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Intervention Effects in the Acquisition of Raising and Control: Evidence from English and Spanish

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Intervention Effects in the Acquisition of Raising and Control:

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by

Victoria Eugenia Mateu Martin

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ABSTRACT OF THE DISSERTATION

Intervention Effects in the Acquisition of Raising and Control:

Evidence from English and Spanish

by

Victoria Eugenia Mateu Martin

Doctor of Philosophy in Linguistics

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Professor Nina Hyams, Chair

This dissertation investigates the delays observed in the acquisition of raising with *seem*-type verbs (e.g. *Mary seems to John _ to be cautious*) and control with *promise*-type verbs (e.g. *Mary promises John _ to be cautious*), two constructions which involve an interpretative dependency between the matrix subject and the unpronounced embedded subject, and an argument intervening between the two.

One prominent explanation for the difficulties with these constructions holds that it is related to the presence of the intervening argument. Crucially for this type of accounts, an intervener is possible with Spanish *prometer* 'promise', but not with the modal-like verb *parecer* 'seem'. The experiments of this dissertation were designed to answer the following questions: i) are the delays observed in these constructions due to intervention effects? If so, ii) are they

grammar- or processing-based? and iii) if they are grammar-based, is the machinery used to by-pass the intervener the same in raising and control?

The results obtained from the raising experiments (Chapter 3) reveal that Spanish-speaking children comprehend sentences with raising *parecer* by age four, while English-speaking children experience difficulties with raising *seem* until at least age six – even when the intervening argument is *not* overtly produced. This cross-linguistic asymmetry suggests that the (overt or covert) intervening argument is the root of the difficulty. Consistent with intervention accounts, the results obtained from the control experiments (Chapter 4) reveal that both English- and Spanish-speaking children show difficulties comprehending control with *promise*/*prometer* until at least age six.

Regarding our second question, an in-depth look into the raising and control data reveal two different groups of children: i) below-chance group: children who lack the grammatical means to circumvent the intervening argument and ii) chance and above-chance group: children who have an adult grammar system but still experience difficulties due to their immature processing system. We refer to this as the Dual Source Intervention hypothesis. Crucially, our results do not show a correlation between performance on raising and control in all children. Some perform below chance with raising and above chance with control and some vice-versa, showing a *grammatical* dissociation between these two constructions.

The dissertation of Victoria Eugenia Mateu Martin is approved.

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CHAPTER 1

Acquisition of Subject-to-Subject Raising and Subject Control

One of the main goals in the area of first language acquisition is to characterize linguistic development so as to explain children's rapid mastery of most aspects of their native language while simultaneously accounting for persistent areas of delay. More generally, we endeavor to determine what these areas can tell us about the underlying grammar of individual languages and human language in general. This dissertation addresses two particular instances of reported delay: subject-to-subject raising (StSR) with verbs such as *seem* and subject control (SC) with verbs such as *promise*.

Although semantically and structurally different in ways we will address later, StSR and SC verbs show interesting similarities – both involve an obligatory interpretive dependency between an overt subject DP in the matrix clause and a lower unpronounced subject in the complement clause, represented pretheoretically as a gap in (1). These similarities are shared with Spanish, as illustrated in (2), the counterparts of (1). However, there are two crucial differences between these languages: (i) an intervening experiencer is impossible with *parecer* when the verb takes an infinitival complement (in contrast to *seem*), and (ii) there is a case-marking preposition *a* heading the benefactive of *prometer* (but not *promise*).

(1) a. Mary seems (to John) [__ to be cautious] (Subject-to-Subject Raising)
b. Mary promised (John) [__ to be cautious] (Subject Control)

(2) a. María parece (*a Juan) [__ ser precavida] (Subject-to-Subject Raising)
b. María prometió (a Juan) [__ ser precavida] (Subject Control)

Despite the extensive literature on the acquisition of StSR and SC constructions in English, the accounts proposed to date continue to be debated. But more strikingly, even though the acquisition of Romance languages has been actively researched, there is, to our knowledge, no experimental work investigating the development of these two constructions in Spanish. Given the differences in (i) and (ii) above, data from Spanish could be extremely helpful in evaluating competing accounts of children's delayed development of StSR and SC. The experiments in this dissertation are mainly designed to answer the following questions:

i) Are the delays observed in StSR with *seem*-type verbs and SC with *promise*-type verbs due to the syntactic presence of an intervening argument?
ii) Are these intervention effects attributable to the competence or the performance domain?
iii) Is the grammatical machinery that children use to circumvent the intervening experiencer in StSR the same as the one they use to circumvent the intervening benefactive in SC?

An experimental comparison of Spanish and English-speaking children should bear fruitfully on all these issues. In addressing these questions we hope to provide a better understanding of what grammatical properties are particularly difficult for children to master and why. A secondary goal of this study is to shed light on the syntactic representation of control constructions, a theoretical debate that has resurfaced in recent years. Finally, this research will add to our understanding of syntactic development in Spanish-speaking children in important (but neglected) areas of grammar.

In the remainder of Chapter 1 we describe previous acquisition research that bears on our investigation of StSR and SC. The focus here will be on English because to date these studies have been almost exclusively conducted in this language. In Chapter 2 we will discuss the

different accounts that have been proposed to explain these delays and lay out the specific predictions they make with respect to English and Spanish. In this context we will also describe in detail the distinctive aspects of Spanish *parecer* and *prometer.*

1.1. Acquisition of subject-to-subject raising

Subject-to-subject raising (StSR) involves promotion of an argument from its thematic base position to the matrix subject position and so it is considered a classic example of A-movement, together with passives and unaccusatives. Some examples of raising predicates include *seem*, *happen*, *used (to)*, *(be) going*, and *tend* – all of which lack an external argument. One piece of evidence that the matrix subject is not in a thematic relation to the matrix predicate comes from the fact that most raising patterns have an unraised counterpart such as the one in (3), where the DP *Mary* appears in the embedded clause, and a non-referring expletive serves as the matrix subject.

(3) It seems (to John) that [Mary is cautious].

In English, a small subset of raising verbs, *seem* and *appear*, permit an optional experiencer argument, requiring the subject to raise past it in StSR constructions, as observed in (1a). Experimental results indicate that while English-acquiring children show delays with StSR verbs that take an experiencer argument, e.g. *seem*, they perform well with StSR verbs that do not select for an experiencer, e.g. *be going, be about* and *tend* (Orfitelli, 2012). More specifically, experimental results have consistently found that children perform poorly with StSR *seem* when it appears with an overt intervening experiencer argument. However, there is less consensus with respect to children's comprehension of StSR *seem* when it appears without an

overt intervening experiencer or with a fronted experiencer. In the next sections we will provide an overview of the results obtained in each of these cases, including a brief review of the only non-English study on the acquisition of StSR that we are aware of, which examined children's comprehension of the Dutch StSR verbs *schijnen* and *lijken* 'seem'.

1.1.1. Children's comprehension of StSR *seem* with an overt intervening experiencer

Hirsch and Wexler (2007b) tested three- to nine-year-olds' comprehension of StSR *seem* with an overt experiencer argument using a sentence-picture matching task. Thought-bubbles were used to convey the notion of thinking about an event, or "experiencing" it. Thus, for the sentence '*Bart seems to Lisa to be playing an instrument*', the target image would depict Lisa with a thought-bubble in which Bart was playing an instrument. They found that children's overall group performance did not differ from chance until the age of seven years. Contrastively, even the youngest group showed adult comprehension of unraised sentences with *seem*, such as '*It seems to Lisa that Bart is playing an instrument*'. The three-year-old group was on average 85.6% accurate on the unraised condition but only 43.9% on the raised counterpart. Thus, poor comprehension of StSR *seem* could not have been attributed to any lexical or conceptual difficulty with the verb *seem*.

Additional experimental investigations using the Truth-Value Judgment Task (TVJT) methodology have since replicated these results. Hirsch, Orfitelli, and Wexler (2007)[1] tested four- to seven-year olds' comprehension of StSR using scenarios with two interacting dolls, Barbie and Ken. In one example scenario, Barbie is searching for her hat and does not realize that it is already on her head. Ken, who is standing far from her, tells the child that he believes

[1] This study is also discussed in Hirsch (2011).

that he can see Barbie is wearing her hat. The child would then be asked to comment on the truthfulness of '*Barbie seems to Ken to be wearing a hat*' (true) or '*Ken seems to Barbie to be wearing a hat*' (false). Children were also tested on two control conditions: the unraised counterparts (e.g. '*It seems to Ken that Barbie is wearing a hat*'), and finite clauses with the verb *think* (e.g. '*Ken thinks Barbie is wearing a hat*'). In order to be included in the study, children were required to score at least seven out of the eight in these two conditions. The results showed that children preformed rather poorly on the StSR condition –none of the age groups obtained an average score above chance. In fact, of the 20 four- and five-year olds, none performed above-chance on this condition, and 17 performed below chance. Upon examining the individual response justifications, some of these children were found to consistently use a strategy that led them to the incorrect answer; namely, they assigned *seem* a *think* interpretation. Thus, when given the false sentence '*Ken seems to Barbie to be wearing a hat*', children would say '*true, because Ken thinks that Barbie is wearing a hat*'. While it is entirely possible that this interpretation was primed by the *think* condition, the crucial finding about this study is that while children perform well with unraised sentences they display a non-adult comprehension of StSR sentences.

More recently, Choe (2012)[2] suggested that children's poor performance with StSR in the previous two studies could be due to the infelicitous use of the experiencer phrase when there are only two characters in the stimuli. She thus created scenarios with three characters to satisfy the felicity condition: Mickey, Donald, and Daisy. In an example scenario, Donald and Daisy are standing inside a hole. Mickey then arrives and believes that Donald is short, even though Donald is in fact the same height as Mickey. Daisy tells Mickey that Donald is not short, but

[2] See also Choe & Deen (2016).

Mickey does not believe her (see Figure 1.1). The children were then asked to judge the target StSR sentence '*Donald seems to Mickey to be short*' (true) or '*Mickey seems to Donald to be short*' (false), as well as two controls, the unraised counterpart, and a sentence with *think*. The results from the study confirmed that three- to six-year-olds do not comprehend StSR sentences with an intervening experiencer, scoring 50% on average.

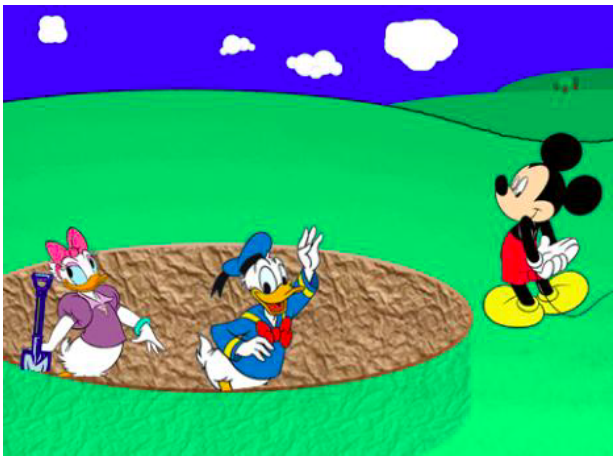


Figure 1.1. Sample picture used in Choe's (2012) TVJT.

Interestingly, Choe found that children showed better performance when the intervener experiencer was a pronoun (67%) as opposed to a lexical DP (50%).[3] However, methodological concerns may make it difficult to interpret this asymmetry. As Hirsch and Wexler (2007b) note, when faced with StSR sentences with an overt intervening experiencer, a child with a non-adult grammar may interpret *seem* as if it takes an external argument, much like *think* or a Raising-to-object/ECM predicate like *believe*. Thus, given the sentence '*Donald seems to him to be short'*,

[3] Choe attributes this asymmetry to performance factors, namely the hypothesis that pronouns are referentially more accessible than lexical DPs, and consequently easier to process according to some memory-based models (see Warren & Gibson, 2002).

the raised argument (*Donald*) may have been interpreted as an experiencer subject and the experiencer (*him*) as the subject argument of the embedded clause, i.e. '*Donald believes him to be short*'. As the author acknowledges, the children being tested were at the exact age where they permit Principle B violations (Chien & Wexler, 1990; Grodzinsky & Reinhart, 1993, *inter alia*). If children interpreted '*Donald seems to him to be short*' as '*Donald believes himself to be short*' they might have answered 'true', since this is exactly what Donald believes at the end of the story, leading to a false positive result. In order to rule out this possibility, Choe included a gender-mismatched condition to rule out a Principle B violation, e.g. '*Bart seems to her to be studying*'. The results were not different from the gender-matched condition. However, children were only given *three* testing items, and given that TVJT judgments are inherently non-parametric data (because the judgments are categorical *yes* vs. *no*) this is not enough data to say that a given child performed statistically differently from chance. Moreover, the children that were tested on the lexical DP condition were not the same children tested in the pronoun condition. Therefore, whether children's performance improves when the intervening experiencer is a pronoun remains inconclusive.

1.1.2. Children's comprehension of StSR *seem* without an overt experiencer

The experimental results for StSR *seem* without an overt experiencer is mixed, with some studies finding good performance (Becker, 2005; 2006), and some poor performance (Hirsch, 2011; Hirsch, Orfitelli, & Wexler, 2007, 2008; Orfitelli, 2012). Becker (2005, 2006) tested three- to four-year-olds' comprehension of StSR predicates, including StSR *seem* (without an overt experiencer) using a TVJT. The scenario used to test StSR *seem* involved a white dog that had walked under a purple light, thus appearing purple (see Figure 1.2). Children were then asked to

judge the truthfulness of the statement '*The dog seemed to be purple*' (true) or '*The dog seemed to be white*' (false). The three-year olds obtained an average score of 71.4% for the combined three raised verb conditions *seem*, *used to*, and *happen*.[4] The four-year olds averaged 83.3%. Becker thus concludes that children have no difficulties with StSR when there is no (overt) experiencer.

Figure 1.2. Sample images used in Becker (2005, 2006), Hirsch (2011), Hirsch et al. (2008), and Orfitelli (2012). The left image shows the dog is white. The right image shows the dog appears to be purple when he is under the light.

However, Hirsch et al. (2008) raise a number of concerns regarding this experiment, one of which we will outline here, as it relates to a particular decision regarding the stimuli of our study. These authors bring up the fact that predicates such as '*be purple*' are ambiguous between an individual level interpretation, in which the dog is interpreted to be permanently purple, and a stage level interpretation, in which the dog is interpreted to be temporarily purple. Given a scenario in which the white dog's fur changes appearance under a purple light, it is entirely plausible that children who have not acquired StSR *seem* would ignore *seem* and assign this predicate a stage level interpretation similar to '*The dog was purple (when he was under the*

[4] Becker does not provide a breakdown of the results by verb.

light)'. This idea is reinforced by the results from a pilot study conducted by Hirsch et al. (2008) (see also Hirsch, 2011). These authors found that both children and adults judged '*The dog was purple*' to be acceptable given the scenario used in Becker's study. In contrast, when given the sentence '*The dog was* really *purple*' (an individual level interpretation), they changed their answer to 'false'.

Hirsch and colleagues thus improved and expanded Becker's methodology by adding a copular declarative condition to test whether children were assigning a stage or individual level interpretation to the complex predicates like '*be purple*'. Additionally, an unraised condition was added to ensure that children knew the meaning of *seem* independent of StSR. Finally, the word *really* was added to all conditions as a modifier to the matrix predicate. They found that while children answered correctly on nearly all copula (average = 100% correct) and unraised items (average = 78.8% correct), they showed non-adult performance on StSR items (average = 42.4% correct), incorrectly answering 'false' to '*The dog really seems to be purple*' in more than half the trials. Children's response justifications for these answers suggest that they were ignoring the verb *seem* entirely in the StSR condition, e.g. '*No, he's not purple. In real life he's white*', but crucially, not in the unraised condition. The authors conclude that children up till the age of six show delayed comprehension of StSR *seem* even when it appears with*out* an overt experiencer.

More recently, Orfitelli (2012) tested four- to six-year-olds' comprehension of StSR predicates: *seem*, *be going, be about* and *tend* using a TVJT. She found that while the four- and five-year olds performed above chance in the two control copula and unraised conditions, the majority of these children (n = 16/20) performed below chance in StSR sentences such as '*The dog really seems to be purple*' (see Figure 1.2). Additionally, she found that all age groups performed above chance with other StSR predicates that do not select an experiencer argument:

be going, e.g. '*The ball is going to be rolling down the hill*', *be about*, e.g. '*The ball is about to be rolling down the hill*', and *tend*, e.g. '*The flower tends to be touching the ground*'.[5] She concludes that it may not be the presence of an overt experiencer DP that is at the root of children's difficulties with StSR *seem*, but rather the explicit or *implicit* presence of an experiencer argument.

1.1.3. Children's comprehension of StSR *seem* with a fronted experiencer

Another pattern that does not involve an overt intervening experiencer is StSR *seem* with a fronted experiencer, as illustrated in (4). In this example, the overt experiencer phrase '*to John*' does not intervene between the matrix subject *Mary* and its base-generated position in the embedded subject position, only the trace of the experiencer phrase does. If children's difficulty with StSR *seem* is the *overt* intervening experiencer, children should have no difficulties comprehending StSR *seem* with a fronted experiencer.

(4) To John_i , Mary_k seems t_i t_k to be cautious.

Hirsch (2011) tested four- to seven-year-olds' comprehension of StSR *seem* with a fronted experiencer argument using a sentence-picture matching task. As in Hirsch and Wexler (2007b), thought-bubbles were used to convey the notion of thinking about an event, or "experiencing" it. Thus, the sentence '*To Lisa, Homer seems to be petting the dog*' would match with image A in Figure 1.3. The results revealed that children performed at chance on sentence-picture matches like the one just described.

[5] Only five- and six-year-olds were tested for *tend* due to the higher complexity of the verb and scenarios.

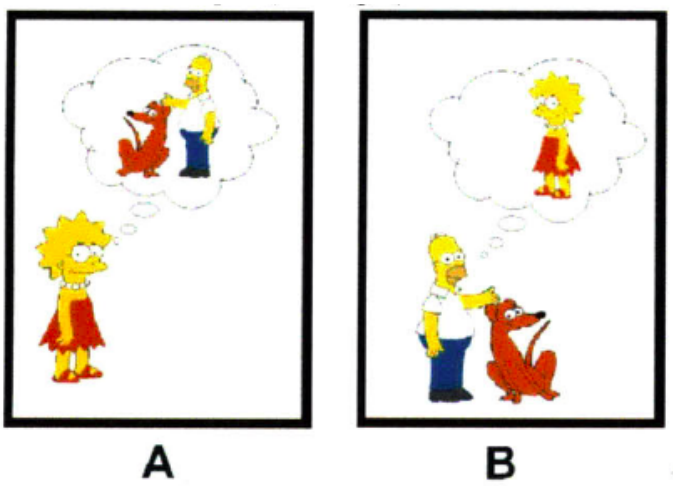


Figure 1.3. Sample set pictures used in Hirsch (2011)

Hirsch argues that the children are ignoring the fronted experiencer in raised sentences, and interpreting *seem* as a copula – thereby taking the raised sentence '*Homer seems to be petting the dog*' to mean '*Home is petting a dog*'. This analysis would explain their chance performance in this condition, since both pictures in (correct and incorrect) presented to the child would match the test sentence if the experiencer is ignored. Overall, Hirsch (2011) concludes that raising is problematic for young children irrespective of the position of the experiencer phrase.

Choe (2012) also tested StSR *seem* with a fronted experiencer using the same TVJT procedure she used for the intervening experiencer experiment. Results show that three- to five-year olds perform as well with StSR with a fronted experiencer (e.g. '*To Mickey, Donald seems to be short*') as with unraised sentences (e.g. '*It seems to Mickey that Donald is short*'), both scoring at 80%. However, as Hirsch (2011) suggests, it could be the case that children were ignoring the experiencer and analyzing the StSR construction using a copula strategy, i.e. by interpreting '*Donald seems to be short*' as '*Donald is short*'. Thus, children may have said 'true'

in this case either because they were assigning the predicate a stage-level reading, as Hirsch et al. (2008) suggest, or simply because children believed Donald was actually short, since he was never shown outside the hole (see Figure 1.1). An obvious test would be to ask children if they thought Donald *really* was short, but the study did not include a copula condition. Therefore, we cannot rule out the possibility that StSR constructions with *seem* are difficult to comprehend even when the experiencer phrase is displaced to the front of the sentence (via A'-movement).

1.1.4. Children's comprehension of StSR with *seem*-type verbs in other languages

The acquisition of StSR with *seem*-type verbs without an overt experiencer has also been examined in Dutch. Koring (2007) investigated the acquisition of the Dutch StSR verbs *schijnen* and *lijken* (both translated as *seem*) using a TVJT. In one example scenario, two crocodiles are eating a dinosaur, and children were asked to judge the truthfulness of '*The crocodiles seem to be eating the dinosaur*'. The trial sentences were in either SVO order or OVS order to ensure that children were not simply ignoring *schijnen* / *lijken* 'seem' and using a 'DP_1 = agent' strategy. She found that Dutch-speaking children experience difficulties with the OVS StSR constructions until as late as eight years old, in line with the English findings of Hirsch (2011); Hirsch et al., (2008) and Orfitelli (2012). However, it is worth noting that these results may not be informative given the following two methodological concerns: i) surprisingly, children also performed poorly on the control OVS constructions in which there was no raising, i.e. '*The crocodiles are eating the dinosaur*', scoring on average 64% in the groups below eight years old. This could indicate children may not have understood the task properly; ii) the author did not include unraised controls with *schijnen* or *lijken*; thus, we cannot exclude the possibility that these children did not understand the lexical properties of these two verbs. In fact, a recent study suggests that

Dutch-speaking do not understand indirect hearsay evidential verbs such as *schijnen* until age nine (Koring & Mulder, 2015). Therefore, children's difficulties with *schijnen* and *lijken* may not be (solely) due to the raising operation.

1.1.5. Summary of the acquisition of StSR

As we have seen, there is relatively strong evidence that young children fail to understand sentences with StSR *seem* when there is an overt intervening experiencer argument. However, the evidence regarding children's comprehension of StSR *seem* without an overt intervening experiencer is mixed, with some studies finding good performance (Becker, 2005; 2006; Choe, 2012), and some poor performance (Hirsch 2011; Hirsch et al., 2008; Orfitelli, 2012). Additionally, because previous studies have tested only the StSR verb *seem* and virtually all studies have been conducted in English, whether children are universally delayed in acquiring StSR predicates that select for an experiencer is still a matter of debate. To address these concerns, in Chapter 3 we perform a within-subjects comparison of overt and covert experiencer StSR with English *seem* and a parallel study on Spanish *parecer*.

Before discussing several proposals that attempt to account for the acquisition of StSR *seem*, we present the experimental results obtained for SC *promise*.

1.2. Acquisition of subject control

In control structures the matrix predicate takes an infinitival complement whose silent subject is referentially linked to an argument in the matrix. Under standard syntactic analyses of obligatory control (e.g. Chomsky, 1981, 1986; Landau 2000, 2006, 2013), the silent subject is PRO. When PRO is in a complement clause, it can be controlled by either the matrix subject

with verbs such as *hope, try, want,* or *promise*, as illustrated in (5), or the matrix object, with verbs such as *tell, force, persuade,* or *urge*, as in (6).

(5) a. $John_i$ hopes [PRO_i to be home by nine].
b. $John_i$ promised Mary [PRO_i to be home by nine].

(6) a. John urged $Mary_k$ [PRO_k to be home by nine].
b. John told $Mary_k$ [PRO_k to be home by nine].

However, since the Minimalist program (Chomsky 1995), there has been an important premium placed on "simpler" theories that eschew both theory internal levels such as D-structure or principles like the Theta Criterion. This has led some syntacticians to derive obligatory control constructions by A-movement, i.e. raising. This analysis, known as the Movement Theory of Control (henceforth MTC; Hornstein 1999, 2001; Boeckx & Hornstein, 2003, 2004, 2006; Hornstein & Polinsky, 2010; Manzini & Roussou 2000; O'Neil 1995), is illustrated in (7).[6]

(7) a. $John_i$ promised Mary [t_i to be home by nine].
b. John told $Mary_k$ [t_k to be home by nine].

Regardless of the approach, there are a few control verbs that, in violation of the Minimal Distance Principle (MDP) / Minimal Link Condition (MLC), require the embedded subject to be controlled by the matrix subject as opposed to the more local c-commanding DP, the object, e.g. *promise.* 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;

[6] For clarity of presentation, we will keep referring to the empty category in the embedded subject position of control complements as PRO. However, we remain agnostic in that respect for now.

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 verb complements by age 3;6-4;0, they persist in assigning object control with the verb *promise.*

C. Chomsky (1969) probed children's comprehension of subject control *promise* and object control *tell* using an act-out task. She found that while children aged five to 10 years correctly pick out the individual referred to by the object to represent PRO in *tell* sentences such as the one in (8a), the interpretation of PRO in *promise* sentences like the one in (8b) goes through three developmental stages: (i) consistently object control, (ii) mixed subject and object control, (iii) consistently subject control. Results also show that the acquisition of *promise* is not significantly correlated with age –some children show mastery by five, and some at nine.

(8) a. Donald tells Bozo to lie down. Make him do it.
b. Donald promises Bozo to lie down. Have him do it.

Prior to presenting the test sentences, the experimenter verified that the child understood the meaning of *promise* by asking 'C*an you tell me what you would say to your friend if you promise him that you'll call him up this afternoon? How would you say that to him?*' and/or '*What do you mean when you make somebody a promise?*'. According to C. Chomsky, all children succeeded with these questions and knew what a promise is, even those in the earliest stage, thus demonstrating that it is the syntactic structure of *promise* that causes difficulties, and not the concept.

In a second experiment, she tested children's comprehension of *tell* and *ask* with sentences such as the ones in (9), in which both *tell* and *ask* (used in request sense) require object

control, and with *wh*- complements as in (10), in which *tell* still requires object control, but *ask* (used in question sense) requires subject control (across the object).

(9) a. Bozo tells Mickey to go first in line.
b. Bozo asks Mickey to go first in line.

(10) a. Would you tell Laura what to feed the doll.
b. Would you ask Laura what to feed the doll.

She found that children initially treat *tell* and *ask* alike in both types of constructions, imposing a *tell* interpretation everywhere, and that children do not consistently interpret *ask* as a subject control verb until approximately nine or 10.

These results were replicated in a later study by Hsu et al. (1985), in which they tested children between the ages of three and eight using an act-out task. Among the 18 different complex sentence types children were tested on, two included object control *tell*, and subject control *promise*. They found that while *tell* obtained 95% correct responses in the youngest age group (3;2-4;0), children showed a mixed pattern of responses with *promise* until after seven years old, with a strong preference for object control until the age of 6;6.

Sherman and Lust (1993) gathered production and comprehension data from 108 children in the 3;0-8;0 age range. In an elicited imitation task, children were required to imitate finite clauses containing lexical pronouns, (11), and infinitive clauses involving object and subject control, (12).

(11) a. Jimmy tells Tom that he will ride the bicycle.
b. Jimmy promises Tom that he will drink the milk.

(12) a. The sister tells the brother to draw a picture.
b. Jimmy promises Tom to watch the baseball game.

In repeating these sentences, children sometimes changed the complement type, replacing the finite complement with an infinitive complement or vice versa. Interestingly, children converted a finite complement into an infinitive one more often when the matrix verb was *tell* than when it was *promise*. Conversely, they changed an infinitive complement into a finite one more often when the matrix verb was *promise* than when it was *tell*. Examples of children's conversions are given in (13).

(13) a. Target: Jimmy tells Tom that he will ride the bicycle.
Response: Jimmy t … tells Tom to ride the bicycle. (3;0)
b. Target: Jimmy promises Tom to watch the baseball game.
Response: Um… Jimmy promises him that he will watch th'ball game. (3;9)

Even though it is not clear why children changed the structures in the way they did, the authors claim that the different response patterns suggest children know something that distinguishes the two kinds of control verbs. Moreover, the experimenters found that overall it was easier for children to imitate sentences including object control verbs (*tell*/*remind*) than sentences including subject control verbs (*promise*), which implies that this latter kind of sentences may be cognitively more taxing to produce and/or comprehend.

In a different part of the study, children were asked to act out the content of sentences like (12). They found that by four, children choose the object as the controller of PRO more often when the matrix verb is *tell.* However, they do not choose the subject more often than the object as the controller of PRO with *promise* until age seven. The authors also noted that this bias with *promise* is observed only when the complement contains an infinitive. When the

complement contains a finite verb, as in (11b), children prefer to take the lexical pronoun to be anaphorically linked to the subject by age five.

Like C. Chomsky, Sherman and Lust, also ensured children understood the meaning of *promise* by asking children '*what does it mean* "to promise"*?*'. According to the experimenters, their responses suggested that even the youngest children they tested (3-year-olds) appear to understand the basic meaning of the verb *promise* (i.e., what Grimshaw, 1989 would term the '*lexical semantic structure'*). That is, they appear to understand that the verb *promise* involves a semantic structure like (14):

(14) *promise*: x asserts to someone else, y, with a sense of obligation on x's part that [z] (where z may be a proposition).

In their Appendix 2B Sherman and Lust provide examples from interviews to illustrate that the children in their study understand the dative properties of the verb "promise". In Grimshaw's (1989) terms, they appear to be aware of the "goal argument" in the lexical semantic structure for this verb. Example (15), from a child aged 3;l0, is illustrative:

(15) Experimenter: Did you ever promise anything?
Child: I promised my mom I'd make the cake.
Experimenter: What did you do?
Child: I made the cake.

Sherman and Lust thus interpret these results as an indication that object control responses with *promise* arise from a grammatical problem establishing subject control with *promise*, and not failure to comprehend its basic meaning.

1.2.1. Summary of the acquisition of SC

Summarizing, there is consistent evidence that shows that young children fail to understand sentences with SC *promise* (and possibly *ask* with *wh*- complements) while they succeed with object control sentences. However, whether children's difficulties with *promise* extend to other SC verbs that take an object argument as well as to languages outside English remains to be tested. Additionally, whether this delay is due to competence or performance factors is still an open question. In Chapter 4, we will present our control study investigating SC *promise* and SC *swear* in English, as well as Spanish SC *prometer* and SC *jurar*. In Chapter 2 we discuss several proposals that attempt to account for the late acquisition SC *promise*

1.3. The proposed study

This study mainly aims to investigate the acquisition of StSR with *seem*-type verbs and SC with *promise*-type verbs in English and Spanish. The goals are twofold: (i) to determine whether the delays reported for these two constructions have the same origin (e.g. difficulties with the grammatical machinery that would overcome intervention) and (ii) to investigate the effects that a limited processing capacity might have in these areas. Comparing children's performance in these two constructions at the individual level will not only further our understanding of the path of normal language development, it will also allow us to test the predictions of several different accounts of children's difficulties. These will be summarized in Chapter 2. Additionally, since neither of these two constructions has been investigated in child Spanish, this study will begin to fill this important empirical gap.

CHAPTER 2

Acquisition Accounts of Subject-to-Subject Raising and Subject Control

In Chapter 1 we provided an overview of the experimental studies that have investigated the acquisition of subject-to-subject raising (StSR) and subject control (SC), two constructions that are notoriously challenging for children to understand until approximately age six or seven. In this chapter we will survey the different theories that have been proposed to account for these delays. We will first discuss the accounts posited for StSR in Section 2.1, and then examine those proposed for SC in Section 2.2.

The accounts for these two constructions will be divided into grammar- and performance-based accounts. As the names might suggest, the core claim of grammar-based theories is that young children's non-adult behavior arises from a difference between their grammar and that of adults, largely irrespective of performance factors. Contrastively, performance-based theories posit a much more direct connection between children's general processing capacity and their linguistic competence. However, it is important to recognize that there are conceptually possible explanations for the acquisition of StSR and SC which appeal to both grammatical and processing factors to varying degrees. In Chapters 3 and 4 we will return to the question of what type of theory or combination of theories best explains the development of StSR and SC structures respectively, in light of the new experimental data presented in this dissertation. Ultimately, we will propose an account that appeals to both grammatical constraints and processing limitations.

2.1. Accounting for the acquisition of subject-to-subject raising

While there exists a vast literature on the acquisition of StSR in English, there is no consensus explanation for its protracted acquisition – partly due to the inconsistency of the results, as discussed in Chapter 1. Accordingly, in reviewing the different grammar-based (Section 2.1.1) and processing-based accounts (Section 2.1.2) for the delay of StSR, we will concentrate on how well they capture the acquisition data that exist to date. Most recent research on the acquisition of StSR has centered around the concept of 'intervention': children have difficulties computing an interpretative dependency across the intervening experiencer, either for grammatical or processing reasons. Crucially, intervention accounts make for testable predictions in languages like Spanish, which has a StSR verb semantically similar to *seem* but which does not select for an (intervening) experiencer argument (cf. Ausín 2001; Boeckx, 1999; Bosque & Torrego 1995; Torrego 1996, 1998, 2002). In Section 2.1.3 we will describe the syntactic and semantic properties of the Spanish raising verb *parecer* and discuss the empirical predictions that the different intervention accounts make for English and for Spanish

2.1.1. Grammar-based accounts

The seminal grammar-based account is Borer and Wexler's (1987, 1992) A-Chain Deficit Hypothesis (ACDH), which states that immature children lack the ability to form A-chains, predicting that structures involving A-movement – such as raising, passives, and unaccusatives – are delayed in acquisition. However, one difficulty faced by the ACDH is that not all A-chains have been found to be difficult for children. Since the formulation of the ACDH, it has been argued that subjects are base-generated inside the VP and only later move up to Spec-TP (VP-internal subject hypothesis; Koopman & Sportiche, 1991). Since the subject undergoes A-

movement, it creates an A-chain. Yet, acquisition data show that children are able to correctly raise the subject out of the VP (Stromswold, 1996), contra the prediction of ACDH. Furthermore, Kirby (2010) found that by four years old children have adult-like comprehension of raising-to-object sentences, in which the embedded subject is assumed to A-move to the matrix object position.

More recent research has provided additional evidence that other structures involving A-movement are acquired early. For instance, experimental work from Snyder, Hyams, and Crisma (1995), and Hyams and Snyder (2005, 2006) shows that French and Italian children as young as two make virtually no errors in auxiliary selection with what they call 'formally (but not semantically) reflexive clitic constructions' (FRCCs), which involve A-movement. Specifically, they found children always correctly use *be* (*être*/*essere*) in FRCCs and *have* (*avoir*/*avere*) elsewhere. The authors take this to indicate that the children have the adult grammar of FRCCs and can effectively perform A-movement. Other studies also seem to corroborate that young children distinguish between unaccusatives and unergatives (see Friedmann, 2007, for Hebrew, and Shimada & Sano, 2007, for Japanese).[1]

Further, it has been shown that not all StSR structures are acquired late either. As mentioned in Chapter 1, Orfitelli (2012) found that while four-to-six year-olds experience difficulties with verbs that select for an experiencer, i.e. *seem*, these problems vanish with other StSR predicates, such as *be going, be about* and *tend*, all of which are semantically incompatible with an experiencer argument, and therefore do not involve raising over an intervening argument.

[1] Babyonyshev, Ganger, Pesetsky, and Wexler (2001) report that Russian children are delayed in acquiring unaccusativity based on a production study of the genitive of negation. However, Potsdam and Polinsky (2011) provide compelling arguments showing that Russian unaccusatives do not involve A-movement.

Recent grammatical accounts have thus proposed that the difficulties observed with StSR *seem* (and passives) may be due to '*intervention effects*', which arise due to the presence of the intervening experiencer phrase (or *by*-phrase). Grammar-based intervention effects can be explained in terms of syntactic principles such as the Minimal Link Condition (MLC; Chomsky, 1995) or Relativized Minimality (RM: Rizzi 1990, 2004). RM states that in the configuration in (1), the dependency between X and Y is blocked iff Z belongs to the same structural type as X, and iff Z c-commands Y:

(1) … X … Z … Y …

StSR constitutes a well-known instance of a structure which successfully overcomes a potential intervention configuration in adult grammars. There is evidence that 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 seem to cross over the position of the intervening experiencer argument, a straightforward violation of MLC/RM. In fact, this problem, which Boeckx (1999) calls the '*experiencer paradox*', has been a long-standing puzzle for 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; Kitahara, 1997; McGinnis, 1998). Indeed, intervention effects can be voided through the adoption of certain structural strategies. However, these may only become accessible to children at later stages in development. Some of the acquisition accounts we discuss in the following section rely on this assumption.

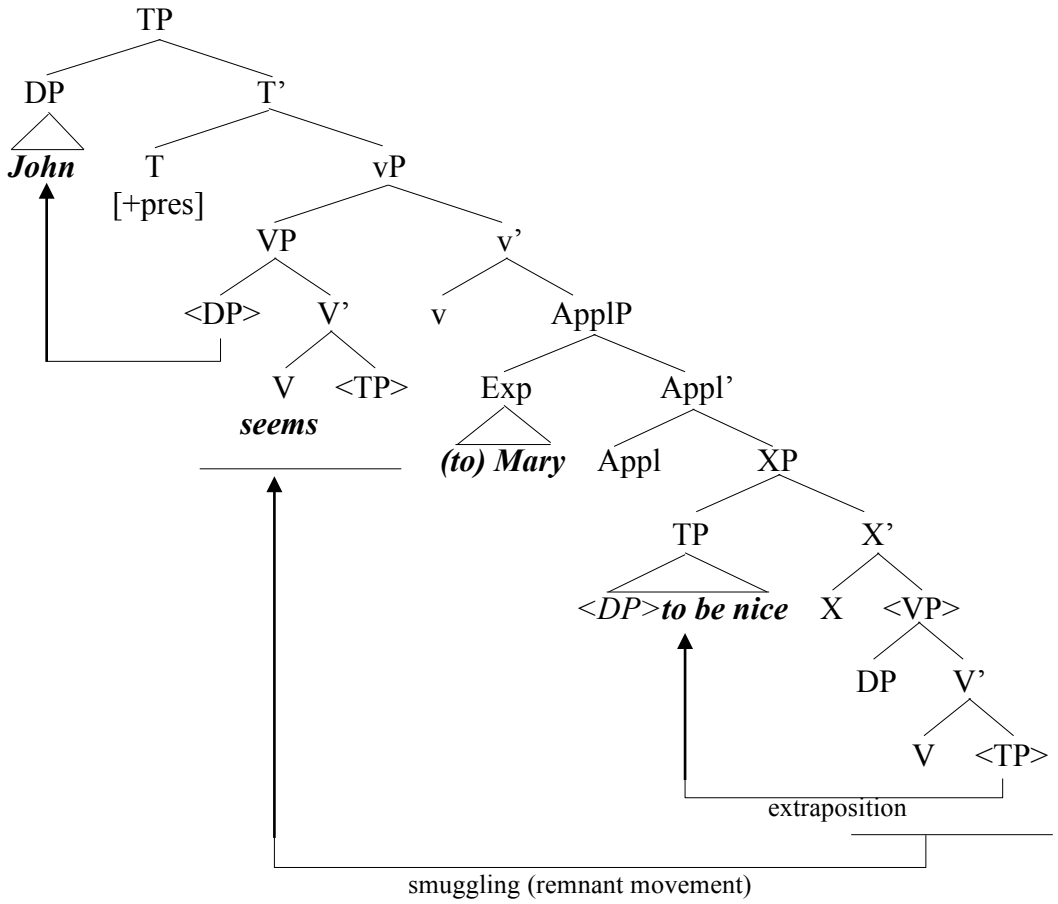
2.1.1.1. The Universal Freezing Hypothesis

Hyams and Snyder (2005, 2006) adopt the syntactic assumptions put forth in Collins' (2005a) smuggling account of StSR. In order to circumvent a locality violation on movement such as RM, Collins proposes that the subject of the embedded clause undergoes smuggling, a process in which a larger piece of verbal structure containing the DP moves past the experiencer, as schematized abstractly in (3), and 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*'. As represented in (4) from this point onward, 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.

(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.



Crucial for Hyams and Snyder's account is Collin's assumption that smuggling necessarily represents an exception to the Freezing Principle developed in Wexler & Culicover (1983), and reformulated by Müller (1998). The Freezing Principle rules out movement from an already moved phrase, which is precisely the operation involved in smuggling. A recent reformulation of the Freezing Principle is given in (5) (from Müller, 1998:124).

(5) *X [Y … <X> …] <Y>.

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, the immature child's grammar is 'zero-tolerance' in this regard.

(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.

As a consequence, 'immature' children will have difficulties with structures involving the smuggling process – such as StSR sentences and verbal passives (Collins, 2005a,b). Importantly, the UFH predicts that children's comprehension of A-movement becomes problematic only when an intervening argument is structurally present, since it is only when the embedded subject needs to raise across the intervening experiencer that smuggling is required. In line with their predictions, children seem to acquire unaccusatives (Friedmann, 2007; Hyams & Snyder, 2005, 2006; Shimada & Sano, 2007; Snyder et al., 1995) and StSR with no experiencer arguments (Orfitelli, 2012) at a young age, since neither of them call for smuggling.

Turning to the central question of implicit arguments, because the UFH is based on Collins' (2005a,b) specific analyses of raising and passives, Hyams and Snyder also assume that an implicit *by*-phrase argument is always structurally present in verbal passives, even when it is not overtly produced (see Orfitelli, 2012, who makes the same assumption). Strong crossover effects (7a), binding (7b,c), the availability of subject-controlled infinitival clauses (7d), subject-oriented modifiers (7e), and purpose adverbials (7f) provide empirical evidence for this idea (see

Baker, Johnson, & Robert, 1989; but cf. Bhatt & Pancheva, 2006).[3]

(7) a. *They$_i$ were killed by themselves$_i$. (Baker et al., 1989:225)
b. Damaging testimony is always given about oneself in secret trials. (Roberts, 1987:172)
c. Such privileges should be kept to oneself. (Baker et al., 1989:228)
d. The boat was sunk to collect the insurance. (Roeper, 1983:268)
e. The book was written deliberately. (Grillo, 2008:100)
f. The book was torn on purpose. (Fox & Grodzinsky 1998:327)

The UFH thus predicts delayed comprehension with passives both when the *by*-phrase is overtly expressed and when it is not. As expected, children show non-adult comprehension of (non-actional)[4] verbal passives until approximately six years old, irrespective of the presence or absence of the *by*-phrase (Hirsch & Wexler, 2006b; Maratsos, Fox, Becker, & Chalkley, 1985; Orfitelli, 2012; but cf. O'Brien, Grolla & Lillo-Martin, 2006).

However, because Collins (2005a) does not make mention of implicit experiencers in StSR constructions, Hyams and Snyder assume that when the experiencer is omitted it is not structurally present and hence smuggling is not required (2005:6, 2006:17). If so, children should perform well on StSR sentences with *seem*-type predicates when the experiencer is not overtly expressed. As we saw in Chapter 1, the experimental results for this type of sentences is mixed, with some studies finding good performance (Becker, 2005; 2006), and some poor performance (Hirsch, 2011; Hirsch, Orfitelli, & Wexler, 2007, 2008; Orfitelli, 2012) (see Section 1.1.2 for a review).

[3] Baker et al. (1989) was the first attempt to define the implicit external argument as being of the same kind as PRO. More recently, Collins (2005) and Gehrke and Grillo (2008) have also assumed this position.

[4] We will return to the question of actional versus non-actional verbal passives in the next section.

A more serious problem for the UFH is that it relies on Collins' smuggling analysis, which has faced a number of theoretical criticisms that the UFH has consequently inherited, the most severe of which is that it poses a look-ahead problem. According to Collins (2005a,b), the lower VP/PartP moves for two reasons: i) because an uninterpretable feature in the verbal morphology needs to be checked, and ii) because movement of the VP/PartP avoids a potential minimality violation since the argument inside VP/PartP is now closer to the probe T than the argument in the Spec of ApplP/vP. This leads to a potential problem, as the proposed VP/PartP movement must precede the insertion of the probe T. That is, if T is projected before the movement of VP/PartP, it will attract the Experiencer/external argument, but if movement happens before T is projected, then we cannot motivate the movement with feature-checking.

2.1.1.2. The Argument Intervention Hypothesis

Given the theoretical concerns just mentioned in the previous section, Orfitelli (2012) proposes an account that abstracts away from the specific details associated with smuggling and freezing to account for children's delay in StSR as well as verbal passives:

(8) Argument Intervention Hypothesis (AIH): Children are delayed in acquiring those structures which require A-movement across a structurally intervening argument.

For example, an StSR sentences like (9) is ungrammatical for children, since it involves movement of the embedded subject 'John' past the experiencer argument 'Mary'.

(9) John_i seems to Mary t_i to be nice.

The AIH is similar to the UFH in that the crucial component of the hypothesis has to do with the presence of an intervening argument. It therefore claims that children have difficulty with StSR when it operates across an experiencer. Thus, it is only the *seem*-type raising predicates that select an experiencer argument (e.g., *seem*, *appear*) that should pose special difficulty for children, while non-experiencer raising predicates (e.g., *tend*, *be going to, be about to*) should be unproblematic. Importantly, the AIH does stipulate that even when there is no overt experiencer argument in *seem*-type StSR sentences, it is nevertheless syntactically projected (Orfitelli, 2012:109-111).

The diagnostics of implicit argumenthood that exist for the passive *by*-phrase are mostly agent-oriented and hence not available for the experiencer of StSR sentences. However, examples from binding (10a-b), 'speaker/experiencer'-oriented modifiers (10c), and instrumental phrases (10d) may suggest the presence of an implicit experiencer argument. For instance, in (10a-b) the implicit experiencer must be disjoint from *Mary* and *Nixon* respectively. Now compare the sentences in (10c) and (10d). In (10c) the implicit experiencer was the one convinced that James loved the woman. However, when *seem*, the licenser of the implicit experiencer, is removed, as in (10d), the sentence becomes severely degraded.[5] Similarly, in (10e), the diamond is perceived to be of good quality by the implicit experiencer, who has observed it with no other instrument but his or her bare eyes.

[5] For some native speakers, (10d) seems to be grammatical but has a slightly different meaning than (10c). In (10c) the presence of *seem* introduces a new experiencer/judge and *convincingly* can be shifted to be interpreted as that person. In (10d), however, there is no predicate that shifts this perspective, and the convinced individual must be interpreted as a default general judge.

(10) a. John seems {__$_i$/to her$_k$} to like Mary$_{*i/*k}$.
[implicit/explicit experiencer of *seem* ≠ Mary]

b. (Because of his arrogance) To implicate Nixon$_{*i/*k}$ seemed {__$_i$/to him$_k$} to be impossible.
[implicit/explicit experiencer of *seem* ≠ Nixon]

c. James killed the woman he so convincingly seemed to love.

d. ??James killed the woman he so convincingly loved.

e. This diamond seems to be of high quality, at least with the naked eye.

While a semantic representation of the experiencer could perhaps suffice to account for the examples in (10c-d), there is no evidence to conclude that the implicit experiencer of *seem* is not syntactically projected.[6] It is also important to recognize that Orfitelli (2012) does not make claims about the adult grammar, but the child's representation. Thus, under the AIH, children's difficulty should persist even when the experiencer phrase is unpronounced. As previously mentioned, the experimental data is divided, although most results seem to support this claim (see Section 1.1.2).

Nevertheless, the predictions of the AIH may overgeneralize in an undesirable way. It is well established that while children show no difficulties with actional passives, they perform poorly with non-actional passives (Bever, 1970; Gordon & Chafetz, 1990; Hirsch & Wexler, 2006b; Maratsos et al. 1985; Orfitelli, 2012). To account for this discrepancy, Borer and Wexler (1992) originally posited that children do not actually comprehend actional passives in an adult-

[6] There is, to our knowledge, no theoretical work on the status of the implicit experiencer of *seem* in adult grammars. We will thus remain agnostic with respect to what the empty category may be. However, it is our hope to convince the reader that at least in child English, the implicit experiencer causes difficulties comparable to those observed with overt experiencers (see Chapter 3, Section 3.3.3); and that this result is compatible with the idea that the implicit experiencer of *seem* is always syntactically projected.

like manner, but are able to use an "adjectival strategy" that leads them to an alternative, closely related interpretation. This is possible because in English, verbal passives with actional verbs (11a) are homophonous with adjectival passives (11b). This strategy, however, is unavailable for non-actional passives (12), leaving immature children with no way of interpreting them.

(11) a. The doll was combed (by the girl).
[describing the 'combing' event]
b. The doll was combed. / The combed doll… / The doll appears combed.
[describing the state of the doll]

(12) a. The doll was liked (by the girl).
[describing the 'liking' state]
b. *The doll was liked. / *The liked doll… / *The doll appears liked.
[describing the state of the doll]

Therefore, in principle, Orfitelli (2012) could account for children's good performance with actional passives like (11a) by assuming that children analyze them as adjectival passives, which, according to Borer and Wexler (1987, 1992), do not involve an (explicit or implicit) external argument or A-movement.[7] However, if one assumes that the adjectival strategy is also

[7] Meltzer-Asscher (2011), McIntyre (2013), Bruening (2014), and Alexiadou, Gehrke, & Schäffer (2014) show that some adjectival passives in English do license *by*-phrases (e.g. '*untouched by human hands'*), as well as instrument and manner modifiers. They conclude that these adjectival passives, like true verbal passives, syntactically project an implicit 'initiator' which can license the *by*-phrase. Note, however, that some adjectival passives can also be based on non-actional verbs (e.g. '*Last night, we lost our very much loved dog'*) and can *also* license *by*-phrases, e.g. '*The dictator remained unsupported/underestimated by the warlords'* (from McIntyre, 2013). Therefore, it is unclear why children would employ an adjectival strategy with actional verbs but not non-actional verbs. Moreover, some of these authors have claimed that adjectival passives also involve A-movement of the internal argument to Spec-AP position (Bruening, 2014), in which case, true verbal passives and adjectival passives should be equally difficult for children to comprehend. The difference, thus, seems to lie in the notion of eventivity (see Section 2.1.1.3).

incompatible with a *by*-phrase, regardless of the verb type, then we should expect children to perform uniformly poorly with both actional and non-actional long passives; i.e. those involving a *by*-phrase. Yet, numerous studies have consistently found an asymmetry between children's performance in actional and non-actional long passives as well. That is, children do well on actional long passives but poorly on non-actional long passives (e.g. Anderson, 2005; Fox & Grodzinky, 1998; Gordon & Chafetz, 1990; Hirsch & Wexler, 2006b; Maratsos, et al., 1985; Orfitelli, 2012).

To account for children's good performance with actional long passives, Fox and Grodzinky (1998) have claimed that children interpret passive *by*-phrases (13a) as nominal "affector/agent" *by*-phrases (13b). This alternative structure would again permit children to interpret long actional passives in a seemingly adult-like manner, but would not extend to long non-actional passives, since the nominal *by* only assigns an "affector/agent" theta role, and not an experiencer theta role (13c-d). This is consistent with the results obtained by Fox & Grodzinsky (1998) among others (e.g. Anderson, 2005; Fox & Grodzinky, 1998; Gordon & Chafetz, 1990; Hirsch & Wexler, 2006b; Maratsos, et al., 1985; Orfitelli, 2012), which show children perform better with actional long passives than non-actional long passives.

(13) a. Mary was kissed by John. (John = agent)
b. The book by John. (John = agent)
c. Mary was loved by John. (John ≠ agent)
d. *The love by John is the sweetest

However, Hirsch and Wexler (2006a) provide convincing evidence this analysis is unlikely to be correct. In an examination of child-directed speech for the 1,051 English-speaking children available in the CHILDES corpus (as of 2006), not a single nominal *by*-phrase was

found, nor was one produced by a child. Moreover, in a Truth-Value Judgment Task, they found that children do not understand the meaning of nominal *by*-phrases (13b) until sometime past five years old, making the explanation untenable.

Going back to the AIH, this account (as well as Hyams & Snyder's [2005, 2006] UFH) predicts that actional *and* non-actional (long) passives should be equally problematic for children, since the structure, just like StSR, involves movement of one argument (thematic object) over another (*by*-phrase) (Collins, 2005a,b). However, the experimental data reviewed in this section indicates that children perform significantly better with actional than non-actional passives (regardless of the presence of the *by*-phrase). The AIH is thus unable to capture this discrepancy.

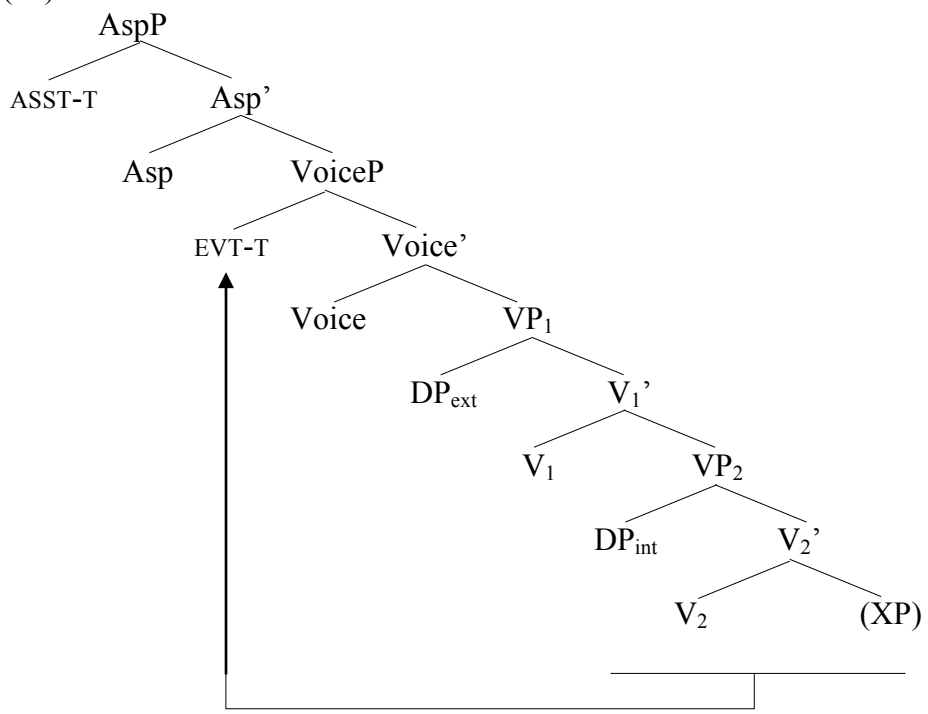
2.1.1.3. The Universal Freezing Hypothesis revisited

More recently, Snyder and Hyams (2015) have proposed a modified version of their original UFH (Hyams & Snyder, 2005, 2006) based on Gehrke and Grillo's (2008) smuggling analysis for the passive. These authors propose a similar analysis to Collins' (2005b) but argue that the movement of the lower VP complex is independent of the promotion of the internal argument to Spec-TP. Instead, they argue that the passive is an operation on event structure, and movement is motivated by interface requirements.

In the spirit of Travis's (2000, *et seq*.) theory of event structure, Gehrke and Grillo employ a VP shell account for the syntactic representation of event structure in which a typical actional predicate has a VP_1, comparable to vP in other theories. VP_1 expresses a causing sub-event and introduces the external argument. V_1 in turn selects a VP_2 that expresses a consequent state and introduces an internal (theme) argument. In a passive, VP_2 moves to Spec-VoiceP to

form a basis for the event time (EVT-T), which subsequently serves as the internal argument of Asp (in the sense of Demirdache & Uribe-Etxebarria, 2000). Importantly, VP_2 serves as the "container" in which the logical object is smuggled past the intervening external argument. From that position the DP can move to Spec-TP and satisfy the EPP. The tree in (14) exemplifies this proposal.

(14)



In essence, Gehrke and Grillo propose that a semantic requirement, some kind of topicalization, singles out the consequent state and assigns it a feature that will determine the movement of the lower VP_2 to a discourse-related projection at the edge of the VP phase, represented as VoiceP. Voice is responsible for grounding the event time in a particular way. In the case of passives the event time is anchored within the consequent state subevent. According to the authors, the feature that triggers movement to VoiceP, π, has two properties: i) it chooses

an element of the atemporal event structure associated with the VP to enrich its semantics by introducing temporality; and ii) it makes it available to the temporal domain and ultimately the discourse domain of the clause; i.e. assertion time, ASST-T, and utterance time, UTT-T (see Grillo, 2008; and Gehrke & Grillo, 2008 for further motivation and details).

One prediction of this analysis is that only transitive verbs associated with an accomplishment or achievement event structure (involving a consequent state) will passivize. Non-actional verbs, on the other hand, are stative, and do not have the internal event-structure needed for passivization. However, the authors further propose that a non-actional verb can still be passivized through semantic coercion, where a type shifting rule adds BECOME (Dowty, 1979) and gives it a related, eventive meaning.[8] In other words, the basic meaning of a verb can sometimes be reconceptualized as the consequent state of an event. For example, the stative sentence in (15a,b) can be coerced into the one in (15c). For adults, the impossibility of semantic coercion (16c) seems to entail the impossibility of passivization (16b) (from Gehrke & Grillo, 2008:14).

(15) a. The news worried / surprised / excited Max.
b. Max was worried / surprised / excited (by the news).
c. Max got into a worried / surprised / excited state.

(16) a. The solution appeals to me / escapes me.
b. *I am appealed / escaped (by the solution)
c. *I got into an appealing / escaping state.

Turning to acquisition, Grillo (2008) briefly argues that children's impoverished

[8] Similar type shift operations have been discussed in the literature. See de Swart (1998) and Travis (2010).

performance in non-actional passives is due to the application of aspectual coercion, which he suggests does not become available until later in development. Snyder and Hyams (2015) adopt this idea and argue for the existence of three different developmental stages. Before the age of four, the child cannot smuggle, as it would constitute a violation of the Freezing Principle. Therefore if an "immature" child tries to move the logical object from its base position up to Spec TP prior to movement of the VP shell, he will need to overcome the RM intervention of the logical subject.[9] Around age four, smuggling becomes available, but only for eventive VP shells. The child can smuggle the logical object past the subject in a VP "container", and then raise it from that VP into Spec TP. This will work, but only for actional verbs.[10] Finally, at about age six, the child gains the ability to coerce non-actional verbs so as to have a consequent state reading, and only then will smuggling be possible in the passive of a non-actional verb.

Importantly for our study, Snyder and Hyams contend that semantic coercion will be needed not only in certain passives, but also in StSR with *seem*-type predicates. Given that *seem* is clearly a non-actional verb, and that only eventive VPs can undergo smuggling, the child will require to coerce the stative VP in which *seem* is contained into having an eventive reading. A derived meaning is exemplified in (17b).

[9] A distinctive feature on the intervener, such as [+Topic], could make this possible. Accordingly, O'Brien et al. (2006) has shown that English-speaking three- and four-year-olds can comprehend long passives, even with non-actional verbs, provided the experimental materials satisfy certain 'pragmatic felicity conditions' for having a *by*-phrase; namely, when the *by*-phrase is made felicitous by having at least one alternative person who *could* have been the logical subject, but is not.

[10] Note that Snyder and Hyams (2015) assume that once children have access to smuggling (at approximately age 4), they interpret actional passives as real verbal passives involving A-movement. Orfitelli (2012) and Borer and Wexler (2003), on the other hand, assume that children analyze those as adjectival passives until approximately age 6. However, as previously mentioned, this analysis does not account for children's good performance with actional long passives, while the revised UFH does.

(17) a. Classical art seems (to many artists) to be less interesting than provocative art.

b. Classical art came to seem (to many artists) to be less interesting than provocative art.

The authors also change their stance with respect to the syntactic status of implicit experiencers and claim that the English raising verb *seem* always has a syntactically projected experiencer argument (following Orfitelli 2012). Therefore, smuggling and semantic coercion will always be required in StSR *seem* sentences.

We will refrain from committing to this syntactic analysis of non-actional passives and StSR *seem*. However, the general idea that children have difficulties applying smuggling with non-eventive predicates receives strong empirical support. Specifically, two different studies have linked children's performance on non-actional passives and StSR with *seem* sentences. Hirsch and Wexler (2007b) investigated children's comprehension of StSR sentences 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 and significant correlation between the scores they obtained in the two constructions, $r(51) = 0.851$, $p < 0.0001$. Similarly, Orfitelli (2012), compared children's comprehension of both (short and long) non-actional passives and StSR *seem* sentences (without an overt experiencer). She found a near perfect correspondence between above chance performance on the two constructions. Importantly, this correspondence was not found with actional passives in either study. This is exactly what Snyder and Hyams' (2015) account predicts. Once semantic coercion – or the ability to smuggle non-eventive VPs – becomes available to a child, its effects are found across the board; i.e. in non-actional passives and StSR *seem* alike.

2.1.2. Performance-based accounts

Another way to explain children's difficulty with interveners is to appeal to performance-based limitations that impinge on the proper functioning of an adult-like grammar system (Bloom, 1990; Valian, 1991). Under this type of accounts, the difficulty associated with StSR (18a) is not unique to this construction, but is merely a reflection of the processing difficulty imposed on children in a variety of other structures that involve crossing dependencies.

Indeed, it has been reported that children up to the age of six experience difficulties with constructions including: *(non-actional) passives* (18b) (Gordon & Chafetz, 1990; Hirsch & Wexler, 2006b; Maratsos et al., 1985; Orfitelli, 2012); *subject control* with *promise* (18c) (C. Chomsky, 1969; Hsu, Cairns, & Fiengo, 1985; Sherman & Lust, 1993); *tough*-movement (18d) (Anderson, 2005; C. Chomsky, 1969); *object relatives* (18e) (Friedmann, Belletti, & Rizzi, 2009; Friedmann & Novogrodsky, 2004; McKee, McDaniel, & Snedecker, 1998); *object wh-questions* (18f) (Avrutin 2000, de Vincenzi, Arduino, Ciccarelli, 1999; Friedmann et al. 2009); and *object topicalizations* (18g) (Friedman & Lavi, 2006).

(18) 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 __.

All these structures manifest what we have been referring to as an "intervention effect" because the difficulty associated with them is believed to arise from the presence of a DP that

interrupts the filler-gap dependency. As opposed to the grammatical accounts we have considered this far, the performance-based account we discuss below links the delay in the acquisition of StSR, and other constructions involving intervention, to processing difficulties that have also observed in adults, albeit to a lesser extent.

2.1.2.1. The Performance-based Intervention Effects Hypothesis

Choe (2012) attributes the delay in the acquisition of StSR to the general difficulty computing an interpretative dependency between a DP, e.g. '*the boy*' in (18a), and the position with which it is associated when a DP intervenes between them, e.g. '*the girl*' (18a), and proposes the Performance-based Intervention Effects hypothesis, described below (Choe, 2012:36):

(19) Performance-based Intervention Effects (PIE): 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.

Crucially, for Choe, the nature of the difficulty is a reflection of a processing limitation of the sort observed in other sentence types and in other populations. She adopts a theory of language processing that has been proposed to explain intervention effects in adults, the Dependency Locality Theory (DLT; Gibson 1998, 2000; Grodner & Gibson, 2005; Warren & Gibson 1999, 2002, 2005).

According to this theory, sentence processing involves integrating lexical items into the existing syntactic and discourse structure, which incurs in *integration cost*, and maintaining open dependencies in working memory, which incurs in *storage cost*. Importantly, under this model,

integration cost is said to increase with the number and type of "discourse referents" that intervene linearly between the elements being integrated within a dependency.[11] A discourse referent is defined as "an entity that has a spatiotemporal location so that it can later be referred to with an anaphoric expression, such as a pronoun for NPs, or tense on a verb." (Gibson, 1998:12). Therefore, under the DLT, any animate or inanimate full DP, proper name, or pronoun would be considered a discourse referent. Let us examine the integration cost predictions of the DLT for the subject and object extracted sentences in (20) (from Gibson, 2000).

(20) a. The reporter who$_i$ the photographer sent t_i to the editor hoped for a story.

b. The reporter who$_i$ t_i sent the photographer to the editor hoped for a story.

In both cases, we have 6 discourse referents, each incurring a cost of 1: *reporter*, *photographer*, *editor*, *story*; and the tensed verbs *sent*, and *hoped*. However, the integration costs of the two sentences differ at the embedded verb *sent*. In the object-extracted sentence, (20a), the object of the verb *sent* must be linked with *who*. In order to form a dependency, it has to cross two discourse referents, *sent* and *photographer*, thus incurring in a cost of 2. Conversely, in the subject-extracted sentence in (20b), the integration of the subject is local, since it does not cross any discourse referent, thus incurring in a cost of 0. These predictions fit the experimental evidence that shows that object relative clauses are more difficult to process than subject relative clauses in adult native speakers (e.g. Ford, 1983; Gibson, Desmet, Grodner, Watson, & Ko,

[11] See Warren and Gibson (1999, 2002, 2005) for evidence that "reference accessibility" affects the distance-based structural integration cost. Specifically, integration cost seems to vary according to the Givenness Hierarchy (Gundel, Hedberg, & Zacharski, 1993). These are (from most central to most peripheral): (1) first-/second-person pronouns; (2) third-person pronouns; (3) first names; (4) full names (including names of famous people and companies); (5) definite descriptions; and (6) indefinite descriptions.

2005; Holmes & O'Regan, 1981; Just, Carpenter, & Keller, 1996; King & Just, 1991; Stromswold, Caplan, Alpert, & Rauch, 1996; Traxler, Morris, & Seely, 2002; Wanner & Maratsos, 1978, *inter alia*).

Going back to acquisition, Choe's (2012) PIE hypothesis makes several predictions, some of which remain untested. First, because processing cost increases with the number of overt DPs that *linearly* intervene between the filler and the gap, children should perform significantly worse when the integration of the embedded predicate with the raised subject requires crossing more lexicalized "discourse referents".[12] That is, we should expect a significant decrease in performance in (21b) with respect to (21a), because it includes an additional DP, '*the girl*' and in (21c) with respect to (21b), because it includes an additional DP, '*the ponytail*'.[13]

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

As already mentioned, the experimental literature on StSR offers divergent results with respect to sentences with covert experiencers (21a). Choe in particular reported that children aged four to six score as well in StSR sentences with fronted experiencers as in unraised sentences. However, as discussed in Chapter 1, there are a number of methodological problems

[12] Although Gibson's DLT makes no mention of implicit arguments, it is conceivable that the processor would also treat them as discourse objects which must be stored and accessed in memory. However, the acquisition hypothesis we will be testing, PIE, specifically predicts intervention effects only with *overt* DPs (Choe, 2012:8, 36-37; Choe & Deen, 2016:123).

[13] 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 (21) as well (see Hawkins, 1994; McElree, 2000; McElree, Foraker, & Dyer, 2003; O'Grady, 1997; O'Grady, Yamashita, Lee, Choo, & Cho, 2000; O'Grady, Lee, & Choo, 2003).

that may cast doubt her conclusions (see Section 1.1.3 for discussion). Furthermore, her proposal does not account for the delays found in constructions such as short (non-actional) passives (Bever, 1970; Gordon & Chafetz, 1990; Hirsch & Wexler, 2006b; Orfitelli, 2012), which should not pose any problem according to the PIE, as there is no overt intervening DP (in the *by*-phrase).

On a related note, if the source of the difficulty in StSR stems not from a grammatical deficit, but from the performance limitations responsible for intervention effects in a variety of other constructions, we should expect to find a correlation in children's performance across different constructions, *ceteris paribus*. That is, if a child lacks the processing capacity to integrate a verb with a moved DP across an intervening DP, then he should equally fail to do so for different structures. Unfortunately, little research has examined the relationship between different constructions in the *same* sample of children, and even more rarely has there been a report on correlations. To our knowledge, the only study that has compared individual performance on different structures that included overt interveners is that of Hirsch and Wexler (2007b).[14] As previously mentioned, while they found a strong positive correlation between performance on StSR and non-actional passives, they did not find one with actional passives. Since the PIE hypothesis does not predict an actional/non-actional asymmetry, this suggests it

[14] Recall Orfitelli (2012) only looked at StSR without an overt experiencer. Also 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.

may be too broad.[15]

Finally, if processing limitations constitute a significant factor underlying children's difficulty in StSR, correct comprehension of StSR sentences should be positively related to an independent measure of processing capacity. Some studies investigating 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; Arosio, Guasti, & Stucchi, 2011; Booth, MacWhinney, & Harasaki, 2000; Felser, Marinis, & Clahsen, 2003; Roberts, Marinis, Felser & Clahsen, 2007). For instance, Roberts et al. (2007) tested priming at the relative clause gap in 5-7 year olds. In this study they also administered a listening span task designed to measure processing capacity (Gaulin & Campbell, 1994), and they found a significant correlation between listening span score and priming in object relative clauses. Similarly, in an object relative clause comprehension task in Italian, Arosio et al. (2011) found that a relation between the score obtained in a digit span task, which is designed to measure short-term memory, and comprehension of these clauses in 9-year-olds. In other words, there is at least some evidence that the delays found in a construction involving intervention may be due to limited memory resources.

Crucially, the PIE hypothesis rejects the hypothesis that grammatical factors are involved in the difficulties observed with StSR and attributes them solely to processing deficits. The experimental results reviewed in this section may reveal a more complex answer; it could also be the case that both grammatical and processing effects influence the development of StSR *seem*.

[15] Unless she assumes, like Fox and Grodzinky (1998), that children analyze actional passives as adjectival and the *by*-phrase is some sort of adjunct, in which case there would be no movement across an intervening experiencer phrase.

By testing the predictions of the PIE hypothesis we may be able to determine whether processing limitations *also* play a crucial role in the acquisition of StSR *seem*.

2.1.3. Directions of the StSR study and summary of predictions

The (revised) Universal Freezing Hypothesis (Snyder & Hyams, 2015), the Argument Intervention Hypothesis (Orfitelli, 2012), and the Performance-based Intervention Effects hypothesis (Choe, 2012; Choe & Deen, 2016) are all based on the notion of intervention. That is, children have difficulty with constructions in which an argument is promoted past a more local one. The theories differ strongly, however, in their predictions regarding the effects of implicit arguments. While the AIH and the revised UFH (Snyder & Hyams 2015) contend that there is an implicit experiencer argument which disrupts the promotion of the subject, the PIE hypothesis maintains that only overt experiencers induce intervention effects. As already mentioned, the experimental literature offers conflicting results with respect to children's performance on StSR *seem* with a covert experiencer. One of the objectives of this dissertation is to provide further evidence relevant to this question by testing the same group of English-speaking children on both explicit and implicit experiencer StSR sentences.

Notably, each of these theories predicts that StSR will not be delayed in cases where there is no explicit or implicit experiencer to bypass. In this regard, the Spanish raising verb *parecer* 'seem' represents an interesting case due to its homophonous nature. When *parecer* appears without an experiencer, it behaves like a modal verb. However, when *parecer* appears with an experiencer, it manifests different syntactic properties and it has a lexical meaning closer to 'think' or 'consider' (Ausín, 2001; Ausín & Depiante, 2000; Bosque & Torrego, 1995; Fernández-Leborans, 1999; Gallego, 2010; Torrego 1989, 1996, 1998, 2002). Crucially, while

the former does not select for an (implicit or explicit) experiencer, the latter does. Therefore, by all accounts, Spanish-speaking children should perform well with the semi-modal *parecer*, but poorly with the lexical verb *parecer*. In the following section we will provide a full overview of the syntactic and distributional properties of these verbs. I will then be able to provide a fuller outline of the predictions of the various theories with respect to English *seem* and Spanish *parecer*.

2.1.3.1. Spanish *parecer* 'seem'

It is commonly reported that in Spanish the presence of an experiencer is incompatible with raising to subject, even when the experiencer is expressed as a clitic, as illustrated in the example below.

(22) a. *Este chico parece ser inteligente*.
this boy seems be intelligent
'This boy seems to be intelligent.'

b. **Este chico me parece ser inteligente*.
this boy 1DAT seems be intelligent
'This boy seems to me to be intelligent.'

However, as Torrego (1996) notes, there are certain combinations of '*parecer* + experiencer + AP/NP' that are grammatical, as shown in (23).

(23) *Este chico me parece inteligente*.
this boy 1DAT seems intelligent
'This boy seems to me (to be) intelligent.'

Analyses of the sentence in (23) have changed over time. Torrego (1996) argued that there is no raising at all in these cases, and that *parece* 'seems' and *inteligente* 'intelligent' form

a single predicate that selects for the subject *este chico* 'this boy'. However, Ausín (2001) convincingly showed on the basis of binding facts that at some point in the derivation the subject in constructions like (24) is c-commanded by the experiencer. In view of this, Torrego (2002) also adopts a movement account in which the subject raises over the experiencer.

(24) a. *[El amigo de Juan$_i$]$_k$ le$_i$ pareció t_k inteligente.
The friend of John 3DAT seems intelligent
'John's friend seemed to him (to be) intelligent.'

b. [El amigo de su$_i$ hermano]$_k$ le$_i$ parece t_k inteligente a todo el mundo$_i$.
The friend of his brother 31DAT seems intelligent to all the world
'His brother's friend seems to him, to everyone, (to be) intelligent.'

Interestingly, the presence of the experiencer is incompatible with raising to subject when the AP is a stage-level predicate, as in (25).

(25) **Este chico me parece cansado*.
this boy 1DAT seems tired
'This boy seems to me (to be) tired.'

Building on Raposo & Uriagereka (1995; 2002), Bosque and Torrego (1995), and Fernández-Leborans (1999), Ausín (2001) suggests that the contrasts in (22-23) follow from the existence of two different verbs *parecer* (see Haegeman, 2006, for a similar analysis of Italian *sembrare* 'seem'):[16]

[16] In accordance with the literature, we refer to the modal-like verb as 'bare *parecer*' and the lexical verb as 'opinion *parecer*'. However, note that in Mateu & Hyams (2016) we refer to them as 'functional *parecer*', F-*parecer*, and 'lexical *parecer*', L-*parecer*.

i) Bare *parecer* (no experiencer): An epistemic semi-modal verb compatible with stage/individual-level predicates.

ii) Opinion *parecer* (requires experiencer): A lexical verb compatible only with individual-level predicates.

Accordingly, (23) should be read as "I *think* this boy is intelligent". Torrego (2002) adds that grammaticalized forms like *me parece* (literally, 'it seems to me') or *¿Qué te parece?* (literally, 'what does it seem to you?') meaning 'I think' and 'What do you think?', respectively, reinforce this analysis. Thus, the ungrammaticality of (25) would simply be due to the fact that in Spanish, opinion verbs take only individual-level small clauses (see Raposo & Uriagereka, 1995), as illustrated in (26).

(26) a. *Considero a Juan inteligente*.
I-consider to John intelligent
'I consider John intelligent.'

b. **Considero a Juan cansado*.
I-consider to John tired
'I consider John tired.'

Bare *parecer* is an epistemic semi-modal, and it is also a raising verb, as evidenced by the standard diagnostics for raising[17] (Torrego 1998). Consider the idiom *De noche todos los gatos son pardos* (literally 'at night, all cats are brown') which means 'when it's dark, physical appearance is not so noticeable'. The possibility of having a subject of a sentential idiom in

[17] This is not surprising if we assume that modals are raising verbs (see Wurmbrand, 1999, 2001, 2007; Wurmbrand & Bobaljik, 1999).

matrix subject position without losing its idiomatic reading shows that the subject of the main clause must have raised from the embedded clause (27).

(27) *De noche, todos los gatos parecen (ser) pardos*.
At night all the cats seem be brown
'In the dark, everyone looks the same.'

The impossibility of embedding '*parecer* XP' under causatives also suggests that raising has taken place (Aissen, 1974):

(28) **Su expresión hacía parecer sufrir a Juan*.
His expression made seem suffer to John
'His expression made seem Juan to suffer.'

Similarly, the scope ambiguities involving quantificational subjects are also a sign of raising (May, 1977). As in the case of English, the subject in the sentence in (29) may have narrow scope or wide scope over *parecer*.

(29) *Poca gente parece leer mucho*.
Few people seem read a-lot
'Few people seem to read a lot.'

Going back to the dual status of the two raising verbs *parecer*, evidence for the homophony is provided by tense, aspect, and mood selection. For instance, both bare *parecer* and opinion *parecer* can appear in the present (30) and imperfect (31), bare *parecer* cannot occur in the preterit (32a), progressive (33a), or perfect (34a).[18]

[18] To my knowledge, bare *parecer* is the only non-canonical modal that disallows the perfective aspect.

(30) a. *Este chico parece (ser) inteligente*. [Bare *parecer*]
this boy seems be intelligent
'This boy seems (to be) intelligent.'

b. *Este chico me parece inteligente*. [Opinion *parecer*]
this boy 1DAT seems intelligent
'This boy seems to me (to be) intelligent.'

(31) a. *Este chico parecía (ser) inteligente*. [Bare *parecer*]
this boy seem-IMPF be intelligent
'This boy seemed (to be) intelligent.'

b. *Este chico me parecía inteligente*. [Opinion *parecer*]
this boy 1DAT seem-IMPF intelligent
'This boy seemed to me (to be) intelligent.'

(32) a. **Este chico pareció (ser) inteligente*. [Bare *parecer*]
this boy seem-PRET be intelligent
'This boy seemed (to be) intelligent.'

b. *Este chico me pareció inteligente*. [Opinion *parecer*]
this boy 1DAT seem-PRET intelligent
'This boy seemed to me (to be) intelligent.'

(33) a. **Este chico está pareciendo (ser) inteligente*. [Bare *parecer*]
this boy is seeming be intelligent
'This boy is seeming (to be) intelligent.'

b. *Este chico me está pareciendo inteligente*. [Opinion *parecer*]
this boy 1DAT is seeming intelligent
'This boy is seeming to me (to be) intelligent.'

(34) a. **Este chico ha parecido (ser) inteligente*. [Bare *parecer*]
this boy has seemed be intelligent
'This boy has seemed (to be) intelligent.'

b. *Este chico me ha parecido inteligente*. [Opinion *parecer*]
this boy 1DAT has seemed intelligent
'This boy has seemed to me (to be) intelligent.'

In addition, bare *parecer* (35a), as in the case of other modals (35b), allows the subjunctive in the subordinate clause, on contrast to opinion *parecer* (35c):

(35) a. *Parece que lloviera*.
seems that rain-SBJV
'It seems that it is raining.'

b. *Puede que lloviera.*
may that rain-SBJV
'It may have rained.'

c. **Le parece que lloviera.*
3DAT seems that rain-SBJV
'It seems to him that it is raining.'

As noted in Torrego (2002), the fact that bare *parecer* allows clitic-climbing with certain verbs is also consistent with this modal analysis:

(36) a. *Juan parece haberlo resuelto*.
John seems have-it solved
'John seems to have solved it.'

b. *Juan lo parece haber resuelto*.
John it seems have solved
'John seems to have solved it.'

(37) a. *Juan podría haberlo resuelto*.
John could have-it solved
'John could have solved it.'

b. *Juan lo podría haber resuelto*.
John it could have solved
'John could have solved it.'

The modal-like behavior of Spanish bare *parecer* can also be observed when associated with standard modals. In Spanish the second modal verb in a sequence of two modals cannot be interpreted as epistemic (Picallo, 1990). Torrego (1998) shows that bare *parecer* imposes the same type of restriction on its adjacent modal. For instance, in (38a) and (38b), *poder* 'may' must have a deontic reading, and it cannot be interpreted as epistemic, i.e. (38a) cannot mean: '*It must be the case that it is possible that Lupe speaks Zapotec*', and (38b) cannot mean '*It seems to be the case that it is possible that Lupe speaks Zapotec*'. Similarly, *parecer* has the same effect

on the second modal verb, *deber* 'must' in (38d) as *poder* 'may' does in (38c).

(38) a. *Lupe debe poder hablar zapoteco*.
Lupe must may speak Zapotec
'Lupe must be able to speak Zapotec.'

b. *Lupe parece poder hablar zapoteco*.
Lupe seems may speak Zapotec
'Lupe seems to be able to speak Zapotec.'

c. **Lupe puede deber hablar zapoteco*.
Lupe may must speak Zapotec
'Lupe may must speak Quechua.'

c. **Lupe parece deber hablar zapoteco*.
Lupe seems must speak Zapotec
'Lupe seems must speak Zapotec.'

Finally, as noted by Fernández-Leborans (1999), neither modals nor bare *parecer* allow pseudo-clefts (39a). However, other (restructuring) verbs do (39b).

(39) a. *Lo que {puede / debe / parece} Juan, es saber la noticia.
It that may must seem John is know the news
'What John {can/ must/ seems}, is to know the news.'

b. Lo que {pretende / quiere} Juan, es saber la noticia.
It that hopes wants John is know the news
'What John {wants/ desires}, is to know the news.'

In sum, bare *parecer* is an epistemic semi-modal verb, and it cannot appear with an experiencer (clitic or full DP) simply because modals do not select for (overt or covert syntactic) experiencer arguments. On the other hand, opinion *parecer* is closer to a lexical verb and selects for an experiencer argument. Crucially, the appearance of an overt experiencer forces this second 'think' reading (Ausín, 2001; Ausín & Depiante, 2000; Bosque & Torrego, 1995; Fernández-Leborans, 1999; Torrego, 2002). Therefore, when *parecer* appears without an overt experiencer, we are dealing with the modal bare *parecer,* which has no explicit nor implicit experiencer.

Conversely, when we find *parecer* with an experiencer, we are always dealing with opinion *parecer*.

This has important consequences for our cross-linguistic study. If English-speaking children perform poorly with StSR *seem* without an overt experiencer, and Spanish-speaking children do well with StSR bare *parecer*, that will provide strong evidence for the idea that implicit arguments may induce intervention effects. If children from both language groups do well, then we can conclude that intervention effects only arise with overt material. Finally, if both language groups perform poorly, the intervention account will become suspect. The first part of the StSR study will address this question by examining children's performance on StSR *seem* (covert experiencer) and StSR bare *parecer* (no experiencer).

In the second part of our study, we will investigate more closely the predictions of the processing account. We will do this in two ways, (i) by manipulating the length of the intervening experiencer in StSR *seem* and StSR opinion *parecer*, and (ii) by comparing the individual scores with two independent measures or processing and short-term memory capacity. Table 2.1. summarizes the predictions we will test in the StSR study (Chapter 3).

Table 2.1. Predictions for the subject-to-subject raising study.

Prediction	**Grammatical Accounts (AIH, UFH [2015])**	**Processing Accounts (PIE)**
Performance on StSR *seem* with covert experiencer in English	Poor	Good
Performance on StSR bare *parecer* (no experiencer) in Spanish	Good	Good
Performance on StSR with *seem*/opinion *parecer* 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. Accounting for the acquisition of subject control

As discussed in Chapter 1, experimental studies investigating the acquisition of SC *promise* predate by decades recent syntactic theories (C. Chomsky, 1969; Hsu, Cairns, & Fiengo, 1985; Sherman & Lust, 1993). However, the results have been consistently replicated – children up till the age of six or seven generally choose the object as the antecedent of PRO, as opposed to the subject, again reflecting an avoidance for what appears to be a minimality violation. In Sections 2.2.1 and 2.2.2 we will discuss the most recent grammar- and processing-based analyses that have been proposed to account for this difficulty. In Section 2.2.3 we will summarize the predictions and lay out the possible outcomes of this study.

2.2.1. Grammar-based accounts

Similarly to the case of StSR *seem*, SC *promise* appears to represent a violation of the Minimal Distance Principle (MDP; Rosenbaum, 1967), now also subsumed under Relativized Minimality (Rizzi 1990, 2004). Evidence showing that the benefactive DP c-commands into the

embedded clause is provided by tests such as Principle C effects (40a), pronominal binding (40b), and NPI licensing (40c).

(40) 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; 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.[19] In the following sections we will discuss two different grammatical accounts, neither of which has been tested yet.

2.2.1.1. The Null Preposition Hypothesis

In an attempt to explain why *promise* is exceptional in allowing the benefactive to be ignored for purposes of minimality, Boeckx and Hornstein (2003), Hornstein and Polinsky (2010), and Boeckx, Hornstein, and Nunes (2010) propose that the benefactive argument, *Mary*, in sentences like (41a), is a concealed PP, which is headed by a null preposition similar to the one found in other SC *promise*-like verbs, such as *swear* and *commit* (41b-c), or the one that appears with nominal complements of *promise*, as in (41d).

[19] We will only discuss those accounts that aim for explanatory adequacy and go beyond the simple postulation of *promise* as an exceptional or "marked" verb (e.g. C. Chomsky, 1969; Hornstein, 1999; Sherman & Lust, 1993).

(41) a. John promised [P_{null} Mary] [to take care of the baby]
b. John swore [to Mary] [to take care of the baby]
c. John committed [to Mary] [to take care of the baby]
d. $John_i$'s promise [to Mary] [to take care of the baby]

Additionally, proponents of the Movement Theory of Control (MTC; Hornstein 1999, *et seq.*; Manzini & Roussou 2000; O'Neill, 1995) appeal to Occam's Razor and contend that obligatory control is derived by A-movement, thus drawing a parallel between SC and StSR. The authors thus point out there are StSR analogs of *promise* in English, as illustrated in (42a) in which one would assume that the experiencer is also headed by a null P.

(42) a. John strikes [P_{null} Mary] [as nice]
b. John seems [to Mary] [to be nice]

The idea that the benefactive of *promise* is embedded in a PP is also bolstered by the observation that in Spanish, among other languages, the benefactive is overtly marked with the inherent case-marking preposition *a*, illustrated in (43).

(43) *Juan prometió* [***a*** *María*] *cuidar del bebé.*
John promised to Mary take-care of-the baby
'John promised Mary to take care of the baby.'

The authors of this 'concealed P' hypothesis thus reason that the benefactive argument of *promise* is not a genuine intervener for the subject because it does not c-command the base

position – the P node that dominates the benefactive makes it invisible for minimality purposes.[20] Turning to acquisition, Hornstein and colleagues propose that children's delay with *promise* is due to the difficulty in "pinning down" this null preposition since the evidence for it, e.g. (41b-d), is too scarce.

They argue that if one adopts a non-relativized version of UTAH (Baker, 1997), then arguments will be projected to grammatical-function positions on the basis of their thematic proto-role (Dowty, 1991). DP arguments that are internal to VP and that have sufficient theme/patient properties will be assigned to direct object positions. Those that do not have enough of such properties but have goal/path/location properties will be treated as oblique and mapped to the object of some preposition. However, there will be cases where the relevant proto-role may be obscure (and/or ambiguous). One way to alleviate obscurity is by syntactic means, e.g., having an overt preposition will signal that a DP is not a direct object of the verb. If such overt evidence is absent, then ambiguity will be rife and more subtle calculations of semantic consequences will be needed to settle matters. According to the proponents of this account, *promise* is such a case.

The authors of what we will refer to as the Null Preposition Hypothesis (NPH) consequently predict a more protracted development for SC *promise* than for SC verbs like *swear*, where the preposition is overtly expressed on the benefactive. Unfortunately, the

[20] The question that then arises is why examples like the ones in (40) suggest the opposite. The authors assume Kitahara's (1997) proposal regarding these same effects in StSR *seem* constructions. Specifically, they claim that at the point when raising takes place (assuming the MTC), the DP is within PP and does not induce intervention effects, as in (41). Later on in the derivation, the DP undergoes covert movement to a position from where it c-commands the clausal complement, thereby inducing the effects observed in (40) (see also Boeckx, 1999). They alternatively suggest that 'to' is a marker of inherent case and that inherently case-marked elements may not induce intervention in A-movement (Nunes, 2007, 2008).

experimental work that exists on the acquisition of SC with *promise*-type verbs is reduced to the verbs *promise* (C. Chomsky, 1969; Hsu et al., 1985; Sherman & Lust, 1993) and *ask* (followed by an indirect *wh*-question) (C. Chomsky, 1969), both of which lack an overt preposition on the intervener. Similarly, because benefactives of SC verbs in Spanish always have an overt case-marking preposition *a*, Hornstein and colleagues also predict Spanish-speaking children to master SC with *promise*-type verbs earlier than English-speaking children. However, there is, to our knowledge, no experimental work on the development of SC *prometer* 'promise' in Spanish-speaking children. These contrasts provide suitable testing ground for the NPH predictions.

2.2.1.2. Difficulties with smuggling

Belletti and Rizzi (2013) have recently proposed that *promise*-type verbs undergo a smuggling-type process which makes the subject the closest controller of PRO. In this sense, subject control across a benefactive would be akin to raising across an experiencer (without necessarily implying a movement theory of control). The authors argue that smuggling operations are costly and will therefore only by acquired in a "relatively late" temporal window.

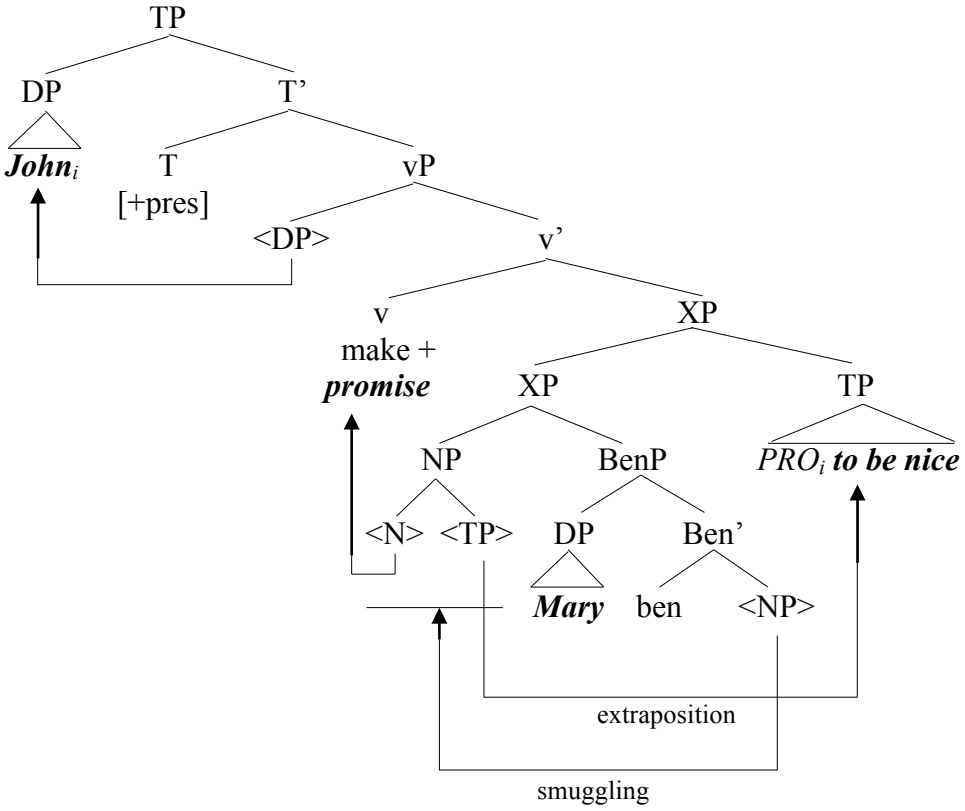
The Belletti and Rizzi analysis is based on lexical decomposition of the verb-structured meaning à la Hale and Keyser (1993) (see also Folli & Harley, 2007; Ramchand, 2008; Travis, 2000). According to the authors, *promise* and *order* allow paraphrases with different light verbs plus nominal elements, as exemplified in (44).

(44) 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 *Bill* 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*, *Bill* 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 Collins' [2005a] raising tree in (4)), hence the representation in (45). Belletti and Rizzi propose that the locality problem can be overcome by a leftward movement of the NP '*promise PRO to go*', which smuggles *promise* to a position suitable for incorporation. 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'*.[21] 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.

[21] The authors suggest that extraposition is made mandatory by the necessity of having *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.

(45) 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 Hyams and Snyder (2005, 2006). Since under this analysis, SC with *promise* involves as much derivational machinery as StSR with *seem*, i.e., smuggling and extraposition (according to Collins' (2005a) analysis, see (4)), they predict the developmental course of the two constructions to be roughly on a par (Belletti & Rizzi, 2013:126).

It is noteworthy to recall that children seem to acquire (long) actional passives earlier than (long) non-actional passives and that performance on the latter construction correlates with children's performance on StSR *seem* (see Section 2.1.1.3). Thus, Snyder and Hyams (2015) hypothesize that after approximately the age of four, children's difficulty is not with the

smuggling operation *per se*, but with semantic coercion, which is a requirement for smuggling non-eventive VPs. Since *promise* is an eventive verb, one may predict it to pattern with actional passives, and yet *promise* does not seem to be mastered until at least age 6 (C. Chomsky, 1969; Hsu et al., 1985; Sherman & Lust, 1993). However, it could still be the case that children's difficulties with passives, StSR *seem*, and SC *promise* are all due to smuggling, and that children use some sort of strategy that allows them to perform in a seemingly adult-like manner with long actional passives (Fox & Grodzinsky, 1998), as mentioned in Section 2.1.1.2. Comparing children's individual performance on StSR *seem*/opinion *parecer* and SC *promise*/*prometer* will allow us to address this question.

2.2.2. Performance-based accounts

Although to our knowledge, there is no performance-based account that has specifically addressed the protracted development of SC *promise*, the same line of argumentation that applies to StSR *seem* would also apply to SC *promise*. That is, the nature of the difficulty with SC would not be grammatical, but a reflection of processing limitations of the kind observed in other construction types that involve crossing dependencies including StSR *seem*, passives, and object relative clauses. In the following section we briefly discuss the specific predictions that an account such as the PIE (Choe, 2012; Choe & Deen, 2016) hypothesis would make for SC *promise*.

2.2.2.1. Performance-based Intervention Effects Hypothesis

As mentioned in Section 2.1.2.1, Choe (2012) did not test SC *promise*; however the PIE hypothesis attributes the delay in a series of constructions, including SC *promise* (Choe,

2012:36), to the general difficulty computing an interpretative dependency between a DP, e.g. '*the boy*' in (46a), and the position with which it is associated when a DP intervenes between them, e.g. '*the girl*' in (46a). According to the DLT (Gibson 1998, 2000; Grodner & Gibson, 2005; Warren & Gibson 2002), the processing theory that Choe bases her PIE hypothesis on, *integration cost* increases with the number of "discourse referents" that intervene linearly between the elements being integrated within a dependency. Therefore, PIE predicts that children should perform significantly worse when the number of referents increases, as in (46a), as opposed to (46b), where the DP '*the ponytail*' must be additionally computed.

(46) a. The boy promised the girl __ to be nice.
b. The boy promised the girl with the ponytail __ to be nice.

Moreover, if processing limitations are indeed a major contributor to children's difficulty in SC *promise*, correct comprehension of this construction should be positively correlated with an independent measure of processing or working-memory capacity, as has been found in the case of object relative clauses in English-speaking children (e.g. Booth et al. 2000; Felser et al. 2003; Roberts et al., 2007) and Italian-speaking children (e.g. Arosio et al. 2009; Arosio et al. 2011).

Finally, if the performance limitations responsible for the difficulties with a variety of structures are also in effect in SC *promise*, we should expect to find a correlation in children's performance across different constructions. That is, we should find a within-subject correlation in children's performance on SC *promise*/*prometer and* StSR *seem*/opinion *parecer* (both with overt interveners), since both involve the presence overt intervening DP between the matrix subject and the embedded subject position it is associated with.

2.2.3. Directions of the SC study and summary of predictions

The Null Preposition Hypothesis (Boeckx & Hornstein, 2003; Boeckx, Hornstein, & Nunes, 2010; Hornstein & Polinsky, 2010) argues that *promise* has a null preposition in its structure similar to the one found in other *promise*-like verbs such as *vow* and *commit*. On this view, subject control obtains because the object of the preposition does not c-command the position that the empty category occupies, at least in overt syntax, and hence does not act as an intervener. They claim that English-speaking children have difficulties obtaining subject control with *promise* because they fail to represent the P_{null}, which shields the benefactive DP from being a potential intervener for the control of PRO (or A-movement, assuming the MTC). This hypothesis thus predicts that children will be more likely to incorrectly show object control with *promise* than with *swear*-type verbs due to the opaqueness of the PP structure. Similarly, they predict Spanish-speaking children to master SC with *prometer* earlier than English-speaking children, as benefactives in Spanish are always headed by the case-marking preposition *a* 'to'. Additionally, the MTC predicts children will perform at a comparable rate with *seem* and *swear*, as both involve StSR and an intervening argument headed by an overt preposition. Also note that if the MTC is correct, and control is derived by A-movement, Orfitelli's (2012) AIH would also predict parallel developmental paths for StSR with *seem*-type predicates and SC with *promise*-type verbs.

Under Belletti and Rizzi's (2013) smuggling analysis, SC *promise* is derived by smuggling, an operation which the authors claim is generally difficult for children. Since both StSR *seem* and SC *promise* require smuggling and extraposition, they predict these two to develop at approximately the same time in the child. However, if as Snyder and Hyams (2015) argue, smuggling (with actional verbs) is available to children after the age of four, we should

expect to find children performing well with SC *promise* at approximately that age, but poorly with StSR *seem* until approximately age 6, as the latter requires the additional step of semantic coercion.

Turning to processing-based explanations, these accounts predict a parallel development of StSR *seem* and SC *promise.* Additionally, a hypothesis like the PIE specifically predicts that children will perform significantly worse when we lengthen the intervening material by adding a DP. Moreover, processing accounts predict that children with more developed processing capacity will be more likely to correctly compute SC *promise* sentences.

In Chapter 4 we will address the main predictions of Hornstein and colleagues' NPH hypothesis by comparing English-speaking children's performance on SC *promise* and SC *swear*, as well as Spanish-speaking children's performance on both *prometer* 'promise' and *jurar* 'swear'. Additionally, we will test the predictions of Choe's PIE by manipulating the length of the intervening experiencer in SC *promise*/*prometer* and by comparing children's individual scores on SC with two independent measures of processing and short-term memory capacity. More generally, we will be investigating if intervention effects also emerge in other SC verbs and languages, and whether these are susceptible to processing limitations. Finally, we will compare children's performance on StSR and SC. Both the grammatical and processing accounts we have considered in this chapter predict there should be a correlation in the development of these two constructions. However, if we find that not to be the case, we will need to evaluate other syntactic analyses of SC *promise* (e.g. Jackendoff & Culicover, 2003; Landau, 2015; Larson, 1991) that dissociate analyses SC promise from StSR *seem*. Table 2.2 summarizes the major predictions we will evaluate in the SC study.

Table 2.2. Predictions for the subject control study.

Prediction	**Grammatical Accounts**		**Processing Accounts**
	(NPH)	**(Smuggling)**	**(PIE)**
Performance on English SC *promise* vs English SC *swear,* and Spanish *prometer* and *jurar*	Worse	Same	Same
Performance on SC *promise/prometer* 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
Performance on SC and StSR	Same [22] (*seem* - *swear*)	Same	Same

[22] Since the proponents of the NPH analyze obligatory control as raising, they would expect there to be 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 a superset 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.

CHAPTER 3

Study 1: Acquisition of Subject-to-Subject Raising

This study investigates the acquisition of subject-to-subject raising (StSR) in English- and Spanish-speaking children. The main questions that we aim to answer are: i) Are the delays observed in StSR with *seem*-type verbs due to intervention effects? And if so, can implicit arguments induce intervention effects? ii) Are these intervention effects caused by grammatical or processing deficits?

As discussed in Chapter 2, despite the extensive literature on the acquisition of StSR in English, there remain competing accounts of the delay. One prominent account holds that the experiencer argument induces an intervention effect, either for grammatical or processing reasons. The most recent grammatical accounts, the (revised) Universal Freezing Hypothesis (UFH; Snyder & Hyams, 2015), and the Argument Intervention Hypothesis (Orfitelli, 2012), both posit that children's difficulties with *seem* in StSR environments are due to a grammatical constraint that bans A-movement over an intervening argument. Specifically, Hyams and Snyder propose that StSR, like non-actional passives, require two operations: smuggling and semantic coercion. In the early acquisition stages, children do not have access to the smuggling and raising strategy that adults use, and so the intervening argument blocks subject promotion in StSR. At approximately age four, children master this operation, but they are still unable to coerce non-actional verbs so as to add the necessary VP shell for smuggling to take place. Therefore, they are able to comprehend actional passives, but they will still experience difficulties with non-actional passives and StSR until approximately age six, when semantic coercion becomes

available. Orfitelli abstracts away from specifics and simply argues that children cannot A-move across a structurally intervening argument. Crucially, however, these two grammatical accounts assume that similar to the covert *by*-phrase in passives, the covert experiencer in StSR is nonetheless syntactically projected (Baker, Johnson, & Roberts 1989; Collins, 2005b; Gehrke & Grillo, 2008). They therefore predict that children will perform poorly with *seem* in StSR contexts even when there is no overt experiencer.

Performance accounts, such as the Processing-based Intervention Effects hypothesis (PIE; Choe, 2012, Choe & Deen, 2016), attribute children's difficulties with StSR *seem* to the increased processing cost associated with the presence of an *overt* DP between the matrix and embedded subject positions. Choe draws a parallel between the effects observed in StSR and other constructions that entail dependencies across an intervener, such as object relative clauses, or subject control *promise*. Importantly, PIE does not make recourse to a non-adult-like linguistic system but instead points to performance-based limitations which impinge on the proper functioning of an adult-like grammar system.

As discussed in Section 1.1.2, the experimental literature offers divergent results with respect to children's performance on StSR *seem* without an overt experiencer. Hirsch, Orfitelli and Wexler (2007), Hirsch (2011), and Orfitelli (2012) found that children do poorly with StSR *seem* without an overt experiencer. Becker (2006), on the other hand, found that English-speaking children were able to understand *seem* sentences when the experiencer was implicit but failed at raising past an overt experiencer. Similarly, Choe (2012) found that children have difficulty comprehending StSR sentences that contain an intervening experiencer, but this difficulty disappears when the experiencer is fronted. One of the aims of our StSR study is to test the same group of English-speaking children on both overt and covert experiencer conditions

using the same methodology and set of stories.

We will also provide novel crosslinguistic data relevant to this question by investigating the acquisition of StSR in a language where *seem* does not select for an experiencer. As discussed in Chapter 2, in Spanish, the modal-like verb *parecer* does not select for an experiencer, as observed in (1b), in contrast with the English example in (1a). On the other hand, the lexical verb opinion *parecer,* which only takes non-verbal small clause complements, does select for an experiencer argument (1c).

(1) a. The boy seems (to his mother) to be smart. (English)

b. *El chico (*le) parece (*a su madre) ser listo*. (Spanish)
The boy 3DAT seems to his mother be smart
'The boy seems (to his mother) to be smart'.

c. *El chico le parece a su madre listo*. (Spanish)
The boy 3DAT seems to his mother smart
'The boy seems to her mother (to be) smart'.

The dual status of *parecer* is also reflected in differences of tense, aspect, and mood selection. Importantly, only if the experiencer is overt do we obtain the opinion reading (Ausín, 2001; Bosque & Torrego, 1995; Gallego, 2010; Torrego, 2002). In other words, we can safely assume that there is no implicit experiencer projected in cases where there is no overt experiencer, i.e. with bare *parecer*.

Therefore, both the grammatical and processing accounts predict that Spanish-speaking children should have no difficulties with StSR sentences with bare *parecer*, since there is no explicit or implicit argument to by-pass. This question will be briefly examined using production data from the Child Language Data Exchange System (CHILDES; MacWhinney, 2000), and by testing children's comprehension of sentences containing StSR *seem* and bare *parecer*.

The second goal of this study is to contribute additional evidence relevant to the question of whether grammatical or processing deficits are behind children's difficulties with StSR. Many processing-based accounts, including Choe's PIE, predict that distance between two related elements in a sentence (e.g., head-dependent or pronoun-antecedent) crucially affects the speed and accuracy with which that sentence is parsed, with greater distance leading to decreasing performance (Baumann, 2014; Demberg & Keller, 2008; Frazier, 1987; Gibson, 1998, 2000; Hawkins, 1994; Kimball, 1973; McElree, 2000; McElree, Foraker, & Dyer, 2003; O'Grady, 1997; Pearlmutter & Gibson, 2001; Phillips, Kazanina, & Abada, 2005; Stevenson, 1994; Vosse & Kempen, 2000; Wagers & Phillips, 2013, *inter alia*).

As mentioned in Section 2.1.2.1, Choe specifically relies on Gibson's Dependency Locality Theory (DLT; Gibson 1998, 2000; Grodner & Gibson, 2005; Warren & Gibson, 2002). According to this model, we should expect increased difficulties with StSR when the intervening experiencer includes an embedded DP, thus lengthening the distance between the filler and the gap, as in (2b), the 'long' counterpart of (2a).

(2) a. The boy seems to the teacher to be smart.
b. The boy seems to the teacher with glasses to be smart.

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 significantly correlated with an independent measure of verbal processing and/or short-term memory capacity, as observed in the case of object relative clauses (e.g. Arosio, Adani, & Guasti, 2009; Arosio, Guasti, & Stucchi, 2011; Booth, MacWhinney, & Harasaki, 2000; Felser, Marinis, & Clahsen, 2003; Roberts, Marinis, Felser & Clahsen, 2007). Competence-based explanations, however, contend

that the observed delays result from a non-adult grammar (e.g. no access to the smuggling or semantic coercion strategy that adults use). Therefore, no such correlation is expected –albeit not excluded either.

Summarizing, the main focus of the experiments discussed in this chapter is to provide a better understanding of what properties of StSR are particularly challenging for children to master and why. In the first study described in this chapter, we investigate if the delays observed with this construction stem from intervention effects, and whether covert experiencers may induce intervention effects. An experimental comparison between children's performance on English StSR *seem* with a covert experiencer and Spanish StSR bare *parecer* (with no experiencer) should bear fruitfully on this question.

In the second study we attempt to determine whether the purported intervention effects are caused by grammatical or processing deficits. In this experiment we will test sentences containing English StSR *seem* with an overt experiencer and Spanish StSR opinion *parecer* with an experiencer. We will address this question by comparing children's performance on sentences with "short" (i.e. DP) and "long" (i.e. DP[PP]) experiencer phrases, as well as by comparing children's performance on these conditions with two independent measures of short-term memory and processing capacity.

3.1. Corpus study

Before moving onto the experimental studies, we briefly report on adults' and children's naturalistic production of sentences with *seem*/*parecer*. The main goal of this corpus study is to assess the predictions of several competing analyses regarding the effects of the (purported) implicit experiencer argument of *seem* and the lack thereof in the case of bare *parecer*.

Utterances containing the raising verb *seem* and (bare/opinion) *parecer* were extracted from all the English and Spanish corpora available in the CHILDES database (MacWhinney, 2000) as of May-July 2016 and classified into two groups –speech produced by adults and speech produced by children younger than 6;11.29. Each utterance was then manually coded for: i) whether it was unraised or raised; ii) the presence and the position of an experiencer; iii) the type of complement.

The results do not include sentences in which it was not possible to determine whther the construction was raised or unraised. This included i) interrupted sentences, as in '*seems'* or '*me parece*' (English: N = 13 in adults, N = 3 in children; Spanish N = 66 in adults, N = 36 in children); ii) sentences missing a(n overt) complement. These only included slifting examples ("sentence lifting", Ross, 1973; Stowell, 2005) with opinion *parecer*, e.g. '*Tengo otro, me parece'* 'I have another, I think' (N = 122 in adults; N = 30 in children). The results are given in the two tables below:

Table 3.1. Results from CHILDES corpus study for English *seem*.

Construction	**Adults**	**Children**
***seem*, covert exp.**	**96% (1213)**	**94.12% (64)**
Unraised *seem*	7% (91)	23.44% (15)
Raised *seem*	93% (1122)	76.56% (49)
***seem*, overt exp.**	**4% (53)**	**5.88% (4)**
Unraised *seem*	42% (22)	50% (2)
Raised *seem*	58% (31)	50% (2)

Table 3.2. Results from CHILDES corpus study for Spanish *parecer*.

Construction	Adults	Children
bare *parecer,* no exp.	**40% (460)**	**65.9% (143)**
Unraised F-*parecer*	19% (87)	22.38% (32)
Raised F-*parecer*	81% (373)	77.62% (111)
opinion *parecer,* overt exp.	**60% (689)**	**34.1% (74)**
Unraised L-*parecer*	64% (440)	75.68% (56)
Raised L-*parecer*	36% (249)	24.32% (18)

The most striking cross-linguistic difference concerns the use of an overt experiencer. While English-speaking adults produce *seem* with an overt experiencer as opposed to a covert experiencer 4% of the time; Spanish-speaking adults use opinion *parecer* (with an experiencer) as opposed to bare *parecer* 60%. It is thus plausible to expect that the higher frequency of opinion *parecer* may lead to earlier acquisition in comparison to the English-speaking children. However, it is essential to recognize that if frequency in the input were the *only* predictor for the acquisition of these constructions, children should perform better with StSR *seem* than unraised *seem*, contrary to the facts (Hirsch, 2011; Hirsch, Orfitelli, & Wexler, 2007; Orfitelli, 2012).

Imprtantly for the accounts we intend to test in this dissertation, we find some interesting differences regarding children's likelihood of using the unraised construction in comparison to adults. Fisher exact tests show that the unraised / raised ratio is significantly different in English-speaking adults and children. Specifically, children are more likely to use the unraised construction compared to the adults. This is not only true for *seem* overall (with and without an experiencer), $p < .001$, but crucially, also for *seem* with a covert experiencer, $p < .001$. These results are in line with grammatical accounts that claim that the implicit experiencer of English

seem is nonetheless syntactically projected thus causing intervention (Orfitelli, 2012; Snyder & Hyams, 2015).[1]

Conversely, Fisher exact tests show that the unraised / raised ratio is not significantly different in Spanish-speaking adults and children, $p = .158$. However, this is not the case for both bare and opinion *parecer*. The distribution of unraised / raised is on a par with the adults only in the case of bare *parecer,* $p = .4$, but with opinion *parecer* children are more likely to use the unraised construction than the adults, $p = 0.032$. These results are compatible with the theoretical analyses presented in Chapter 2 regarding bare *parecer;* namely, that bare *parecer* is a functional verb with no argumental structure (no implicit or explicit experiencer is projected) and is therefore not expected to cause difficulties related to intervention. As we will see, these results are also compatible with the results from our comprehension tasks, which we present in the following sections.

3.2. Part I: Intervention study

In this comprehension study, we i) corroborate whether English-speaking children perform poorly or well on StSR *seem* with an implicit experiencer; ii) provide novel cross-linguistic data from Spanish-speaking children's comprehension of bare *parecer*, a StSR verb semantically close to *seem* that does *not* select for an experiencer argument. Grammatical accounts such as the UFH (Snyder & Hyams, 2015) or the AIH (Orfitelli, 2012) predict that children will do poorly in English, since they assume that the covert experiencer of *seem* is nonetheless syntactically projected, and will thus induce intervention effects. On the other hand,

[1] The data values regarding children's use of the unraised / raised construction with overt experiencers are too small to be able to reject the null hypothesis.

processing accounts, such as PIE (Choe, 2012; Choe & Deen, 2016), predict that children will do well with StSR *seem* as long as the experiencer is not overtly expressed. Both accounts, however, predict Spanish-speaking children will perform well with bare *parecer*, since there is no experiencer argument structurally present that needs to be by-passed.

3.2.1. Subjects

A total of 30 monolingual English-speaking children (14 boys, 16 girls) were included in the English part of this study. Data from an additional seven were excluded because they failed to correctly answer at least five out of the six trials in the control conditions of the subject-to-subject raising experiment ($N = 7$). Children were grouped into three age categories: four-, five-, and six-year olds. Participant details are shown in Table 3.3.

Table 3.3. English-speaking participant details

Group	**Age Range**	**Mean Age**	**N**
4-year-olds	4;2 - 4;11	4;73	10
5-year-olds	5;4 - 5;10	5.49	10
6-year-olds	6;1 - 6;7	6.40	10
Total	**4;2 - 6;7**	**5;53**	**30**

Testing was conducted primarily in a childcare center in West Los Angeles and in an elementary school in Ventura County. A small number of participants were tested on the UCLA campus in a dedicated testing facility. Testing occurred over two sessions with no longer than nine days between the first and second part.

A total of 36 monolingual Spanish-speaking children (18 boys, 18 girls) were included in

the Spanish part of this study. Data from an additional two were excluded because they failed to correctly answer at least five out of the six trials in the control conditions of the subject-to-subject raising experiment ($N = 2$). Children were again grouped into three age categories: four-, five-, and six-year olds. Participant details are shown in Table 3.4. All children included in this study were considered to have normal hearing, vision, and language development.

Table 3.4. Spanish-speaking participant details

Group	**Age Range**	**Mean Age**	**N**
4-year-olds	4;5 - 4;11	4;70	12
5-year-olds	5;6 - 5;11	5.70	12
6-year-olds	6;5 - 6;11	6.66	12
Total	**4;5 - 6;11**	**5;53**	**36**

The experiments were conducted in a preschool and a primary education center in Granada, Spain. Testing occurred over two sessions with no longer than six days between the first and second part.

Ten native English-speaking adults between the ages of 22 and 66 years old (mean age 34.22) were tested in the English subject-to-subject raising and subject control experiments (see Chapter 4) in one single session. Three adults were excluded because they allowed *seem* to have a copular interpretation, scoring below 5/6 in the 'unraised' control condition.

For the Spanish component, twelve native Spanish-speaking adults between the ages of 25 and 61 years old (mean age 31.66) were tested in the Spanish subject-to-subject raising and subject control experiments in one single session. One adult was excluded because she failed to score at least five out of six in the control conditions. The methodology used with the adults was

the same as with the children except that the trial sentences were presented by the computer instead of a puppet.

3.2.2. Materials and method

The methodology employed was a Truth-Value Judgment task (TVJT; Crain & McKee 1985; Crain & Fodor 1993). In this paradigm, the child observes a story and then a puppet comments on it. The task of the child is to indicate whether the puppet commented truthfully or not. In this experiment, the story was depicted through a series of images displayed on a computer screen. The events were observed by a monkey puppet named Coco. The child was told that Coco was somewhat silly and would sometimes make mistakes when commenting on the stories. The child was asked to serve as Coco's teacher, and to let him know when he was right or wrong, and why, so that he could learn to do better. The stories for the English and Spanish tests were digitally recorded in audio files by an English and Spanish native speaker respectively and played through loudspeakers connected to a personal computer presenting the test.

Two training trials preceded each test session to ensure the child understood the task, and that he or she would correct the puppet when the comment was inappropriate. As in the test trials, each consisted of two pictures and an accompanying story. Neither of the trainings contained a subject-to-subject raising or subject control structure. Six unique test scenarios were used to keep children engaged in the task. In general, the stories were similar to those employed in Hirsch, Orfitelli and Wexler (2008), Becker (2006), and Orfitelli (2012). However, in our experiments, all stories involved individual-level predicates in order to match the Spanish stimuli of the second study (see Section 2.1.3.1 for a discussion of the distribution of opinion *parecer*).

Another crucial difference was the introduction of a third character to allow for the felicitous experiencer phrase in the Grammar vs Processing study (see O'Brien, Grolla, and Lillo-Martin, 2006, who found an improvement in children's performance on long passives when the task satisfied a felicity condition by having an additional character in the story).

In one of the scenarios, the first picture showed a white dog is standing next to a grey light, and another animal (e.g. a parrot) is watching him. In the second picture, the dog has gone under the light, making his fur look grey. A new animal arrives (e.g. a cat) and mentions that he remembers the dog being white but that maybe he was wrong, and the dog is actually grey. An example is shown on Figure 3.1. In all cases, following the story, the first image becomes dark but is not completely removed, to remind the child of what happened. Coco, the puppet, then comments on the second, illuminated picture (i.e. the dog standing under the grey light).

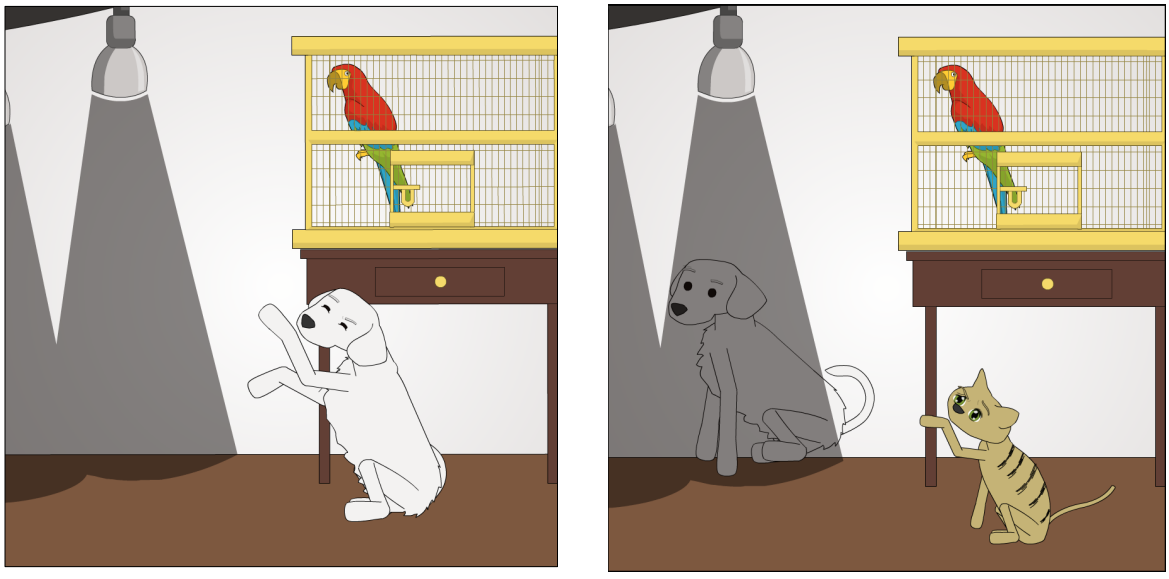


Figure 3.1: Subject-to-subject raising experiment sample pictures: The left picture shows reality – the dog is white. The right picture shows appearance – the dog seems grey.

Following the story, Coco, the puppet, comments on story using one of three sentence types. In the case of English, these were: finite clauses with the copula, unraised sentences, and StSR sentences without an overt experiencer. Sentences with adult judgments are provided in Table 3.5. All the experimental items for the StSR English experiment are included in Appendix A.

Table 3.5. Subject-to-subject raising test items for the English Intervention experiment.

Condition	**True Test Items**	**False Test Items**
Copula	The dog is definitely white.	The dog is definitely grey.
Unraised	It seems that the dog is grey.	It seems that the dog is white.
StSR *seem*, covert exp., TP	The dog definitely seems to be grey.	The dog definitely seems to be white.

In the case of Spanish, the puppet produced one of the following three sentence types: a finite clause with a copula, the equivalent of the English sentence, an unraised sentence, equivalent to the English unraised sentence, or a StSR sentence with bare *parecer* (i.e. no experiencer), superficially the same as the StSR English sentence with a covert experiencer. Sentences with adult judgments are provided in Table 3.6. All the experimental items for the StSR Spanish experiment are included in Appendix B.

Table 3.6. Subject-to-subject raising test items for the Spanish Intervention experiment.

Condition	**True Test Items**	**False Test Items**
Copula	El perro es definitivamente blanco. *'The dog is definitely white'*	El perro es definitivamente gris. *'The dog is definitely grey.'*
Unraised	Parece que el perro es gris. *'It seems that the dog is grey'*	Parece que el perro es blanco. *'It seems that the dog is white.'*
StSR bare *parecer*, no exp., TP	El perro definitivamente parece ser gris. *'The dog definitely seems to be grey'*	El perro definitivamente parece ser blanco. *'The dog definitely seems to be white.'*

The inclusion of *definitely/definitivamente* 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* grey *when* he stands under the light. We added the modifier on the raising without an experiencer condition to match the copula condition.

The unraised condition primarily served to verify that children had an understanding of the lexical properties of the verb *seem/parecer*. Their performance on the StSR conditions must therefore be considered separate from this consideration. Children scoring less than 5 out of 6 items correct on either the copula or unraised conditions were not included in subsequent analyses. In the English experiment, seven children were excluded for failing to pass the unraised condition. In the Spanish study, one participant failed in the copula condition, and one failed in the unraised condition.

Crucially, the inclusion of Spanish StSR bare *parecer* condition served to determine whether children perform well with StSR when there is no experiencer (either covertly or overtly). This is in contrast to the English situation, where the experiencer is syntactically present but not overtly expressed.

Importantly for this experiment, each of the experimental stories hinges upon the difference between reality and appearance. While the dog seems to be grey when he stands under the light, in reality his fur is white. This distinction is important given the finding, discussed in Section 1.1.2, that when faced with a StSR structure, immature children may ignore *seem/parecer* and assign the sentence a copular interpretation. Any child using such a strategy will show a consistent below-chance pattern of answers.

Each test item was read with primary stress placed on the main clause predicate (i.e. *be/ser* or *seem/parecer*), and secondary stress placed on either *definitely/definitivamente*, or on the experiencer DP. Half of the test items were judged 'true' by adult speakers, and half were 'false'. Each condition was tested six times, once for each scenario, for a total of 18 test items. No child was presented with both the 'true' and 'false' test items for the same scenario. The order of presentation for these items was pseudo-randomized such that children were never tested on either the same condition or the same scenario in two consecutive trials. Finally, in order to avoid carryover effects and minimize the number of experimental sessions, we interspersed the trials of the two parts of the subject-to-subject raising study and the subject control experiments and divided the task in two balanced sessions. However, for expository purposes, we present the different parts of the experiments and their results separately.

3.2.3. Part I StSR results for the English-speaking group

Let us first discuss the results from the English-speaking group. As stated in the previous section, when children did not perform at or above five out of the six trials on the copula and unraised control conditions (see Table 3.3), their data were not included in further analysis. This resulted in a total of seven children not being included. In all cases it was the 'unraised'

condition that these children failed on, indicating that they did not understand the meaning of *seem* independent of raising. By performing well on these conditions, the remaining 30 children included in the analysis demonstrated that they understood the task and had mastered the lexical meaning of *seem*. Thus, their performance on the raised condition can be considered independently of these issues.

Our results show that while the English-speaking children did well with the unraised *seem* trials, scoring 93.83% (M = 5.63/6), they performed rather poorly in the raised *seem* trials, 52.83% (M = 3.17/6). A Wilcoxon signed-rank test, used to compare nonparametric related samples, confirmed this difference is significant, $Z = -4.4$, $p < .001$. The subjects' performance by age group on the three different conditions is shown in Figure 3.2.

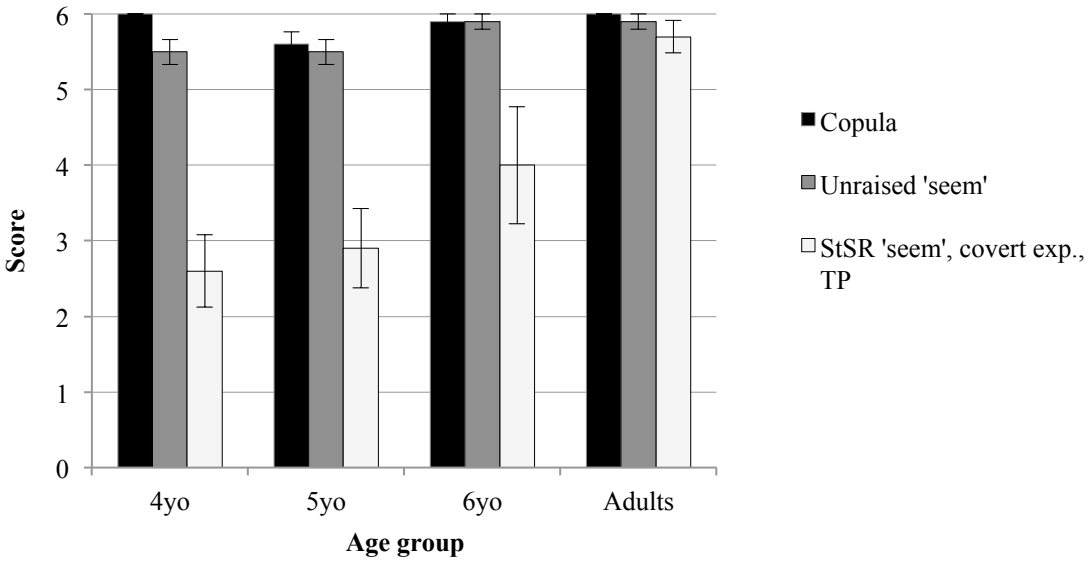


Figure 3.2. English subject-to-subject raising Intervention experiment results by age group and condition.

Results from the three age groups show a similar pattern. All age groups performed significantly worse in the 'StSR *seem* with a covert experiencer' than in the 'unraised' condition.

Wilcoxon signed-rank tests confirmed this, Z = -2.82, p = .005 (four-year-olds), Z = -2.83, p = .005 (five-year-olds), Z = -2.04, p = .041 (six-year-olds). These results replicate the findings in Hirsch et al. (2007), Hirsch (2011), and Orfitelli (2012), and contradicts those of Becker (2006) and Choe (2012), who found good performance when the experiencer was covert or fronted. This result suggests that children experience difficulties with movement over structurally intervening arguments even when they are not overtly expressed.

3.2.4. Part I StSR results for the Spanish-speaking group

Recall that in Spanish, the StSR verb bare *parecer* behaves like a modal verb and does not select for an experiencer argument. If the covert experiencer of English StSR *seem* is effectively the source of children's difficulty, then we should expect Spanish-speaking children to do well with StSR bare *parecer*, since there is no covert experiencer to by-pass. As in the case of the English experiment, when children did not correctly answer at least five out of the six trials on the copula and unraised control conditions (Table 3.4), their data were not included in further analysis. This resulted in a total of two children not being included.

In stark contrast to the English-speaking children, we found that Spanish-speaking children showed nearly adult-like performance with StSR bare *parecer* sentences, obtaining an average of 91.67% (M = 5.5/6), just barely below the score they obtained in the 'unraised' condition, 93% (M = 5.58/6). Wilcoxon signed-rank tests confirmed this difference was not significant, Z = -.456, p = .648. The subjects' performance by age group on the three different conditions is shown in Figure 3.3.

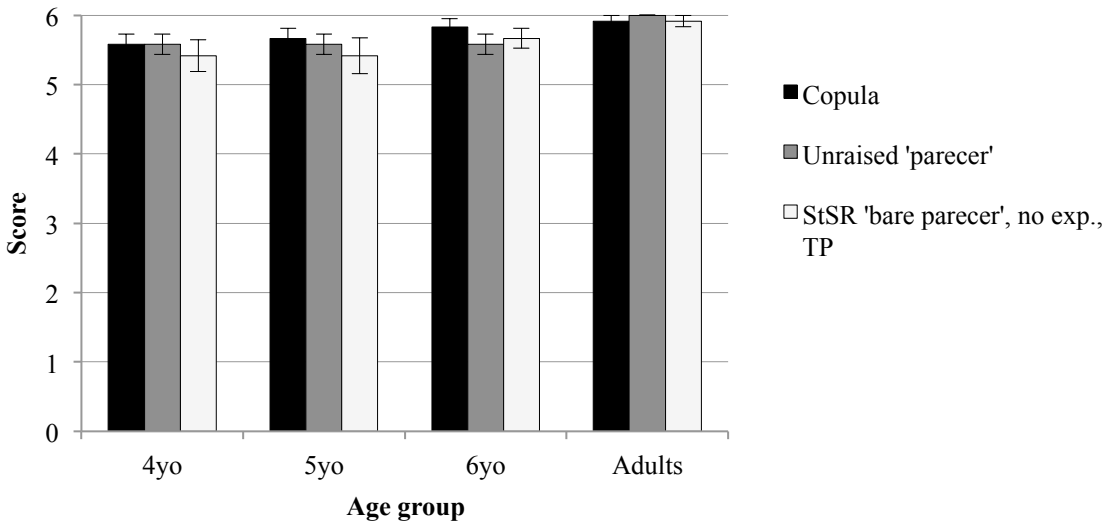


Figure 3.3. Spanish subject-to-subject raising Intervention experiment results by age group and condition.

As for the tree child groups, we found that even in the youngest group they all did as well in the 'StSR bare *parecer*' condition as in the 'unraised' condition, Z = -.743, p = .46 (four-year-olds), Z = -.302, p = .763 (five-year-olds), Z = -.447, p = .655 (six-year-olds). It thus seems that by age four, Spanish-speaking children can comprehend raised sentences with bare *parecer*.

Summarizing, the results of this experiment show that, in contrast to English-speaking children, who perform poorly with StSR *seem*, e.g. '*The dog seems to be grey*', Spanish-speaking children succeed on superficially analogous sentences with bare *parecer*, e.g. '*El perro parece ser gris*'. This asymmetry strongly suggests that the covert experiencer argument of *seem* is syntactically represented in English, inducing intervention effects even when it is not overtly expressed, as suggested by some representation accounts (Orfitelli, 2012; Snyder & Hyams, 2015), and contra the predictions of processing accounts that attribute the problem to *overt* intervening material (e.g. Choe's PIE, 2012). Moreover, the results from the Spanish part of this study are in line with the syntactic theories discussed in Section 2.1.3.1, which claim that bare

parecer behaves like an epistemic modal verb and does not select for an experiencer. Because there is no overt or covert experiencer to by-pass, no intervention effect arises.

3.3. Part II: Grammatical versus processing effects study

In the second study, we investigate whether these intervention effects are due to an immature grammar or an immature processing system. In order to investigate this question, we will: i) manipulate the length of the intervening material, and ii) compare children's performance on StSR *seem* and StSR opinion *parecer* with two independent measures of processing and short-term-memory capacity.

A number of processing-based accounts, including Choe's (2012) PIE, predict that children will show worse performance if we increase the number of DPs between the filler and the gap (Gibson 1998, 2000; Grodner & Gibson, 2005; Warren & Gibson 2002). Thus, we should expect increased difficulties comprehending StSR when the intervening experiencer DP includes an embedded DP, as in the example in (2b), the 'long' counterpart of (2a). Furthermore, if the difficulties that we observe with StSR are due to processing or short-term memory limitations, we should expect to find a positive correlation between children's performance on this construction and an independent measure of verbal processing and/or short-term memory capacity.

Competence-based explanations, such as the UFH (Snyder & Hyams, 2015) or the AIH (Orfitelli, 2012), argue that the observed delays result from a non-adult grammar (e.g. no access to the smuggling strategy adults use). Therefore, no such difference or correlation is expected.

3.3.1. Subjects

The subjects were the same as in the Intervention study just described (see Section 3.2.1).

3.3.2. Materials and method

3.3.2.1. Truth-value judgment task

The procedure we followed was the same as for the Intervention study just described. The exclusion criteria was also the same; therefore, only children that correctly scored at least 5/6 in the copula and unraised conditions were included in this part of the study.

The conditions most relevant to this part of the study are the ones that contained an experiencer. However, we will present the results in comparison to a baseline, the 'StSR *seem* with a covert experiencer' condition for English (repeated in Table 3.7), and the 'StSR bare *parecer* with no experiencer' condition for Spanish. Example test sentences for the English experiment can be found in Table 3.7.

Table 3.7. Subject-to-subject raising test items for the English Grammar vs Processing experiment.

Condition	**True Test Items**	**False Test Items**
StSR *seem*, covert exp., TP	The dog definitely seems to be grey.	The dog definitely seems to be white.
StSR *seem*, short exp., TP	The dog seems to the cat to be grey.	The dog seems to the cat to be white.
StSR *seem*, long exp., TP	The dog seems to the cat with stripes to be grey.	The dog seems to the cat with stripes to be white.

Example test sentences for the Spanish experiment are included in Table 3.8. As mentioned in Section 2.1.3.1, because Spanish opinion *parecer* only selects for a small clause AP

or DP complement, we also included a condition with bare *parecer* followed by an AP for comparative purposes. We expect children to perform well in this condition, as there is no overt or covert experiencer, but poorly on the conditions with opinion *parecer* because they involve A-movement over an intervening experiencer phrase.

Table 3.8. Subject-to-subject raising test items for the Spanish Grammar vs Processing experiment.

Condition	**True Test Items**	**False Test Items**
StSR bare *parecer*, no exp., AP	El perro definitivamente parece gris. *'The dog definitely seems grey'*	El perro definitivamente parece blanco. *'The dog definitely seems white.'*
StSR opinion *parecer*, short exp., AP	El perro le parece al gato gris. *'The dog seems to the cat (to be) grey'*	El perro le parece al gato blanco. *'The dog seems to the cat (to be) white'*
StSR opinion *parecer*, long exp., AP	El perro le parece al gato con rayas gris. *'The dog seems to the cat with stripes (to be) grey'*	El perro le parece al gato con rayas blanco. *'The dog seems to the cat with stripes (to be) white'*

In order to ensure 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 actual experiencer had an additional trait (e.g. stripes). Moreover, because Spanish has grammatical gender, we controlled for the gender of the subject and the experiencer so that they were one of the two configurations in (3).

(3) a. DP1MASC … DP2MASC … <DP1MASC>.
b. DP1FEM … DP2FEM … <DP1FEM >.

Although there is evidence that gender features do not enter into the computation of intervention,

or Relativized Minimality in Romance languages (Belletti, Friedmann, Brunato, & Rizzi, 2012[2]), we nevertheless controlled for grammatical gender in the Spanish stimuli so that the subject and the intervener shared the same gender features. This ensured the English and Spanish test sentences did not differ in terms of feature cues.

3.3.2.2. Competing language processing task

In order to assess verbal processing capacity we administered a developmentally appropriate version of Daneman and Carpenter's (1980) listening span task, a version of the Competing Language Processing Task (CLPT; Gaulin & Campbell, 1994). In this test, children are presented with a series of an increasing number of sentences. Children are asked to recall as many sentence-final words as possible. Additionally, to ensure the children were processing the sentence, as opposed to adopting a strategy of focusing only on the last word of each sentence, they were also asked to judge the sentence as 'true' or 'false'.

The classic CLPT protocol consists of two trials at six different levels. At Level 1, children are required to comprehend only one statement and to recall the last word of that sentence. The number of statements per group increases by one for each level; consequently, at the highest level (Level 6), six statements must be analyzed and six words recalled for each of the two trials. However, to our knowledge, this version of the task has only been tested on school-aged children (5;8-9;7 in Ellis Weismer, Evans, & Hesketh, 6;0-12;0 in Gaulin & Campbell 1994; 1999; 8;4-12;2 in Mainela-Arnold & Evans, 2005; 11;0-14;0 in Campbell, Needleman, Riess & Tobin, 2000). Given the younger age of our participants (4-6 years old), we

[2] Although the effects of gender mismatch on the computation of RM has only been tested in Italian children, the authors extend this prediction to any language in which gender is not part of the ϕ-set attracting the subject, i.e. all Romance languages, including Spanish.

modified the task to include only three levels, but increased the number of trials per level from two to three. There were a total of 18 sentences, three for Level 1 (one sentence per trial), six for Level 2 (two sentences per trial), and nine for Level 3 (three sentences per trial). A trial example for each level is given in Table 3.9.

Table 3.9. Three trial examples for each of the three levels of the CLPT in English and Spanish.

Level	Test Items in English	Test Items in Spanish	Veracity
1	Cows have wings.	Las vacas tienen alas.	F
2	Mice eat cheese.	Los ratones comen queso.	T
	Babies drive cars.	Los bebés conducen coches.	F
3	Bees make honey.	Las abejas hacen miel.	T
	Zebras have stripes.	Las cebras tienen rayas.	T
	Fish drink milk.	Los peces beben leche.	F

Two training trials preceded the test trials. One was a Level 1 trial, and the other was a Level 2 trial. At the end of each sentence, the child was asked if the sentence was 'true' or 'false', and at the end of each trial, the child was asked '*What was the last word of each sentence?*' The sequence of word recall did not have to match that of sentence presentation; that is, responses were scored as correct if the child produced the last word of sentences within the target group regardless of whether the order of recall corresponded to that in which the sentences had been presented.

As in the original CLPT task, the comprehension aspect of the task was intended to be quite easy for young children in that it consisted of simple sentences such as '*cows have wings'*. Sentence length, grammatical complexity, and vocabulary level were held constant across the

three levels of the task. Each sentence consisted of three words in English and four in Spanish due to the requirement of the definite article on plural generics in subject position in Spanish. All trials involved subject-verb-object, so that the last word of each sentence was always a noun. It was administered and scored as described in Gaulin & Campbell (1994), but for a more direct comparison to the StSR study, the raw score of total correct items rather than the percentage was used in the analysis. Therefore, in this task, the participant's CLPT score was the number of sentence-final words he or she could recall with a correct 'true'/'false' answer. The CLPT was completed after the first TVJT session.

3.3.2.3. Digit span task

Given that Arosio, Guasti, and Stucchi (2011) and Booth, MacWhinney, and Harasaki (2000) and found digit span modulates comprehension of object relative clauses but not listening span, albeit in nine-year-olds, we also administered an auditory digit span test task, a measure of verbal short-term memory resources, that is, the phonological loop capability of storing limited amounts of verbal information over a short interval.

The procedure used was as described in Gathercole and Adams (1993); however, the longest list of digits was reduced from nine to seven. Series of digits of increasing length were randomly ordered in lists, starting at Length 2. Each level consisted of four trials, and each number of the list was given with the same intonation at a rate of about one digit per second. Children were asked to listen carefully to the lists of digits and repeat them aloud in the same order. Two practice trials preceded the test items, one was two-digits long, and the other one was three-digits long. As in Gathercole and Adams, span was scored as the maximum length at which the child correctly recalled at least two lists. Additionally, for a more direct comparison to the

CLPT, we calculated a separate score for total correct lists repeated, as in Gaulin and Campbell (1994), and following the manual of the Auditory Sequences subtest in the Illinois Test of Psycholinguistic Abilities (ITPA; Kirk, McCarthy, & Kirk, 1968). This task was completed after the second TVJT session.

3.3.3. Part II StSR results for the English-speaking group

Let us first discuss the truth-value judgment task results from the English-speaking group. In this part of the study, we wanted to determine whether the intervention effects we observe in subject-to-subject raising are due to grammatical or processing deficiencies. In order to answer this question we included three conditions, one with a covert experiencer, to serve as a baseline, one with a short experiencer (DP), and one with a long experiencer (DP[PP]). If children perform significantly worse in the 'long experiencer' condition than in the 'short experiencer' condition, we would conclude that the difficulty with the intervening material is related to the increased number of elements that must be computed.

Moving to the results, when children did not perform at or above five out of the six trials on the copula and unraised control conditions (see Table 3.5), their data were not included in this part of the study either. As mentioned in Section 3.2.3, this resulted in a total of seven children not being included.

Our results reveal that, as expected, children performed poorly in the 'StSR *seem* with a short experiencer' condition scoring an average of 56.17% (M = 3.37/6). Interestingly, children did not perform significantly worse in this condition than in the 'StSR *seem* with a covert experiencer' condition, in which they obtained 52.83% (M = 3.17/6). Wilcoxon signed-rank tests with Bonferroni's correction for two comparisons (i.e. 0.05/2 = 0.025) confirmed this, Z = -.8, p

= .42. The subjects' performance by age group on the three conditions is shown in Figure 3.4.

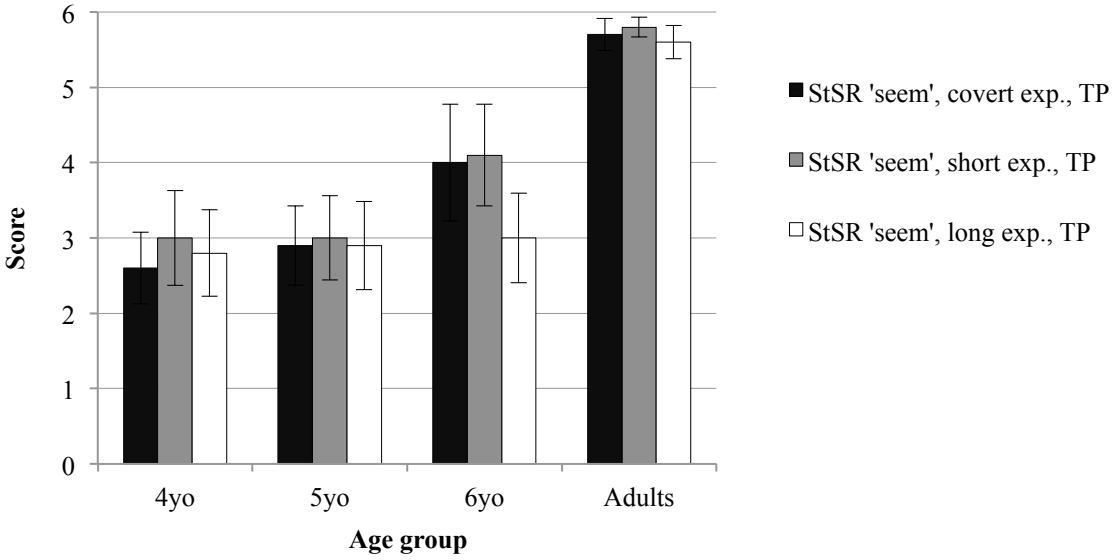


Figure 3.4. English subject-to-subject raising Grammar vs. Processing experiment results by age group and condition.

The three age groups also showed comparable results in these two conditions, Z = -.88, *p* = .38 (four-year-olds), Z = -.33, *p* = .74 (five-year-olds), Z = -1.08, *p* = .91 (six-year-olds). That is, having an overt short experiencer did not lead to a decrease in their performance as compared to sentences with a covert experiencer. This result reinforces our conclusion from the Intervention study, namely, that children have difficulties with StSR both when the experiencer is overtly expressed and when it is not.

Importantly for this part of our study, we examined the difference between children's performance in the 'StSR with a short experiencer' condition, in which they scored an average of 56.17% (*M* = 3.37/6), and 'StSR with a long experiencer' condition, in which they obtained 48.33% (*M* = 2.9/6) of correct responses. We found no significant difference between the two

conditions in the whole group of 30, Z = -1.62, p = .11, or in any of the three age groups, Z = -.35, p = .73 (four-year-olds), Z = -.26, p = .79 (five-year-olds), Z = -1.88, p = .06 (six-year-olds). Therefore, children overall did not perform significantly worse when we lengthened the intervening material. This result provides counterevidence to processing-based accounts that claim that in general children's difficulties are due to interference in working memory caused by the presence of intervening NPs (Choe, 2012).[3] Having said that, we will see that for certain children processing difficulty does have a significant effect. We discuss this in Section 3.3.9.

3.3.4. Part II StSR results for the Spanish-speaking group

We included three conditions in the Spanish experiment as well: one with StSR bare *parecer* with no experiencer, one with StSR opinion *parecer* with a short experiencer, and one with StSR opinion *parecer* with a long experiencer, all three with a small clause AP complement, since opinion *parecer* does not select for TPs. Recall that bare *parecer* behaves like an epistemic modal verb and does *not* select for an experiencer argument. Therefore, we expect children to perform well in this condition as they did in the Intervention study with bare *parecer* when it was followed by a TP complement. However, we included this condition with bare *parecer* and an AP complement to ensure that any behavioral difference between children's performance on the bare *parecer* and opinion *parecer* conditions is exclusively due to the presence of the intervening experiencer in the case of opinion *parecer*, and not in any way related to the differences in the type of complement (i.e. TP or AP).

Moreover, in this part of the study we tested the hypothesis that Spanish-speaking

[3] Alternatively, one could interpret that the additional DP discourse object in the long experiencer condition did not add enough processing cost to be noticeable. However, the results discussed in Section 3.3.9 suggest that at least for some children, having an additional linearly intervening DP did significantly affect their performance.

children would do worse with StSR opinion *parecer* when the intervening experiencer is longer, i.e. when it includes a PP embedded in the DP.

Let us now turn to the results. As previously mentioned, when children did not perform at or above five out of the six trials on the copula and unraised control conditions (see Table 3.6), their data were not included in this part of the study either. As mentioned in Section 3.1.4, this resulted in a total of two children not being included.

As expected, we found children did do worse with StSR opinion *parecer* as compared to bare *parecer*. They obtained an average score of 88.5% (M = 5.31/6) in the 'StSR bare *parecer* followed by an AP' condition, as compared to an average score of 75% (M = 4.5/6) in the 'StSR opinion *parecer* with a short experiencer, followed by an AP'. Wilcoxon signed-rank tests were conducted with a Bonferroni correction applied for two comparisons confirmed this difference, $Z = -2.726$, $p = .006$. The subjects' performance by age group on the three conditions is shown in Figure 3.5.

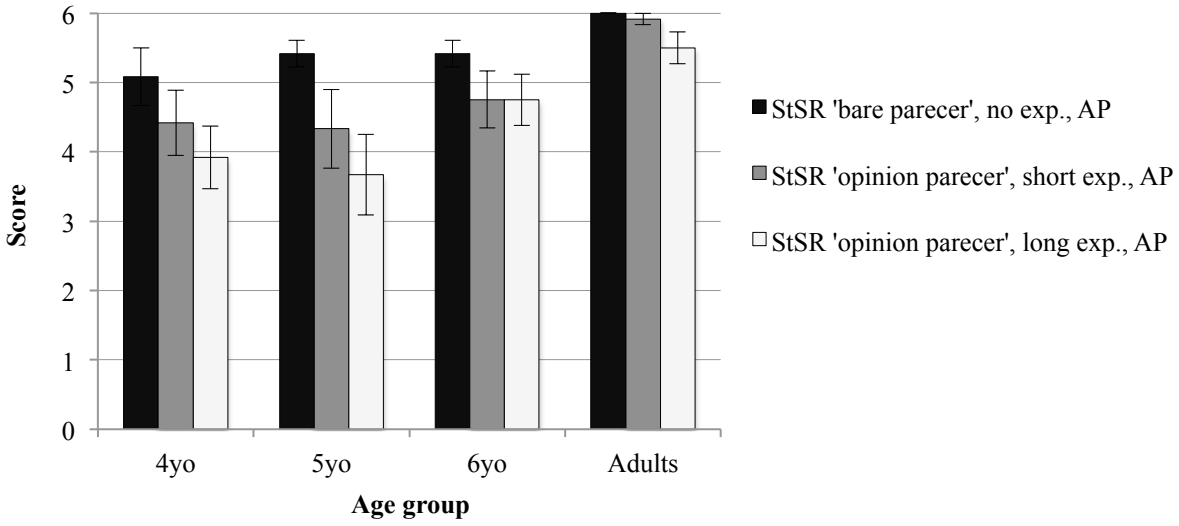


Figure 3.5. Spanish subject-to-subject raising Grammar vs. Processing experiment results by age group and condition.

This difference was not as noticeable in all the age groups. We found a marginal but not a significant difference between these two conditions in two of the three age groups, Z = -1.165, *p* = .244 (four-year-olds), Z = -1.725, *p* = .084 (five-year-olds), Z = -2.121, *p* = .03 (six-year-olds).[4] However, we can clearly conclude from the group results that, as predicted by representational accounts, English-speaking children perform as poorly with StSR *seem* with a covert experiencer as with StSR *seem* with a short experiencer, while Spanish-speaking children perform significantly worse with StSR opinion *parecer* with a short experiencer than with StSR bare *parecer* (i.e. no experiencer). This result reinforces our interpretation of the results of the Intervention study. Namely, that the reason why Spanish-speaking children do well with StSR bare *parecer* is because there is no overt or covert experiencer argument to by-pass, hence no intervention effect arises. Conversely, when children must A-move the subject over a structurally intervening argument, they experience difficulties.

An additional and interesting difference that can be observed between the two language groups is the relatively good scores that the Spanish-speaking children obtained with opinion *parecer* (72.22% correct responses on average between the short and long experiencer conditions), as opposed to the English-speaking children (52.22% on average between the short and long experiencer conditions). While it is not possible for us to ascertain the root of this cross-linguistic discrepancy, two potential reasons may account for it. The first one could be related to differences in input frequencies. As mentioned in Section 2.1.3.1, Spanish opinion *parecer* is best translated as 'think', and it is, as expected, found ubiquitously in everyday speech. English *seem*, on the other hand, is not regularly used with an overt experiencer. Results

[4] This suggests that the difference between the two conditions may be too small to be detected given a small sample size, i.e. 12, but is easily detectable given a greater sample size, i.e. 36.

from our corpus study (see Section 3.1) indicate that while opinion *parecer* makes up 59.97% (689/1,149) of all the *parecer* instances produced by adults, English *seem* only appears with an overt experiencer only 4.19% (53/1,266) of the times. Crucially, while *seem* + overt experiencer appears in a raised construction only 31 times in the whole CHILDES corpus; Spanish opinion *parecer* (with experiencer) appears in a raised construction 249 times, eight times more often than in English.

Opinion *parecer* and *seem* also differ with respect to aspect selection, a crucial feature of Snyder and Hyams' (2015) UFH, based on Gehrke and Grillo's (2008) smuggling analysis. Recall that under this analysis, children's delays in non-actional passives and StSR *seem* are related to their difficulties smuggling non-eventive VPs, since those may require an additional operation that is unavailable to children until school-age, i.e. semantic coercion. However, as Grillo (2008: 135ff) argues, some stative predicates may be easier to associate with an eventive reading than others. We speculate that this may be the case of opinion *parecer*. As discussed in Section 2.1.3.1, opinion *parecer* may appear in both the preterit and the progressive, two characteristic features of achievement and accomplishment verbs respectively, which are not so readily available to stative verbs. If Spanish-speaking children hear opinion *parecer* in the preterit and the progressive – and CHILDES results show they do (N = 40) – but English-speaking children do not hear *seem* in the progressive, it is reasonable to assume that Spanish-speaking children will have an easier time 'aspectually coercing' opinion *parecer*.

Let us now go back to the results and consider the effects of length of the intervener. We compared children's performance in the two StSR opinion *parecer* conditions. Overall, children scored 75% (M = 4.5/6) in the 'short experiencer' condition, and 69.5% (M = 4.17/6) in the 'long experiencer' one. Despite the trend, we found that there was no significant difference between

the two, Z = -1.64, p = .1. The three age three groups showed the same pattern, Z = -1.027, p = .305 (four-year-olds), Z = -1.634, p = .102 (five-year-olds), Z = .0, p = 1 (six-year-olds). This shows that, overall the Spanish-speaking children's performance did not decline significantly when the intervening experiencer was longer. We will thus conclude, for now, that the predictions of processing accounts are not borne out, at least not for all children.

3.3.5. Competing language processing task results and StSR

In addition to manipulating the length of the intervening experiencer, in order to test the predictions of processing accounts we included two independent measures of processing and short-term memory capacities. We will first report on the results from the verbal processing capacity task. If processing limitations constitute a significant factor underlying children's poor comprehension of StSR, the score they obtained in the relevant conditions should be positively correlated with an independent measure of processing capacity. We chose to administer a listening span task, which consisted of an abridged version of the Competing Language Processing task (CLPT). In this task, children had to correctly say if a statement was true or false and recall the last word of each utterance after listening to *all* the sentences of the trial, i.e. one in Level 1, two in Level 2, and three in level 3. There were a total of 18 sentences. Table 3.10 presents the English-speaking group means, standard deviations, and ranges for the CLPT.

Table 3.10. Results from the CLPT in the English-speaking group.

Age group	*M*	*SD*	Range
4-year-olds	11.00	3.92	5-16
5-year-olds	10.10	5.15	3-18
6-year-olds	12.50	3.57	4-18
Total	**11.20**	**4.24**	**3-18**

The three English-speaking age groups performed rather similarly. The six-year-olds obtained the highest score in this task; however, there was no significant difference between the three age groups based on Friedman test, $\chi^2(2) = 1.68, p = 0.43$. On average, the English-speaking children were able to recall 11.2/18 sentence-final words while they simultaneously judged if the sentences were true or false.

In order to examine the relationship between children's performance in the subject-to-subject raising experiment and processing capacity we compared the scores obtained in the three raised conditions, i.e. those that require A-movement over an explicit or implicit experiencer, and the CLPT. We found no significant correlation between the two scores, $r_s(28) = .317, p = .09$. This is further illustrated in Figure 3.6.

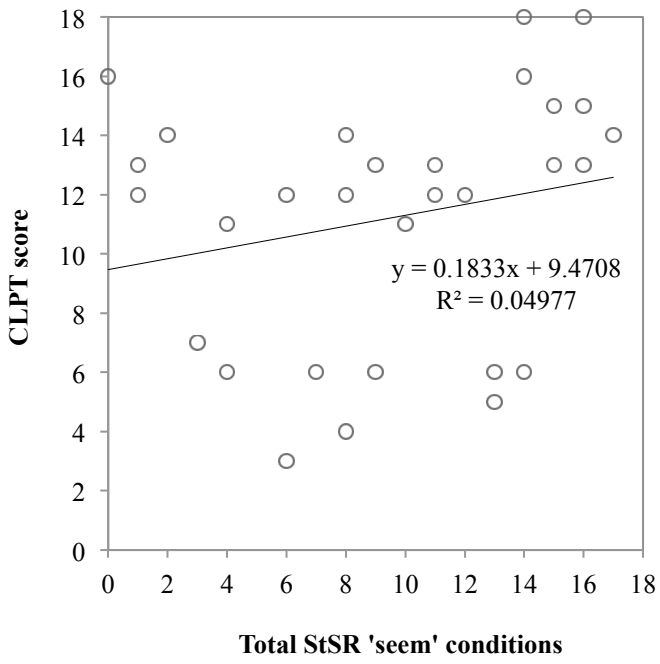


Figure 3.6. Correlation between the score obtained in the English raised conditions and the score obtained in the CLP task (sample of 30 children).

If low processing capacity were the main obstacle to comprehending StSR sentences we would expect children with high processing capacity to perform better in the StSR conditions than those with low processing capacity. As illustrated in Figure 3.6, this does not seem to be the case for all the English-speaking children. In fact, 4/7 of the children who performed below chance obtained higher than average CLPT scores. Therefore, the overall group results fail to support the predictions of processing-based accounts (e.g. Choe, 2012), and are in accordance with grammar-based accounts, which do not predict such correlation (e.g. Hyams & Snyder, 2005, 2006; Orfitelli, 2012; Snyder & Hyams, 2015).

The Spanish-speaking group means, standard deviations, and ranges for the CLPT are presented in Table 3.11.

Table 3.11. Results from the CLPT in the Spanish-speaking group.

Age group	*M*	*SD*	Range
4-year-olds	8.17	5.04	3-17
5-year-olds	12.58	3.99	5-16
6-year-olds	14.08	3.70	7-18
Total	**11.61**	**4.88**	**3-18**

In the case of the Spanish-speaking group, we did find a significant difference between the three groups, based on Friedman test, $\chi^2(2) = 9.64$, $p = 0.01$. Post hoc analysis with Wilcoxon signed-rank tests was conducted with a Bonferroni correction applied for three comparisons, resulting in a significance level set at $\alpha = 0.02$. A significant difference was found between the five- and four-year olds, $Z = -2.655$, $p = .008$, the five-year olds performing better, and between the six- and four-year olds, $Z = -2.755$, $p = .006$, the six-year-olds performing better. No difference was found between the six- and five-year olds, $Z = -1.295$, $p = .195$. While we found sharper differences between the three age categories in the Spanish-speaking group, the average of recalled words was comparable to the English-speaking group, $M = 11.61/18$.

With regards to the relationship between children's scores obtained in the CLPT and the two StSR opinion *parecer* conditions, i.e. those that entail A-movement over a structurally intervening argument in Spanish, we found no significant correlation, $r_s(34) = .255$, $p = .134$. That is, as in the English-speaking group, processing capacity does not seem to accurately predict the overall rate of success in the constructions that involve StSR over an experiencer, i.e. with opinion *parecer*. This is further illustrated in Figure 3.7.

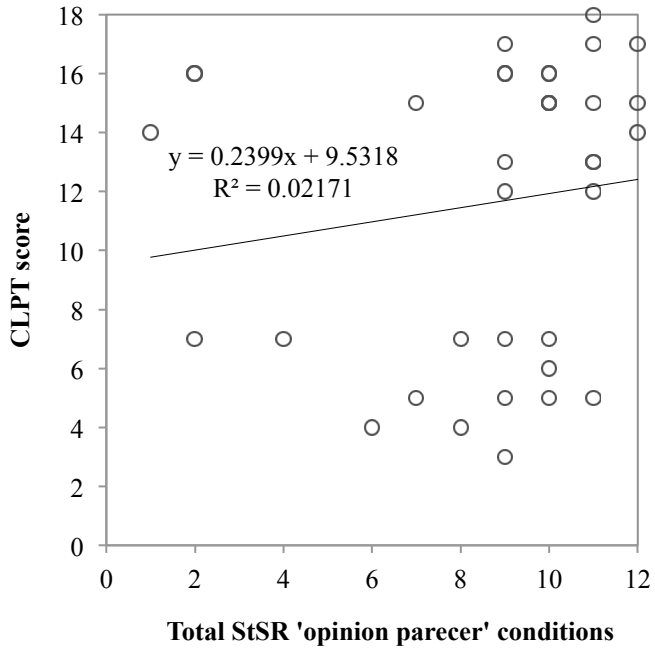


Figure 3.7. Correlation between the score obtained in the Spanish raised with an experiencer conditions and the score obtained in the CLP task (sample of 36 children).

3.3.6. Digit span task results and StSR

In order to investigate whether it is short-term memory limitations, and not verbal processing capacity limitations, that is the cause of children's poor performance with StSR, we administered a digit span task, in which the children had to repeat increasingly long lists of digits. The English-speaking children group means, standard deviations, and ranges for the digit span task are given in Table 3.12. The task in scaled scores, as it is typically reported, ranged from 2-7. The same task scored by item, as done in Gaulin and Campbell (1994) for more direct comparison with the CLPT scores, ranged from 1-24.[5]

[5] As mentioned in Section 3.3.2.3 the digit span task may be scored as the maximum length at which the child correctly recalled at least two lists of numbers (Gathercole & Adams, 1993). We refer to this score as the 'scaled

Table 3.12. Results of the digit span task from the English-speaking group.

	Scaled scores			Itemized scores		
Age group	***M***	***SD***	**Range**	***M***	***SD***	**Range**
4-year-olds	4.70	0.67	4-6	13.80	1.75	11-16
5-year-olds	4.70	0.48	4-5	14.50	1.78	12-17
6-year-olds	4.70	1.25	4-7	13.50	4.03	10-22
Total	**4.70**	**0.84**	**4-7**	**13.93**	**2.68**	**10-22**

Once again, we obtained very similar results in the three English-speaking age groups and we found no significant difference between them based on Friedman test for either the scaled scores, $\chi^2(2) = 1.15, p = 0.562$, or for the itemized scores, $\chi^2(2) = 2.58, p = 0.275$. On average they were able to correctly repeat 4.7-digit long lists (scaled score) and they correctly repeated an average of 13.93/24 digit lists (itemized score).

We found no significant correlation between the scores obtained in the digit span task and in the three StSR conditions, regardless of the scoring method used in the digit span task, $r_s(28) = .094, p = .62$ for the scaled scores, and $r_s(28) = .156, p = .411$ for the itemized scores. That is, short-term memory capacity by itself cannot successfully predict the rate of success in the constructions that involve StSR in English.

The Spanish-speaking group means, standard deviations, and ranges for the digit span task are presented in Table 3.13.

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score'. However, it may also be scored as the total number of digit lists that were correctly repeated (Gaulin & Campbell, 1994). This is the 'itemized score'.

Table 3.13. Results of the digit span task from the Spanish-speaking group.

	Scaled scores			Itemized scores		
Age group	***M***	***SD***	**Range**	***M***	***SD***	**Range**
4-year-olds	3.75	0.75	3-5	10.41	1.98	7-14
5-year-olds	4.00	0.60	3-5	11.25	2.38	8-16
6-year-olds	4.50	0.79	3-6	13.58	2.57	9-18
Total	**4.08**	**0.77**	**3-6**	**11.75**	**2.63**	**7-18**

We again found the mean scores to be more reflective of chronological maturation, with the six-year-olds performing best, and the four-year-olds performing worst. However, we only found a marginal statistical difference between the three age groups based on Friedman test for the scaled scores, $\chi^2(2) = 6.09$, $p = 0.048$ for the scaled scores. Post hoc analysis with Wilcoxon signed-rank tests was conducted with a Bonferroni correction applied for three comparisons. We only found a marginal difference between the six- and four-year old groups, $Z = -2.165$, $p = .03$. Differences between the four- and five-year olds and between the five- and six-year-old were not significant, $p = .1$.

As for the itemized scores, we again found a significant difference between the three groups, Friedman test, $\chi^2(2) = 9.805$, $p = 0.01$. Post hoc analysis with Wilcoxon signed-rank tests was conducted with a Bonferroni correction applied for three comparisons. The six-year old group did significantly better than the four-year-old group, $Z = -2.539$, $p = .011$. The difference between the six- and five-year-old group was only marginally significant, $Z = -2.138$, $p = .033$. No significant difference was found between the four- and three-year-old groups, $Z = -.773$, $p = .439$. That is, under both scoring methods, the six-year-olds did better than the four-year olds. On

average, the Spanish-speaking children were able to correctly repeat 4.1-digit long lists (scaled score) and they correctly repeated an average of 11.75/24 digit lists (itemized score).

With respect to the relationship between the digit span task and the scores obtained in the two StSR opinion *parecer* conditions, we found no significant correlation between them, regardless of the scoring method used in the digit span task, $r_s(34) = .116$, $p = .5$ for the scaled scores, and $r_s(34) = .207$, $p = .225$ for the itemized scores. This suggests that, as for the English-speaking children, short-term memory deficits are not likely to be the sole cause of Spanish-speaking children's difficulties with constructions that involve StSR over an experiencer.

3.3.7. Interim summary of StSR results

As predicted by all accounts, both English- and Spanish-speaking children performed rather poorly in the StSR conditions that included an experiencer argument. However, as a whole, we found no significant difference between the short and long experiencer conditions, in either the English-speaking group, where they scored an average of 56% and 48% respectively, or in the Spanish-speaking group, where they obtained an average of 75% and 69% respectively. That is, adding more intervening material did not significantly decrease their performance.

Moreover, we found no significant correlation between children's overall performance in the StSR conditions involving an experiencer and CLPT score, nor between performance in StSR and digit span score in either English or Spanish. This strongly suggests that children's difficulties with StSR over an experiencer cannot be *solely* due to processing difficulties, and that the poor performance with interveners has a grammatical basis.

However, a closer look at the data may reveal a more complex answer. Unlike 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 English- and Spanish-speaking children whose responses were not consistently wrong, but who were not performing significantly differently from what could have been simple guessing. However, their response justifications lead us to believe this was not the case. While these children assigned a non-adult interpretation to many of the StSR trials, in some of them they provided justifications that were fully consistent with an StSR interpretation in which they had to circumvent the intervener experiencer. In the following section we provide evidence that suggests that our subject do not constitute a homogeneous group; that the children who perform below chance truly lack the grammatical means to by-pass the intervening experiencer, while the children that perform at chance and above have an adult grammatical system, but still experience difficulties applying the rule consistently due to performance factors.

3.3.8. A closer look into the individual StSR data

These differences become apparent when we look at the individual subject data. Let's begin with the English data. A binomial distribution was used to establish whether the children's overall StSR score could have resulted from chance. Out of the 18 StSR test items that included a covert or overt intervening experiencer, the inclusive range 0-5 constituted below chance (BC) performance, 6-12 constituted chance (C) performance, and 13-18 constituted above chance (AC) performance, all with a confidence interval of >95%. The number of children that were classified as behaving below, at, and above chance in StSR constructions in given in Table 3.14.

Table 3.14. Individual results from the English subject-to-subject raising experiment classified according to below chance, chance, or above chance performance in the subject-to-subject raising conditions.

Age group	**BC** (0-5)	**C** (6-12)	**AC** (13-18)
4-year-olds	2	6	2
5-year-olds	3	3	4
6-year-olds	2	3	5
Total	**7**	**12**	**11**

In the case of Spanish, we found that a majority of children performed above chance in the two StSR opinion *parecer* conditions (i.e. those that included an experiencer argument), but we still observe a large proportion of children performing at chance. Out of the 12 test items, the inclusive range 0-2 constituted below chance performance, 3-9 constituted chance performance, and 10-12 constituted above chance performance. The number of children that were classified as behaving below chance, chance, and above chance in StSR constructions with opinion *parecer* (with an experiencer) is given in Table 3.15.

Table 3.15. Individual results from the Spanish subject-to-subject raising experiment classified according to below chance, chance, or above chance performance in the opinion *parecer* conditions.

Age group	**BC** (0-2)	**C** (3-9)	**AC** (10-12)
4-year-olds	1	7	4
5-year-olds	2	4	6
6-year-olds	1	3	8
Total	**4**	**14**	**18**

Children performing below-chance are children that cannot obtain an adult interpretation of StSR and are analyzing the StSR sentences in a consistent, non-adult way; specifically, their performance is consistent with an analysis in which they ignore the StSR predicate entirely and assign the sentence a copular interpretation. Response justifications from children performing below chance on the StSR conditions support the hypothesis that they are ignoring the raising predicate, leading them to interpret both the StSR and copula items identically, as simple copular declaratives. For example, in the English responses given in (17), the justifications given by the child for the copula sentence and for the *seem* sentence are virtually identical. In (17a) the child correctly answers that the statement given by the puppet is false because the dog in the story is actually white, not grey. In (17b) the child responds incorrectly, presumably because he interprets the StSR sentence as '*The dog is white*', as opposed to '*The dog seems to be white*', in which case, the response should be 'true', because although the dog is standing under a grey light, his real fur color is white.

(17) a. Puppet: *The dog is definitely grey!* [F]
BC Child: *Funny […] because he's actually white but he's standing under the grey light.* [F]
b. Puppet: *The dog definitely seems to be white!* [F]
BC Child: *True […] because he's actually just standing under a grey light but he's actually white.* [T]

Interestingly, children performing below chance not only interpreted StSR *seem* as a copula in the condition where the experiencer is not overtly expressed, they generally also did so in the 'short' and 'long experiencer' conditions. The examples in (18) illustrate that a child that performed below chance in the raised sentences interpreted *seem* as a copula even when the

experiencer was overtly produced. This suggests that these children were possibly ignoring the experiencer altogether.

(18) a. Puppet: *The dog seems to the cat to be white!* [F]
BC Child: *True […] because the dog is really white.* [T]
b. Puppet: *The dog seems to the cat with stripes to be white!* [F]
BC Child: *True […]. It's because he's actually white but he looks grey when he's standing under the grey light.* [T]

The same was true for the Spanish-speaking children who performed below chance with opinion *parecer*. Namely, they would interpret opinion *parecer* as a copula and they seemed to ignore the experiencer, as illustrated below in (19a). The same child, however, would consistently assign the correct interpretation to bare *parecer*, as shown in (19b).

(19) a. Puppet: *¡La actriz le parece a la mujer con el micro morena!* [T]
'The actress seems to the woman with the mic to be brunette!'
BC Child: [shakes head, i.e. 'no'] *¡Rubia!*
'Blonde!' [F]
b. Puppet: *¡La actriz definitivamente parece ser rubia!* [F]
'The actress definitely seems to be blonde!'
BC Child: *¡No! […]*
'No!' [F]
Exp: *¿Por què? ¿Cómo lo sabes?*
'Why? How do you know?'
BC Child: [silence]
Exp: *¿Tú qué crees, que parece ser rubia o parece ser morena?*
'What do you think? That she seems to be blonde or she seems to be brunette?'

BC Child*: ¡Morena!*

'Brunette!'

This contrasts with the justifications obtained in Hirsch and Wexler (2007b), Hirsch, Orfitelli, and Wexler (2007), and Hirsch (2011) where children typically interpreted *seem* as *think* when the raised construction had an overt experiencer. That is, a sentence like '*Bart seems to Lisa to be kicking the ball'* was interpreted as '*Bart thinks Lisa is kicking the ball'*. Our stories, however, were built to be unambiguous both with regards to who has the quality denoted in the embedded clause, i.e. who looks grey in Figure 3.1, and also to whom it appears that the event in the embedded clause is occurring, i.e. who is thinking that someone is grey in Figure 3.1. These did not change across trials, as they did in the above-mentioned experiments. That is, in the dog scenario, it was always the dog the one that seemed to be grey, and the hamster or cat, the ones that thought he was white or grey. We suspect this may have reinforced the idea that the property being judged was always with respect to the dog, and not the other characters. However, we did find one child whose justifications indicated she was using a *think*-strategy to interpret *seem* in StSR conditions, as shown in (20).

(20) Puppet: *The dog seems to the cat with stripes to be grey!* [T]

Child: *Funny […] because he said that he* [points to dog] *thinks that he* [points to cat] *is grey.* [F]

Similarly, we also found a Spanish-speaking child whose response justifications seemed to indicate that he was assigning opinion *parecer* (with an experiencer) a reversal of theta-roles, as observed in (21), where the child takes the subject of the matrix clause as the experiencer, rather than the intervening argument, i.e. the child seems to interpret that it is the squirrel's

appearance of fatness that is being judged, as opposed to the snake's.

(21) Puppet: *¡La serpiente le parece a la ardilla gorda!* [T]
'The snake seems to the squirrel to be fat!'
Child: *No […] porque la ardilla no es gorda.* [F]
'No, because the squirrel is not fat.'

Crucially, while children who have yet to master StSR may apply different compensatory heuristics when seeking the real-time interpretations of these sentences, children do have an adult-like comprehension of *seem*/*parecer* constructions in unraised contexts. They only fail to obtain an adult interpretation of *seem*/opinion *parecer* constructions in StSR contexts that include an intervening experiencer (overt or covert).

Contrastively, children performing above chance on the StSR conditions with *seem*/opinion *parecer* display full or almost full adult mastery of StSR and show an understanding that the subject of the sentence only appears to have a certain quality (to a particular character, the experiencer), but is not so in reality, as shown in (22) for English, and (23) for Spanish.

(22) Puppet: *The dog seems to the cat with stripes to be white!* [F]
AC Child: *Funny […] because he seems to be grey, but Coco said he seems to be white!* [F]

(23) Puppet: *¡La chica le parece a la vecina con el sombrero joven!* [F]
'The girl seems to the neighbor with the hat to be young!'
AC Child: *No, ¡le parece vieja!* [F]
'No, she seems to her to be old!'

As mentioned above, in some previous studies (Hirsch et al. 2007; Orfitelli, 2012), they found children performed uniformly below or above chance, but not at chance. However, in our study we found a large proportion of children who performed at chance as shown in Tables 3.12 and 3.13. Interestingly, their response justifications suggest that these children were not merely guessing 'true' or 'false', but rather, they were sometimes interpreting StSR *seem*/opinion *parecer* as a copula, and some others, they were correctly interpreting the StSR *seem*/opinion *parecer* sentences, which would require the ability to A-move across an intervening experiencer argument. The examples in (24) were given by the *same* English-speaking child during the *same* experimental session. We observe that while the response justification to the trial in (24a) suggests that her incorrect response is due to her interpreting *seem* as a copula, she is able to correctly respond 'false' to the trial in (24b) and further explains that the dog only has the appearance of being grey but is not so in reality.

(24) a. Puppet: *The dog seems to be grey!* [T]
C Child: *Funny […] because the dog is really white.* [F]
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 grey when he stands under the light.* [F]

Similarly, the examples in (25) correspond to the same Spanish-speaking child. In (25b), he incorrectly responds 'true' and affirms that the dog *is* white. In (25a), however, he responded correctly to a StSR sentence with opinion *parecer*, and justifies it by explaining why the dog has the appearance of being grey.

(25) a. Puppet: *¡El perro le parece al gato con rayas blanco!* [F]
'The dog seems to the cat with stripes to be white!'

C Child: *Porque es blanco y se pone en la luz gris.*
'Because he's white and he goes under the grey light.'

Exp: *Muy bien, entonces lo que ha dicho él, ¿es verdad o es mentira? Él ha dicho "El perro le parece al gato con rayas blanco".*
'Very good, then what he said, is it true or false? He said "The dog seems to the cat with stripes to be white."'

C Child: *¡Verdad!* [T]
'True.'

b. Puppet: *¡El perro le parece al gato gris!* [T]
'The dog seems to the cat to be grey!'

C Child: *Porque está debajo de una luz gris.*
'Because he's under a grey light.'

Exp: *Entonces, ¿es verdad o es mentira?*
'Then is it true or false?'

C Child: *¡Es verdad!* [T]
'It's true!'

These results suggest two possible scenarios: i) children performing at chance have a non- adult grammatical system that bans A-movement over arguments, ii) children performing at chance have an adult grammatical system. If (i) is correct, we would need to account for why they are sometimes able to obtain an adult interpretation of StSR over an intervening experiencer. If (ii) is correct, we would need to account for why they still show difficulties with these constructions. In the following section we present and investigate our hypothesis.[6]

[6] One could argue that the mixed judgments in the group of children that performed at chance could be due to lack of attention or metalinguistic abilities. However, these same children correctly answered at least five out of the six trials in both the 'copula' and the 'unraised' condition. The fact that the children could correctly judge the control sentences suggests that they had developed the attention span and the metalinguistic skills required to make the truth-value judgments on the experimental sentences.

3.3.9. Our hypothesis: The Dual Source Intervention hypothesis

Given the evidence discussed in the previous section, we hypothesize that we are actually dealing with two groups. For one group, the one made up of children that performed below chance, the problem is grammar-based, i.e. these children lack the grammatical means to escape intervention altogether. For another group, the one made up of children that performed at chance and above, the problem is processing-based, i.e. these children have the grammatical machinery that allows them to by-pass the intervener, but have problems implementing it due to processing limitations. We will henceforth refer to this hypothesis as the Dual Source Intervention hypothesis (DSI).

To investigate this hypothesis, we first excluded from the correlation analysis the English-speaking children that performed below chance in the StSR conditions, i.e. those that scored 5 or less out of the 18 StSR *seem* trials ($N = 7$) and again calculated the Spearman's rank order correlation coefficient. As predicted, the correlation between the performance on the StSR conditions and the CLPT score then becomes significant, $r_s(21) = .6, p = .003$. This is further illustrated in Figure 3.8, which is equivalent to Figure 3.6 except that it does not include the data points from the children who performed below-chance in the StSR conditions, suggesting that these were greatly influencing the slope of the regression line, masking the clear trend observed in this group. In fact, results from a Spearman's rank order correlation coefficient between the children that performed below chance with StSR *seem* and their CLPT score, show that these variables are in fact negatively correlated, $r_s(5) = -.82, p = .024$. It is difficult to interpret this result given the small dataset ($N = 7$) and the limited scale of below-chance performance (0-5/18). However, it could reflect the fact that children with higher processing capacity may be better at abiding by their own grammar, in this case, a non-adult grammar. Importantly, however,

we find no positive correlation between StSR and CLPT scores, as we do with the children that perform at chance and above.

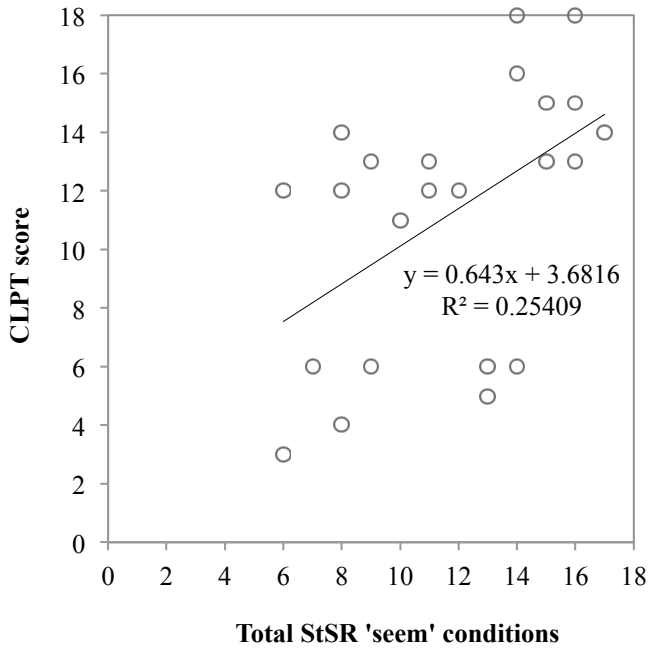


Figure 3.8. Correlation between the score obtained in the English raised conditions and the score obtained in the CLP task in the chance and above chance children (sample of 23 children).

We did not find a correlation between the performance on the StSR conditions in the 'chance' and 'above chance' children and their digit span scores, regardless of the scoring method used in the digit span task, $r_s(21) = .114, p = .604$ for the scaled scores, and $r_s(21) = .141, p = .52$ for the itemized scores. Moreover, in a multiple regression model, with the score obtained in the StSR *seem* conditions as dependent variable and the CLPT score, digit span score, and chronological age as independent variables, only CLPT score added statistically significantly to the prediction, $p = .032$. Neither digit span, $p = .49$, nor chronological age $p = .176$ were statistically significant.

That is, only processing capacity, and not short-term memory span, can predict the scores in the StSR conditions in the 'chance' and 'above chance' children. This idea is reinforced when we compare the performance of the chance and above chance children on the short and long experiencer conditions. A Wilcoxon signed-rank test was conducted showed that these children performed significantly worse in the long experiencer condition than in the short experiencer one ($Z = -2.04$, $p = .04$), supporting our hypothesis, that children who perform at or above chance (i.e. children that are not in the non-adult grammar stage) show effects of processing costs associated with longer intervening DPs.

For the Spanish-speaking child participants as well, we excluded from the correlation analysis the children that performed below chance in the StSR opinion *parecer* conditions, i.e. those that scored 2 or less out of the 12 StSR opinion *parecer* trials ($N = 4$) and again calculated the Spearman's rank order correlation coefficient. Once again, and as predicted by the DSI hypothesis, the correlation between the performance on the 'StSR opinion *parecer*' conditions and the CLPT score then becomes significant, $r_s(30) = .414$, $p = .01$. That is, among the 'chance' and 'above chance' children, those that did worse with opinion *parecer* also had a lower score in the language processing task, while those that did very well with opinion *parecer* obtained a high score in the processing task. This is further illustrated in Figure 3.9, which is equivalent to Figure 3.7 except that it does not include the below-chance children. On the other hand, and as expected, among the children that performed below chance with StSR opinion *parecer,* i.e. at 2/12 or below (N = 4), we found no significant correlation between StSR and CLPT scores, $r_s(2) = .27$, $p = .73$.

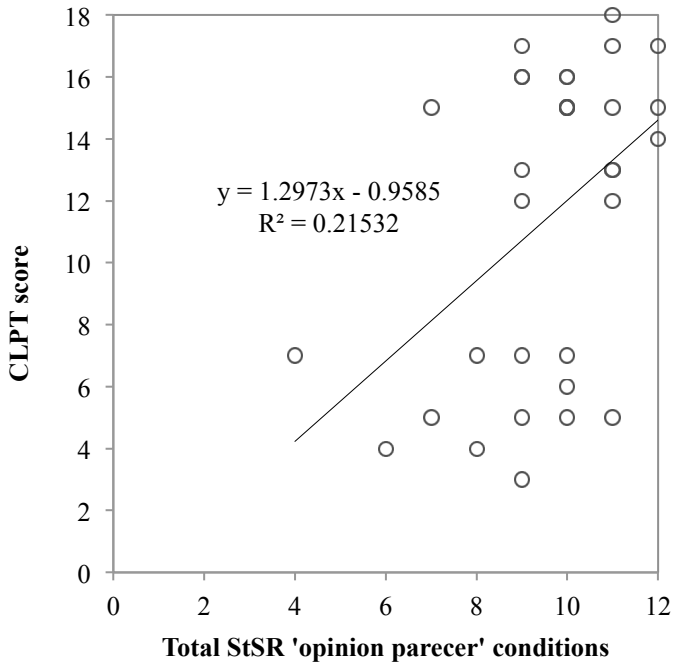


Figure 3.9. Correlation between the score obtained in the Spanish raised conditions and the score obtained in the CLP task in the chance and above chance children (sample of 32 children).

Again, we failed to find a correlation between the performance on the StSR opinion *parecer* conditions in the 'chance' and 'above chance' children and their digit span scores, regardless of the scoring method used in the digit span task, $r_s\,(30) = .225$, $p = .22$ for the scaled scores, and $r_s\,(30) = .284$, $p = .12$ for the itemized scores. Moreover, in a multiple regression model, with score in the two StSR opinion *parecer* conditions as a dependent variable, and CLPT score, digit span score, and chronological age as independent variables, we only found CLPT to be a marginally significant in predicting the score in the 'StSR opinion *parecer*' conditions, $p = .054$. Neither digit span, $p = .631$, nor chronological age $p = .633$ were significant.

Because PIE (Choe, 2012; Choe & Deen, 2016) and other adult processing accounts (see Gibson 1998, 2000; Grodner & Gibson, 2005; Hawkins, 1994; McElree, 2000; Warren & Gibson

1999, 2002, 2005) predict a difference in performance between the short and long experiencer conditions, we again compared the performance of the 'chance' and 'above chance' children on these two conditions. A Wilcoxon signed-rank test was conducted and we found a significant difference between the two conditions ($Z = -2.11$, $p = .034$), suggesting that 'chance' and 'above chance' children (i.e. children who are in the adult grammar stage by our hypothesis) have more difficulty processing longer interveners.

It thus seems, that both English- and in Spanish-speaking children can be subdivided into two groups: i) those whose difficulty with StSR is grammatical in nature, i.e. those who perform below-chance in the StSR conditions that involve raising over an experiencer, and ii) children whose problem is due to limited processing capacity, i.e. those who perform at chance and above. Children who belong to the first group do not have the appropriate rule system and may lack the grammatical mechanism to circumvent intervention. Therefore, they consistently interpret StSR with *seem*/opinion *parecer* in a non-adult-like manner, regardless of the length of the intervener, and regardless of their processing capacity. On the other hand, children in the second group may have access to the adult rule system; however, because these children do not have a fully developed processing capacity, computing constructions that involve StSR over an intervening argument may surpass their processing capacity. This correctly predicts that the chances of correctly interpreting these structures is predicted by their processing capacity, and that their performance on StSR over a longer experiencer argument will be particularly affected by their processing limitations.

While we did not find any significant correlation with age, one may assume that these groups represent different developmental stages. That is, initially, children have a grammar that block A-movement over intervening arguments. Subsequently, they acquire the grammatical rule

that allows them to circumvent intervention; however, children with poor processing capacity will still experience difficulties with these constructions. Eventually, they have both the adult grammar, and the memory capacity required to process these sentences. Nevertheless, since children mature at different rates with respect to both grammar and processing capacity, it could also be the case that some children develop the necessary processing capacity before they acquire the grammatical mechanism to by-pass interveners. Our current data sample does not provide evidence for a particular chronological order. Crucially, however, we have found evidence for both grammar- and processing-based difficulties associated with the delays observed with StSR both in English and in Spanish.

3.4. Conclusion

In this chapter, we addressed two main questions. The first was to determine whether the delay observed with subject-to-subject raising is due to intervention effects. We answered this question in two ways: i) by replicating findings showing that English-speaking children have difficulties with StSR with *seem*-type verbs even when there is no overt experiencer; and ii) by looking at the acquisition of StSR in a language in which *seem,* or any other conceptually similar verb, does not select for an experiencer, i.e. Spanish bare *parecer*.

Our results confirm that English-speaking children up to the age of six have difficulties comprehending raised but not unraised constructions with *seem*. On average, English-speaking children scored 53% in the 'StSR *seem* covert experiencer' condition. This result is in clear contrast with the Spanish-speaking children, who obtained an average score of 92% in the 'StSR bare *parecer*' condition, just 1% less than in the 'unraised' condition. Given that the methodology and the experimental materials were identical and the sentences were superficially

analogous in the two languages, we interpret this behavioral difference as evidence for a syntactic difference; namely, English *seem* always has a syntactically projected experiencer argument, as claimed by Orfitelli (2012) and Snyder and Hyams (2015), while Spanish bare *parecer*, which is a modal-like verb, does not, as has been argued in the syntactic literature (Ausín, 2001; Bosque & Torrego, 1995; Gallego, 2010; Torrego, 2002). In other words, the reason why we observe intervention effects in English but not Spanish in StSR sentences without an (overt) experiencer is because children must circumvent an intervening experiencer in English, but not in Spanish. These ideas are also supported by the results from our corpus study, which reveal that English-speaking children are less likely to produce raised *seem* constructions than adults, even when the experiencer is not overt. Contrastively, Spanish-speaking children are only less likely to use the raised construction in comparison to the adults with opinion *parecer*, but not with bare *parecer*.

Our second goal was to determine if these intervention effects have a grammatical (e.g. Hyams & Snyder, 2005, 2006; Orfitelli, 2012; Snyder & Hyams, 2015) or processing base (e.g. Choe, 2012). We addressed this question in two different ways: i) by including 'short' and 'long experiencer' conditions, and ii) by comparing children's StSR proficiency with two independent measures of processing capacity (CLPT) and short-term memory (digit span). When comparing children's performance between the short and long experiencer conditions we found no significant difference when we included all the children in the analysis in either the English-speaking group, where they scored an average of 56% and 48% respectively, or in the Spanish-speaking group, where they obtained an average of 75% and 69%, respectively. Moreover, we found no significant correlation between children's overall performance in the three StSR conditions and CLPT score, nor between their performance in StSR and digit span score when

we included all of the children either in English or in Spanish. These two results suggested that children's difficulties with StSR over an experiencer cannot be solely due to processing difficulties, and that they must have a grammatical base.

However, a closer look at the data revealed that children who performed at chance or above in the StSR conditions did behave as expected by processing accounts: they showed a strong correlation with the CLPT score, and they performed significantly worse in the 'long experiencer' conditions. We hypothesized that there are two different factors affecting children's difficulties with StSR with *seem*-type verbs and proposed the Dual Source Intervention hypothesis. On the one hand, there is a grammatical constraint that blocks children from A-moving over an intervening (overt or covert) argument. Children in this group are thus unable to comprehend StSR sentences, irrespectively of their processing capacity. On the other hand, children who are able to interpret these sentences in an adult-like manner – even if only some of the time, as their response justifications indicate – may have the grammatical machinery needed to A-move the subject overt the experiencer, but the processing cost associated with effecting the movement operation across an intervening argument is too great, causing children to fail on these type of constructions.

CHAPTER 4

Study 2: Acquisition of Subject Control

Since C. Chomsky's (1969) pioneering work, several studies have investigated children's acquisition of *promise* in English (C. Chomsky, 1969; Hsu, Cairns, & Fiengo, 1985; Sherman & Lust, 1993). An established result is that while children correctly interpret the embedded subject (i.e. PRO) in complements of object control (OC) verbs, as in the one in (1a) from relatively early, they fail to do so with subject control (SC) *promise,* (1b), until school age, approximately at the same time children acquire StSR with *seem*.

(1) a. The mom_i told the boy_k PRO_k to be loyal and honest.
b. The boy_i promised his mom_k PRO_i to be loyal and honest.

Boeckx and Hornstein (2003), Hornstein and Polinsky (2010), and Boeckx, Hornstein, and Nunes (2010) propose a grammatical account for this delay. According to these authors, the benefactive of *promise*, '*his mom*' in (1b), is headed by a null preposition, similar to the one observed in other SC verbs such as *swear* (2a) or *commit*, and as observed in other languages, such as Spanish *prometer* 'promise' (2b). They claim that English-speaking children have difficulties obtaining subject control with *promise* because they fail to represent the P_{null}, which shields the benefactive DP from being a potential intervener for the control of PRO.[1] We labeled

[1] These authors are proponents of the Movement Theory of Control (Hornstein 1999, *et seq.*; Manzini & Roussou 2000; O'Neill, 1995). They would thus treat PRO as a trace in cases of obligatory control such as the ones in (1) and (2).

this account as the Null Preposition Hypothesis (NPH). This hypothesis thus predicts that children should be more likely to incorrectly show object control with *promise* than with *swear*-type verbs due to the opaqueness of the prepositional complement structure. The extant literature on the acquisition of subject control of *promise*-type verbs is limited to the English verb *promise* and predates by decades recent syntactic theories (C. Chomsky, 1969; Hsu, Cairns, & Fiengo, 1985). We will test this hypothesis experimentally by looking at English-speaking children's comprehension of subject control sentences with *swear*, and compare them to their performance with analogous sentences with *promise*. Additionally, we will test Spanish-speaking children on both *prometer* 'promise' and *jurar* 'swear', both of which take a benefactive headed by the case-marking preposition *a* 'to', as illustrated in (2b). The prediction of the NPH thus is that English-speaking children will show better performance with *swear* than with *promise* and that Spanish-speaking children will do relatively well with both verbs.

(2) a. The boy$_i$ swore to his mom$_k$ PRO$_i$ to be loyal and honest.
b. *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'

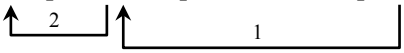
The authors of the NPH are also proponents of the idea that the difficulties with SC *promise* and StSR *seem* are connected. Although there exists no experimental evidence linking children's observed delays observed with these two constructions, recent theoretical discussions have related these difficulties by proposing either a similar syntactic derivation, or a comparable processing cost. As mentioned in Chapter 1, an alternative view to the standard theory of control is that obligatory control is derived by A-movement. This analysis, known as the Movement Theory of Control (MTC; Hornstein 1999, 2001; Manzini & Roussou 2000; O'Neil 1995, *inter*

alia), is illustrated in (3).[2] Hornstein (1999) claims that since both *seem*- and *promise*-type predicates violate the Minimal Distance Principle (MDP; Rosenbaum, 1970), they are to be considered "marked", and predicted to emerge (approximately equally) late. However, since both StSR *seem* (3a) and SC *swear* (3b) have intervening arguments headed by an overt preposition, these two constructions should be mastered relatively earlier than SC *promise* (3c), as per the NPH.

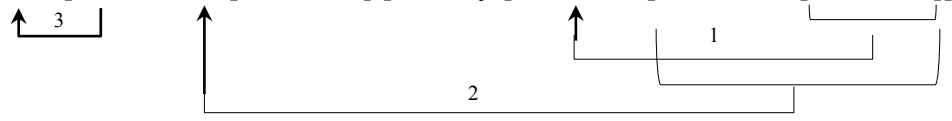
(3) a. $[_{DP}$ The girl$]_i$ seems $[_{PP}$ to her mom] t_i to be cautious.
b. $[_{DP}$ The girl$]_i$ swore $[_{PP}$ to her mom] t_i to be cautious.
c. $[_{DP}$ The girl$]_i$ promised $[_{PP}$ P_{null} her mom] t_i to be cautious.

Yet another grammatical account builds on the idea that SC *promise,* like StSR *seem*, undergoes a smuggling-type process, as illustrated abstractly in (4), and in (5) for StSR *seem* (as per Collins' [2005a] analysis), and in (6) for SC *promise*.[3] This operation makes the subject the closest controller for PRO, and subject control obtains.

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



(5) a. John seems to Mary to be nice.
b. John [~~John~~ seems [~~to be nice~~] [to Mary [to be nice [~~John seem [to be nice~~]]

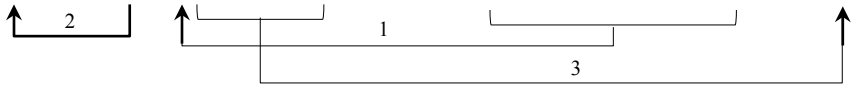


[2] However, note the NPH can be tested without necessarily adopting a movement approach to control.

[3] As discussed in Section 2.2.1.2, under Belletti and Rizzi's analysis, the benefactive 'Mary' does not c-command PRO after step 1, hence it does not structurally intervene between the subject and PRO. Subject control thus obtains.

(6) a. John promised Mary to be nice.

b. John v_{make}+promise [~~promise~~ [~~PRO to go~~] [Mary *ben* [~~promise [PRO to go~~]]] [PRO to go].



Belletti and Rizzi (2013) claim that smuggling operations are acquired late and that children's difficulties with SC can be looked as a case of delay similar to the one children experience with StSR *seem*.[4] If the intervening argument is bypassed using the *same* type of operation in both constructions, we should expect children to master them at roughly the same time. If no such relationship is found, it may indicate that children fail to by-pass the intervener in StSR and SC constructions for independent reasons.

While there are no performance-based hypotheses that specifically address the difficulties children have with *promise*, if we extend the Performance-based Intervention Effects hypothesis (PIE, Choe, 2012; Choe & Deen, 2016) to SC verbs like *promise*, we would expect that children have an adult-like representation of SC, but that as in the case of StSR, the added processing cost associated with mediating the dependency between the surface subject and the implicit subject (PRO) in the embedded clause over an intervening benefactive might prove too great, and cause children to fail. A processing story along these lines thus predicts i) significantly worse performance when we increase the number of DPs that linearly intervene between the subject and PRO (see Gibson 1998, 2000; Grodner & Gibson, 2005; Warren & Gibson, 1999, 2002,

[4] Belletti & Rizzi assume Collins' (2005a) smuggling analysis, which does not involve semantic coercion. However, if one adopts Gehrke and Grillo's (2008) smuggling analysis, as Snyder and Hyams' (2015) propose for non-actional passives and StSR *seem*, these would also involve semantic coercion (see Section 2.1.1.3). If SC *promise* (an actional verb) is derived via smuggling, and StSR *seem* (a non-actional verb) requires both smuggling and semantic coercion, we should expect to find the former to be acquired earlier than the latter, as is the case of actional passives with respect to non-actional passives.

2005); ii) a correlation between performance with SC *promise*/*prometer* and memory-span tasks; iii) a correlation between performance with StSR *seem*/opinion *parecer* (see Chapter 3) and SC *promise*/*prometer*.

The main focus of the two experiments described in the following sections is to provide a better understanding of what properties of SC *promise/prometer* are particularly challenging for children and why that would be the case. In particular, we would like to determine if the delays observed with this construction are related to children's difficulty detecting the null preposition heading the benefactive of *promise*, as suggested by Hornstein and colleagues, if they are related to the grammatical operation that is required to circumvent the intervener, or if they are simply related to the increased processing cost associated with the presence of an intervening DP between the controller subject and the embedded subject PRO, as suggested by some processing accounts. We will address these questions in the first part of this chapter.

A secondary goal of this study is to determine whether the grammatical mechanism that derives SC with *promise*-type verbs is in fact the same that derives StSR with *seem*-type verbs, e.g. via smuggling, as suggested by Belletti and Rizzi (2013), or A-movement, as argued by the Movement Theory of Control. In the second part of this chapter we will address this question by examining whether the grammatical developments of StSR and SC are related at the individual performance level.

4.1. Part I: Subject Control study

4.1.1. Subjects

The same 30 monolingual English-speaking children that participated in the subject-to-subject raising (StSR) experiment participated in the subject control (SC) experiment (see (Section 3.2.1). As mentioned in the previous chapter, data from an additional seven children were excluded because they failed to correctly answer at least five out of the six trials in the control conditions of the StSR (N = 7); two of these children also failed on the control items in the SC experiment. Testing was conducted primarily in a childcare center in West Los Angeles and in an elementary school in Ventura County. A small number of participants were tested on the UCLA campus. Testing occurred over two sessions with no longer than nine days between the first and second part.

Given the inter-speaker variation with respect to the grammaticality of English *promise* followed by a benefactive and a nonfinite complement,[5] we asked parents to evaluate the grammaticality of (7a) and (7b) in a three-point Likert scale (1 = weird, 3 = natural). No parent reported (7b) to be 'unnatural' nor any less natural than (7a), i.e. the sentence (7b) was judged as 'natural' as often or more than (7a) in all cases.

(7) a. Mary seems to John to be smart.
b. Mary promised John to be cautious.

[5] In an unpublished study, Hirsch, Orfitelli, and Wexler tested over 5,000 native English speakers on the grammaticality of '*promise* + DP + to VP'. They found that approximately 70% of speakers permit it, and 30% do not. Neither age, nor dialect, nor gender, nor SES was able to account for the distribution. However, they may have found a possible genetic relationship –parents and children, and siblings, including fraternal twins, are significantly correlated. Additionally, identical twins are more closely correlated than fraternal twins (Orfitelli, p.c.).

The same 36 monolingual Spanish-speaking children that participated in the StSR study took part in the Spanish SC study. As previously mentioned, data from an two children were excluded because they failed to correctly answer at least five out of the six control trials in the StSR experiment. The experiments were conducted in a preschool and a primary education center in Granada, Spain. Testing occurred over two sessions with no longer than six days between the first and second part.

The same ten native English-speaking adults and twelve native Spanish-speaking adults that were included as controls in the StSR study completed the SC study. As in the StSR study, the methodology used with the adults was the same as with the children except that the trial sentences were presented by the computer instead of a puppet.

4.1.2. Materials and methods

As in the StSR experiments, the SC experiments used a Truth-Value Judgment Task (TVJT; Crain & McKee 1985; Crain & Fodor 1993). The children observed narrated scenario, and subsequently a puppet named Coco commented on it. The child was asked to be Coco's teacher: to listen carefully and to tell him when he said something right and when he said something wrong, and why.

Two training trials preceded each test session to ensure the child understood the task, and that he or she would correct the puppet when the comment was untruthful. As in the test trials, each training trial consisted of two pictures and an accompanying story. Neither of the trainings contained a StSR or SC structure. We designed six different scenarios, all of which included three characters: one that performed the action in the embedded clause, and two potential benefactives in order to satisfy the felicity condition in the overt benefactive trials.

In an example scenario, a knight and a wizard discuss who should go out to defend the castle after another character – a princess – announces they are being attacked. In the *tell/ordenar* conditions the characters are talking, while in the *promise/prometer* and *swear/jurar* conditions they are making a pinky promise. In order to ensure that the children understood the meaning of *promise/prometer* and *swear/jurar*, the experimenter would ask the child if he or she knew what a "pinky promise" or "pinky swear" was when the first SC trial appeared. The vast majority of the children said 'yes', but regardless of their answer, 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*'.

In the second image the knight is outside the castle wielding his sword. In the long benefactive condition, there are two characters of the same sort and the benefactive has an identifying trait (e.g. a knight with a cape). This guaranteed the felicity of the PP (e.g. '*with the cape*'). An example for a short benefactive condition is given in Figure 4.1.

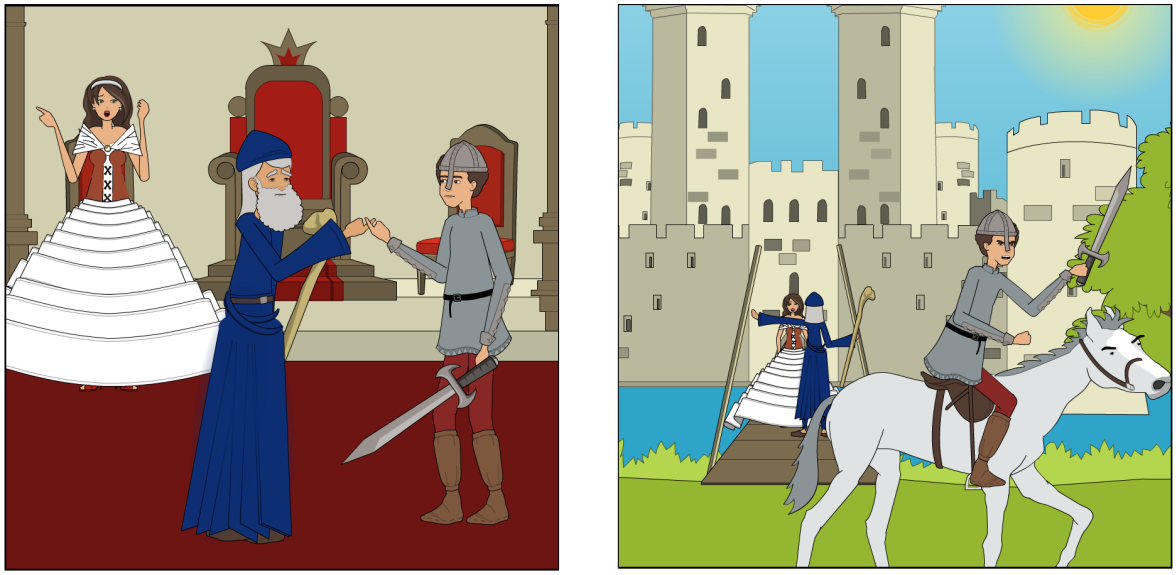


Figure 4.1: Subject control experiment sample pictures: The left picture shows the character making the promise. The right picture shows the character fulfilling his promise.

The stories were digitally recorded in audio files by an English and a Spanish native speaker respectively and played through loudspeakers connected to a personal computer administering the test. Following the story, the puppet comments on it using one of six sentence types: object control with English *tell* or Spanish *ordenar* 'order'; subject control with *promise*/*prometer* without a benefactive, subject control with *promise/prometer* with a short benefactive, subject control with *promise*/*prometer* with a long benefactive, subject control with *swear*/*jurar* without a benefactive, subject control with *swear/jurar* with a short benefactive. Sentences with adult judgments are provided in Table 4.1 for English and 4.2 for Spanish. All the experimental items for the SC experiments are included in Appendices C and D.

Table 4.1. Subject control test items for the English study.

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 p*romise*, 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.

Table 4.2. Subject control test items for the Spanish study.

Condition	True Test Items	False Test Items
OC *ordenar*	El mago ordena al caballero defender el castillo. *'The wizard orders the knight to defend the castle.'*	El caballero ordena al mago defender el castillo. *'The knight orders the wizard to defend the castle.'*
SC *prometer*, no ben.	El caballero promete seriamente defender el castillo. *'The knight seriously promises to defend the castle.'*	El mago promete seriamente defender el castillo. *'The wizard seriously promises to defend the castle.'*
SC *prometer*, short ben.	El caballero promete al mago defender el castillo. *'The knight promises the wizard to defend the castle.'*	El mago promete al caballero defender el castillo. *'The wizard promises the knight to defend the castle.'*
SC *prometer*, long ben.	El caballero promete al mago con la capa defender el castillo. *'The knight promises the wizard with the cape to defend the castle.'*	El mago promete al caballero con la capa defender el castillo. *'The wizard promises the knight with the cape to defend the castle.'*
SC *jurar*, no ben.	El caballero jura seriamente defender el castillo. *'The knight seriously swears to defend the castle.'*	El mago jura seriamente defender el castillo. *'The wizard seriously swears to defend the castle.'*
SC *jurar*, short ben.	El caballero jura al mago defender el castillo. *'The knight swears to the wizard to defend the castle.'*	El mago jura al caballero defender el castillo. *'The wizard swears to the knight to defend the castle.'*

The conditions without a benefactive primarily served to verify that children had the appropriate lexical understanding of the verbs *promise/prometer* and *swear/jurar*. Their performance on the 'benefactive' conditions must therefore be separate from this consideration. Children scoring less than 5 out of 6 items correct on either the OC or SC without a benefactive conditions were not included in subsequent analyses. This resulted in the exclusion of two English-speaking children, as noted above. No Spanish-speaking child was excluded for failing the SC experiment controls. Moreover, the inclusion of *seriously/seriamente* on the 'no

benefactive' conditions served to emphasize the intentionality of the promisor to fulfill his pledge, as well as to partially match the StSR 'no experiencer' conditions with the adverb *definitely*/*definitivamente*.

The condition with English *swear* followed by a short benefactive served to test the predictions of the Null Preposition Hypothesis; namely, that the overt preposition introducing the benefactive of *swear* would improve children's performance relative to their performance with *promise*, which has a hidden preposition by hypothesis. Similarly, we included the Spanish conditions with *prometer* 'promise' and *jurar* 'swear' because the NPH also predicts Spanish-speaking children should do equally well in both, since in both cases the benefactive is headed by the case-marking preposition *a* 'to'.

Lastly, the *promise*/*prometer* conditions with a long benefactive were included to be compared with the 'short benefactive' conditions, to test processing accounts which predict a decline in performance when the intervening material is lengthened (i.e. by containing an additional DP).

Each test item was read with primary stress placed on the main clause predicate (i.e. *tell/ordenar*, *promise/prometer*, or *swear/jurar*), and secondary stress placed on either *seriously/seriamente*, or on the benefactive. Half of the test items were judged true by adult English speakers, and half were false. Each condition in Tables 4.1 and 4.2 was tested six times, once for each scenario, for a total of 36 test items. The test design was between-subjects so that no child would be presented with the 'true' and 'false' test items for the same scenario. The order of presentation for these items was pseudo-randomized such that children were never tested on either the same condition or the same scenario in two consecutive trials. Finally, recall that in order to avoid carryover effects, we interspersed the trials of the StSR and SC experiments and

divided the task into two balanced sessions. For expository purposes, we present the experiments and their results separately.

Crucially for the SC experiment, the main manipulation is whether the promisor or the benefactive carries out the action in the embedded clause. While the wizard is the one who tells the knight to defend the castle, it is the knight that promises to do so. This distinction is important given the findings, discussed in Section 1.2, that when faced with a SC structure with *promise*-type verbs, immature children choose the object as the antecedent of the embedded subject PRO, as opposed to the subject. Any child using such a strategy will therefore lead to a consistent non-adult pattern of answers.

4.1.3. SC results for the English-speaking group

Let us first discuss the TVJT results from the English-speaking group. As mentioned in the previous section, when children were not correct on at least five out of the six trials on the 'OC *tell'* or the 'SC *promise*/*swear* without a benefactive' conditions, their data were not included in the analysis. Two English-speaking children scored three out of six with 'promise without a benefactive'.[6] These two children were also excluded from the analysis for failing to pass the control condition in the StSR experiment. The remaining children, who performed well on these conditions and were included in the analysis, demonstrated that they were able to: i) establish a Control dependency between a DP in the matrix clause and the embedded subject PRO; and ii) understand the lexical meaning of *promise* and *swear*. The subjects' performance by age group on the six different conditions is shown in Figure 4.2.

[6] The response justifications from these two children and its theoretical implications will be discussed in Section 4.1.10.

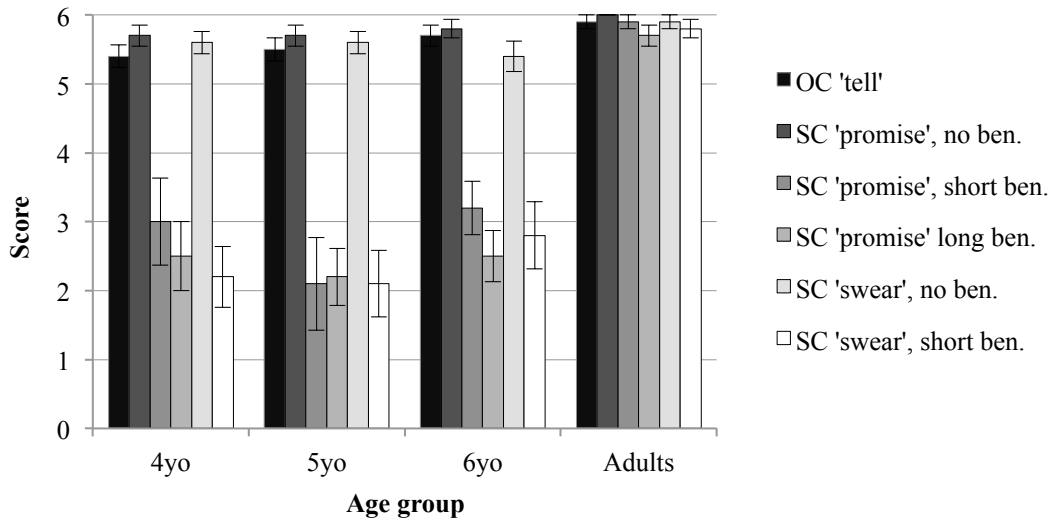


Figure 4.2. English subject control experiment results by age group and condition.

As regards to the two 'short benefactive' conditions, as expected, children performed significantly worse in the 'SC *promise* with a short benefactive', 46.17% (M = 2.77/6) than in the 'SC *promise* without a benefactive' conditions, 95.5% (M = 5.73/6). Wilcoxon signed-rank tests with Bonferroni's correction for three comparisons (i.e. 0.05/3 = 0.02) confirmed this, Z = -4.59, $p < .001$. This difference was also observed in each of the three age groups, Z = -2.55, p = .011 (four-year-olds), Z = -2.66, p = .008 (5-year-olds), Z = -2.83, p = .005 (6-year-olds).

Similarly, children performed significantly worse in the 'SC *swear* with a short benefactive' 39.5% (M = 2.37/6) condition than in the 'SC *swear* without a benefactive' condition, 92.17% (M = 5.53/6), Z = -4.8, $p < .001$. The three age groups also showed this pattern, Z = -2.81, p = .005 (four-year-olds), Z = -2.83, p = .005 (five-year-olds), Z = -2.87, p = .005 (six-year-olds). These results indicate that while children easily assigned subject control to PRO when there was no benefactive, they showed difficulties interpreting PRO when there was an intervening benefactive.

Crucial to the question of whether children would perform better when the benefactive is headed by a preposition, we found no significant difference between the 'SC *promise* with a short benefactive' and the 'SC *swear* with a short benefactive' conditions, Z = -1.76, p = .078. In fact, children did slightly better with *promise,* 46.17% (M = 2.77/6) than with *swear,* 39.5% (M = 2.37/6), despite the fact that all of them understood of the meaning of both verbs. Therefore, the presence of the preposition *to* on the benefactive of *swear* did not aid children in circumventing the minimality violation, as hypothesized in Boeckx and Hornstein (2003). Additionally, even with few subjects we found a significant correlation between children's performance on *promise* and *swear*, $r_s(28) = .649$, $p < .001$. Such strong trend suggests that children's difficulties with these two constructions are likely due to the same underlying factor.

Finally, we wanted to examine whether lengthening the intervening benefactive argument would worsen children's performance, as expected by processing accounts. We examined the difference between children's performance in the 'SC *promise* with a short benefactive', 46.17% (M = 2.77/6) and 'SC with a long benefactive' conditions, 40% (M = 2.4/6) and we found there was no significant difference. Wilcoxon signed-rank tests with Bonferroni's correction for three comparisons confirmed this, Z = -1.67, p = .1. None of the three age groups showed a significant difference either, Z = -1.41, p = .16 (four-year-olds), Z = -.173, p = .86 (five-year-olds), Z = -2.33, p = .02 (six-year-olds). This shows that children, as a whole, did not perform significantly worse when we lengthened the intervening benefactive. As expected, the adults did not perform significantly differently depending on the condition based on Friedman test, $\chi^2(5) = 4.44$, p = 0.487.

Summarizing the group results, English-speaking children did well in the 'object control *tell*' condition as well as in the 'SC *promise* and *swear* without a benefactive' conditions, but

they did significantly worse in the 'short' and 'long benefactive' conditions. This result replicates previous findings (Chomsky, 1969; Hsu et al., 1985), and suggests that children experience difficulties establishing a control dependency over a structurally intervening argument. However, there was no effect of intervener length, suggesting that as a whole, the English-speaking children do not behave as expected on processing accounts that would predict a decrease in performance with longer intervening material (e.g. Bloom, 1990; Choe, 2012; Gibson, 1998, 2000; Wagers & Phillips, 2013, *inter alia*).

4.1.4. SC results for the Spanish-speaking group

Let us now turn to the Spanish-speaking experimental data obtained from the TVJT. As in the case of the English experiment, when the Spanish-speaking children did not correctly answer at least five out of the six trials on the object control or subject control without a benefactive conditions, their data were not included in the analysis. None of the Spanish-speaking children were eliminated for this reason. The subjects' performance by age group on the six different conditions is shown in Figure 4.3.

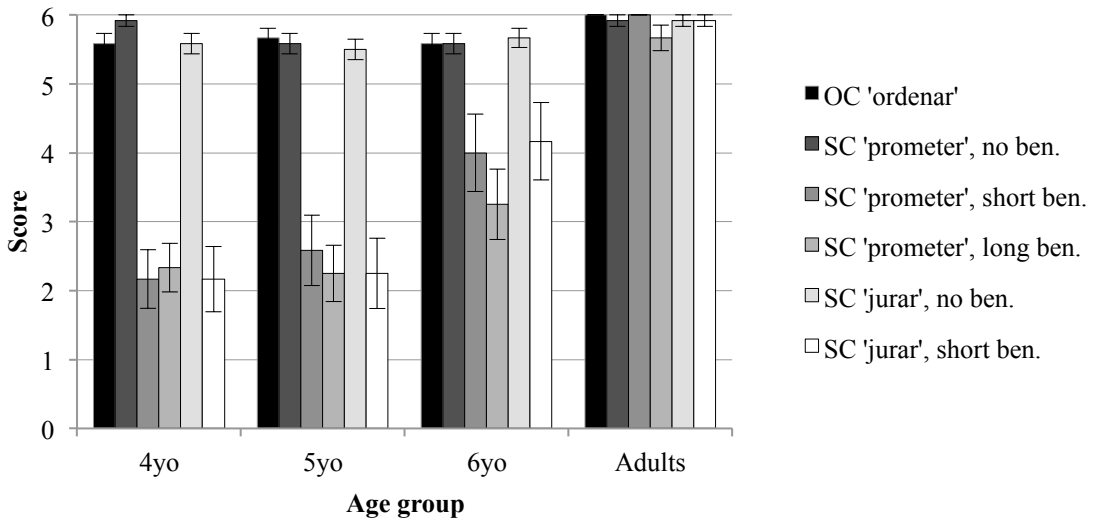


Figure 4.3. Spanish subject control experiment results by age group and condition.

The Spanish results largely replicated the English ones. Overall, children performed poorly on both the 'benefactive' conditions, just like the English-speaking children did. Specifically, Spanish-speaking children performed significantly worse in the 'SC *prometer* with a short benefactive', 48.67% (M = 2.92/6) condition, than in the 'SC *prometer* without benefactive' condition, 94.83% (M = 5.69/6). Wilcoxon signed-rank tests with Bonferroni's correction for three comparisons confirmed this, Z = -4.88, $p < .001$. This was also observed in the four-, Z = -3.08, $p = .002$, five-, Z = -3.071, $p = .002$, and six-year-olds, Z = -2.254, $p = .024$.

Likewise, children performed significantly worse in the 'SC *jurar* with a short a benefactive', 47.67% (M = 2.86/6) condition than in the 'SC *jurar* without a benefactive' condition 93% (M = 5.58/6), Z = -4.8, $p < .001$. This was also true for the four-, Z = -3.09, $p = .002$, five-, Z = -3.07, $p = .002$, and six-year-olds Z = -2.207, $p = .027$. It thus seems that an overt preposition heading the benefactive does not aid Spanish-speaking children with *promise*-type verbs.

We additionally examined the difference between children's performance with *prometer* in the 'short benefactive' 48.67% (M = 2.92/6) and 'long benefactive' conditions, 40% (M = 2.4/6) and we found there was no difference. Wilcoxon signed-rank tests with Bonferroni's correction for three comparisons confirmed this for the whole group, Z = -1.362, p = .173, and for each of the three age groups, Z = -.816, p = .414 (four-year-olds), Z = -.794, p = .427 (five-year-olds), Z = -1.809, p = .07 (six-year-olds). That is, Spanish-speaking children, like the English-speaking children, did not perform significantly worse when we lengthened the intervening benefactive, contra processing accounts that would predict a decline in children's performance on the 'long benefactive' condition. As expected, the adults did not perform significantly differently depending on the condition based on Friedman test, $\chi^2(5) = 10.00$, p = 0.08.

Summarizing the group results, Spanish-speaking children, like English-speaking children, did well in the OC condition with *ordenar* 'order/tell' as well as in the 'SC *prometer* and *jurar* without a benefactive' conditions, but they did significantly worse in the conditions that included an intervening benefactive. This result is comparable to the results obtained for English, and show that Spanish-speaking children also experience difficulties establishing a control dependency over a structurally intervening argument. Importantly, this is case even when the benefactive is headed by English 'to', as in the case of *swear*, or by the Spanish case-marked preposition *a* 'to', as in the case of both *prometer* and *jurar*. The homogeneity of the results further suggests that Boeckx and Hornstein's (2003) Null Preposition Hypothesis is not responsible for the delays that had been long observed with English SC *promise*.

Additionally, as in the case of the English-speaking children, we found no significant difference between the 'short' and 'long benefactive' conditions with *prometer*. Therefore, the

results as a whole also fail to support the predictions of purely processing-based accounts that would predict a significant decrease in performance with longer intervening arguments (e.g. Bloom, 1990; Choe, 2012; Gibson, 1998, 2000; Wagers & Phillips, 2013, *inter alia*).

4.1.5. Competing language processing task results and SC

In addition to manipulating the length of the intervening benefactive, in order to test the predictions of processing accounts we administered two tasks that measure processing and short-term memory capacities respectively. We will first report on the results from the verbal processing capacity task. If processing limitations constitute a significant factor underlying children's poor comprehension of SC with *promise*-type verbs, the score they obtained in the three 'benefactive' conditions should be positively correlated with an independent measure of processing capacity. We chose to administer a listening span task, which consisted of an abridged version of the Competing Language Processing task (CLPT; Gaulin & Campbell, 1994). In this task, children had to correctly say if a statement was true or false and recall the last word of each utterance after listening to *all* the sentences of the trial, i.e. one in Level 1, two in Level 2, and three in level 3. There were a total of 18 sentences. The methodological details of the task are discussed in Section 3.3.2.2, and the results can be found in Section 3.3.5.

If low processing capacity were the *single* cause of poor performance with subject control (e.g. Choe's PIE hypothesis, 2012), we should not expect to find children with high processing capacity performing below chance with subject control. However, that was not the case, as illustrated in Figure 4.4; we found no significant correlation between the two scores, $r_s\,(28) = .01$, $p = .97$. In fact, 8/11 children who performed below chance in the 'benefactive' conditions (≤5/18) obtained a higher than average score in the CLPT ($M = 11.2/18$).

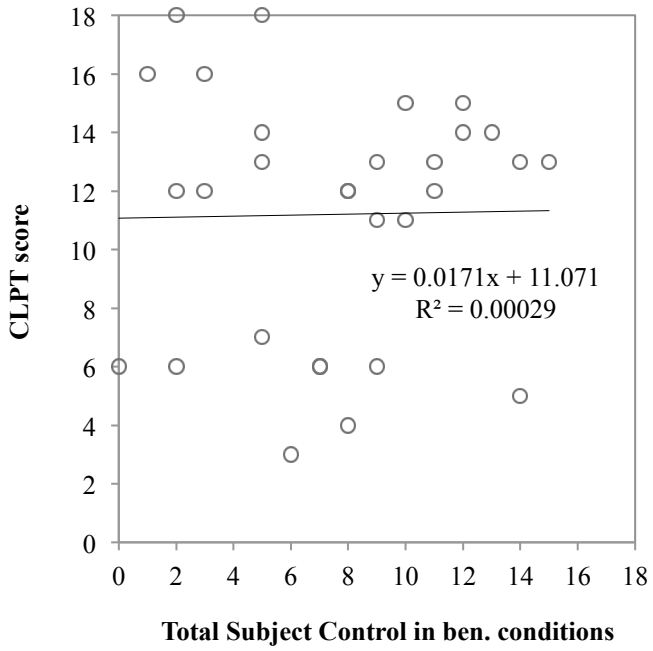


Figure 4.4. Correlation between the score obtained in the English subject control conditions that included an overt benefactive and the score obtained in the CLP task (sample of 30 children).

Turning to the Spanish-speaking children's scores obtained in the CLPT and the three subject control conditions that entail a control dependency over an argument, we also found there to be no significant correlation, $r_s\,(34) = .225$, $p = .188$. That is, as in the English-speaking group, processing capacity does not seem to accurately predict the rate of success in the SC constructions in all children. This is further illustrated in Figure 4.5.

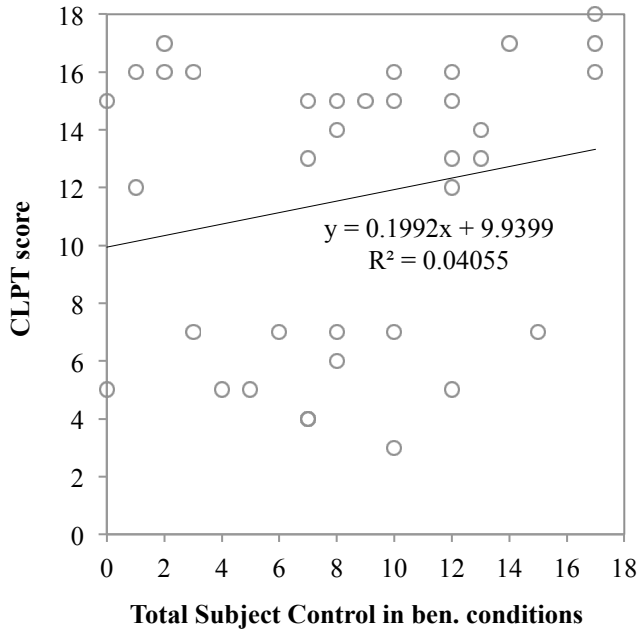


Figure 4.5. Correlation between the score obtained in the Spanish subject control conditions that included an overt benefactive and the score obtained in the CLP task (sample of 36 children).

4.1.6. Digit span task results and SC

We also wanted to investigate whether it could be short-term memory limitations, rather than verbal processing capacity limitations, that are the cause of children's poor performance with SC with *promise*-type verbs. For this purpose, we administered a digit span task, which consists on repeating increasingly long lists of digits. The methodological details of the task are discussed in Section 3.3.2.3, and the results can be found in Section 3.3.6. In the English-speaking group, we found no significant correlation between the two scores, regardless of the scoring method used in the digit span task, $r_s(28) = -.068$, $p = .722$ for the scaled scores, and r_s

(28) = -.071, p = .71 for the itemized scores.[7] That is, short-term memory capacity does not accurately predict the rate of success in the constructions that involve SC with an intervening benefactive.

As for the Spanish-speaking group, we also found no significant correlation between the two scores, regardless of the scoring method used in the digit span task, r_s (34) = .009, p = .959 for the scaled scores, and r_s (34) = .12, p = .486 for the itemized scores. This suggests that, as for English, short-term memory deficits are not likely to be the cause of Spanish-speaking children's difficulties with constructions that involve SC over a benefactive.

4.1.7. Interim summary of SC results

In this study we found that both English- and Spanish-speaking children up to the age of six experience difficulties comprehending subject control sentences with *promise*/*prometer* and *swear*/*jurar* when followed by a benefactive*,* but not object control with *tell*/*ordenar*. On average, English-speaking children scored 46% in the 'SC *promise* with a short benefactive' condition, 39% in the 'SC *swear* with a short benefactive' condition, and 92% in the 'OC *tell*' condition. The results from Spanish parallel those of English –on average, Spanish-speaking children scored 48% in the 'SC *prometer* with a short benefative' condition, 47% in the 'SC *jurar* with a short benefactive' condition, and 93% in the 'OC *ordenar*' condition. From this we concluded that English-speaking children's reported difficulty with SC *promise* cannot be due to

[7] As discussed in Section 3.3.2.3 the digit span task is typically scored as the maximum length at which the child correctly recalled at least two lists of numbers (Gathercole & Adams, 1993). We refer to this as the 'scaled score'. However, for more direct comparison with the CLPT score, which was calculated as the total number of correctly repeated words, we also calculated a separate score for total correct digit lists repeated, as in Gaulin and Campbell (1994). This is the 'itemized score'.

their difficulty identifying the P_{null} heading the benefactive, as Boeckx and Hornstein (2003), Hornstein and Polinsky (2010), and Boeckx, Hornstein, and Nunes (2010) propose. If that were the case, we would have found children perform significantly better with SC *swear*. Moreover, the Spanish-speaking children performed as poorly as the English-speaking children in those two conditions, despite the fact that both the benefactive of *prometer* and *jurar* are headed by the case-marking preposition a 'to'.

In order to evaluate processing accounts, we manipulated the length of the benefactives and administered two different memory tasks. However, the predictions failed for both English and Spanish. As a whole, we found no significant difference between the 'short' and 'long benefactive' conditions, either in the English-speaking group, where they scored an average of 46% and 40% respectively, or in the Spanish-speaking group, where they obtained an average of 49% and 43% respectively. That is, adding intervening material did not significantly decrease their performance.

Moreover, we found no significant correlation between performance on the three 'SC benefactive' conditions and the Competing Language Processing Task, nor between those 'SC benefactive' conditions and the digit span task in neither English nor Spanish. Therefore, the results of both the English- and the Spanish-speaking groups, *as a whole*, fail to support the predictions of processing accounts.

4.1.8. A closer look into the individual SC data

Having discussed the group data, let us now look at individual results. Recall that examining the individual data of the StSR experiments forced us to revise our conclusions regarding the effects of processing limitations upon closer scrutiny. As in the StSR study, we

found once again that a large proportion of children showed mixed responses along with coherent justifications, suggesting they were not merely guessing. Hsu et al. (1985) also found a group of children that behaved in this way and argued that these "mixed subject-object control children" are not answering randomly to the TVJT, but rather, they are in a transitional stage in which they have already developed a new rule system but they may still lack the grammatical knowledge as to when the new system should be applied. An alternative explanation for this behavior was given to account for the mixed pattern of responses in the StSR experiments, namely that 'chance' and 'above chance' children have the adult grammars, but still show difficulties computing these complex derivations due to an immature performance system, as we found for the StSR case. We will continue to refer to this hypothesis as the Dual Source Intervention (DSI) hypothesis.

We begin by discussing the distribution of the individual data. A binomial distribution was used to establish whether their overall StSR score could have resulted from chance. Out of the 18 SC test items that included an intervening benefactive, the inclusive range 0-5 constituted below chance (BC) performance, 6-12 constituted chance (C) performance, and 13-18 constituted above chance (AC) performance, all with a confidence interval of >95%. The number of children that were classified as behaving below, at, and above chance in three SC constructions is given in Table 4.3 for English, and Table 4.4 for Spanish.

Table 4.3. Individual results from the English subject control experiment classified according to below, at, or above chance performance in the subject control conditions that included a benefactive.

Age group	**BC** (0-5)	**C** (6-12)	**AC** (13-18)
6-year-olds	3	6	1
5-year-olds	5	4	1
4-year-olds	3	5	2
Total	**11**	**15**	**4**

Table 4.4. Individual results from the Spanish subject control experiment classified according to below, at, or above chance performance in the subject control conditions including a benefactive.

Age group	**BC** (0-5)	**C** (6-12)	**AC** (13-18)
4-year-olds	4	7	1
5-year-olds	4	8	0
6-year-olds	2	4	6
Total	**10**	**19**	**7**

Below-chance performance indicates that children were analyzing the SC sentences with a benefactive in a consistent, non-adult way; specifically, their performance is consistent with an analysis in which the embedded subject (PRO) is controlled by the object, as for *tell*/*ordenar*. Response justifications from children performing below chance on the SC conditions support the hypothesis that they analyzed *tell*/*ordenar,* and *promise*/*prometer* and *swear*/*jurar* followed by a benefactive identically, i.e. as object control. Examples for their similar pattern of responses and justifications are given in (8) for English and (9) for Spanish.

(8) a. Puppet: *The wizard tells the knight to defend the castle!* [T]
BC Child: *True […] 'cause he needs to fight some bad guys!* [T]
Exp: *who?*
BC Child: *the knight!*

b. Puppet: *The cowboy promises the farmer to feed the horse!* [F]
BC Child: *True […] I think the cowboy talks to the farmer and he* [points to the farmer] *needs to feed the horse.* [T]
Exp: *Uh-huh. So you think the cowboy promises the farmer to feed the horse, or the farmer promises the cowboy to feed the horse?*
BC Child: *The cowboy promises… The cowboy promises the farmer to feed the horse.*

(9) a. Puppet: *¡El bombero ordena al policía subir la escalera!* [F]
'The firefighter tells the policeman to climb up the ladder!'
BC Child: *No, el policía ordena al bombero subir la escalera* [F]
'No, the policeman tells the firefighter to climb up the ladder.'

b. Puppet: *¡El bombero jura al policía subir la escalera!* [T]
'The firefighter swears to the policeman to climb up the ladder!'
BC Child: *No, el policía jura al bombero.* [F]
'No, the policeman swears to the firefighter.'

Response justifications from children performing above chance on the overt benefactive conditions show that they assign subject control to the subject of the embedded clause, and not object control. Responses from 'above chance' children are given in (10) for English, and (11) for Spanish.

(10) Puppet: *The wizard promises the knight to defend the castle!* [F]
AC Child: *No, the knight promises the wizard!* [F]

(11) Puppet: *¡El mago jura al caballero defender el castillo!* [F]
'The wizard swears to the knight to defend the castle!'
AC Child: *No, se lo jura el caballero al mago* [F]
'No, the knight swears it to him, to the wizard.'

As for the justifications from the children that performed at chance, they may again suggest that the majority of them were not simply providing a 'true' or 'false' response at random. These children were sometimes incorrectly assigned object control, and at other times they answered correctly and gave response justifications that were compatible with subject control. The examples in (12) were given by the same English-speaking child. In (12a), her response justification suggests that she has incorrectly assigned subject control. However, she is able to correctly respond 'true' to the trial in (12b) and further explains that the one who ended up climbing the ladder is the firefighter, not the policeman with the scarf, and so, it must be the firefighter who made the promise.

(12) 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]

Similarly, the examples in (13) are from the same Spanish-speaking child. In (13b), she incorrectly responds 'true' and repeats the incorrect target sentence, albeit with the (grammatical) addition of a dative clitic preceding the verb *prometer*. This suggests that the child was not simply repeating the sentence, but actually filtered it through her own grammar, but

mistakenly established control with the closest antecedent, the benefactive, instead of the matrix subject. In (13a), however, she responded correctly to a SC sentence with *promise*. Notice she produced several disfluencies and repairs, which is a hallmark of processing difficulties (see Levelt, 1983, 1989; Oviatt 1995; Shriberg 1996).

(13) a. Puppet: *¡La cocinera promete a la camarera cerrar la puerta!* [F]
'The cook promises the waitress to lock the door!'

C Child: [nods head, i.e. 'yes'] *[...] La cocinera le promete a la camarera cerrar la puerta.* [T]
'The cook promises to her, the waitress to lock the door.'

b. Puppet: *¡La camarera promete a la cocinera con el delantal cerrar la puerta!* [T]
'The waitress promises the cook with the apron to lock the door!'

C Child: *La camarera le ha dicho… ¡qué va! La cocinera le ha prom… ¡qué va! La camarera le ha prometido a la cocinera que va a cerrar la puerta. ¡Es que no me salía!* [T]
'The waitress has said… no way! The cook has prom… no way! The waitress has promised the cook that she'll lock the door. It wouldn't come out!'

Patterns of mixed control have also been observed in Chomsky (1969), and Hsu et al. (1985). In those studies, they found that an object-oriented stage was followed by a transitional stage. Hsu et al. argued that children with 'mixed subject-object grammars' represent a transitional stage whereby they variably attach the nonfinite clause at different levels, explaining the mixed patterns of object control in both *promise* and adjunct control sentences.[8] Instead, we

[8] As we noted for the StSR study, an alternative explanation of these mixed responses might be that children that performed at chance are merely 'noisy' responders. That is, they may be less attentive or they may have trouble understanding the task instructions. However, all of the 'chance' children correctly responded to at least 5/6 trials for

will again interpret this "transitional period" as one in which the child has grammatical mastery of control, but lacks the processing capacity to apply the adult rule system consistently.

4.1.9. The Dual Source Intervention hypothesis and SC

Given the evidence discussed in the previous section, we again hypothesize that children may not form a homogenous group. Rather, for one group, the one made up of children that performed below chance, the problem is grammar-based, i.e. these children lack the grammatical means to by-pass the intervening benefactive. For the other group, the one made up of 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. We labeled this premise the Dual Source Intervention hypothesis.

To test this hypothesis with respect the SC results, we again excluded from the correlation analysis the English-speaking children that performed below chance in the SC with 'short and long benefactive' conditions, i.e. those that scored 5 or less out of the 18 SC benefactive trials (N = 11) and calculated the Spearman's rank order correlation coefficient. Strikingly, we again observe that the correlation between the performance on the SC with a benefactive conditions and the CLPT score then becomes significant, $r_s\,(17) = .583, p = .009$. This is further illustrated in Figure 4.6, which is equivalent to Figure 4.4 except that it does not include the below-chance children. On the other hand, and as expected, among the children that

---

OC *tell*, and SC *promise* and *swear* without an overt benefactive, as well as for the copula and unraised conditions in the StSR experiment. Thus, lack of understanding or attention cannot explain the fact that these children show correct responses on several construction types.

performed below chance with SC on *promise/swear* with a benefactive, i.e. at 5/18 or below (N = 11), we found no significant correlation between SC and CLPT scores, $r_s(9) = .23$, $p = .49$.

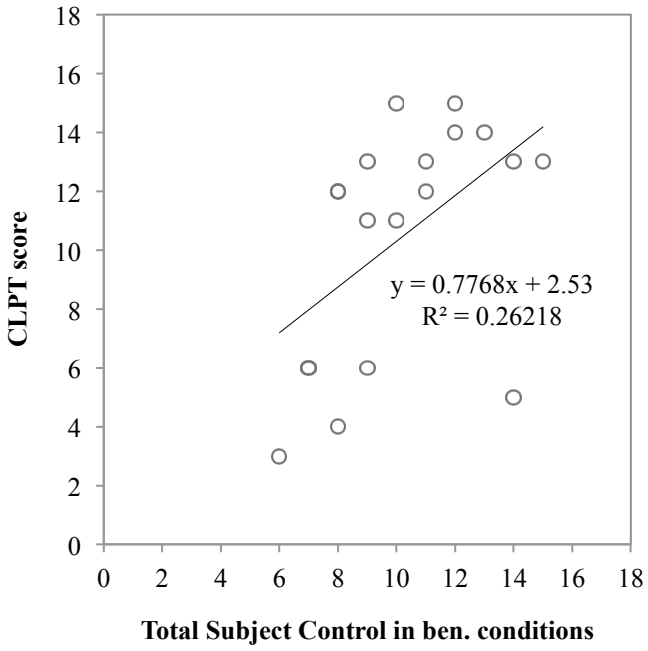


Figure 4.6. Correlation between the score obtained in the English subject control conditions that included a benefactive and the score obtained in the CLP task in the chance and above chance children (sample of 19 children).

Turning to the digit span task, we found no correlation between the SC with a benefactive conditions in the 'chance' and 'above chance' children and their digit span scores, regardless of the scoring method used in the digit span task, $r_s(17) = .059$, $p = .812$ for the scaled scores, and $r_s(17) = .016$, $p = .95$ for the itemized scores.

In a multiple regression model, we included the score of the SC with a benefactive conditions as the dependent variable, and CLPT score, digit span, and chronological age as independent variables. Even though the whole model was not able to account for a significant amount of the variability, $F(3, 15) = 1.97$, $p = .1$, $R^2 = .293$, perhaps due to the small dataset,

only CLPT score added statistically significantly to the prediction, $p = .029$. Neither digit span, $p = .53$, nor chronological age $p = .98$ were statistically significant.

Thus, processing, but not short-term-memory capacity seems to predict the performance of the 'chance' and 'above chance' children in the conditions that entail SC over an intervening benefactive. Our hypothesis is strengthened when we compare the performance of these children on the 'short' and 'long benefactive' conditions. A Wilcoxon signed-rank test showed a significant difference between the two conditions ($Z = -1.98, p = .047$), suggesting that, as hypothesized, 'chance' and 'above chance' children may have the appropriate grammatical representation, but still show difficulties when they try to process these constructions, particularly when the intervening material is long.

We followed the same procedure for the Spanish-speaking group. We excluded from the correlation analysis the children that performed below chance in the SC with a benefactive conditions, i.e. those that scored 5 or less out of the 18 trials ($N = 10$) and again calculated the Spearman's rank order correlation coefficient. Once again, and as predicted by our hypothesis, the correlation between the performance on the benefactive conditions and the CLPT score then becomes significant, $r_s\,(24) = .49, p = .01$. This is further illustrated in Figure 4.7, which is equivalent to Figure 4.5 except that it does not include the below-chance children. On the other hand, the correlation between the performance on the benefactive conditions and the CLPT score among the children that performed below chance was not significant, $r_s\,(8) = -.23, p = .53$.

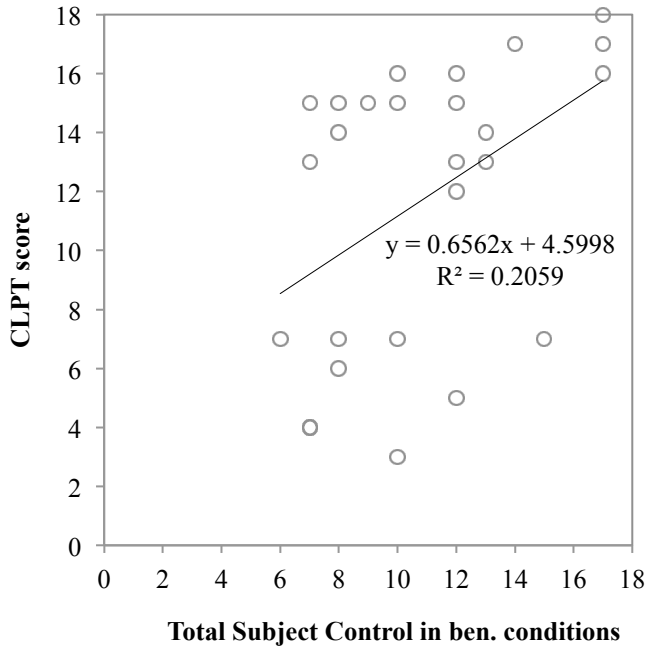


Figure 4.7. Correlation between the score obtained in the Spanish subject control in the conditions that included a benefactive and the score obtained in the CLP task in the chance and above chance children (sample of 26 children).

As for English, we found no correlation between the performance on the SC with a benefactive conditions in the 'chance' and 'above chance' children and their digit span scores, regardless of the scoring method used in the digit span task, $r_s\,(24) = .312$, $p = .12$ for the scaled scores, and $r_s\,(24) = .309$, $p = .125$ for the itemized scores.

In a multiple regression model, we included the score of the SC with a benefactive conditions as the dependent variable, and CLPT score, digit span, and chronological age as independent variables. The whole model was able to account for a significant amount of the variability, $F(3, 22) = 5.79$, $p = .004$, $R^2 = .44$. In this particular case, we found chronological

age to be a significant predictor, p = .02. CLPT score, however, was not, p = .145.[9] Digit span was not a significant predictor either, p = .387.

Our hypothesis is again reinforced when we compare the performance of the chance and above chance children on the short and long benefactive conditions. A Wilcoxon signed-rank test was conducted and we found a significant difference between the two conditions (Z = -2.173, p = .03), suggesting that 'chance' and 'above chance' children do display even greater processing difficulties when there is a longer intervening argument.

While the sample size may be too reduced to draw strong conclusions from the positive correlation found with the CLPT, the strikingly similar results from the English- and Spanish-speaking children seem to suggest that we are in fact dealing with a heterogeneous group of children. On the one hand, there are children who have a non-adult grammar that has no mechanism to circumvent the intervening object. Therefore, these children will consistently fail to assign subject control, and will establish object control instead, regardless of their processing capacity. On the other hand, there are children who seem to have access to the grammatical rule system that allows for subject control over an intervening argument, but they still do not have the processing wherewithal to effectively comprehend SC with an intervening argument. In this case, children will be as successful in the SC conditions as their processing capacity allows.

Before we turn to our second part of this study, which addresses the hypotheses that predict children's difficulties with StSR and SC are due to the same underlying problem, we will discuss some questions that emerged in this study.

[9] This result may be due to the high correlation between CLPT scores and age in the Spanish-speaking children, r_s (36) = .54, $p < 0.001$.

4.1.10. Unsolved issues: On the syntactic status of the implicit benefactive

At this point, we would like to discuss the possibility of an implicit benefactive in the subject control sentences that had no overt benefactive. While our experiment was not designed to investigate the syntactic status of an implicit benefactive, we believe it is important to address this question. Recall that in the StSR study, English-speaking children performed poorly in the StSR covert experiencer condition, i.e. '*The dog seems to be grey*', but Spanish-speaking children performed well on superficially equivalent sentences, i.e. with bare *parecer* (no experiencer), '*El perro parece ser gris*', presumably because English *seem* always selects for an experiencer argument, while Spanish bare *parecer* does not. We concluded from this that children exhibit intervention effects even when the structurally represented intervening argument is not overtly expressed. However, in this study, we found that a vast majority of children did well with subject control when the benefactive was not expressed, i.e. '*The knight promises to defend the castle*'. This could be due to one of two reasons: i) the benefactive of *promise*-type verbs is not syntactically projected when it is not overtly realized (but the experiencer of English *seem* is); ii) the children in our experiment were aware that *promise* takes a benefactive argument and they did experience difficulties establishing control across this intervening argument, but they used a compensatory strategy that resulted in seemingly adult-like behavior.

Let us begin by examining the first hypothesis. Where and how implicit arguments are represented is an open, hotly debated question: they could be completely abstract entities, pragmatically inferred in specific situations; argument positions, or θ-roles, in argument structures (θ-grids); or *bona fide* null syntactic categories, pronominal or variable-like in nature (see Epstein, 1984; Williams, 1985, Rizzi, 1986, Chomsky, 1986, Roeper, 1987; Brody & Manzini, 1987; Jackendoff, 1987; Safir, 1991; Bhatt & Pancheva, 2006; Landau 2010, *inter*

alia). While the status of the implicit benefactive of *promise* has not been researched in much detail, there is some evidence from Principle C effects that it may be structurally present, as shown in Williams' (1985: 306) example in (14). However, there is also evidence that may cast doubt on this assumption. Five native English speakers were informally asked whether the implicit benefactives in (15) could be interpreted as Mary, Lady Catelyn, and Abraham respectively, they all judged this reading as grammatical without reservations.

(14) Mary went to the doctor's office, and she promised {__$_i$/him$_k$} that the doctor$_{*i/*k}$ would not see her again until she was really sick.
[implicit/explicit benefactive of *promise* ≠ the doctor]

(15) a. In his wedding vows, John promised {__$_i$/her$_k$} to be good to Mary$_{i/*k}$.
[implicit benefactive of *promise* = Mary, explicit benefactive of promise ≠ Mary]

b. Brienne promised {__$_i$/her$_k$} to bring Lady Catelyn$_{i/*k}$'s daughters back to Winterfell.
[implicit benefactive of *promise* = Lady Catelyn, explicit benefactive of promise ≠ Lady Catelyn]

c. God promised {__$_i$/him$_k$} to bless the prophet$_{i/*k}$ and make his descendants as numerous as the stars in the sky.
[implicit benefactive of *promise* = the prophet, explicit benefactive of promise ≠ the prophet]

Given the weak syntactic evidence that would support the structural presence of the covert benefactive, we cannot rule out the possibility that for adults and children alike, the benefactive argument of *promise* is syntactically represented only when it is overtly expressed,

as suggested by the examples in (15). This would explain why most children were able to score at least 5/6 in the SC conditions that had no benefactive, i.e. because there was no syntactically represented intervener, as in the case of bare *parecer*.

On the other hand it may be that the benefactive is syntactically projected even when covert. If that were the case, we would need to account for children's success with SC *promise* and not with StSR *seem* with a covert intervener. It is possible that a methodological flaw in the SC study may have resulted in apparent adult-like behavior with *promise* in the 'no benefactive' condition. Recall that in the StSR experiment, 'below chance' children consistently ignored *seem* or assigned it a copula interpretation as a compensatory strategy for not being capable of circumventing the covert experiencer. In our SC experiments we depicted the promise event by showing the promisor and the benefactive doing a "pinky promise" and then having the promisor fulfill the promise (see Figure 4.1). That means that if some children were ignoring the predicate *promise* or giving it a different interpretation (e.g. *decide*), they could still have given an adult-like response. For instance, given the scenario where the firefighter promises to climb the ladder (and then does so), and the sentence '*The firefighter promises to climb the ladder*', immature children may have correctly answered 'true' by pairing the subject of the matrix clause, '*the firefighter*', with the predicate of the embedded clause, '*to climb the ladder*', and not by virtue of deriving SC over a covert benefactive in an adult-like manner (assuming there is a covert benefactive). In other words, the methodology used in the SC experiment does not allow us to rule out the possibility that children may also have problems correctly interpreting SC *promise*/*prometer* sentences even when they do not contain an overt benefactive.

The methodological problem notwithstanding, we have multiple reasons to believe the children in our experiment understood the lexical and argumental properties of *promise*, and that

the reason for their non-adult-like comprehension of subject control was in fact due to the difficulty of establishing control over an overt (or covert) benefactive. First, we ensured that each child understood the meaning of *promise*/*prometer* and *swear*/*jurar* when they encountered a *promise*/*prometer* or *swear*/*jurar* trial for the first time. Even though the vast majority of the children knew what it meant, the experimenter gave a definition: '*it's when you say you are going to do something and you really mean it*' in every case. Secondly, many of the children's response justifications in the benefactive conditions included the verb *promise* or some sort of communicative verb, as observed in the examples (8b), (9b), (10), (11), or (13), suggesting they were not consistently ignoring it. Lastly, in a search of the entire set of American English corpora on CHILDES (as of 2016), we found that adults produced 197 utterances with *promise*, and children produced 76. This number exceeds the number of utterances containing verbs that most researchers (and parents) would judge young children to know, including *shout*, *clap*, *crawl*, *repeat*, *sneeze*, *fill*, *add*, *swallow*, *splash*, or *waste* (Li, 2001)[10]. The youngest age at which *promise* was produced was 1;11.23 ('*I think you promised her*', Nelson corpus, 829296.cha file, line 71), and while the child who uttered the sentence was clearly precocious, we found multiple instances of the verb *promise* in the speech of two- and three-year-olds. The assumption that children aged four and above know the meaning of *promise* is also maintained by Sherman and Lust (1993). In the interview part of their control experiment they asked children to explain what a promise is and if they had ever made a promise to their parents or siblings. They found that even the youngest children they tested (3-year-olds) understood the basic meaning of the verb '*promise*' and were aware of the goal argument in the lexical semantic structure of the verb, namely, the existence of a goal of the '*promise*'.

[10] This frequency list does not distinguish verb forms from noun forms.

We will thus assume, as in C. Chomsky (1969), Hsu, et al. (1985), and Sherman and Lust (1993) that young children know what a promise is, and that they are able to use and correctly interpret sentences containing 'promise' in other syntactic environments that do not entail a control relationship (over a benefactive), e.g. cases in which '*promise*' is followed by a finite clause. However, when immature children encounter SC '*promise*' they may use different compensatory heuristics to interpret the sentence in real-time. When '*promise*' does not appear with an overt benefactive, and assuming there is one structurally represented, they may either ignore the verb, or assign it a meaning similar to other subject control verbs, such as '*decide*', e.g. '*The firefighter decides to climb up the ladder*'. As we have just mentioned, because in the subject control scenarios the promisor did fulfill his promise, children may have easily used this strategy when the benefactive was not overtly expressed, and obtained a good score in this condition, even if they were not correctly assigning SC *promise* an adult grammatical structure, (i.e. potentially with a syntactically represented covert benefactive).

On the other hand, when these immature children hear a sentence with SC *promise* followed by a benefactive, they may assign it a meaning closer to object control *tell*, i.e. '*The firefighter tells the policeman to climb up the ladder*'. Under this analysis, we would assume that initially, children always exhibit problems when establishing a subject control dependency across an overt or covert intervening argument, and that due to their inability to circumvent the intervener they assign a non-adult interpretation to these structures.

This would be reminiscent to the case of StSR. As has been previously reported (Hirsch & Wexler, 2007b; Hirsch, Orfitelli, & Wexler, 2007; Hirsch 2011; Orfitelli, 2012), and as we found in our own English StSR experiment, when faced with raising sentences with a covert experiencer, immature children tend to ignore the element of the sentence that causes them

difficulty, the StSR predicate, and assign the sentence a copular interpretation. That is, given the scenario where the white dog goes under the grey light appearing to be grey, and the sentence '*The dog seems to be grey*', some children interpret that that dog *is* grey, and thus incorrectly answer 'false'. On the other hand, some of these same studies (Hirsch & Wexler, 2007b; Hirsch, Orfitelli, & Wexler, 2007; and Hirsch 2011) have shown that when the StSR utterance contains an overt experiencer, English-speaking children that have not mastered StSR may interpret *seem* as *think* as a compensatory strategy. That is, given the sentence '*The dog seems to the cat to be grey*' these children would interpret it as '*The dog thinks that the cat is grey*'.[11] This implies that while children show an adult comprehension of *seem* in unraised contexts, they may use different compensatory strategies when attempting to interpret StSR *seem* in covert and overt experiencer sentences. In the former case, they assign *seem* a copular interpretation, while in the latter case they may assign it a *think* interpretation.

Our SC study was not designed to test for children's use of interpretive strategies. We did not have control items like the copula condition for the StSR studies, and so we cannot be certain that children were adopting any particular strategy to understand sentences with *promise* without a benefactive because of a failure to compute the SC relationship. Therefore, our interpretation of the results with respect to the syntactic status of the covert benefactive is merely speculative. Nevertheless, what is clear and crucial to us is that children show intervention effects with SC *promise*/*prometer* when the benefactive is overtly produced, despite their adult comprehension of the meaning of *promise*/*prometer*. In this respect, our results replicate previous findings (Chomsky, 1969; Hsu et al. 1985, Sherman & Lust, 1993).

[11] This was also the case for a few of the children we tested in the StSR study (see Sections 3.3.3 for English and 3.3.4 for Spanish), although the majority of children seemed to apply the copula strategy in these sentences too and ignore the experiencer phrase all together.

4.2. Part II: Subject-to-subject raising and subject control

A key question that remains is whether children's individual performance on subject-to-subject raising with *seem*/opinion *parecer* and subject control with *promise*/*prometer* is positively correlated, as predicted by theories that relate the adult structures of StSR with *seem*-type verbs and SC with *promise*-type verbs. According to proponents of the Movement Theory of Control these constructions are both derived by A-movement (Boeckx & Hornstein 2003; Boeckx, Hornstein, & Nunes, 2010; Hornstein 1999, *et seq.*; Hornstein & Polinsky, 2010; Manzini & Roussou 2000; O'Neill, 1995). Similarly, Belletti and Rizzi (2013) argue that both StSR *seem* and SC *promise* are derived via smuggling. Therefore, these two accounts predict a correlation between individual performance on StSR and SC.

Finally, processing-based accounts such as the Performance-based Intervention Effects hypothesis (PIE; Choe 2012; Choe & Deen, 2016) claim that any structure in which a DP intervenes between the filler and the gap will cause difficulties in children (and adults). These would not only include StSR with *seem*-type predicates and SC with *promise*-type verbs, but also passives, *tough*-constructions, object relatives, object topicalizations, and object *wh*-questions. If processing limitations are the *only* factor hindering the proper functioning of an adult-like grammar system then we expect to find a correlation between individual children's performance on StSR *seem* and SC *promise* and processing capacity (as measured by the CLPT) in *all* children. As discussed in Sections 3.3.9 and 4.1.9, we found that the children in our study do not constitute to a homogeneous class. For some children, those that performed below chance in the StSR and SC conditions that included in intervening argument, the problem seemed to be purely grammatical. They did not perform significantly different in the 'short' and 'long experiencer/benefactive' conditions, nor did they show a correlation between performance on

these constructions and processing capacity. However, those that performed at chance and above did behave as predicted by processing accounts, showing an effect of length of intervener and also a relationship with the verbal processing capacity task scores. Thus, we attribute the poor performance of the 'at- and above-chance' children on both StSr and SC to non-grammatical, processing factors. We therefore predict that the results for individual children on these two structures may be correlated. We will examine this hypothesis after discussing the below-chance children.

For the below-chance children, there are two possibilities: i) the grammatical constraint behind the delay with StSR *seem*/opinion *parecer* is the same as the one for SC *promise*/*prometer* (e.g. no access to smuggling operations) 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 constraint behind the delay with StSR *seem*/opinion *parecer* is indeed not the same (i.e. the two constructions are not derived in the same way), in which case, the children that performed below chance with StSR may not be the same children that performed below chance with SC.

In order to investigate this question, we will compare the results of the StSR and SC truth-value judgment tasks from both the English- and Spanish-speaking children. Note that ideally this question should be answered by comparing the scores of the children that performed below chance in either of these conditions. Unfortunately, this group constitutes a very small data sample for statistical purposes. Therefore, we will discuss the results individually and then proceed to calculate the correlation including all children.

The crucial idea is that if StSR and SC are difficult to acquire due to the *same* grammatical constraint, then we should expect children to perform rather similarly in the two

conditions. That is, if a child performs below chance in one construction, he should also perform below chance in the other construction, since he lacks the common grammatical means to by-pass the intervener. Conversely, if the child performs at chance or above in one construction, he should also perform approximately at chance or above in the other, since he has the adult grammatical system to derive both constructions, but may still experience difficulties (with both constructions) due to his immature processing system.

Regarding the group results, if we find a significant correlation when we include all children it could be due to the effect of the group of children that perform at chance and above, since the problem for these children with both StSR and SC is mainly processing-based, as discussed above. We would therefore have to interpret this result with caution. On the other hand, if we do not find an overall correlation between children's performance on StSR and SC, it may indicate that the children who perform below chance in both conditions are in fact children with very different grammatical deficiencies.

4.2.1. StSR and SC in the English-speaking group

Let us first investigate this question in English. We started by comparing children's performance on the 'StSR *seem* with a short experiencer' condition and the 'SC *swear* with a benefactive' condition, these two being the most equivalent, since both have an intervening argument headed by a preposition. If children's difficulty with these two constructions is due to the same constraint 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 the grammatical constraints blocking good performance in the two constructions are different. Therfore, the predictions of the Null Preposition Hypothesis and the Movement Theory of Control are not borne out in our study (Boeckx & Hornstein, 2003, Boeckx, Hornstein, & Nunes, 2010, and Hornstein & Polinsky, 2010).[12]

Secondly, we compared children's performance on StSR with *seem* in the 'short' and 'long experiencer' conditions, and SC with *promise* in the 'short' and 'long benefactive' conditions. Out of the 11 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 eight performed either below chance with StSR and above chance with SC (n = 1), below chance with StSR and at chance with SC (n = 2), 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 the StSR conditions but poorly in the SC conditions, and vice-versa. These results thus constitute evidence against the predictions of Belletti and Rizzi (2013), who claim that children's delays with StSR and SC stem from the same difficulty with smuggling operations. If that were the case, we would expect the two constructions to be acquired at roughly the same time in each individual child. However, the vast differences in performance in some of these children suggest otherwise.

We additionally conducted a second Spearman's rho test comparing all 30 children's

[12] Recall also that the NPH predicted a significant improvement in performance in the *swear* condition as opposed to the *promise* condition and yet that was found not to be the case (see Section 4.1.3).

performance in these two constructions. The results indicate that there is no significant correlation between the two constructions, $r_s\,(28) = .225, p = .232$, reinforcing the individual results just discussed. The data is illustrated in Figure 4.8. The correlation remains non-significant even when we include the data from the StSR without an overt experiencer condition and the SC with *swear* and a short benefactive condition, $r_s\,(28) = .245, p = .191$. This result reinforces the idea that the grammatical difficulties observed with StSR are not attributable to the same grammatical difficulties observed with SC.

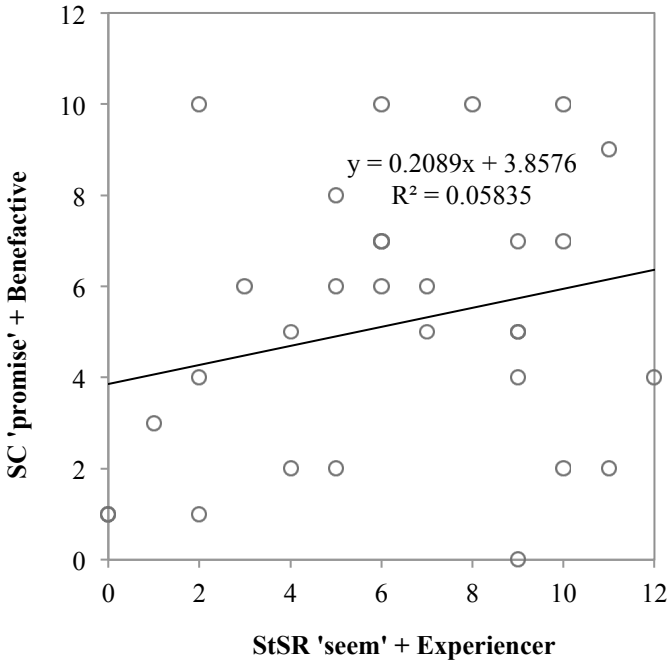


Figure 4.8. Correlation between the score obtained in the English StSR conditions with an experiencer and the score obtained in the SC conditions with a benefactive (sample of 30 children).

Returning to the children that perform at chance and above with StSR and SC, our hypothesis is that their difficulty is due processing limitations. If a child's processing capacity

enables him to comprehend StSR most of the time, we should also expect him to comprehend SC most of the time. In other words, we predict that there should be a correlation in the performance of 'at- and-above chance' children in both experiments due to the processing effects that we observe in this group. A Spearman rho test of partial correlation (Conover, 1999) was run to determine the relationship between the StSR and SC scores in the at- and above-chance children whilst controlling for processing capacity as measured by the CLPT. Zero-order correlations showed that there was a significant, positive correlation between StSR and SC performance in this group, $r_s(16) = .62$, $p = .007$). However, there was no significant correlation between StSR and SC performance when controlling for CLPT scores, $r_s(15) = .46$, $p = .07$.

These results support our hypothesis that processing capacity is the principle determinant of performance in both the StSR and SC constructions in the at- and above-chance children, as we concluded in Chapter 3 and in this chapter, Section 4.1.9. However, because as a whole, StSR and SC performance are not correlated, this suggests that the grammatical deficiencies of the children that performed below chance in the StSR experiment, and the grammatical deficiencies of those that performed below chance in the SC experiment are unlikely to be of the exact same nature.

4.2.2. StSR and SC in the Spanish-speaking group

Let us now turn to the Spanish data. We will again start comparing the individual results obtained in the StSR opinion *parecer* conditions with a 'short' and 'long experiencer', and in the SC with *prometer* conditions with a 'short' and 'long benefactive'. Out of the 10 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 two children performed below chance in both conditions.

The other eight performed either below chance with StSR and at chance with SC (n = 2), at chance with StSR and below chance with SC (n = 1), or above chance with StSR and below chance with SC (n = 5). That is, as in the English-speaking group, there were children who performed well with the StSR conditions but poorly in the SC conditions, and vice-versa. These results reinforce the idea that children's delays with StSR and SC may not be due to the same grammatical difficulty.

We additionally conducted a Spearman's rho test comparing the performance of all 36 children with StSR with opinion *parecer* in the 'short' and 'long experiencer' conditions, and SC with *prometer* in the 'short' and 'long benefactive' conditions. The results indicate that there is no significant correlation between the two constructions, $r_s\,(34) = .246, p = .149$, there were children who obtained high scores in the StSR experiment and low scores in the SC experiment, and vice-versa. The correlation is illustrated in Figure 4.9.

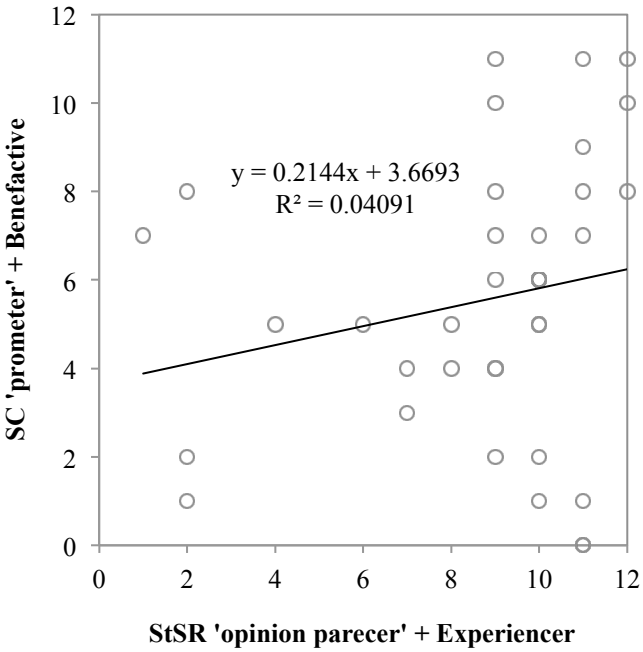


Figure 4.9. Correlation between the score obtained in the Spanish StSR conditions with an experiencer and the score obtained in the SC conditions with a benefactive (sample of 36 children).

If we again consider the hypothesis that only the below-chance children have a non-adult grammatical rule-system for that particular construction, while the at- and above-chance children are hindered by processing limitations, we again expect a correlation in the performance of at- and above-chance children in both experiments due to the processing effects that we observe in these groups. We ran a Spearman rho test of partial correlation to determine the relationship between the StSR and SC scores in these children (N = 26) whilst controlling for processing capacity as measured by the CLPT. Zero-order correlations showed that there was a significant, positive correlation between StSR and SC performance in this group, $r_s(24) = .657$, $p = .001$. However, there was no significant correlation between StSR and SC performance when controlling for CLPT scores, $r_s(23) = .52$, $p = .08$. That is, as in the case of the English-speaking

group, processing imitations seem to be the main reason why the 'chance' and 'above chance' children perform poorly with both StSR opinion *parecer* and SC *prometer*. However, individual and group results indicate there is no clear correspondence between children's performance on these two constructions, which suggests that whichever grammatical operations children must use to circumvent the intervening argument are not entirely overlapping, contra the predictions of the MTC (Hornstein 1999, 2001; Manzini & Roussou 2000; O'Neil 1995, *inter alia*) and Belletti and Rizzi (2013).

4.2.3. Discussion

The results presented in the previous two sections show that the group of children that performed below chance in the StSR experiment and the group that performed below chance in the SC experiment do not completely overlap. This result strongly suggest that the grammatical deficit of the children that performed below chance in the StSR experiment, and the grammatical deficit of those that performed below chance in the SC experiment cannot be *exactly* the same. Some children may have an adult-like representation of StSR but not SC and some vice-versa. In other words, there seems to be a disconnect between the *grammatical* development of StSR with *seem*-type verbs and SC with *promise*-type verbs, contrary to the predictions of Belletti and Rizzi (2013) and Hornstein and colleagues (Boeckx & Hornstein, 2003; Boeckx, Hornstein, & Nunes, 2010; Hornstein & Polinsky, 2010). What then is the mechanism that gives children trouble in these two constructions? Though our study was not designed to specifically answer this question in what follows we offer some speculation.

One source of difficulty that would make StSR with *seem* uniquely problematic for children as compared to SC *promise* was discussed in Section 2.1.1.3. Snyder and Hyams (2015)

have recently proposed that StSR structures with *seem* undergo both smuggling and semantic coercion in the same way it has been claimed for non-actional passives (Grillo, 2008; Gehrke & Grillo, 2008, 2010). According to Gehrke and Grillo, a stative verb can only be passivized if it can be semantically coerced into a consequent state. This type-shifting rule presumably gives it a related, eventive meaning, which allows for smuggling and for passivization to take place.

Snyder and Hyams claim that children are able to smuggle eventive shells from approximately age four but they cannot coerce psych-verbs into their eventive counterpart until approximately age six, hence children's ability to comprehend actional passives, but not non-actional passives (with or without a *by*-phrase). Therefore, children's main difficulty between the age of four and six would not be in applying smuggling, but with semantic coercion. Because *seem* is a stative verb, Snyder and Hyams argue that the same mechanism is involved in StSR, i.e. semantic coercion takes place and the eventive VP smuggles the subject across the intervening experiencer. This hypothesis would also account for the perfect one-to-one correspondence found between performance with non-actional passives and StSR with *seem*-type predicates in English-speaking children (Hirsch & Wexler, 2007b; Orfitelli, 2012).

This factor alone, however, could not have lead to the uncorrelated results we observe between StSR and SC. If we assume both share the same structure and are derived similarly, e.g. via smuggling or by A-movement, and that children's problems with StSR *seem* are due to their difficulties with semantic coercion, we should expect a subset-superset pattern of results. That is, we should find that children that have mastered StSR with *seem,* which involves smuggling and semantic coercion, should have also mastered SC with *promise*, since purportedly SC with *promise* only involves smuggling (and/or A-movement), but not semantic coercion. As we show in the results discussed in Section 4.2.1 and 4.2.2, this is not the case, some children did perform

better on StSR than on SC.

Our experiment was only designed to test the analyses of *promise* discussed in Chapter 2. However, whichever analysis of *promise* one adopts, it should incorporate an operation that makes the derivation of SC *promise/prometer* (and *swear*/*jurar*) uniquely problematic for children up to the age of six. Table 4.6 offers a summary of the findings of the SC experiment that the right analysis should be able to account for.

Table 4.6. Summary of findings from the SC study and theoretical implications regarding children's difficulties with SC *promise*.

The fact that…	**implies that children's difficulties with SC *promise* is…**
children perform well with object control…	unrelated to establishing control.
children perform equally poorly on English *promise* and *swear;* and poorly on Spanish *prometer* and *jurar*…	unrelated to the (lack of) the preposition *to* heading the intervening benefactive.
there is a below-chance group who consistently chooses the benefactive as the controller of PRO…	related to the presence of the intervening argument.
there is a below-chance group whose performance is not predicted by processing capacity (as measured by the CLPT)…	related to the grammatical operation that derives SC in *promise*-type verbs.
there is no overlap between the below-chance group in the StSR study and the below-chance group in the SC study…	unrelated to difficulties with A-movement across an intervening argument; and unrelated to the operation of smuggling

At this point we would like to briefly discuss a recent proposal by Landau (2015) and speculate about how this analysis may be compatible with our experimental results. For four decades, two research traditions on control have proceeded mostly disconnected and in parallel:

one in formal semantics (e.g. Chierchia, 1989; Culicover & Jackendoff, 2001; Farkas, 1988; Jackendoff & Culicover, 2003; Sag & Pollard, 1991), the other in generative syntax (e.g. Boeckx & Hornstein, 2003, 2004; Hornstein, 1999; Landau, 2000; Larson, 1991; Williams, 1980) (see Landau [2013] for an overview of these accounts). Fundamental assumptions in the formal semantic treatments of control and fundamental assumptions in the syntactic treatments have often been mutually incompatible. For example, prevalent semantic explanations of the *de se* reading[13] of PRO in obligatory control cannot explain the basic fact that PRO displays grammatical agreement with the controller, a syntactic fact. Conversely, prevalent syntactic explanations of the agreement fact cannot come to terms with the *de se* semantics. Landau's (2015) analysis of obligatory control is the first serious attempt to formulate a theory of control that lies on the syntax-semantic interface.

Concretely, Landau proposes that obligatory control is achieved either through *predicative* control or through *logophoric* control. Empirically, the latter applies in attitude complements[14] (i.e. complements of verbs such as *want*, *believe*, *hope*, *offer*, *promise*) the former

[13] *De se* refers to a belief that holds true of the individual's self-perception. Let us imagine a scenario in which Mary participates in a singing contest. A radio show is broadcasts the Mary's song prior to the announcement of who won. Listening to this rebroadcast, Mary does not recognize herself when she hears herself sing but she says: "this contestant is surely the best one. I hope she wins first prize". Consider the following reports of this situation:

i. Mary_i hopes [that she_i will win first prize]

ii. Mary_i hopes [PRO_i to win first prize]

(i) has a true reading under this scenario. This is known as a *de re*, non *de se* construal of the pronoun *she*. On the other hand, (ii) is clearly false in the given scenario. Mary did not express the hope: "I will win". This is described as an obligatory *de se* construal of the silent PRO subject of the infinitival complement.

[14] Attitude contexts are domains in which the denotation of the linguistic expression is determined relative to the epistemic or bouletic state of a participant in the reported situation, and not relative to the actual world. Given that specific descriptions may have different meanings for different people (i.e., pick different referents in their

in nonattitude complements (i.e. complements of verbs such as *manage, avoid, begin, be able, force*).

Predicative complements establish control via simple syntactic predication and does not require any specific ingredient. The two key components in predicative subject control constructions are a referential argument, i.e. the controller, and a predicate, i.e. the infinitive. Predicative object control constructions additionally have a Relator Phrase, RP, serving as the complement of the causative verb. The structure in (16) (from Landau, 2015:31) illustrates the derivation for predicative object control. In terms of agreement, PRO's [ϕ:] bundle is valued by the DP controller, *Mary*. This step could is the result of a direct Agree (*Mary*,PRO) operation.

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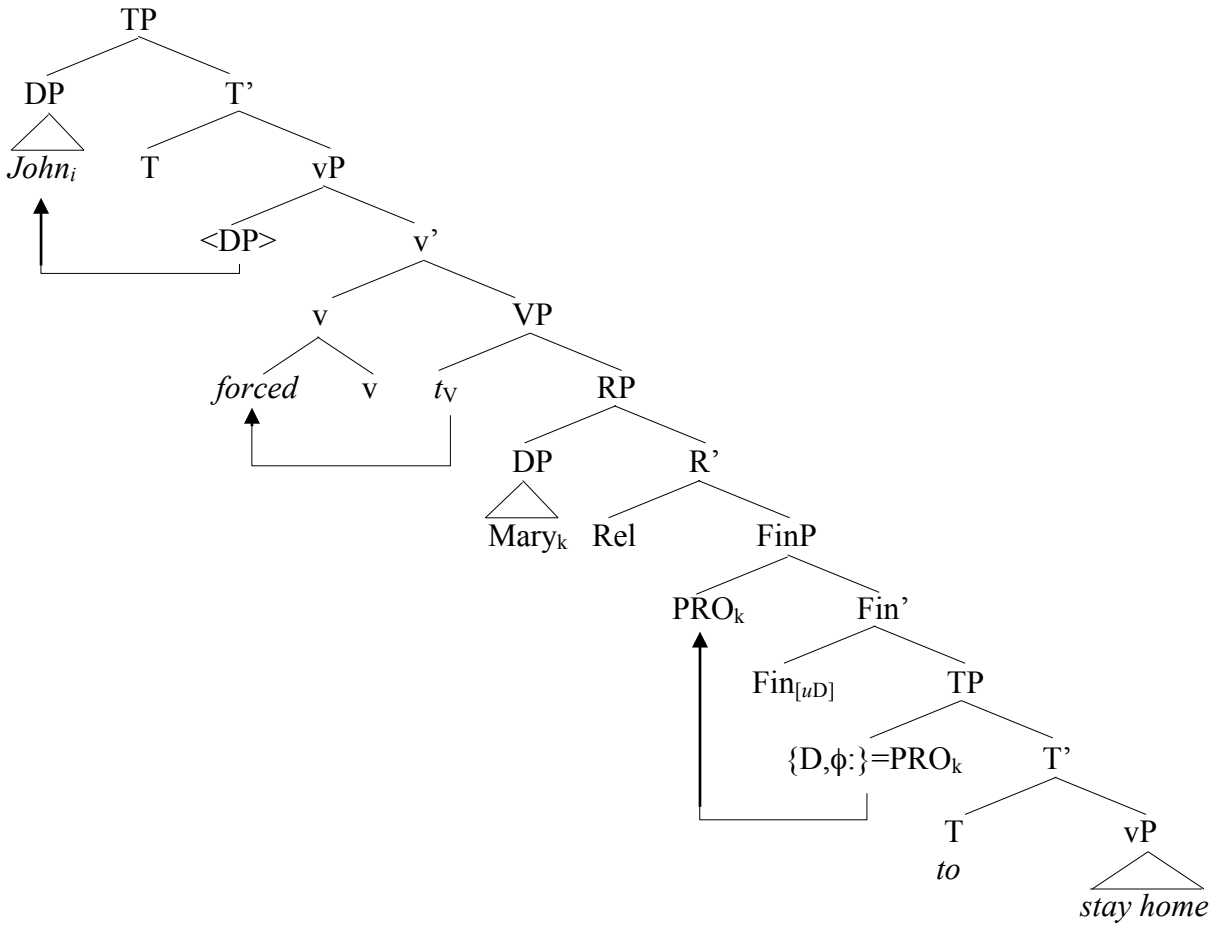
respective belief worlds), referential expressions are known to give rise to the *de re-de dicto* ambiguities. Thus, the presence of such an ambiguity is a standard test for attitude contexts.

(16)



Logophoric complements, on the other hand, establish control via predication and variable binding, the bound variable being a projected coordinate of the embedded context of evaluation. Specifically, attitude verbs select clausal complements whose left periphery represents the context of speech (or thought) of the matrix event. This context consists of the following coordinates: author, addressee, time, and world. This information resides in the C head of the clause and affects the interpretation of any context-sensitive element under the scope of C. What is special about this C is that it must *syntactically* project one of its individual coordinates. Landau labels the projected author coordinate as *pro*$_x$ and to the projected addressee coordinate

as pro_y, and this *pro*, which consists of the feature bundle [D, ϕ:], is the one in predicative relation with PRO.

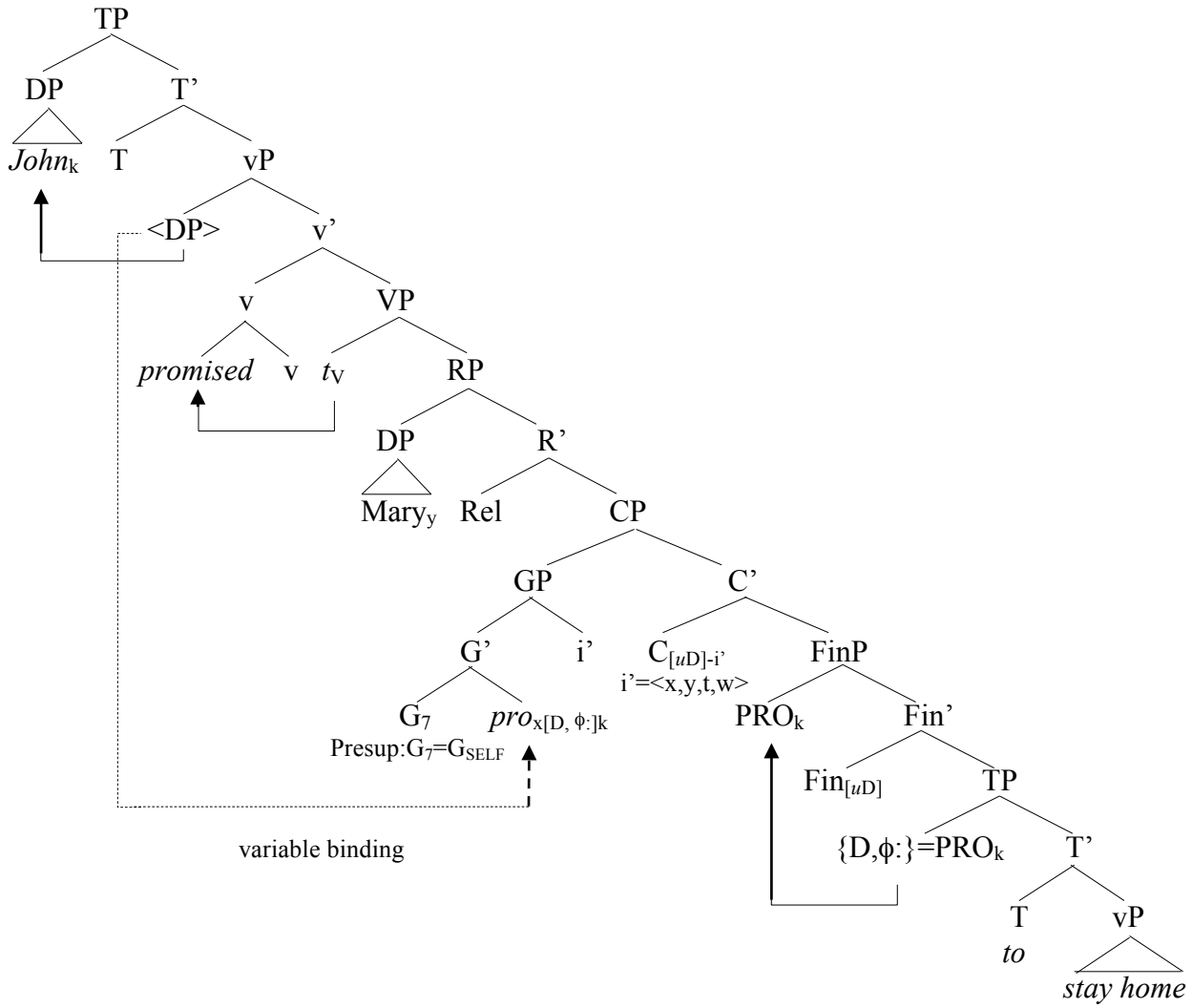
The characteristic *de se* presupposition is also induced by C via a concept generator,[15] G, which picks a specific coordinate of the context as the description of the individual that the attitude holder is acquainted with. That G is G_{SELF} if read *de se* (i.e. the attitude holder is known to himself as the author of his thoughts). The structure in (17) (based on Landau's tree in (54)) illustrates the derivation for *promise*, a logophoric subject control verb that has a goal or benefactive argument. A derivation of OC *tell* would have a pro_y coordinate bound by the addressee, thus obtaining object control.

[15] A concept generator is a one to one function between the individuals an attitude holder is acquainted with and the individual concepts the attitude holder has for those individuals. Consider the classic scenario in which Ralph sees a man in a trenchcoat and believes that the man is a spy. In this context, one can say:

(i) Ralph believes that Ortcutt is a spy.

In order to derive truth conditions accurately reflecting that Ralph does not *de dicto* believe Ortcutt is a spy, one can utilize concept generators to pick out the relevant acquaintance-based concept Ralph has for Ortcutt. Thus, we can posit that there is a concept generator G such that "Ortcutt" would return "the man in the trenchcoat". Intuitively, our *de re* belief report in (i) is true because the man in the trenchcoat is identical to Ortcutt at the actual world, and because Ralph's *de dicto* belief about the man in the trenchcoat applies, at the actual world, to Ortcutt.

(17)



Crucially, whether the coordinate is *pro*x bound by the matrix author, or *pro*y, bound by the matrix addressee, is entirely up to post-LF interpretive processes. In other words, in logophoric control the choice of the controller is not grammatically encoded in the structure, but semantically underpinned. This would account for why control may shift from the matrix subject to object and vice versa under complex semantic and pragmatic conditions. Favorable conditions include: reversal of authority relations (18a), "de-agentivized" complements (18b), or with *be-*

allowed-to complements (18c):

(18) a. The prisoner$_i$ begged/asked the guard$_k$ *pro*$_i$ PRO$_i$ to smoke a cigarette.
b. John$_i$ convinced Mary$_k$ *pro*$_i$ PROi to be included generously in his last will.
c. The teacher$_i$ promised John$_k$ *pro*$_k$ PRO$_k$ to be allowed to leave early.

As Landau (2015) acknowledges, different verbs allow control shift to varying degrees (e.g., *offer/propose* are quite flexible, *urge/recommend* are quite resistant), and the precise effect of each of the three conditions listed above is known to vary considerably across speakers and languages (see Bresnan, 1982; Chierchia, 1984; Comrie 1984, Farkas, 1988; Sag & Pollard, 1991, Rooryck, 2000; Jackendoff & Culicover 2003). However, as predicted by his theory, it is only logophoric control verbs that display control shift; predicative control verbs consistently exclude it.

So why might children experience difficulties deriving subject control with *promise* (a logophoric control verb) under this theory of control? One possibility could be that children initially misrepresent logophoric control complements as predicative control ones, hence their consistent choice of the most local antecedent to PRO. One of the predictions of this hypothesis is that children in this stage would not require PRO to be read *de se* (see fn. 13 in this chapter), since this presupposition is induced by the special complementizer of logophoric complements. This remains an open question. Alternatively, it may be the case that children correctly represent the CP tier in logophoric complements, but consistently choose the addressee as the binder of *pro* due to locality principles. Once they learn to override this principle in determining the choice of antecedent in logophoric control constructions, they will be able to appeal to more abstract semantic notions and effectively establish subject control with verbs like *promise*. Importantly, problems with *promise* would only surface in SC constructions, and not when it takes a finite

clause.

Although these ideas are admittedly speculative, the results from our experiments lead us to believe that children's grammatical difficulties with SC with *promise*-type verbs have a different origin from their difficulties with StSR with *seem*-type predicates. Landau's (2015) analysis of SC *promise* is one possible analysis that would lead us to expect exactly what we find in our results – an uncorrelated pattern of results.

4.3. Conclusion

In our SC study, we addressed three questions. The first was to examine whether the delays observed with subject control *promise* also extend to other subject control verbs that have an intervening benefactive, but are headed by a preposition, such as *swear* in English or both *prometer* 'promise' and *jurar* 'swear' in Spanish. As mentioned in Chapter 2, Boeckx and Hornstein (2003), Hornstein and Polinsky (2010), and Boeckx, Hornstein, and Nunes (2010) have argued that children's delays with *promise* are due to their difficulties detecting a null preposition heading the benefactive, causing intervention effects. They therefore predict that children are more likely to show object control with *promise* than with other subject control verbs that have a benefactive headed by an overt preposition. We investigated this hypothesis by testing children on *promise,* which takes a DP benefactive, and *swear*, which takes a PP benefactive. Contrary to their predictions, children scored equally poorly on both conditions, 46% for *promise* and 39% for *swear*. The results were not significantly different, and were in fact correlated, suggesting that whatever difficulty children have with *promise* is likely to be the same as the one they have with *swear*. Similarly, Spanish-speaking children scored 48% in the 'SC *prometer* with a short benefactive' condition, and 47% in the 'SC *jurar* with a short

benefactive' condition. That is, having an overt case-marking preposition on the benefactive did not facilitate obtaining subject control, contra the predictions of Boeckx and Hornstein (2003).

The second goal of the SC study was to determine whether the acquisition delays observed with subject control are due to children having a non-adult grammar or to processing limitations. As we did in the case of StSR, we investigated this question using two different approaches, i) including short and long benefactive conditions for *promise/prometer*, and ii) comparing children's SC proficiency with two independent measures of processing capacity (CLPT) and short-term memory (digit span). We found no significant difference between children's performance in the short and long benefactive conditions, scoring an average of 46% and 40% respectively in English, and 48% and 44% respectively in Spanish. Moreover, we found no significant correlation between children's overall performance in the three SC conditions and CLPT score, or between performance in SC and digit span score, in either English or Spanish. This again suggests that the overall difficulties with *promise*-type verbs is not *exclusively* a processing problem, as Choe's (2012) Performance-based Intervention hypothesis would predict.

However, as in the case of StSR, when looking more closely at the data, we found a subgroup of children for whom the processing predictions are borne out: the children that performed at chance and above in both the English and Spanish experiments. In this group, we found i) a significant correlation between their performance on the three conditions that included a benefactive and their CLPT scores, and ii) a significant difference between the short and long benefactive conditions. Therefore, we hypothesize that children that perform below chance with SC simply do not have the grammatical machinery that allows them to circumvent intervention, while 'chance' and 'above chance' children may have the correct representation and rule system, but still manifest processing difficulties. This correctly predicts that children that perform below

chance, i.e. those that still have a non-adult grammatical representation, will fail to comprehend the SC sentences even if they have a high processing capacity, and they will consistently assign object control. This is clearly observed in Figures 4.4 and 4.5, where we find multiple cases of children that performed below chance in the benefactive conditions but obtained above average and even perfect scores in the CLPT. Contrastively, those that perform at chance or above chance may still show difficulties, but these are most likely processing-based.

Finally, we sought to determine if children's grammatical difficulties with StSR with *seem*-type verbs and SC with *promise*-type verbs stem from the same underlying source, as suggested by some authors who derive these two structures in a parallel fashion, e.g. by smuggling, as in Belletti and Rizzi (2013), or by A-movement, as proposed by proponents of the MTC (Boeckx & Hornstein, 2003, 2004; Boeckx et al., 2010; Hornstein 1999, 2001; Hornstein & Polinsky, 2010). The results show that the group of children that performed below chance with StSR were not the same as the group that performed below chance with SC in neither the English- nor the Spanish-speaking group. In fact, some children performed above chance in one construction and below chance in the other and vice-versa. This suggests that the grammatical machinery that allows them to by-pass the intervening experiencer in the case of StSR is unlikely to be the same as the one that allows them to by-pass the intervening benefactive in the case of SC.

Recent advances in psycholinguistic methodology also corroborate this conclusion. In an oft-cited neurolinguistic study, Featherston, Gross, Munte, and Clahsen, (2000) report a significant difference in the Evoked-Related Potentials elicited at the empty subject positions in German StSR constructions and SC constructions. They interpret this as evidence for the different nature of the empty categories, specifically, an A-trace versus PRO. This result has

been taken to provide support for standard analyses of Control such as Landau's (2000, 2003, 2013, 2015), as opposed to Movement analyses such as Hornstein's (1999, 2001, 2003 *et seq*.). It is important to take experimental results with caution, particularly when they have not been yet replicated. However, it is our hope that strengthening the bridge between theoretical syntax and psycholinguistics will advance both fields.

CHAPTER 5

Implications, extensions, and conclusion

This dissertation investigates how English- and Spanish-speaking children comprehend subject-to-subject raising (StSR) and subject control (SC). This final chapter begins by summarizing the findings of each of the studies described in Chapters 2 and 3 and discusses their theoretical implications. The second section examines some potential extensions of these findings and suggests further research questions. The third section concludes the dissertation by highlighting the main results.

5.1. Summary of the findings and implications

In Chapter 3, a set of experiments was conducted to examine children's comprehension of StSR sentences. Previous experimental studies of the acquisition of StSR *seem* with a covert experiencer found mixed results (Section 1.1.2). Thus, the purpose of the first part of this study was to provide a better assessment of children's comprehension of this structure (e.g., '*The dog definitely seems to be grey*'), and to compare it to children's comprehension of StSR with a verb that does *not* take an experiencer argument but is otherwise semantically similar to *seem*, i.e. Spanish bare *parecer*, (e.g. '*El perro definitivamente parece ser gris*') (see Section 2.1.3.1). The results of this experiment showed that six-year-old children still have difficulty comprehending StSR sentences with an implicit experiencer in English – a finding consistent with the results of Hirsch, Orfitelli, and Wexler (2008) and Orfitelli (2012). On the other hand, we provided new experimental evidence from Spanish showing that children as young as four perform above

chance with StSR bare *parecer* in superficially matching sentences. This result provides support for the idea that StSR constructions are not universally acquired late. It is only those structures that involve A-movement across an (overt or covert) intervening argument that seem to give rise to difficulties, in accordance to Orfitelli's Argument Intervention Hypothesis (AIH, 2012) and Snyder and Hyams' Universal Freezing Hypothesis (UFH, 2015).

In the second part of the study, we examined children's performance on StSR *seem* and opinion *parecer,* both with an overt experiencer argument. As anticipated by grammatical accounts (e.g. AIH, UFH), English-speaking children perform as poorly with an overt experiencer as they do with a covert experiencer. In contrast, Spanish-speaking children perform significantly worse with opinion *parecer* than bare *parecer*, as only the former requires A-movement over an experiencer argument, presumably achieved through smuggling and semantic coercion, two operations that are difficult for children to master (Gehrke & Grillo, 2008; Grillo, 2008; Snyder & Hyams, 2015).

Interestingly, we found that children who performed statistically at and above chance with StSR *seem*/opinion *parecer* were occasionally able to answer correctly and provide response justifications that were fully compatible with an adult representation of StSR, and hence would have had to raise the subject past the experiencer. We hypothesized that the 'at- and above-chance' children, have an adult grammatical system, but may still experience difficulties due to processing limitations, while the below-chance children have grammatical difficulties, i.e. they have a non-adult-like grammar system, which does not allow them to move the subject across the intervening experiencer. Accordingly, we divided the children into two groups: i) below-chance and ii) at- and above-chance, and found that the latter, but not the former group showed a positive correlation between performance on StSR and processing capacity as

measured by the Competing Linguistic Processing Task (CLPT; Gaulin & Campbell, 1994). They also performed significantly better in the short experiencer condition (e.g. '*The dog seems to the cat to be grey*') than in the long experiencer condition (e.g. '*The dog seems to the cat with stripes to be grey'*). Thus, in this respect, the below-chance group performed as predicted by grammatical accounts and the at- and above-chance group performed as predicted by processing accounts such as the Processing-based Intervention Effects hypothesis (PIE; Choe, 2012, Choe & Deen, 2016). We take these results to indicate that the intervention effects observed with StSR are *both* grammar- and processing-based. That is, children are delayed in acquiring structures that require A-movement across a structurally intervening argument due to two independent factors: i) lack of the grammatical operation that enables this movement (e.g. 'semantic coercion' according to Snyder & Hyams, 2015); and ii) limited processing capacity, which impinges on the proper functioning of the adult-like grammar system.

In Chapter 3 we investigated the acquisition of Subject Control (SC) with *promise*-type verbs in English and Spanish. The experiments included the SC verb *promise*/*prometer*, which takes a DP benefactive in English ('*The knight promises the wizard to protect the castle*') but a PP in Spanish (e.g. '*El caballero promete al mago proteger el castillo*'), and *swear/jurar*, which takes a PP benefactive in both languages (e.g. '*The knight swears to the wizard to protect the castle*', and '*El caballero jura al mago proteger el castillo*'). The results demonstrated that English-speaking children's difficulties with SC *promise* are also observed with *swear* and also extend to the Spanish SC verbs, contra the predictions of the Null Preposition Hypothesis (NPH, Boeckx & Hornstein, 2003; Hornstein & Polinsky, 2010; Boeckx, Hornstein, & Nunes, 2010).

In order to gain a better understanding of the source of difficulty with SC patterns, we followed the same protocol as in the StSR study, namely, we manipulated the length of the

intervening argument, and compared children's performance on SC with their CLPT scores. As in the StSR study, we found that the children who performed statistically at chance and above showed a positive correlation between performance with SC and the CLPT score, and performed significantly better on the short benefactive condition than the long benefactive condition. These hallmarks of processing limitations were not found in the below-chance group. We thus proposed the Dual Source Intervention (DSI) hypothesis to explain children's difficulties with both StSR and SC, i.e. two different components are responsible for the delays observed with these two constructions: (i) a non-adult grammatical system; (ii) and an underdeveloped processing capacity.

According to most memory-based processing theories (e.g. Dependency Locality Theory; Gibson 1998, 2000; Grodner & Gibson, 2005; Warren & Gibson 1999, 2002, 2005; Similarity-based Interference; Lewis, 1996, 2000; Gordon, Hendrick, & Levine, 2002; Van Dyke & Lewis, 2003; Activation-based model; Lewis & Vasishth, 2005; Lewis, Vasishth, & Van Dyke, 2006), any construction that involves a dependency across an intervening DP should impose processing difficulties, regardless of whether the dependency is A-movement (e.g. StSR *seem*, verbal passives), control (e.g. SC with *promise*), or A'-movement (e.g. object relative clauses, object topicalizations). Hypotheses such as PIE thus predict that children's performance in all these constructions should be correlated, provided the DP features are kept constant; e.g. two masculine singular DPs, or two plural pronouns (Choe, 2012:). However, our DSI hypothesis is sensitive to both, processing *and* grammatical factors. Therefore, it predicts that we should only find a significant correlation between children's performance on two constructions if they both circumvent the intervening argument through the same *grammatical* operation(s). For instance, Hirsch & Wexler (2007b) and Orfitelli (2012) found a near perfect correspondence between

performance on StSR *seem* and non-actional passives. Such a tight relationship suggests that children's delay is likely to be caused by a shared underlying grammatical operation, e.g. smuggling and semantic coercion (Snyder & Hyams, 2015).

In order to investigate this question with respect to StSR *seem*/opinion *parecer* and SC *promise*/*prometer*, we compared children's performance on these two constructions in the second part of Chapter 3. Individual and group results suggest that these are not likely to be derived through the same *grammatical* mechanism. As expected by the DSI hypothesis, we found that children who performed at chance and above in *both* constructions showed a positively correlated pattern, presumably due to the processing effects observable in those groups. Crucially, however, we did not find a correspondence between performance with StSR and SC in *all* children – some children performed below chance with StSR and above chance with SC and some children performed above chance with StSR and below chance with SC. That is, some children lack the grammatical operation that enables raising the subject past the experiencer, but are able to establish subject control across the benefactive; and some other children are able to raise the subject past the experiencer argument, but fail at establishing subject control across the benefactive argument.

These results not only provide evidence against hypotheses that claim children's difficulties are *uniquely* processing-based, i.e. Choe's (2012) PIE, they also constitute the first experimental evidence against theories that link the syntactic derivation of these two constructions, such as the Movement Theory of Control (MTC; Hornstein 1999, *et seq.*; Manzini & Roussou 2000; O'Neill, 1995), which claims both are derived by A-movement, or Belletti and Rizzi's (2013) hypothesis, which attributes the delay of SC *promise* and StSR *seem* to children's general difficulties with smuggling (Collins, 2005a). Given the uncorrelated pattern of results we

obtained, we conclude, with a reasonable level of confidence, that these cannot be derived in exactly the same way, at least not in children. This idea is illustrated abstractly in Figure 5.1.

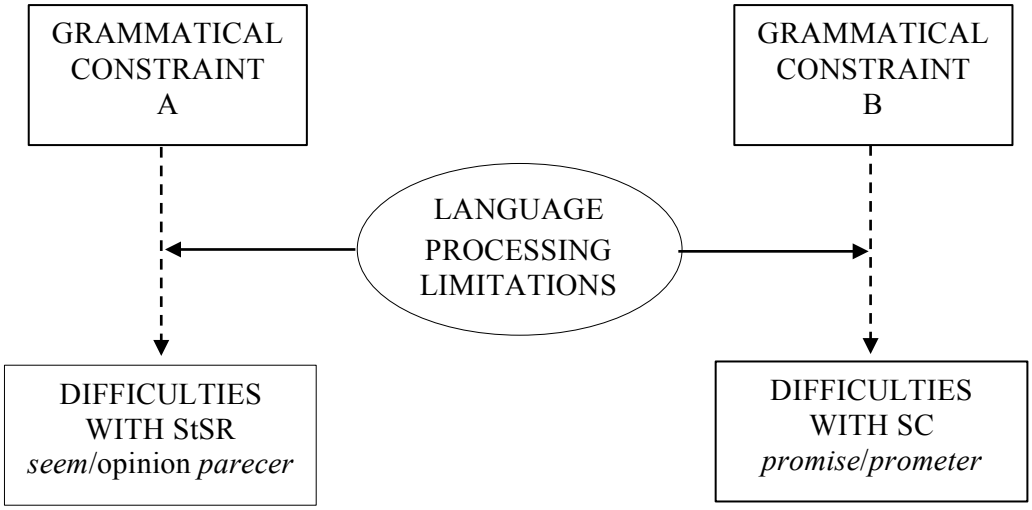


Figure 5.1. Diagram representing the potential causal relationship between processing limitations and two independent grammatical constraints, one affecting StSR *seem*/opinion *parecer* and one affecting SC *promise*/*prometer*.

Although the experiments in this dissertation were not designed to uncover which specific syntactic analyses of StSR and SC one should adopt, we speculated about some possibilities. As discussed in Section 2.1.1, Collins (2005a,b) has argued that passive and StSR *seem* constructions do not violate Relativized Minimality because the logical object/subject is 'smuggled' across the logical subject/experiencer. Additionally, Gehrke and Grillo (2008), and Grillo (2008) have claimed that non-eventive VPs must undergo 'semantic coercion' before they can move and smuggle the object in the case of non-actional passives. This operation enriches its semantics by introducing temporality. Given that *seem* is also non-eventive verb, Snyder and Hyams (2015) contend that the delays observed with both non-actional passives and StSR *seem*

must be due to children's inability to apply semantic coercion. This would account for the asymmetry in performance between actional (eventive) and non-actional (non-eventive) verbal passives, and the within-subjects correlation found between StSR *seem* and non-actional verbal passives in (Hirsch & Wexler, 2007b) and Orfitelli (2012).

Regarding SC *promise*, none of the hypotheses or syntactic analyses we described in Section 2.2.1 predict our pattern of results. Thus, in Section 4.2.3 we entertained a new perspective: Landau's (2015) Two-tiered Theory of Control. Although this analysis of control does not make reference to acquisition, several factors involved in the derivation of subject control with *promise* could account for children's delays. One of them could be children's misrepresentation of logophoric control structures as predicative control structures. This would account for below-chance children's consistent choice of the benefactive as the controller, i.e. they choose the closest c-commanding DP, as is done in the case of predicative control. Another possibility could be children's failure to project the (correct) coordinate pro_x, which could again lead to a syntactically local choice over a semantically underpinned one. Further work needs to be done before we can solidify any of these hypotheses. Crucially, given our results, it is highly unlikely that StSR with *seem* and SC with *promise* are derived in a similar way.

5.2. Extensions of our findings

The set of findings reported in this dissertation has several implications for our understanding of the acquisition of raising, control, and of intervention effects in general, and thus provides for potential extensions to this line of investigation. In the following sections we will describe a few constructions, some of which involve an intervening argument, and discuss the predictions about their acquisition.

5.2.1. StSR beyond English and Spanish

The ramifications of our StSR study results extend beyond English and Spanish. We concluded from the results of our study that by age four, children comprehend StSR sentences that have no overt or *covert* intervening experiencer argument. Contrastively, children experience difficulties with StSR constructions involving an intervening argument until age six. We thus predict that StSR will be acquired early in languages in which there is no intervening experiencer.

Icelandic raising verbs can appear with or without a dative experiencer. When the experiencer argument is not produced, the subject of the embedded clause can raise to matrix subject position, as in (1a). However, when there is a dative experiencer subject raising is not possible (1c), i.e. the nominative subject must remain in the embedded infinitival clause. However, in this case the dative experiencer appears in subject position (1b) (to satisfy verb second requirements (for a much fuller discussion of examples of this kind, see Sigurðsson, 1989: 81-100).

(1) a. *María virðist [vera glöð].*
Mary-NOM seems be happy
'Mary seems to be happy.'

b. *Mér virðist [María vera glöð].*
me-DAT seems Mary-NOM be happy
'Mary seems to me to be happy.'

c. **María virðist mér [vera glöð].*
María-NOM seems me-DAT be happy
'Mary seems to me to be happy.'

If we assume that (1a) does not have a syntactically represented intervening experiencer argument (as is the case with Spanish bare *parecer*), we would predict that Icelandic-speaking children will have no difficulties comprehending this raising pattern, provided they understand the lexical properties of the verb. Similarly, Icelandic-speaking children should perform well with (1b) sentences, as there is no movement over the experiencer argument.[1]

Turning to Romance languages, it has been claimed that for some speakers of Italian (reported in Cinque, 2004), Italian *sembrare* 'seem' has dual status, in a way similar to Spanish *parecer*. On the one hand, it is a lexical verb that selects an experiencer argument (2a) and does not allow clitic climbing (2b).[2] On the other hand, it behaves like a functional restructuring verb (2c), incompatible with an experiencer argument, but allowing clitic climbing, as in (2d) (see Haegeman, 2006).

(2) a. *Maria non mi sembra [apprezzare il mio sforzo].*
Mary not me-DAT seems appreciate the my effort
'Mary doesn't seem to appreciate my effort.'

b. **Maria non mi lo sembra apprezzare abbastanza.*
Mary not me-DAT it seems appreciate enough
'Mary doesn't seem to me to appreciate it.'

c. *Maria sembra [apprezzare l'arte].*
Mary seems appreciate art
'Mary seems to appreciate art.'

[1] We are currently investigating this hypothesis (Hyams, Mateu, Sigurjónsdóttir, in progress).

[2] Note that contrary to Spanish opinion *parecer*, lexical *sembrare* can select a non-finite verbal complement.

d. *Maria lo sembra [apprezzare].*
Mary it-M.SG seems appreciate
'Mary seems to appreciate it.'

Italian-speaking children are thus predicted to show a similar developmental path to that observed with Spanish *parecer*. That is, children should comprehend sentences with StSR functional *sembrare* by age four, and will perform significantly worse on lexical *sembrare* for a prolonged period of time.

Interestingly, Haegeman (2006) argues that the French cognate of *sembrare*, *sembler*, behaves *only* like a lexical verb and never like a functional head, and bases this claim on restructuring facts. Namely, French *sembler*, as opposed to Italian *sembrare*, does not allow for clitic climbing, even when there is no overt experiencer argument, (3). If that is the case, and French *sembler* is closer to English *seem* in that it always syntactically represents an experiencer argument, we predict French-speaking children, like English-speaking children, will experience overall problems with StSR *sembler* until age six or seven –assuming both are derived through the same grammatical operations, e.g. smuggling (Collins, 2005a) (and semantic coercion; Gehrke & Grillo, 2008; Grillo, 2008).

(3) a. *Il a semblé [avoir parlé d'eux].*
He has seemed have talked of-them
'He seems to have talked about them.'

b. **Il en a semblé [avoir parlé].*
He of-them has seemed have talked
'He seems to have talked about them.'

As discussed in Chapter 1, to date, very few languages have been examined with the respect to the acquisition of StSR –English, Dutch, and now Spanish. Investigating the developmental path of acquisition of this construction in other languages, such as Icelandic, Italian, or French, would allow us to strengthen (or reject) our hypotheses, and also fill an important empirical gap.

5.2.2. Need

The verb *need* can be associated with two modalities: a subject-teleological flavor, which expresses necessity in light of the subject's priorities (4a), and a deontic flavor, which expresses necessity in light of third party rules (4b) (see Abenina-Adan & Angelopoulos, 2016; Harves, 2008; Harves &Kayne, 2012; Kratzer, 1981, 1991, 2012; Rubinstein, 2012).

(4) a. I need to drink coffee every day (to feel awake).
b. I need to drink coffee every day (because I am participating in a study that investigates the effects of caffeine).

Abenina-Adan and Angelopoulos (2016) argue that when *need* expresses a subject-teleological modality, as in (4a), it has a control structure, i.e. *need* is thematically related to the matrix subject.[3] On the other hand, when it expresses a deontic necessity, as in (4b), it behaves as a raising verb, i.e. *need* is not in a thematic relation with the matrix subject. This ambiguity would explain why *need* can pass both control and raising diagnostics. For example, *need,* like other raising verbs and unlike control verbs, can take an expletive *there* subject, as shown in (5).

[3] See Rubinstein (2012) for experimental evidence demonstrating that *need* is not always compatible with a raising analysis, and that non-raising *need* constructions only allow subject-teleological readings.

(5) a. There seems to be no one at home.
b. *There wants to be John a millionaire.
c. There needs to be a doctor present.

Conversely, *need* behaves like a control verb with respect to pseudoclefting, (6). Raising verb complements cannot be pseudoclefted; (some) control verbs complements can. The possibility of pseudoclefting *need* complements shows that it is not uniformly a raising verb.

(6) a. *What John is likely is to be examined by the doctor.
b. What John wants is to be examined by the doctor.
c. What Bill needs is to be taken care of.

Consider the scenario in (7), a deontic-inducing context, and the sentence uttered in light of this context in (7a): Though *a professional* is in matrix subject position, the 'needer' is interpreted to be a third party, i.e. the three roommates. Unambiguous raising cases would include sentences with expletive *there* or inanimate subjects, (7b), which cannot be interpreted as 'needers', and as predicted, are only compatible with a deontic interpretation.

(7) *Deontic-inducing context: John, Peter, and Mike live together in an apartment and have not cleaned the bathroom in over a year. At this point, the stains are too difficult to remove with regular cleaning products, and they realize they need to bring a professional cleaner:*
a. A professional needs [$_{TP}$ to clean the bathroom]
b. The bathroom needs [$_{TP}$ to be cleaned by a professional]

Now, compare the example in (7a) to the one provided in (8a). While (7a) is assumed to involve raising given the deontic-inducing context in (7) (in light of a third-party's priorities), the superficially similar example in (8a) would have a control analysis given the teleological-inducing context in (8) (in light of the subject's priorities). Evidence for the incompatibility of subject-teleological readings and a raising analysis (guaranteed by the use of an inanimate subject) is given in (8b).

(8) *Subject-teleological-inducing context: John obsesses about the cleanliness of the bathroom. Although the bathroom is already clean…*

a. John needs [$_{TP}$ to clean the bathroom]

b. #The bathroom needs [$_{TP}$ to be cleaned by John]

Crucially for us, Abenina-Adan and Angelopoulos (2016) argue that the impossibility of a raising analysis in (8b) follows if it involves an implicit 'needer' argument in the matrix clause whose reference is constrained by Condition C, as in (9a). In the raising *need* construction, the overt matrix subject raises from the subject position in the embedded clause to the matrix Spec-TP (across the internal subject *pro*). Compare now to the control *need* structure in (9b). In this case, the matrix subject *is* thematically related to *need,* and raises from internal subject position to Spec-TP. The subject of the embedded clause is PRO, controlled by the matrix subject.

(9) a. Raising *need* (deontic reading)
John$_i$ [*pro*$_k$ needs [t_i to clean the bathroom] (according to the house rules)

b. Control *need* (subject-teleological reading)
John$_i$ [needs [PRO$_i$ to clean the bathroom] (because he is concerned about germs)

Turning now to acquisition, if the analyses in (9) are correct, the development of control *need* is predicted to precede that of raising *need,* all else being equal (i.e. provided they do not have more difficulties understanding the teleological modality), as the latter, but not the former, involves movement over a structurally intervening argument. An acquisition experiment that manipulated the context to promote either a deontic or a subject-teleological reading of *need* could investigate this question. Additionally, if we assume that *need,* a non-eventive verb, requires the same grammatical operations as StSR *seem* in order to circumvent the intervening argument, we predict that the same children that experience difficulties with non-actional passives and *seem*, will experience difficulties with raising *need*.

5.2.3. Inverse copulas

Copular sentences are distinguished in two ways (e.g. Higgins, 1973). In predicational sentences, the referential DP comes before the copula with the predicate following it (10a). In specificational sentences, the referential DP is post-verbal, and the predicative constituent is in initial position (10b). Moro (1997) proposes that both (10a) and (10b) are derived from the structure in (10c), which involves a small clause with two DPs.

(10) a. The pictures on the wall were the cause of the riot.
b. The cause of the riot was the pictures on the wall.
c. be [$_{DP}$ the pictures on the wall] [$_{DP}$ the cause of the riot]

According to Moro, the canonical, non-inverted copular structure, i.e. (10a), involves raising the referential DP to spec-DP; while the inverted copula structure, i.e. (10b), involves

raising the predicative DP instead. This analysis has interesting implications for acquisition given our findings.

There is, to our knowledge, only one study that has investigated the acquisition of these constructions. Hirsch and Wexler (2007a) report that four- to seven-year-old children have difficulties in the comprehension of inverse copula constructions, namely specificational copular ones, until approximately age six or seven. Using a picture-matching methodology they tested comprehension of sentences of the sort exemplified in (11a) and (11b), uttered in the context of pairs of pictures depicting opposite events. While children showed near perfect comprehension of non-inverted copula structures, their performance was significantly worse with inverted copula sentences.[4]

(11) a. Which picture shows [the pig is the animal who helps].
b. Which picture shows [the animal who helps is the rabbit].

The authors attribute children's difficulties with inverse copulas to children's inability to perform A-movement (across a defective *v* phase). However, contrary to the experimental evidence reviewed in Chapter 2, this hypothesis would incorrectly predict unaccusatives, actional passives, and non-experiencer StSR constructions to be acquired as late as non-actional passives and StSR with *seem* (i.e. at around age six or seven). On the other hand, according to

[4] It could be the case that having a subject relative clause in the predicative DP may have increased the difficulty of these sentences. Experimental results from a self-paced reading experiment with adults show that subject relative clause modifiers, e.g. '*The nurse who was from the clinic…*') are significantly harder to process than PP subject modifiers, e.g. '*The nurse from the clinic…*' (Grodner & Gibson, 2005). Future acquisition studies should thus isolate the relevant inverse copula structure from other constructions, as in (i):

(i) a. Which picture shows [the pig is the helpful animal].
b. Which picture shows [the helpful animal is the rabbit].

Orfitelli, the AIH (2012: 124-125) predicts inverse copulas to be delayed due to the presence of an intervener interrupting the A-movement dependency.[5]

Our prediction is again dependent upon the specific syntactic analysis we assume. If inverse copula sentences are derived by simple A-movement, as Moro (1997) proposes (see fn. 5 in this chapter) or A'-movement (as proposed by Hatakeyama, 1997; Heggie & Iwasaki, 2012), and not smuggling and semantic coercion, as we are assuming in the case of StSR *seem*, the difficulties observed in Hirsch and Wexler (2007b) may be solely due to *processing*-based intervention effects. If that is the case, we should find that processing capacity modulates comprehension of these sentences in all children, and not only those that perform at chance and above. That is, we should *not* find children with a high processing capacity performing poorly on inverse copula sentences, as we did observe in the case of StSR.

On the contrary, if inverse copulas are derived through the same smuggling (Collins, 2005a) (and semantic coercion operation; Gehrke & Grillo, 2008; Grillo, 2008) used in StSR *seem*, we would predict a parallel development of these two constructions. An experimental study comparing children's performance on these two constructions should bear fruitfully on this question.

[5] It is not entirely clear whether the subject DP is in an intervening position under Moro's analysis. More argues that "as far as locality is concerned, the chain established by the predicative DP is essentially no different from the chain of the subject DP in a canonical sentence: in both cases, the raised DP passes through the Spec-position of the local c-commanding head activating agreement on it." (1997: 41). However, this seems rather stipulative, since his main evidence for this point is the fact that inverse copulas are possible.

5.2.4. Middles

Another potential example of A-movement is the case of middle constructions, (12). A prototypical English middle construction consists of the following syntactic elements: (a) a preverbal DP, which is the logical object of the verb; (b) a transitive verb which agrees with the preverbal element; and (c) a manner or adverbial expression, like *easily*. Semantically, middles attribute properties to entities; that is, they are non-eventive (stative) statements.

(12) These cookies sell easily.

Much of the theoretical discussion on middles has centered on whether the preverbal nominal in middles is generated in that position (no movement analysis; e.g. Ackema & Schoorlemmer 1995; Mendikoetxea, 1998, 1999) or whether it moves to that position (movement analysis; e.g. Ahn & Sailor, 2010; Hoekstra & Roberts 1993; Massam, 1992; Stroik 1992). Additionally, there is debate as to whether the external argument is projected in the syntax (Bruening, 2012; Hoekstra & Roberts 1993; Stroik 1992) or not (Ackema & Schoorlemmer 1995; Ahn & Sailor, 2010; Rapoport, 1999).

From an acquisition perspective, if the structural subject has moved from internal position, and if the external argument is projected as an intervening argument, we expect to observe intervention effects similar to those of StSR *seem*. Contrastively, if the object moves to subject position but there is no intervening argument, or if it is in a non-intervening position (i.e. adjunct position), we should not find delays.

The middle does not pass all of the standard syntactic tests for covert arguments, including modification by volitional adverbs (13) and control into purpose clauses (14) (from Roberts, 1986); but anaphor binding suggests the opposite (15) (from Stroik, 1992).

(13) *This bureaucrat bribes deliberately.

(14) *This bureaucrat bribes easily [to avoid the draft].

(15) a. Books about oneself never read easily.

b. The poets admired each other so much, that even each other's worst work actually seemed to read well.

Additionally, the middle permits modification by a *for*-PP whose argument seems to reflect the intended agent (16), and Stroik (1992) argues is the overt realization of the external argument of the middle, just as the *by*-phrase is the overt realization of the external argument of the passive.

(16) Epic fantasy novels read easily for Mary.

Given the results of this dissertation, we can make a clear prediction concerning the interaction of syntax and acquisition: if the middle has a syntactically projected logical subject (and assuming a movement analysis of the logical object), intervention should cause a delay in their acquisition. If there is no syntactically projected logical subject, children should show early acquisition. Additionally, if we assume that middles are derived by smuggling, as suggested by Ahn and Sailor (2010), and that they require semantic coercion due to their non-eventive nature (Gehrke & Grillo, 2008), we predict that the same children who experience difficulties with non-actional passives and *seem*, will experience difficulties with middles.[6] This is one of the many

[6] Note that Orfitelli's AIH (2012) also predicts a similar delay with middle constructions. However, we only expect to find a correlation with other constructions if they also require smuggling and semantic coercion, i.e. those involving non-eventive verbs; while the AIH predicts equal difficulty in all constructions involving A-movement

potential examples for acquisition data could inform syntactic theory.

Let us now turn to Spanish middles. A prototypical Romance 'middle' consists of the following elements: (a) the clitic *se/si*; (b) a preverbal DP, which is the internal argument of the verb; (c) a transitive verb which agrees (in number) with the preverbal element; and (d) a manner or adverbial expression, like fácilmente 'easily', as in (17).

(17) *Estas camisas se arrugan fácilmente.*
These shirts SE wrinkle easily
'These shirts wrinkle easily.'

Competing analyses consider this *se/si* to be either an argument (Raposo & Uriagereka, 1995 for European Portuguese; Sportiche, 2010, for French; and Teomiro, 2010, for Spanish) or a nonargumental clitic (Mendikoetxea, 2008). However, even accounts that argue that the clitic is in a nonargument position claim that the argument position in *se* impersonal constructions is occupied by a null pronominal of some kind, such as a non-referential or generic *pro* (see Suñer 1990, Mendikoetxea, 2008). Therefore, if the object does undergo movement across the s*e/si* (+*pro*) and is not directly merged in a pre-verbal position, we would expect children to experience difficulties with this construction due to intervention. If we additionally assume that the same grammatical operations that are employed in StSR opinion *parecer* are used to derive middles in Spanish, we would also predict both to be (grammatically) acquired at roughly the same time.

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across an intervening argument regardless of whether the predicate is stative or eventive (i.e. including actional verbal passives).

5.2.5. *Tough*-movement

The standard analysis of *tough*-movement constructions involves an empty operator moved from object position to an initial position in the embedded clause. The empty operator in turn is identified by coindexation with the higher referential subject DP. The derivation is represented in (18a) (see Chomsky, 1977, 1981, 1995; Haegeman, 1994; Rifkin, 2001). *Tough*-movement constructions can also appear with an overt experiencer phrase (18b), which is assumed to be generated in Spec-AP. The surface word order would be obtained by moving the adjective from A (head of the AP) to a (head of the aP) (see Anderson, 2005, for further discussion).

(18) a. Our dog_i was easy [Op_i [$PRO_{k/arb}$ to train t_i]].
b. Our dog_i was easy for us_k [Op_i [PRO_k to train t_i]].

In both cases, we observe movement of an argument from internal argument position to matrix subject position across the embedded subject argument, i.e. PRO; however, only in the latter case (18b) do we have an *overt* DP disrupting the semantic link between the matrix subject and the object gap in the embedded clause. Processing-based intervention accounts such as Choe's PIE (2012) would thus predict difficulties with sentences such as (18b), but not sentences like (18a). On the other hand, intervention accounts that (also) rely on grammatical representations and dependencies may such as the one defended here predict difficulties with both.

Experimental studies that have investigated the acquisition of *tough*-movement constructions such as the one in (18a) have consistently found that children err in their interpretation until quite late in development, around age six to eight years according to C.

Chomsky (1969), and 10 to 11, according to Cromer (1970, 1972).[7] More recent investigations (Anderson, 2005) have likewise found that children give at best inconsistent interpretations, and at worst consistently incorrect interpretations, until approximately age six.[8] The vast majority of errors consist of children's construing the subject of the sentence as the semantic subject of the lower predicate. That is, (18a) would be interpreted to mean '*it is easy for the dog to train someone.*' Interestingly, all three studies report on a large group of children assigning these sentences an inconsistent interpretation, i.e. sometimes they assign it a non-adult-like interpretation and sometimes they assign it an adult-like interpretation. We have taken this pattern to indicate that target-like knowledge of the structure has been acquired by this stage, but its deployment remains unreliable due to insufficient processing capacity.

Additional difficulties arise if we assume the experiencer phrase of (18b) is also syntactically represented in (18a), given that PRO is presumably controlled by the experiencer across the intervening operator (i.e. the raised direct object from the embedded clause that corefers with the matrix subject). This would effectively predict that *tough*-movement constructions will develop particularly late, as they incur two crossed dependencies: (i) the operator – object gap, crossing PRO; (ii) the experiencer – PRO, crossing Op. As far as we know, no study has investigated the acquisition of *tough*-movement constructions with overt experiencers. Future studies comparing the development of these constructions with and without

[7] It is interesting to note that Cromer (1972) also tested children on two actional passive sentences (e.g. *The duck is bitten by the wolf*) and found that the same group of children that performed poorly on *tough*-constructions, performed above 90% on actional passives.

[8] See Becker, Estigarribia, and Gylfadottir (2012) who demonstrate that four- to seven-year-olds can correctly categorize novel adjectives construed as *tough* (*daxy* ≈ *easy*) or control adjectives (e.g. *greppy* ≈ *eager*) based on context cues alone. However, they note children experience more difficulties when the subject is animate or "animized" (e.g. a doll), given that it gives rise to an ambiguity between a *tough* or control analysis.

an overt experiencer could also inform us about the status of the experiencer argument in the non-overt experiencer construction and intervention.

5.3. Conclusions

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 an overt or *covert* intervening argument. Further, not all constructions involving an intervening argument are acquired at the same time – our results indicate that the development of StSR and SC are not mastered concurrently in all children, suggesting that these are derived through different grammatical operations, which must be mastered independently.

On the other hand, we found processing capacity also plays a role in children's performance with both constructions, leading us to propose the Dual Source Intervention (DSI) hypothesis –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 enough processing capacity to parse constructions that involve a crossing dependency (i.e. all structures involving an intervener will cause processing difficulties). Such a theory not only unifies the seemingly incompatible grammatical and processing accounts proposed for the acquisition of this type of constructions, but may also provide a diagnostic to adjudicate between competing syntactic analyses.

Our hypothesis awaits future confirmation, or disconfirmation, which ideally will involve more extensive and cross-linguistic testing of children's abilities to interpret various

constructions involving intervention (or not). It is hoped that, ultimately, the results of our investigative efforts will serve to inspire others to test the claim that both deficient syntactic knowledge *and* deficient processing capacity underlie the child's non-target-like interpretation of StSR with *seem*-type verbs and SC with *promise*-type verbs until a relatively advanced age.

APPENDIX A

StSR English test items by condition

Condition	Test item	Adult judgment
Copula	The dog is definitely white.	TRUE
	The drummer is definitely tall.	TRUE
	The horse is definitely big.	TRUE
	The actress is definitely blonde.	TRUE
	The girl is definitely young.	TRUE
	The snake is definitely skinny.	TRUE
	The dog is definitely grey.	FALSE
	The drummer is definitely short.	FALSE
	The horse is definitely small.	FALSE
	The actress is definitely brunette.	FALSE
	The girl is definitely old.	FALSE
	The snake is definitely fat.	FALSE
Unraised	It seems that the dog is grey.	TRUE
	It seems that the drummer is short.	TRUE
	It seems that the horse is small.	TRUE
	It seems that the actress is brunette.	TRUE
	It seems that the girl is old.	TRUE
	It seems that the snake is fat.	TRUE
	It seems that the dog is white.	FALSE
	It seems that the drummer is tall.	FALSE
	It seems that the horse is big.	FALSE
	It seems that the actress is blonde.	FALSE
	It seems that the girl is young.	FALSE
	It seems that the snake is skinny.	FALSE
StSR *seem*, covert experiencer, TP	The dog definitely seems to be grey.	TRUE
	The drummer definitely seems to be short.	TRUE
	The horse definitely seems to be small.	TRUE
	The actress definitely seems to be brunette.	TRUE
	The girl definitely seems to be old.	TRUE
	The snake definitely seems to be fat.	TRUE
	The dog definitely seems to be white.	FALSE
	The drummer definitely seems to be tall.	FALSE
	The horse definitely seems to be big.	FALSE
	The actress definitely seems to be blonde.	FALSE
	The girl definitely seems to be young.	FALSE
	The snake definitely seems to be skinny.	FALSE

StSR *seem*, short experiencer, TP	The dog seems to the cat to be grey.	TRUE
	The drummer seems to the guitarist to be short.	TRUE
	The horse seems to the dog to be small.	TRUE
	The actress seems to the woman to be brunette.	TRUE
	The girl seems to the neighbor to be old.	TRUE
	The snake seems to the squirrel to be fat.	TRUE
	The dog seems to the cat to be white.	FALSE
	The drummer seems to the guitarist to be tall.	FALSE
	The horse seems to the dog to be big.	FALSE
	The actress seems to the woman to be blonde.	FALSE
	The girl seems to the neighbor to be young.	FALSE
	The snake seems to the squirrel to be skinny.	FALSE
StSR *seem*, long experiencer, TP	The dog seems to the cat with stripes to be grey.	TRUE
	The drummer seems to the guitarist with glasses to be short.	TRUE
	The horse seems to the dog with spots to be small.	TRUE
	The actress seems to the woman with the mic to be brunette.	TRUE
	The girl seems to the neighbor with the hat to be old.	TRUE
	The snake seems to the squirrel with the peanut to be fat.	TRUE
	The dog seems to the cat with stripes to be white.	FALSE
	The drummer seems to the guitarist with glasses to be tall.	FALSE
	The horse seems to the dog with spots to be big.	FALSE
	The actress seems to the woman with the mic to be blonde.	FALSE
	The girl seems to the neighbor with the hat to be young.	FALSE
	The snake seems to the squirrel with the peanut to be skinny.	FALSE

APPENDIX B

StSR Spanish test items by condition

Condition	Test item	Adult judgment
Copula	El perro es definitivamente blanco. 'The dog is definitely white.'	TRUE
	El batería es definitivamente alto. 'The drummer is definitely tall.'	TRUE
	El caballo es definitivamente grande. 'The horse is definitely big.'	TRUE
	La actriz es definitivamente rubia. 'The actress is definitely blonde.'	TRUE
	La chica es definitivamente joven. 'The girl is definitely young.'	TRUE
	La serpiente es definitivamente delgada. 'The snake is definitely skinny.'	TRUE
	El perro es definitivamente gris. 'The dog is definitely grey.'	FALSE
	El batería es definitivamente bajo. 'The drummer is definitely short.'	FALSE
	El caballo es definitivamente pequeño. 'The horse is definitely small.'	FALSE
	La actriz es definitivamente morena. 'The actress is definitely brunette.'	FALSE
	La chica es definitivamente vieja. 'The girl is definitely old.'	FALSE
	La serpiente es definitivamente gorda. 'The snake is definitely fat.'	FALSE
Unraised	Parece que el perro es gris. 'Is seems that the dog is grey.'	TRUE
	Parece que el batería es bajo. 'Is seems that the drummer is short.'	TRUE
	Parece que el caballo es pequeño. 'Is seems that the horse is small.'	TRUE
	Parece que la actriz es morena. 'Is seems that the actress is brunette.'	TRUE
	Parece que la chica es vieja. 'Is seems that the girl is old.'	TRUE
	Parece que la serpiente es gorda. 'Is seems that the snake is fat.'	TRUE
	Parece que el perro es blanco. 'Is seems that the dog is white.'	FALSE

	Parece que el batería es alto. 'Is seems that the drummer is tall.'	FALSE
	Parece que el caballo es grande. 'Is seems that the horse is big.'	FALSE
	Parece que la actriz es rubia. 'Is seems that the actress is blonde.'	FALSE
	Parece que la chica es joven. 'Is seems that the girl is young.'	FALSE
	Parece que la serpiente es delgada. 'Is seems that the snake is skinny.'	FALSE
StSR bare *parecer*, no experiencer, TP	El perro definitivamente parece ser gris. 'The dog definitely seems to be grey.'	TRUE
	El batería definitivamente parece ser bajo. 'The drummer definitely seems to be short.'	TRUE
	El caballo definitivamente parece ser pequeño. 'The horse definitely seems to be small.'	TRUE
	La actriz definitivamente parece ser morena. 'The actress definitely seems to be brunette.'	TRUE
	La chica definitivamente parece ser vieja. 'The girl definitely seems to be old.'	TRUE
	La serpiente definitivamente parece ser gorda. 'The snake definitely seems to be fat.'	TRUE
	El perro definitivamente parece ser blanco. 'The dog definitely seems to be white.'	FALSE
	El batería definitivamente parece ser alto. 'The drummer definitely seems to be tall.'	FALSE
	El caballo definitivamente parece ser grande. 'The horse definitely seems to be big.'	FALSE
	El actriz definitivamente parece ser rubia. 'The actress definitely seems to be blonde.'	FALSE
	El chica definitivamente parece ser joven. 'The girl definitely seems to be young.'	FALSE
	La serpiente definitivamente parece ser delgada. 'The snake definitely seems to be skinny.'	FALSE
StSR bare *parecer*, no experiencer, AP	El perro definitivamente parece gris. 'The dog definitely seems (to be) grey.'	TRUE
	El batería definitivamente parece bajo. 'The drummer definitely seems (to be) short.'	TRUE
	El caballo definitivamente parece pequeño. 'The horse definitely seems (to be) small.'	TRUE
	La actriz definitivamente parece morena. 'The actress definitely seems (to be) brunette.'	TRUE
	La chica definitivamente parece vieja. 'The girl definitely seems (to be) old.'	TRUE
	La serpiente definitivamente parece gorda. 'The snake definitely seems (to be) fat.'	TRUE

	El perro definitivamente parece blanco. 'The dog definitely seems (to be) white.'	FALSE
	El batería definitivamente parece alto. 'The drummer definitely seems (to be) tall.'	FALSE
	El caballo definitivamente parece grande. 'The horse definitely seems (to be) big.'	FALSE
	La actriz definitivamente parece rubia. 'The actress definitely seems (to be) blonde.'	FALSE
	La chica definitivamente parece joven. 'The girl definitely seems (to be) young.'	FALSE
	La serpiente definitivamente parece delgada. 'The snake definitely seems (to be) skinny.'	FALSE
StSR opinion *parecer*, short experiencer, AP	El perro le parece al gato gris. 'The dog seems to the cat (to be) grey.'	TRUE
	El batería le parece al guitarrista bajo. 'The drummer seems to the guitarist (to be) short.'	TRUE
	El caballo le parece al perro pequeño. 'The horse seems to the dog (to be) small.'	TRUE
	La actriz le parece a la mujer morena. 'The actress seems to the woman (to be) brunette.'	TRUE
	La chica le parece a la vecina vieja. 'The girl seems to the neighbor (to be) old.'	TRUE
	La serpiente le parece a la ardilla gorda. 'The snake seems to the squirrel (to be) fat.'	TRUE
	El perro le parece al gato blanco. 'The dog seems to the cat (to be) white.'	FALSE
	El batería le parece al guitarrista alto. 'The drummer seems to the guitarist (to be) tall.'	FALSE
	El caballo le parece al perro grande. 'The horse seems to the dog (to be) big.'	FALSE
	La actriz le parece a la mujer rubia. 'The atress seems to the woman (to be) blonde.'	FALSE
	La chica le parece a la vecina joven. 'The girl seems to the neighbor (to be) young.'	FALSE
	La serpiente le parece a la ardilla delgada. 'The snake seems to the squirrel (to be) skinny.'	FALSE
StSR opinion *parecer*, long experiencer, AP	El perro le parece al gato con rayas gris. 'The dog seems to the cat with stripes (to be) grey.'	TRUE
	El batería le parece al guitarrista con gafas bajo. 'The drummer seems to the guitarist with glasses (to be) short.'	TRUE
	El caballo le parece al perro con manchas pequeño. 'The horse seems to the dog with spots (to be) small.'	TRUE
	La actriz le parece a la mujer con el micro morena. 'The actress seems to the woman with the mic (to be) brunette.'	TRUE

La chica le parece a la vecina con el sombrero vieja. 'The girl seems to the neighbor with the hat (to be) old.'	TRUE
La serpiente le parece a la ardilla con el cacahuete gorda. 'The snake seems to the squirrel with the peanut (to be) fat.'	TRUE
El perro le parece al gato con rayas blanco. 'The dog seems to the cat with stripes (to be) grey.'	FALSE
El batería le parece al guitarrista con gafas alto. 'The drummer seems to the guitarist with glasses (to be) tall.'	FALSE
El caballo le parece al perro con manchas grande. 'The horse seems to the dog with spots (to be) big.'	FALSE
La actriz le parece a la mujer con el micro rubia. 'The actress seems to the woman with the mic (to be) blonde.'	FALSE
La chica le parece a la vecina con el sombrero joven. 'The girl seems to the neighbor with the hat (to be) young.'	FALSE
La serpiente le parece a la ardilla con el cacahuete delgada. 'The snake seems to the squirrel with the peanut (to be) skinny.'	FALSE

APPENDIX C

SC English test items by condition

Condition	Test item	Adult judgment
OC *tell*	The policeman seriously tells the firefighter to climb the ladder.	TRUE
	The wizard seriously tells the knight to protect the castle.	TRUE
	The cowboy seriously tells the farmer to feed the horse.	TRUE
	The vet seriously tells the girl to walk the dog.	TRUE
	The pianist seriously tells the singer to practice the song.	TRUE
	The cook seriously tells the waitress to lock the door.	TRUE
	The firefighter seriously tells the policeman to climb the ladder.	FALSE
	The knight seriously tells the wizard to protect the castle.	FALSE
	The farmer seriously tells the cowboy to feed the horse.	FALSE
	The girl seriously tells the vet to walk the dog.	FALSE
	The singer seriously tells the pianist to practice the song.	FALSE
	The waitress seriously tells the chef to lock the door.	FALSE
SC *promise*, no benefactive	The firefighter seriously promises to climb the ladder.	TRUE
	The knight seriously promises to protect the castle.	TRUE
	The farmer seriously promises to feed the horse.	TRUE
	The girl seriously promises to walk the dog.	TRUE
	The singer seriously promises to practice the song.	TRUE
	The waitress seriously promises to lock the door.	TRUE
	The policeman seriously promises to climb the ladder.	FALSE
	The wizard seriously promises to protect the castle.	FALSE
	The cowboy seriously promises to feed the horse.	FALSE
	The vet seriously promises to walk the dog.	FALSE
	The pianist seriously promises to practice the song.	FALSE
	The cook seriously promises to lock the door.	FALSE
SC *promise*, short benefactive	The firefighter promises the policeman to climb the ladder.	TRUE
	The knight promises the wizard to protect the castle.	TRUE
	The farmer promises the cowboy to feed the horse.	TRUE
	The girl promises the vet to walk the dog.	TRUE
	The singer promises the pianist to practice the song.	TRUE
	The waitress promises the cook to lock the door.	TRUE
	The policeman promises the firefighter to climb the ladder.	FALSE
	The wizard promises the knight to protect the castle.	FALSE
	The cowboy promises the farmer to feed the horse.	FALSE
	The vet promises the girl to walk the dog.	FALSE

	The pianist promises the singer to practice the song.	FALSE
	The cook promises the waitress to lock the door.	FALSE
SC *promise*, long benefactive	The firefighter promises the policeman with the scarf to climb the ladder.	TRUE
	The knight promises the wizard with the cape to protect the castle.	TRUE
	The farmer promises the cowboy with the beard to feed the horse.	TRUE
	The girl promises the vet with the hat to walk the dog.	TRUE
	The singer promises the pianist with the bun to practice the song.	TRUE
	The waitress promises the cook with the apron to lock the door.	TRUE
	The policeman promises the firefighter with the scarf to climb the ladder.	FALSE
	The wizard promises the knight with the cape to protect the castle.	FALSE
	The cowboy promises the farmer with the beard to feed the horse.	FALSE
	The vet promises the girl with the hat to walk the dog.	FALSE
	The pianist promises the singer with the bun to practice the song.	FALSE
	The cook promises the waitress with the apron to lock the door.	FALSE
SC *swear*, no benefactive	The firefighter seriously swears to climb the ladder.	TRUE
	The knight seriously swears to protect the castle.	TRUE
	The farmer seriously swears to feed the horse.	TRUE
	The girl seriously swears to walk the dog.	TRUE
	The singer seriously swears to practice the song.	TRUE
	The waitress seriously swears to lock the door.	TRUE
	The policeman seriously swears to climb the ladder.	FALSE
	The wizard seriously swears to protect the castle.	FALSE
	The cowboy seriously swears to feed the horse.	FALSE
	The vet seriously swears to walk the dog.	FALSE
	The pianist seriously swears to practice the song.	FALSE
	The cook seriously swears to lock the door.	FALSE
SC *swear*, short benefactive	The firefighter swears to the policeman to climb the ladder.	TRUE
	The knight swears to the wizard to protect the castle.	TRUE
	The farmer swears to the cowboy to feed the horse.	TRUE
	The girl swears to the vet to walk the dog.	TRUE
	The singer swears to the pianist to practice the song.	TRUE
	The waitress swears to the cook to lock the door.	TRUE
	The policeman swears to the firefighter to climb the ladder.	FALSE
	The wizard swears to the knight to protect the castle.	FALSE

The cowboy swears to the farmer to feed the horse.	FALSE
The vet swears to the girl to walk the dog.	FALSE
The pianist swears to the singer to practice the song.	FALSE
The cook swears to the waitress to lock the door.	FALSE

APPENDIX D

SC Spanish test items by condition

Condition	Test item	Adult judgment
OC *ordenar*	El policía ordena seriamente al bombero subir la escalera. *'The policeman seriously orders the firefighter to climb the ladder.'*	TRUE
	El mago ordena seriamente al caballero defender el castillo. *'The wizard seriously orders the knight to defend the castle.'*	TRUE
	El vaquero ordena seriamente al granjero alimentar al caballo. *'The cowboy seriously orders the farmer to feed the horse.'*	TRUE
	La veterinaria ordena seriamente a la chica pasear al perro. *'The vet seriously orders the girl to walk the dog.'*	TRUE
	La pianista ordena seriamente a la cantante practicar la canción. *'The pianist seriously orders the singer to practice the song.'*	TRUE
	La cocinera ordena seriamente a la camarera cerrar la puerta. *'The cook seriously orders the waitress to lock the door.'*	TRUE
	El bombero ordena seriamente al policía subir la escalera. *'The firefighter seriously orders the policeman to climb the ladder.'*	FALSE
	El caballero ordena seriamente al mago defender el castillo. *'The knight seriously orders the wizard to defend the castle.'*	FALSE
	El granjero ordena seriamente al vaquero alimentar al caballo. *'The farmer seriously orders the cowboy to feed the horse.'*	FALSE
	La chica ordena seriamente a la veterinaria pasear al perro. *'The girl seriously orders the vet to walk the dog.'*	FALSE
	La cantante ordena seriamente a la pianista practicar la canción. *'The singer seriously orders the pianist to practice the song.'*	FALSE
	La camarera ordena seriamente a la cocinera cerrar la puerta. *'The waitress seriously orders the cook to lock the door.'*	FALSE
SC *prometer*, no benefactive	El bombero promete seriamente subir la escalera. *'The firefighter seriously promises to climb the ladder.'*	TRUE
	El caballero promete seriamente defender el castillo. *'The knight seriously promises to defend the castle.'*	TRUE
	El granjero promete seriamente alimentar al caballo. *'The farmer seriously promises to feed the horse.'*	TRUE
	La chica promete seriamente pasear al perro. *'The girl seriously promises to walk the dog.'*	TRUE

	La cantante promete seriamente practicar la canción. *'The singer seriously promises to practice the song.'*	TRUE
	La camarera promete seriamente cerrar la puerta. *'The waitress seriously promises to lock the door.'*	TRUE
	El policía promete seriamente subir la escalera. *'The policeman seriously promises to climb the ladder.'*	FALSE
	El mago promete seriamente defender el castillo. *'The wizard seriously promises to defend the castle.'*	FALSE
	El vaquero promete seriamente alimentar al caballo. *'The cowboy seriously promises to feed the horse.'*	FALSE
	La veterinaria promete seriamente pasear al perro. *'The vet seriously promises to walk the dog.'*	FALSE
	La pianista promete seriamente practicar la canción. *'The pianist seriously promises to practice the song.'*	FALSE
	La cocinera promete seriamente cerrar la puerta. *'The cook seriously promises to lock the door.'*	FALSE
SC *prometer*, short benefactive	El bombero promete seriamente al policía subir la escalera. *'The firefighter seriously promises the policeman to climb the ladder.'*	TRUE
	El caballero promete seriamente al mago defender el castillo. *'The knight seriously promises the wizard to defend the castle.'*	TRUE
	El granjero promete seriamente al vaquero alimentar al caballo. *'The farmer seriously promises the cowboy to feed the horse.'*	TRUE
	La chica promete seriamente a la veterinaria pasear al perro. *'The girl seriously promises the vet to walk the dog.'*	TRUE
	La cantante promete seriamente a la pianista practicar la canción. *'The singer seriously promises the pianist to practice the song.'*	TRUE
	La camarera promete seriamente a la cocinera cerrar la puerta. *'The waitress seriously promises the cook to lock the door.'*	TRUE
	El policía promete seriamente al bombero subir la escalera. *'The policeman seriously promises the firefighter to climb the ladder.'*	FALSE
	El mago promete seriamente al caballero defender el castillo. *'The wizard seriously promises the knight to defend the castle.'*	FALSE
	El vaquero promete seriamente al granjero alimentar al caballo. *'The cowboy seriously promises the farmer to feed the horse.'*	FALSE

	La veterinaria promete seriamente a la chica pasear al perro. *'The vet seriously promises the girl to walk the dog.'*	FALSE
	La pianista promete seriamente a la cantante practicar la canción. *'The pianist seriously promises the singer to practice the song.'*	FALSE
	La cocinera promete seriamente a la camarera cerrar la puerta. *'The cook seriously promises the waitress to lock the door.'*	FALSE
SC *prometer*, long benefactive	El bombero promete seriamente al policía con la bufanda subir la escalera. *'The firefighter seriously promises the policeman with the scarf to climb the ladder.'*	TRUE
	El caballero promete seriamente al mago con la capa defender el castillo. *'The knight seriously promises the wizard with the cape to defend the castle.'*	TRUE
	El granjero promete seriamente al vaquero con la barba alimentar al caballo. *'The farmer seriously promises the cowboy with the beard to feed the horse.'*	TRUE
	La chica promete seriamente a la veterinaria con el gorro pasear al perro. *'The girl seriously promises the vet with the hat to walk the dog.'*	TRUE
	La cantante promete seriamente a la pianista con el moño practicar la canción. *'The singer seriously promises the pianist with the bun to practice the song.'*	TRUE
	La camarera promete seriamente a la cocinera con el delantal cerrar la puerta. *'The waitress seriously promises the cook with the apron to lock the door.'*	TRUE
	El policía promete seriamente al bombero con la bufanda subir la escalera. *'The policeman seriously promises the firefighter with the scarf to climb the ladder.'*	FALSE
	El mago promete seriamente al caballero con la capa defender el castillo. *'The wizard seriously promises the knight with the cape to defend the castle.'*	FALSE
	El vaquero promete seriamente al granjero con la barba alimentar al caballo. *'The cowboy seriously promises the farmer with the beard to feed the horse.'*	FALSE
	La veterinaria promete seriamente a la chica con el goroo	FALSE

	pasear al perro. *'The vet seriously promises the girl with the hat to walk the dog.'*	
	La pianista promete seriamente a la cantante con el moño practicar la canción. *'The pianist seriously promises the singer with the bun to practice the song.'*	FALSE
	La cocinera promete seriamente a la camarera con el delantal cerrar la puerta. *'The cook seriously promises the waitress with the apron to lock the door.'*	FALSE
SC *jurar*, no benefactive	El bombero jura seriamente subir la escalera. *'The firefighter seriously swears to climb the ladder.'*	TRUE
	El caballero jura seriamente defender el castillo. *'The knight seriously swears to defend the castle.'*	TRUE
	El granjero jura seriamente alimentar al caballo. *'The farmer seriously swears to feed the horse.'*	TRUE
	La chica jura seriamente pasear al perro. *'The girl seriously swears to walk the dog.'*	TRUE
	La cantante jura seriamente practicar la canción. *'The singer seriously swears to practice the song.'*	TRUE
	La camarera jura seriamente cerrar la puerta. *'The waitress seriously swears to lock the door.'*	TRUE
	El policía jura seriamente subir la escalera. *'The policeman seriously swears to climb the ladder.'*	FALSE
	El mago jura seriamente defender el castillo. *'The wizard seriously swears to defend the castle.'*	FALSE
	El vaquero jura seriamente alimentar al caballo. *'The cowboy seriously swears to feed the horse.'*	FALSE
	La veterinaria jura seriamente pasear al perro. *'The vet seriously swears to walk the dog.'*	FALSE
	La pianista jura seriamente practicar la canción. *'The pianist seriously swears to practice the song.'*	FALSE
	La cocinera jura seriamente cerrar la puerta. *'The cook seriously swears to lock the door.'*	FALSE
SC *jurar*, short benefactive	El bombero jura seriamente al policía subir la escalera. *'The firefighter seriously swears to the policeman to climb the ladder.'*	TRUE
	El caballero jura seriamente al mago defender el castillo. *'The knight seriously swears to the wizard to defend the castle.'*	TRUE
	El granjero jura seriamente al vaquero alimentar al caballo. *'The farmer seriously swears to the cowboy to feed the horse.'*	TRUE
	La chica jura seriamente a la veterinaria pasear al perro. *'The girl seriously swears to the vet to walk the dog.'*	TRUE

La cantante jura seriamente a la pianista practicar la canción. *'The singer seriously swears to the pianist to practice the song.'*	TRUE
La camarera jura seriamente a la cocinera cerrar la puerta. *'The waitress seriously swears to the cook to lock the door.'*	TRUE
El policía jura seriamente al bombero subir la escalera. *'The policeman seriously swears to the firefighter to climb the ladder.'*	FALSE
El mago jura seriamente al caballero defender el castillo. *'The wizard seriously swears to the knight to defend the castle.'*	FALSE
El vaquero jura seriamente al granjero alimentar al caballo. *'The cowboy seriously swears to the farmer to feed the horse.'*	FALSE
La veterinaria jura seriamente a la chica pasear al perro. *'The vet seriously swears to the girl to walk the dog.'*	FALSE
La pianista jura seriamente a la cantante practicar la canción. *'The pianist seriously swears to the singer to practice the song.'*	FALSE
La cocinera jura seriamente a la camarera cerrar la puerta. *'The cook seriously swears to the waitress to lock the door.'*	FALSE

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