task (Table 5.1), however, she does not report on correlations and does not provide the raw results, only whether they did generally poorly, or generally well. Therefore, it is not possible calculate if there is a significant correlation between children's performance on those two constructions.

also involve Smuggling. As far as we know, this type of construction has not been tested in children. However, the prediction is that young children would also have difficulties comprehending *V by +MPs*, e.g., *who weighs more*, until they are approximately 6 years old. As a last example, we draw the same connection with *que* + clitic interrogatives in French, e.g., '*Que lui dit-elle?*' (lit. '*What to him says she?*', '*What is she saying to him?*'). According to Polletto and Pollock (this volume) this type of question structure also requires a smuggling derivation. As such, we predict children will have difficulties interpreting them, e.g., *who said what to whom*, until they reach school age. These, among other constructions explored in this volume highlight new avenues of acquisition research, which can in turn provide valuable empirical support (or counterevidence) to theoretical analyses involving Smuggling and beyond.

5. Conclusion

The results of our experimental work provide strong support for intervention effects in early grammatical development. We submit that the dependencies involved in raising and control themselves do not present an acquisition difficulty *per se*. Rather, children experience difficulties with these structures when a dependency crosses a structurally intervening argument. Further, not all constructions involving an intervening argument develop at the same rate – our results indicate that the development of StSR and SC are not mastered concurrently in most children, suggesting that these are derived through different grammatical operations which must be mastered independently.

Our results show that processing capacity also plays a role in children's performance in both constructions. Thus, adult-like performance depends on: (i) having access to the necessary grammatical operation that will derive a particular structure (i.e. each operation needs to be acquired independently); and (ii) having sufficient processing capacity to parse constructions that involve a crossing dependency (i.e. all structures involving an intervener will cause processing difficulties). Some of the children in our study lacked the grammatical mechanism (e.g. smuggling) necessary to circumvent the intervening argument, while others were grammatically mature but had insufficient processing capacity to implement all the grammatical options available to them. Such a theory unifies the seemingly incompatible grammatical and processing accounts proposed for the acquisition of this type of constructions.

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