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The use of control information in dependency formation: an eye-tracking study

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Abstract

Recent research has shown much evidence that sentence comprehension can be extremely predictive. However, we currently know little about the limits of predictive processing. In the two eye-tracking experiments, we examined whether predictive information in dependency formation is inevitably given priority over a well-known structural preference in syntactic ambiguity resolution. Experiment 1 used sentences including control nouns like *order*, (e.g. *After Andrew's order to wash the kids came over to the house*). If predictive dependency information is given priority over disambiguation preferences, then readers could immediately interpret *the kids* as the ones who have been ordered to wash, thus avoiding the garden path at the main verb *came.* However, garden path effects were found irrespective of control information, although the garden path difficulty was reduced when the lexical control information highlighted the globally correct analysis (as in the above example), relative to when it did not. Experiment 2 replicated these results with *adjunct control*, where the relevant dependency is obligatory (e.g. *After refusing to wash the kids came over to the house*). Again, control information did not influence initial disambiguation, but did affect the difficulty of garden path recovery. Overall, the results suggest that there are limitations on the influence of predictive dependency formation on on-line structural disambiguation.

***Keywords*:** nominal control construction; adjunct control construction; PRO; cataphora; structural ambiguity resolution; eye-tracking

Introduction

Recent research has shown much evidence that sentence comprehension can be extremely predictive; incoming words and phrases are matched against expectations based on top-down information, rather than always being recognized in a purely bottom-up manner (see Altmann & Kamide 1999; Federmeier 2007; Federmeier & Kutas 1999; Kamide et al. 2003a; Kamide et al. 2003b; Lau et al. 2006; McRae et al. 2005; Phillips 2006; Staub & Clifton Jr. 2006; Van Berkum et al. 2005; Wagers & Phillips 2009; Wlotko & Federmeier 2007 among others). However, despite the wealth of evidence that predictive processing takes place, we currently know little about its limits. Are predictions always used in situations where the relevant information is available, and how much priority is given to predictive information in the comprehension process? The present paper examines the limits of predictive processing in relation to syntactic dependency formation. In the two eye-tracking experiments reported below, we examine whether predictive information is inevitably given priority over well-known structural preferences in syntactic ambiguity resolution.

The idea of predictive (or *active*) dependency formation can be illustrated in relation to sentence in (1), which requires a dependency relation between the reflexive pronoun *himself* and its antecedent *the king*.

(1) After reminding **himself** about the letter, **the king** immediately went to the meeting at the office.

The claim that a dependency is formed *predictively* implies that, once the left-hand element of the dependency (e.g. *himself* in (1)) has been encountered in the input, the parser predicts features of the right-hand element, and then actively attempts to match these features with subsequent input, with the dependency being formed when the match occurs. For example, in (1), the parser might predict a subject noun phrase with a masculine feature, based on the masculine reflexive *himself,* and this matches with the features of *the king* when it appears in the input. In fact, as we discuss below, there is considerable evidence that predictive mechanisms are used in the processing of a number of types of dependency.

One way in which active dependency formation may affect sentence processing is to pre-activate features of a predicted phrase (for example, its gender or number) before that phrase is reached in the input. For example, the pre-activation of features has been argued to apply to the processing of backwards anaphora, where a pronoun precedes its antecedent, as in (2) below:

(2) van Gompel and Liversedge (2003), Experiment 1

a. When **he** was fed up, **the boy** visited the girl very often. (gender matched)

b. When **he** was fed up, **the girl** visited the boy very often. (gender mismatched)

A number of researchers have found evidence for a *mismatch cost* in the processing of backwards anaphora: processing difficulty is found when the features of the first available antecedent (e.g. *the girl*) are not compatible with those of the preceding pronoun (e.g. *he*), as in (2)-(b), relative to when the features match, as in (2)-(a) (see Kazanina, Lau, Lieberman, Yoshida, & Phillips, 2007; van Gompel & Liversedge, 2003). Kazanina et al (2007) argued that this effect is due to the processor predicting the features of the antecedent in advance.

Converging evidence for active dependency formation comes from a study reported by Kreiner, Sturt, & Garrod (2008). Their experiments involved gender role nouns that were either definitional (e.g. *king*; being male is part of the word's definition), or stereotypical (e.g. *minister*; the role is typically filled by a male, but this is not by definition), and preceding gender-matched (e.g. *himself*) and mismatched (e.g. *herself*) reflexives. In their Experiment 2, they examined backwards anaphor dependencies, as in (3):

(3) Kreiner et al. (2008), Experiment 2

After reminding **himself/herself** about the letter, **the minister/king** immediately went to the meeting at the office.

The results showed a gender mismatch cost for definitional gender nouns at the critical word (e.g. *king*), but there was no such gender mismatch cost for stereotypical nouns (e.g. *minister*). This pattern is consistent with the idea that the gender feature of the matrix subject is predicted actively in advance, based on the information in the preceding subordinate clause (i.e. the gender of *minister,* whether male or female, is specified by the form of the reflexive; *himself* or *herself*). Kreiner et al. argued that this prediction allowed the strereotypical noun *minister* to be immediately integrated without the need to infer the stereotypical gender information, leading to the lack of a mismatch effect for this condition.

A second, and stronger, influence that active dependency formation may have on sentence processing is that it may change the priorities in syntactic ambiguity resolution. This idea has been explored particularly in studies of filler-gap processing with wh-dependencies. For example, in filler-gap sentences, such as (4)-(a) below, much evidence suggests that when a displaced element, such as *who* in (4)-(a) is detected, the parser prioritizes the postulation of the corresponding gap (___) above other structure-building options, without waiting for specific bottom-up information to confirm this prediction. In doing so, the parser predicts the appropriate grammatical position for the gap. This prioritization of gap-filling is known as the *Active filler strategy* (Frazier & Clifton, 1989; Boland, Tanenhaus, Garnsey, & Carlson, 1995; Garnsey, Tanenhaus, & Chapman, 1989; Pickering & Traxler, 2003; Traxler & Pickering, 1996). Thus, when there is an overt pronoun, such as *us*, in the presumed gap position (i.e. object of *bring*), processing difficulty occurs, as manifested by slower reading times at *us* in (4)-(a) than in (4)-(b) (*Filled-gap effects*: Crain & Fodor, 1985; Stowe, 1986).

(4) Filled-gap experiment sentences (Stowe, 1986)

a. My brother wanted to know who Ruth will bring us home to __ at Christmas.

b. My brother wanted to know if Ruth will bring us home to Mom at Christmas.

This Active filler strategy has been argued to interact with structural ambiguity resolution, and can override otherwise strong structural preferences. For example, in (5), both interpretations (5)-(b) and (5)-(c) are logically possible depending on the attachment of *Mary.* According to *Late closure,* and other recency-based heuristics, *Mary* should be attached as the direct object of *tell* as in (5)-(c), and this results in the interpretation of the sentence as a question about which person left the country (according to what Fred told Mary). On the other hand, according to the Active filler strategy, *Mary* has to be attached as the subject of the complement clause as in (5)-(b), as the direct object position of *tell* has already been predicted as the gap position, and is therefore not available for *Mary*. The sentence would then be interpreted as a question about which person Fred told about Mary's leaving the country.

(5) Structurally ambiguous sentence with anaphoric dependency (Frazier & Clifton, 1989)

a. Who did Fred tell Mary left the country?

b. Who$_i$ did Fred tell ___$_i$ Mary left the country?

c. Who$_i$ did Fred tell Mary ___$_i$ left the country?

Frazier and Clifton (1989) argue, on the basis of intuition, that the reading of (5)-(b) is strongly preferred. If Frazier and Clifton's intuitions are correct, this suggests that the Active filler strategy takes priority over Late closure.

The claim that the Active filler strategy can override structural preferences is further supported by a recent self-paced reading study by Cai, Sturt, & Pickering (2013) (although this was not their main research objective). Cai et al. (2013) examined sentences like (6) below:

(6) Experimental sentences in Cai et al. (2013)

a. Object cleft/ no comma

Because it was John that Ralph threatened the neighbour recorded their conversation.

b. Object cleft/with comma

Because it was John that Ralph threatened, the neighbour recorded their conversation.

Similarly to Frazier and Clifton's (1989) example, the attachment of *the neighbour* could be influenced by both the Active filler strategy and Late closure. If Late closure has priority, *the neighbour* should be attached as the object of *threatened,* while if the Active filler strategy has priority, this position would already have been predicted as the gap-site, for the clefted *John.* If Late closure had been the preferred strategy, the initial misattachment of *the neighbour* as the object of *threatened* should have led to a garden path effect at the main verb *recorded,* with longer reading times for the no-comma condition (6)-(a) relative to the comma condition (6)-(b). However, this garden path effect at the main verb was not found. This suggests that, by the time the verb had been reached, participants had settled on *John* as the direct object of the subordinate verb *threatened,* in accordance with Active filler strategy, and this analysis prevented the overt noun phrase *the neighbor* from being attached as the object of *threatened.* This was despite the fact that there was evidence that the Late closure analysis was considered during processing before the disambiguating main verb was reached in the input: *the neighbor* in (6)-(a) was read more slowly than relevant controls, suggesting that the two strategies might have momentarily conflicted with each other, causing competition at the ambiguous NP position.

The conclusion that the Active Filler strategy may override Late Closure is particularly striking given that Late Closure is known to be a very strong preference, and is hard to override with competing constraints[1]. For example Pickering, Traxler and Crocker (2000) found evidence for Late Closure even when this structural preference contradicted lexical preferences that went in the opposite direction. Thus, in their Experiment 3, Pickering et al (2000) used verbs like *cheered,* that are statistically biased against taking a direct object, and yet they found that Late Closure led to such verbs being treated as transitive, leading to garden path effects in sentences using such as *While the supporters cheered the team that came in bothered the coaching staff.*

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[1] The strength of Late Closure as a preference applies particularly to examples where the two competing attachment sites are in different clauses, as in (5) and (6). The preference is much less strong, and more variable, in cases where the relevant attachment sites are in the same clause (see, for example, Cuetos & Mitchell, 1988).

Taken together, the evidence discussed above suggests that the Active Filler Strategy may indeed override strong structural preferences, even when such preferences cannot be overridden by other constraints such as lexical subcategorization biases. In this paper, we use the term *Strongly active processing* to describe the type of processing behavior where the requirement to form an active dependency takes priority over other aspects of processing, and can override the preferences that would normally apply to syntactic ambiguity resolution. As seen above, previous evidence for Strongly active processing has been mainly discussed in relation to filler-gap dependencies, where the gap position is structurally licensed through a *wh*-dependency with a preceding filler, as in examples (4,5,6) above. However, in order to judge whether Strongly active processing is a general feature of human sentence processing, we need to test it in relation with other types of dependency, which have different properties.

In this paper, we consider the generality of Strongly active processing, by examining whether it applies to two different types of control constructions in English, namely Nominal control (Experiment 1), and Adjunct control (Experiment 2). Nominal control, which we examine in Experiment 1, is illustrated in (7):

(7) a. Giver control

a. After $John_i$'s promise/vow/offer/guarantee/obligation/pledge/oath/commitment $PRO_{i/*j}$ to stop, the $teenagers_j$ felt nervous on the bus.

b. Recipient control

After $John_i$'s order/instructions/encouragement/reminder/invitation $PRO_{*i/j}$ to stop, the $teenagers_j$ felt nervous on the bus.

The nominal control construction is analyzed as a semantic relation between a control nominal (i.e. a noun such as *promise, order, vow*) and PRO (the phonologically unexpressed subject of *to stop*). The interpretation of PRO is determined by thematic roles regardless of syntactic structures or positions (Culicover & Jackendoff, 2001, 2006). For example, the sentences in (7) above are analyzed based on the semantic roles of the control nominals. That is, the nominal control sentences involving control nouns like *promise, vow, offer, guarantee, obligation, pledge, oath,* and *commitment* as in (7)-(a) are analyzed as examples of the *giver control* construction, as the NPs receiving a giver theta role from the control nominals serve as the controller for PRO (for example, in (7a), the person who stops is the *giver* of the promise/vow/offer, etc). On the other hand, the nominal control sentences involving *order, instruction, encouragement, reminder,* and *invitation* as in (7)-(b) are analyzed as examples of the *recipient control* construction, as the NPs receiving a recipient theta role serve as the controller (in other words, in (7b), the person who stops is the *recipient* of the order/encouragement/reminder, etc). Accordingly, in (7)-(a), PRO is co-indexed with the preceding subject *John,* while in (7)-(b), PRO is coindexed with *the teenagers,* which follows PRO in the input.

Control dependencies such as those in (7) differ from *wh*-dependencies in various ways that allow a good testing of the limits of Strongly active processing. Recall from the discussion above that existing evidence for Strongly active processing has come from *wh*-dependencies, where the gap position is structurally predicted. It may be the case that structural licensing is a necessary property for a dependency to be processed in a strongly active way. It is therefore desirable to test whether Strongly active processing also applies to dependencies that do not share this property. Nominal control is a good test case, because proper interpretation of PRO is complex, with the position of the antecedent depending on semantic properties of the control nominal with which PRO is associated, rather than on structural information. In Experiment 1, we test the generality of Strongly active processing by examining whether dependency formation in nominal control sentences like (7a) and (7b) is prioritized over the application of the Late closure preference. A second way in which nominal control differs from *wh*-dependencies is that, given a *wh*-filler like *who,* the presence of a subsequent gap is obligatory*,* while PRO is not grammatically required to have an intra-sentential antecedent in nominal control. In Experiment 2, we examine the role of obligatoriness by examining another type of control dependency, namely *adjunct control,* where there is a much stronger requirement for an intra-sentential antecedent, relative to nominal control (we will introduce the details of adjunct control later).

To summarize, there is considerable evidence that the predictive mechanisms are used in the processing of a number of types of dependency. For example, semantic or structural features of an upcoming word are predicted in advance based on the information available already. Furthermore, in *wh* filler-gap sentences, predictive dependency formation has been reported to interact with structural disambiguation preferences, in a phenomenon that we have called *Strongly active processing*. In this paper, we use Control dependencies as a test-case to probe the generality of Strongly active processing. We employed an eye-tracking methodology in both experiments, as it provides detailed patterns of participants' eye-movements. With an extremely high-temporal resolution, the eye-tracking data are very helpful in understanding continuously unfolding structure building processes during sentence processing.

Experiment 1

The question that we address in Experiment 1 is whether control information can be accessed in on-line sentence processing and can reliably trigger Strongly active dependency formation. In fact, although there have not been many studies on the processing of the control construction (Bever & Sanz, 1997; Betancort, Carreiras, & Acuña-Fariña, 2006; Boland, Tanenhaus, & Garnsey, 1990; Demestre, Meltzer, García-Albea, & Vigil, 1999; Featherston, Gross, Münte, Clahsen, 2000; Frazier, Clifton & Randall, 1983; Kwon, Monahan, & Polinsky, 2010; Kwon & Sturt, ms; Mauner, Tanenhaus, & Carlson, 1995; McElree & Bever, 1989; Nicol, 1988; Sag & Fodor, 1995; Walenski, 2002), previous results suggest that control information is activated early enough to constrain long-distance dependencies. For example, using a word-by-word self-paced stop-making-sense task, Boland et al. (1990) reported the evidence of early use of control information in dependency formation. Their experimental stimuli included two potential controllers for PRO (e.g. plausible controller: *the outlaw*; implausible controller: *the horse* in (8)), which appeared in a position that is linearly close to or far away from PRO.

(8) Boland et al. (1990), Experiment 1

a. The cowboy$_i$ signaled the horse$_j$ PRO$_{*i/j}$ to surrender to the authorities.

b. The cowboy$_i$ signaled the outlaw$_j$ PRO$_{*i/j}$ to surrender to the authorities.

c. Which horse$_j$ did the cowboy$_i$ signal PRO$_{*i/j}$ to surrender to the authorities?

d. Which outlaw$_j$ did the cowboy$_i$ signal PRO$_{*i/j}$ to surrender to the authorities?

The results showed the implausible controller condition elicited more processing effort than the plausible controller condition immediately after the infinitival verb, suggesting that control verb information is available early enough in on-line sentence processing and can be used to constrain initial long-distance dependency formation (see also Bever & Sanz, 1997; Betancort et al. 2006; Demestre, et al., 1999; Kwon & Sturt, ms; Mauner et al., 1995; but cf. Frazier et al, 1983).

Now going back to the goals of Experiment 1, recall that our research question is whether active search based on control properties can override structural preferences. To this goal, we investigated the processing of the nominal control construction, manipulating control type (giver vs. recipient control: Culicover & Jackendoff, 2006) and ambiguity (temporarily ambiguous vs. unambiguous), as shown in (9).

(9) Experiment 1 sample stimuli

a. Ambiguous giver control

Before Andrew$_i$'s refusal PRO$_{i/*k}$ to wash the kids$_k$ came over to the house.

b. Ambiguous recipient control

Before Andrew$_i$'s order PRO$_{*i/k}$ to wash the kids$_k$ came over to the house.

c. Unambiguous giver control

Before Andrew$_i$'s refusal to wash, the kids$_k$ came over to the house.

d. Unambiguous recipient control

Before Andrew$_i$'s order PRO$_{*i/k}$ to wash, the kids$_k$ came over to the house.

In both types of nominal control, the infinitival verb position (*to wash*) is the first position that PRO can be postulated, as the infinitival complement clause is syntactically optional in the sentences. However, while the dependency involving PRO is already complete in the giver control conditions at the infinitival verb position, it is incomplete in recipient control conditions. This is because in the giver conditions (9)-(a, c), *Andrew* carries a giver thematic role and it is already available at the infinitival verb position. Thus, PRO will be immediately assigned its controller (*Andrew*), completing the dependency at the position. On the other hand, in the recipient control conditions (9)-(b, d), there is no antecedent available for PRO at this position, leaving the dependency incomplete. Accordingly, we predict that i) there should be measurable processing difficulty in the recipient control conditions relative to the giver control conditions around the infinitival verb position, and that ii) an active search of an antecedent for PRO will be launched in the recipient control construction. Such a finding would be analogous to previous results reported in the literature for overt pronouns, where a pronominal element without referential information elicits processing difficulty and triggers a forward search of its potential antecedent (Filik and Sanford, 2008; Kazanina et al., 2007; van Gompel and Liversedge, 2003). More importantly, if active search based on control information interacts with structural disambiguation preferences (i.e. if processing is strongly active), it could resolve the temporary structural ambiguity of NP *the kids*. As *the kids* is the first NP that could serve as a controller in the recipient control construction, the parser could analyze *the kids* as the controller. However, *the kids* cannot be the controller for PRO in the direct object position of the infinitival clause verb. This is because a referential NP (*the kids*) cannot be structurally within the scope of a coindexed anaphoric expression (PRO) (Principle C) as shown in (10)-(b). If Strongly active processing is a general property of human sentence processing, the parser should prefer to resolve an incomplete dependency that results in the assignment of an antecedent to PRO as soon as possible, prioritizing this requirement over all other structure building operations. This would predict that *the kids* will be parsed as the main clause subject contra Late closure (Late closure: Frazier & Clifton, 1989) as shown in (10)-(a).

(10) Recipient control:

a. After Andrew's order [PRO$_k$ to wash] [the kids$_k$ came...

b. *After Andrew's order [PRO$_k$ to wash the kids$_k$] came...

If so, it is predicted that the ambiguous recipient condition (9)-(b) will not show a garden path effect (i.e. ambiguity cost) at the disambiguating main clause verb position in comparison to its unambiguous counterpart (9)-(d). In contrast, all theories would predict that the ambiguous giver condition (9)-(a) will elicit a garden path effect in comparison to its unambiguous counterpart (9)-(c). This would mean that, in the ambiguous giver condition (9)-(a), because PRO will already have taken *Andrew* as its antecedent, the locally ambiguous region *the kids* will be incorrectly parsed as the direct object of the preceding infinitival verb in accordance with Late closure, leading to the necessary structural revision of *the kids* at the disambiguating main verb. Thus, if Strongly active processing is applied for nominal control dependencies, we expect an interaction between control type and ambiguity at the main verb, with an ambiguity cost for the giver-control conditions, and no such cost for the recipient-control conditions.

Alternatively, if Strongly active processing is not applied to nominal control dependencies, one of two patterns could be predicted at the main verb. According to one possibility, nominal control information might not influence initial ambiguity resolution, but might have an influence at a later stage, during the recovery process following the garden path. This account would predict that *the kids* would be taken as the object of *wash* regardless of nominal control type, leading to a garden path effect for both control types at the main verb. However, the recovery from the garden path would be eased in recipient control, where the main clause subject attachment of *the kids* would be facilitated, relative to giver control. This account predicts an interaction whereby the ambiguity cost for recipient control is reduced, but not eliminated, relative to giver control. A second possibility is that nominal control information is not used at any stage in on-line ambiguity resolution. This would predict an equal-sized garden path effect at the main verb for both control types.

Method

Participants

Thirty two native English speakers at Edinburgh University participated in the experiment. All had normal or corrected-to-normal vision. They received course credit for their participation.

Materials

There were 32 sets of experimental sentences. Four lists were created using a Latin square design. The stimuli are included in the Appendix. There were 72 fillers, including 36 from an unrelated experiment on number agreement. The experiment had four conditions as shown in (9) above. All the infinitival clause verbs (e.g. *to wash* in (9)) were statistically biased toward a low to mid transitive reading (Gahl, Jurafsky, & Roland, 2004). This was to allow us the maximum chance of observing the effects of control, without being swamped by effects of local ambiguity. Although the globally correct analysis of the sentence was always consistent with the intransitive structure analysis, the low to mid transitivity of verbs could create temporary ambiguity such that the following NP is interpreted either as direct object of the infinitival clause or as the main clause subject. Sentential complement (SC) ambiguity was minimized by using verbs with low SC bias (mean 2.4% usage for SC construction, as examined using Schulte im Walde, 1998). That is, those verbs which are likely to be used with a sentential complement (e.g. *pretend*: *She pretended that she didn't see me*) were excluded from the study.

The experimental sentence was divided into the following regions for the purpose of analysis:

Region 1: After Andrew's (start of sentence)

Region 2: refusal/order (control noun)

Region 3: to wash (*to* plus infinitive)

Region 4: the kids (temporarily ambiguous NP)

Region 5: came (critical main verb)

Region 6: over to the house (end of sentence)

Region 1 consisted of the initial preposition followed by a possessive name. Region 2 consisted of either giver (e.g. *refusal*) or recipient (e.g. *request*) control nouns. Region 3 consisted of the word *to* followed by the verb of the infinitival clause. In the unambiguous conditions, Region 3 also contained the comma. Region 4, the temporarily ambiguous noun phrase, consisted of the determiner *the* followed by a noun. Region 5, the disambiguating region, contained the main clause verb. Finally, Region 6 consisted of all the remaining words of the sentence.

Procedure

The experimental and filler sentences were pseudo-randomized such that no two items from the same experimental condition appeared in a row. In order to provide a minimal context, and to provide uniformity with the other study with which this experiment was run, each sentence was presented with a title on the first line, in upper case letters (the list of stimuli in the Appendix includes the titles that were used). The sentence was presented on the next line. There were three initial practice sentences. Participants read a single sentence while their eye movements were tracked with an EyeLink 1000 eye-tracker. The device was fully calibrated before the experiment began, and recalibration was performed as necessary through the experiment. The tracker sampled pupil location at a rate of 1000Hz. At the start of each trial, a black square appeared on the left central portion of the screen, marking the position of the first character of text. When the tracker successfully detected a fixation on this square, it was automatically replaced by the stimulus text. Participants were instructed to read each sentence at a natural pace. Comprehension questions were presented for half of all sentences, including fillers. Participants responded by pressing a left or right button. Subsequent analysis showed that mean comprehension accuracy was 90% (range 77%-100%).

Data analysis

An automatic procedure pooled short fixations. Before analysis, fixations of less than 80ms were incorporated into larger fixations within one character, and then any remaining fixations of less than 80ms were deleted. Fixations longer than 1200ms were also removed prior to analysis. Data will be reported for four eye-movement measures in all the regions from Region

2 onwards. First pass reading times are the sum of initial fixations in the region, following the first entry into the region, until the region is first exited, either to left or right. Go-past times (sometimes called Regression path times) are the sum of fixation from the first entry into the region from the left, until that region is first exited to the right – in other words, the time taken for the reader to go past the region following the first forward saccade into the region. First-pass regressions out is a measure of the percentage of trials in which the first exit of the region was to the left. For these three measures, if the relevant region was skipped in the first pass reading, the trial was excluded from the analysis, and the mean for that design cell was calculated from the remaining data. Total time is the sum of all fixations in the region. Trials in which the region was not fixated at all were not included in the analysis of Total time.

Data for the eye-movement measures were analyzed using Linear Mixed Effects Regression (LMER) analysis (Baayen, 2008; Baayen, Davidson, & Bates, 2008; Jaeger, 2008). The lme4 R package was used. For all eye-movement measures reported here, an LMER model was constructed, incorporating the fixed effects of Matching and Control and their interaction. The regressions out measure was analysed using a Generalized LME model, with a binomial link function. Factor labels were transformed into numerical values, and centered prior to analysis, so as to have a mean of 0 and a range of 1. This procedure minimizes collinearity between variables (Baayen, 2008), and in combination with sum coding of contrasts, allows coefficients to be interpreted in an analogous way to the main effects and interactions in an Analysis of Variance. The analysis yields coefficients, standard errors and t-values (z-values for the logit model) for each fixed effect and interaction. For the linear models, a given coefficient was judged to be significant at $\alpha = 0.05$ if the absolute value of t exceeded 2 (Baayen, 2008). For the binomial logit model, p-values were taken from the Z score. The regression models for both experiments reported here incorporated crossed random intercepts for participants and items. When justified by model comparison, random slope parameters were included (Baayen, 2008). Random slope parameters for main effects and interactions were added sequentially, first for participants, and then for items. The final model incorporated only those random slope parameters whose inclusion resulted in a better model fit than simpler models. As the procedure involves exploring the full space of random effect structures, it results in the maximal model that is justified by the data. Each successive pair of random-slope models was compared using a log-likelihood ratio test, evaluated against the χ^2 distribution, taking as the degrees of freedom the difference in number of parameters between the two models (see Baayen, 2008, p. 276). When the random slope term for the interaction was included, the model also included the random slope terms for the main effects (to satisfy the hierarchy principle). Where planned (paired) contrasts are reported following interactions, the analysis was carried out on the subset of data corresponding to the relevant pair of conditions. Region 2 (the control noun) differed between the recipient control and giver control conditions (e.g. *refusal* vs. *order*). The statistical model for this region included word length (in characters) and word form frequency (log transformed frequencies from the written portion of the British National Corpus[2]), and the interaction between these two variables. This was to control for any processing effects due to irrelevant lexical differences between the two control nouns.

[2] *The British National Corpus*, version 2 (BNC World). 2001. Distributed by Oxford University Computing Services on behalf of the BNC Consortium. URL: http://www.natcorp.ox.ac.uk/

Results and Discussion

Table 1 shows the empirical means for the four conditions from the critical Region 5, in addition to the preceding Regions 2, 3, 4, and the sentence final Region 6, in the four eye-movement measures. Table 2 shows the results of the LMER analysis for the main effects and interactions for the reading time measures. Table 3 shows the Generalized LMER analysis for the proportion of regressions out. Figures 1 and 2 show the means for the critical region for Go-past and Total time, respectively.

INSERT TABLE 1 ABOUT HERE

INSERT TABLE 2 ABOUT HERE

INSERT TABLE 3 ABOUT HERE

INSERT FIGURE 1 ABOUT HERE

INSERT FIGURE 2 ABOUT HERE

The results showed main effects of control and ambiguity, and an interaction of these two factors in several regions. In the following, we will discuss these effects in turn in relation to the experimental predictions.

Main effects of control

The main effect of control was significant in Go-past and marginally significant in First-pass regressions out in Region 3 (*to* plus infinitive), with the Recipient control taking longer to read, and eliciting more regressions out relative to Giver control. This effect could be related to an incomplete dependency in the Recipient control condition. As the infinitival complement clauses were optional, the infinitival verb is the first position that PRO can be postulated. In the Giver control condition, as a NP (*Andrew* in Region1) is already available for a giver thematic role at this position, it can be immediately assigned to PRO as its controller. On the other hand, in the Recipient control condition, the controller of PRO is yet to be identified. Consequently, the dependency between PRO and its controller is complete in the giver control but not in the recipient control condition at Region 3, and this incomplete dependency in the recipient control condition could have triggered more processing difficulty (see Filik & Sanford, 2008 for comparable effects with an overt pronominal; also see Chen, Gibson and Wolf, 2005, and Gibson1991, 1998, 2000, for a discussion of processing costs associated with an incomplete dependency). That is, the main effect of control in Region 3 could be taken to suggest that the control information constrains the interpretation of PRO during the early paring stages.

Main effects of ambiguity

There was a main effect of ambiguity for First-pass regressions out and Total time in Region 3 (to infinitival verb), in all the measures at Regions 4 (the temporarily ambiguous NP) and 5 (the critical main verb), and in Go-past at Region 6 (the end of the sentence). In all of these measures except for First-pass regressions out in Region 3, the ambiguous condition took reliably longer to read and elicited more regressions than the unambiguous condition. These higher processing costs in the ambiguous condition can be interpreted as a Garden path related to structural reanalysis; the temporarily ambiguous NP of Region 4 was initially misanalysed as the direct object of the preceding infinitival verb and later reanalyzed as the main clause subject when the disambiguating main verb became available. The garden path effect at Region 5 (the disambiguating main verb) is expected, but the ambiguity effect in First-pass, Go-past and First-pass regressions out at Region 4 seems to be a preview effect; although the main verb information does not become available until Region 5, if the following main clause verb *came* could have been previewed, this should have forced the parser to reanalyze *the kids* as the main clause subject, leading to a similar Garden path effect. More regressions at Region 4 could be also due to the lack of a comma in the ambiguity conditions. Previous studies showed that readers are less likely regress across a comma (Hirotani, Frazier, Rayner, 2006; Staub 2007). Likewise, readers in the current study could have regressed less across the clause boundary after a comma in the unambiguous conditions.[3] We note that a similar preview-like effect was observed by Slattery et al (2013), in the region before a disambiguating verb, in a related construction (see Slattery et al, Experiment 1). On the other hand, first-pass regressions out in Region 3 showed a reversed pattern with the unambiguous condition eliciting more regressions than the ambiguous condition, which did not seem to be related to structural reanalysis. We interpret this as a clause wrap-up effect associated with the comma in the unambiguous conditions (see also Rayner, Sereno, Morris, Schmauder, & Clifton, 1989).

The interaction of control and ambiguity

The interaction of control and ambiguity was found in First-pass regressions out at Region 4 (the temporarily ambiguous NP) and in all the measures of Region 5 (critical main verb) (although the effect was marginal in First-pass regressions out). The overall form of the interaction in these measures is such that the difference between ambiguous and unambiguous conditions (i.e. ambiguity cost) was larger for the giver control sentences than for the recipient control sentences. For example, the ambiguity cost at Region 5 was only marginal for Go-past (a

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[3] We thank an anonymous reviewer for bringing this alternative account to our attention.

difference of 82 msec; t=1.82) or non-reliable for First pass (-2ms, |t|<1) in the recipient control condition, while it was reliable for both measures in the giver control condition (Go-past: 218 msec, t=3.74, p <.05; First pass: 35 msec, t=2.89, p < .05). However, in the rest of the measures, the ambiguity cost was actually reliable for both types of control even though the difference is still larger for the giver control sentences: First-regressions out at Region 4 (giver control: a difference of 14.1%, Z=-4.49, p < .001; recipient control: 6.2%, Z=-2.08, p < .05) and Total Time and Regressions out at Region 5 (Total Time: Giver Control: 138 msec; t=4.95, p < .05; Recipient Control: 67 msec; t=2.18, p < .05; Regressions out: Giver control: 16%, Z=4.36, p < .001; Recipient Control: 8.8%, Z=2.19, p < .05).

We initially predicted that there should be processing difficulty in the recipient control conditions relative to the giver control conditions around the infinitival verb position (Region 3) due to the incomplete dependency in the former, and this result was found. We also predicted that the incomplete dependency could launch an active search of an antecedent for PRO. Although the main effect of control in Region 3 is not direct evidence for active search, it should be noted that the parser was sensitive to the control type at the first position that an incomplete dependency could be identified in the recipient control condition (Region 3). Given this, it is natural to assume that after registering the incomplete dependency and the lack of a suitable antecedent in the left context, the parser initiated a search for the missing antecedent at Region 3. Importantly, the Strongly active account also predicted that the active search of an antecedent for PRO could affect initial structural ambiguity resolution at a later sentential position. In fact, as presented above, there was indeed an interaction of control and ambiguity at Regions 5. However, the overall results do not seem consistent with the Strongly active processing hypothesis. The results for Regions 4 and 5 show evidence of a garden path effect for both control types, even though this was smaller for recipient control relative to giver control.

The earliest region that the ambiguity effect was found in first pass measures is Region 4. Although Region 5 was the disambiguating region, the early measures at Region 4 suggest that the garden path was initiated in this region, through preview, in at least some trials; there was a main effect of ambiguity in First-pass, Go-past and First-regressions out. In addition, although the interaction of control and ambiguity was found in the measure of First-pass regressions out at Region 4, the ambiguity cost was reliable for both types of control. If Region 4 was indeed the position that the structural ambiguity was resolved due to a preview effect, then the effects at Region 5 can be taken as reflecting further necessary structural revision. In fact, all the measures at Region 5 elicited both the main effect of ambiguity and the interaction of ambiguity and control type. Although First-pass reading times at Region 5 do not show reliable differences between the recipient ambiguous and unambiguous condition, all other three measures at the region (First-past regressions out, Go-past, Total times) showed that the ambiguity effect was visible even in the recipient ambiguous condition. These results suggest that active dependency formation based on control information is not a strong enough cue to prevent an initial structural misanalysis due to Late closure, but that it can aid the recovery process.

Thus far, we have examined the processing of giver and recipient nominal constructions to test the generality of Strongly active processing. The results clearly show that nominal control interacts with structural ambiguity; indeed, to our knowledge, this is the first demonstration of on-line effects of nominal control. However, the form of the interaction suggests that, contra the Strongly active processing hypothesis, nominal control information cannot prevent the initial application of the Late closure preference, even though it may be used at a later stage in processing.

The overall results from Experiment 1 are different from those suggested by previous studies on *wh* filler-gap dependencies (e.g. Cai et al, 2013). As mentioned in the introduction, there are various reasons why *wh*-dependencies might impose a stronger constraint than nominal control dependencies. One relevant difference is that a gap is obligatory, given a *wh*-filler, while nominal control does not impose a strong requirement for an intra-sentential antecedent of PRO. Consider again the recipient control condition from Experiment 1, repeated below as (11):

(11) Before Andrew's order PRO to wash(,) the kids came over to the house.

Although the phrase *order to wash* allows for an antecedent of PRO to be found later in the sentence (such as *the kids* in (11)), there is no strong requirement for this. For example, there is a possible interpretation of (11) in which somebody other than *the kids* is ordered to wash. Moreover, the sentence can continue without any explicit mention of an antecedent of PRO, as in (12):

(12) Before Andrew's order PRO to wash, the boiler stopped working.

Thus, the delayed use of control information in Experiment 1 might have been a consequence of the lack of a strong requirement for an intra-sentential antecedent of PRO. It might be the case that initial ambiguity resolution can only be influenced if the antecedent of cataphoric PRO is being obligatorily predicted at the point where the ambiguity first arises, and the recipient control condition of Experiment 1 might not have induced a strong enough requirement for an antecedent to allow prediction to take place.

We conducted Experiment 2 to evaluate the hypothesis that stronger interpretational constraints could potentially influence ambiguity resolution during the early parsing stages. To this aim, in Experiment 2 we included an adjunct control construction, another structure forming a dependency based on control information, but with stronger interpretational requirements than the recipient nominal control construction.

Experiment 2

The purpose of Experiment 2 was to re-examine the use of control information in structural disambiguation using a grammatical construction that places a stronger requirement for an intra-sentential antecedent of PRO. To this aim, we employed the adjunct control construction, as illustrated in (13):

(13) Before $\text{PRO}_{i/*k}$ failing to wash, the kids_i came over to the house.

The adjunct control construction illustrated in (13) requires an antecedent of PRO to appear in subject position of the main clause, and thus, *the kids* is the only possible referent of the subject of *failing to wash.* Moreover, the sentence becomes infelicitous when a plausible antecedent of PRO is not available in the main clause, as in (14), as attested in a norming study reported below.

(14) #Before PRO_i failing to wash, the boiler_k stopped working.

Experiment 2 therefore compared adjunct control with giver nominal control as a baseline, in a design that was similar to that of Experiment 1. Given the stronger constraints of adjunct control, we can then determine whether the delayed use of control information in structure disambiguation observed in Experiment 1 was due to the relatively weak constraints of recipient nominal control, or whether it was due to the more general characteristics of active dependency formation based on control information. Before proceeding with the experiment, we first ran a norming study to assess the interpretational requirements of the adjunct control construction.

Norming study

A norming study was carried out to examine the preferences for co-reference between PRO and the matrix subject in the three different control types, namely, giver control, recipient control, and adjunct control. This was done using a questionnaire in which participants had to judge the plausibility of sentences involving these control types. Thirty-six participants from the University of Edinburgh community were paid to participate in the study. The design crossed control type (adjunct vs. giver vs. recipient) with the animacy of the main clause subject (animate vs. inanimate). Thirty-six stimuli were created on the model of (15) below. Thirty-two of these sentences were adapted from those of Experiments 1 and 2, and four extra items were added, to allow a full rotation of the latin square for six conditions.

(15) Example sentences

(a) Adjunct control/animate

Before refusing to stop, the driver caused a lot of confusion.

(b) Adjunct control/inanimate

Before refusing to stop, the signpost caused a lot of confusion.

(c) Giver control/animate

Before Jeremy's refusal to stop, the driver caused a lot of confusion.

(d) Giver control/inanimate

Before Jeremy's refusal to stop, the signpost caused a lot of confusion.

(e) Recipient control/animate

Before Jeremy's order to stop, the driver caused a lot of confusion.

(f) Recipient control/inanimate

Before Jeremy's order to stop, the signpost caused a lot of confusion.

The stimuli were designed such that the main clause subject would always be a plausible referent of PRO when animate and implausible when inanimate. For example, in (15), it is plausible for a driver to refuse to stop, but implausible for a signpost to do so. The six conditions of each item were distributed across six lists such that, for any given item, only one condition appeared in each list, and overall, each list contained an equal number of all six conditions. The resulting stimuli were combined with 63 fillers, of varying plausibility, and were randomized such that no two experimental items appeared adjacent to each other. Three separate randomizations were used in the experiment. The randomized stimuli were printed onto paper booklets, in which participants were instructed to judge the naturalness of each sentence on a scale of 1 to 7 (with 1 equating to “very unnatural”, and 7 equating to “very natural”). The instructions pointed out that “naturalness” was to be interpreted as referring to how much sense each sentence makes, and participants were given two example sentences, one of which intuitively made a lot of sense, and the other very little sense.

We assumed that, for a given control type, the degree of preference to select the main clause subject as the referent of PRO would be related to the size of the difference in perceived plausibility between the animate and inanimate conditions. Specifically, a strong preference for coreference between PRO and the main clause subject should correspond to a large degradation in perceived plausibility for the inanimate, relative to the animate condition.

The giver control conditions (15)-(c,d) were included as a baseline, in which participants were not expected to interpret PRO as referring to the main clause subject. For example, in (15)-(c,d), as *refusal* is a giver control noun, people should interpret *Jeremy* as the person who refused to stop, and will therefore not attempt to interpret either *the driver* or *the signpost* as the referent for PRO. Thus, any plausibility differences between the two giver control conditions are likely to be due to differences in the plausibility of the main clause content, rather than due to differences in the plausibility of the main clause subject as the referent of PRO.

The adjunct control conditions, on the other hand, are expected to show a large plausibility difference. This is because, by hypothesis, the co-reference of PRO with the main clause subject is obligatory in the adjunct control conditions, and so the animacy effect for these conditions will reflect the difference in plausibility between *the driver* and *the signpost* refusing to stop.

In the recipient control conditions, there is the possibility for co-reference between PRO and the main clause subject, but, as noted above, this is not obligatory. If people sometimes initially compute the co-reference relation between PRO and the main clause subject for the recipient control sentences, the plausibility of this initial co-reference will be judged to be poor in the inanimate condition. If people base their final judgment for the sentence on this implausible co-reference, then the animacy effect for the recipient control sentences will be larger than that of the giver control baseline, but smaller than that of the adjunct control sentences, where co-reference is obligatory. However, as the co-reference is not obligatory for recipient control, this may drive participants to consider an alternative co-reference, for example co-reference between PRO and an individual unmentioned in the sentence (note that (15f) can receive an interpretation where an unmentioned driver has been ordered to stop). Thus, if participants ultimately base their sentence judgment on that (presumably plausible) alternative co-reference, then the size of the animacy effect may be similar to that of the giver control baseline.

Therefore, if main-clause subject co-reference is obligatory for the adjunct control sentences, but not for the recipient control sentences, then the overall pattern of results is expected to show a large animacy effect for the adjunct control sentences, and a smaller (possibly null) animacy effect for the recipient control sentences.

The results of norming study are given in **Table 4** below.

INSERT Table 4 ABOUT HERE

The data were analysed using Linear Mixed effects. Control type was treated as a categorical factor, and the animacy factor was centered prior to analysis. The initial model treated giver control as a baseline; in other words, giver control was the reference level for the control factor. This initial model included two separate interaction terms, which compared the giver control animacy effect with that of recipient control and adjunct control respectively. Based on model comparison, the model also included random slopes for the full interaction by items. The results of this model showed that the animacy effect for adjunct control was reliably larger than that of giver control ($t=6.35$, $p < .001$), but that there was no difference in the size of the animacy effect between recipient control and giver control ($t=1.19$, ns). A second model, treating recipient control as the baseline, showed that the adjunct control animacy effect was also reliably larger than that of recipient control ($t = 4.88$, $p < .001$).

Overall, the results from the norming study suggested that the adjunct control construction has a stronger requirement for an intra-sentential antecedent for PRO than giver and recipient nominal control construction, as we predicted. The lack of the animacy effect for the recipient control sentences suggests that, in the inanimate condition, participants often based their ultimate plausibility judgment on an alternative interpretation in which PRO referred to an unmentioned individual, even if they may have sometimes initially computed the co-reference between PRO and the main clause subject. Thus the large animacy effect for the adjunct control sentences supports the claim that this alternative interpretation was not available for these sentences; in other words the co-reference relation is likely to be obligatory for adjunct control, at least, for the types of sentences used in the current experiment. Based on these results, we proceeded to the eye-tracking component of Experiment 2.

Method

Participants

36 native speakers of English from the University of Edinburgh community were paid to take part in the Experiment. All had normal or corrected-to-normal vision.

Materials

Experiment 2 included thirty-two experimental stimuli. The design manipulated *control type* (giver control vs. adjunct control) and *ambiguity* (temporarily ambiguous vs. unambiguous). The giver control stimuli were based on the relevant conditions of Experiment 1. An example is given in (16), and the Appendix gives a full list.

(16) Experiment 2 sample stimuli

a. Ambiguous giver control

Before Andrew's failure to wash the kids came over to the house.

b. Ambiguous adjunct control

Before failing to wash the kids came over to the house.

c. Unambiguous giver control

Before Andrew's failure to wash, the kids came over to the house.

d. Unambiguous adjunct control

Before failing to wash, the kids came over to the house.

The sentences were divided into regions in an analogous way to Experiment 1. However, because of differing numbers of words between the two control types in the early part of the sentences, the analysis concentrated only on Regions 3-6, which are defined, as in Experiment 1, as follows:

Region 3: to wash (to plus infinitive)

Region 4: the kids (temporarily ambiguous NP)

Region 5: came (critical main verb)

Region 6: over to the house (end of sentence)

If Strongly active processing is used for all obligatory dependencies, the control information of the adjunct control construction should be used during the early parsing stages. If so, an interaction of control and ambiguity will be found around the disambiguation position (Region 5, *came*), with a garden path effect for the ambiguous giver control condition but no such effect for the ambiguous adjunct control condition. If, however, the delayed use of control information in structure disambiguation in Experiment 1 is due to the more general limitation of active dependency formation based on control information, a garden path effect will be found regardless of control type, though, given the results of Experiment 1, the size of the garden path effect is expected to be larger in the giver control condition, relative to adjunct control

Procedure

Procedure was identical to Experiment 1. The fillers were identical to those of Experiment 1. Mean comprehension accuracy for Experiment 2 was 89% (range 69%-100%).

Data analysis

Analysis procedures were analogous to Experiment 1. Below, we report results for each region.

Results and Discussion

Table 5 shows the means in the four eye-movement measures for the four conditions from Region 3, 4 and 5, and the sentence final Region 6. Table 6 shows the results of the LMER analysis for the main effects and interactions for the reading time measures. Table 7 shows the

Generalized LMER analysis for the proportion of First-pass regressions out. Figures 3 and 4 show the means for the critical region, for Go-past and Total time respectively.

INSERT Table 5 ABOUT HERE

INSERT Table 6 ABOUT HERE

INSERT Table 7 ABOUT HERE

INSERT FIGURE 3 ABOUT HERE

INSERT FIGURE 4 ABOUT HERE

Main effects of control

There was a main effect of control type in Total times at Region 4 (temporarily ambiguous NP) and First pass, Go-past and Total times at Region 5 (critical main verb) with longer reading times for giver control, relative to adjunct control. The main effect of control is hard to interpret in the current experiment, due to the lexical and syntactic differences between adjunct and giver control in the early part of the sentence, and indeed, the experiment was not designed to investigate this effect. However, we suspect that the effect may have been due to the fact that the giver control sentences included one extra discourse referent in comparison with adjunct control, and thus required extra processing resources.

Main effects of ambiguity

The main effect of ambiguity was found in First pass regressions out and Total times at Region 3 (to plus infinitive), in all the measures at Region 4 (temporarily ambiguous NP) and Region 5 (critical main verb), and in First Pass time at Region 6 (end of sentence). With longer reading times and more regressions in the ambiguous conditions relative to the unambiguous conditions, the main effects of ambiguity in Total times at Region 3 and all the measures at Region 4 and Region 5 clearly showed that a garden path effect was elicited regardless of control types. The effects at Region 4 may have been driven by parafoveal processing of the disambiguating 5th region, as in Experiment 1. On the other hand, First-pass regressions out at Region 3 and First Pass time at Region 6 show a reversed pattern, with a higher proportion of regressions and longer reading times for unambiguous conditions relative to ambiguous conditions respectively. The effect at Region 3 replicates a similar finding in the equivalent region in Experiment 1, and is likely to be driven by extra wrap-up processes triggered by the comma in the unambiguous conditions. However, the reverse garden-path effect in first pass times at the final region, Region 6, deserves further comment, and is discussed in the next paragraph below.

Given that Region 6 is a (fairly long) four word region, the first-pass reading time measure needs to be interpreted with caution, due to the potential impact of early regressions out of the region. Specifically, when readers experience processing difficulty (as they do, by hypothesis, in the ambiguous conditions), they are likely to make early regressions out of the region, after having made only one or two initial fixations. The relatively small number of initial fixations in the region could result in misleadingly short first-pass times. Conversely, when readers experience less processing difficulty (as in the unambiguous conditions), they are more likely to continue to make further initial fixations as they progress forwards through the region, and all of these fixations would be included in the first pass measure, leading to relatively long first pass times. This effect will be exacerbated in a long region, where multiple progressive fixations are expected when processing load is light.

In order to test whether early regressions might have been the explanation for the reverse garden path effect in Region 6, we first analyzed the proportion of trials in which an early regression took place (defined as trials in which the region was exited to the left after one or two initial fixations). We found that there were more early regressions in the ambiguous conditions than in the unambiguous conditions (46% vs. 37.5%; $Z = 3.12$, $p < .01$). After removing trials in which an early regression took place, first-pass times no longer showed the reverse garden-path effect, but instead showed a numerical (but statistically non-significant) trend in the opposite direction, with longer first-pass reading times for the ambiguous than the unambiguous conditions (954 msec vs. 936 msec, $|t| < 1$). This suggests that the reverse garden-path effect in Region 6 was due to processing difficulty in the ambiguous condition, leading to more early regressions out of the final region, and thus, counter-intuitively, shorter first-pass times. This explanation of the first-pass result fits with the data for Go-past times, where there was a marginal trend for longer reading times in the ambiguous than the unambiguous conditions. This is expected, given that Go-Past time includes the time spent during regressions out of the analysis region, while First Pass time does not. A similar reverse garden path effect in the final region in First Pass time was reported by Sturt (2007), for analogous reasons (see also Betancort, Carreiras & Sturt, 2009).

The interaction of control and ambiguity

The interaction of control and ambiguity was found in Total times at Region 3 (to plus infinitive), and in Go-past (albeit only marginal; $p = .074$; calculated using model comparison) and Total times at Region 5 (critical main verb). In these measures, the difference between ambiguous and unambiguous conditions (i.e. ambiguity cost) was greater for the giver control conditions than for the adjunct control conditions. Pairwise comparison revealed that the ambiguity cost was reliable for the giver control condition (a difference of 134 msec, $p < .05$) but was marginal for the adjunct control condition (64 msec, $p = .06$) at Region 3. The effects at Region 3 are likely to have arisen from regressions from the disambiguating region (Region 5). At Region 5, the ambiguity cost was significant for both control types in Go-Past (giver control: a difference of 206 msec; adjunct control: 102 msec, both p's < .05) but was reliable only for the giver control condition in Total time (giver control: 119 msec, $p < .05$; adjunct control: 40 msec, ns).

Overall, the main findings of Experiment 1 were replicated in Experiment 2. An interaction of ambiguity and control was observed at Region 5 (although it was significant only in Total times), with a bigger garden path effect in the giver control condition than in the adjunct control condition. In addition, the interaction was accompanied by a main effect of ambiguity in all the measures at Region 4 and 5. This is similar to Experiment 1 which showed an interaction in First-regressions out at Region 4 and in First pass, Go-past, and Total times at Region 5, and a main effect of ambiguity in all the measures at the corresponding regions. Thus, there is no evidence that the adjunct control construction used in Experiment 2 resulted in stronger or earlier influence on ambiguity resolution than the recipient control construction used in Experiment 1; in fact, if anything, the results suggest that the reverse is the case, with more interactions in Experiment 1 than in Experiment 2.

In summary, the overall results of Experiment 2 are not different from Experiment 1. Despite stronger interpretational requirements in adjunct control than giver and recipient control as attested in the norming study, control information of the adjunct control construction did not resolve structural ambiguity during the early parsing stages. The incomplete dependency, however, helped readers recover from the initial misanalysis during reanalysis.

General discussion

Results from previous studies examining *wh*-dependencies have suggested that the Active filler strategy in filler-gap dependencies interacted with structural disambiguation during the early parsing stages (Frazier & Clifton, 1989; Cai et al. 2013). However, while wh-dependencies appear to be processed in a strongly active way, these dependencies are based on structural information, and we sought to determine whether strongly active processing can generalize to control dependencies, where interpretation depends on more complex semantic information. In Experiment 1, we examined the processing of a long-distance dependency based on nominal control information. The results showed that nominal control information is not a strong enough cue to prevent an initial structural misanalysis due to Late closure, but that it can aid the recovery process. However, PRO in the nominal control construction does not require an intra-sentential antecedent, while the gap in a filler-gap wh-dependency is obligatory. Thus, it is possible that active dependency formation based on control information did not affect initial ambiguity resolution due to this optionality. To exclude this possibility, in Experiment 2 we examined the processing of adjunct control construction. As shown in a norming study, the adjunct control construction has stronger interpretational requirements than both the giver and the recipient nominal construction that PRO should be assigned its referent from an intra-sentential antecedent. The results from Experiment 2, however, did not differ from those from Experiment 1. That is, despite its stronger interpretational requirement, control information in the adjunct control construction did not prevent a garden path effect due to Late closure during the initial parsing stages. Active dependency formation in the adjunct control construction only helped readers recover from the misanalysis in the later parsing stages, similarly to that in the recipient nominal control construction.

Our claim that control information has affected garden path recovery rather than proactively guiding initial ambiguity resolution appears at first glance to be inconsistent with the results of a study reported by Pynte & Colonna (2002), who examined the processing of adjunct predicate constructions in French. Their experiments 1 and 2 examined sentences like the following (originally in French, but translated here into English for clarity of exposition).

(17) Object sentences

a. Matching condition: PRO Tired (feminine) of calling the woman she left the room

b. Mismatching condition: PRO Tired (masculine) of calling the woman he left the room

(18) Subject sentences

a. Transitive verb: PRO Tired (feminine) of calling the woman left the room.

b. Intransitive verb: PRO Tired (feminine) of chatting the woman left the room

In French adjunct predicate constructions, PRO is usually co-indexed with the main clause subject (Schwartze, 1988). In other words, in the object sentences (17), the person who is tired of calling the woman is *she/he,* while in the subject sentences (18) it is *the woman* who is tired of calling. Pynte and Colonna found that readers slowed down in (17a) relative to (17b) at the region *she left.* This processing difficulty might indicate that *the woman* in (17a) was initially attached as the main clause subject, in order to become an antecedent for PRO, and that this subsequently led to a garden path when *he/she* was encountered in the input (note that this is not possible in (17b), as PRO does not match *the woman* in gender). If this is the correct account of the processing difficulty for (17a), it would suggest that the control dependency was computed in a strongly active way, overriding Late Closure, contrary to the account that we offer in the present paper. However, there are other possible accounts of this processing difficulty. For example, when the pronoun *she* is reached in (17a), participants might momentarily have attempted to link it with the gender-matching *the woman* as an antecedent, but, as this co-reference is not grammatically possible, this dependency would ultimately have to be rejected, leading to processing difficulty. Note that this explanation does not require the assumption of Strongly active processing: the effect could have been found even if *the woman* had not been pro-actively interpreted as the antecedent of PRO.

Moreover, Pynte and Colonna's data for the Subject sentences in (18) are not consistent with Strongly active processing, and instead show evidence for the Late Closure preference. Specifically, reading times around the main verb *left* were longer in (18a) than in (18b), presumably because readers initially interpreted *the woman* as the object of *calling* in 18a (note that the object analysis is not possible in 18b because *chatting* is intransitive). This garden path effect is consistent with our interpretation of the experiments reported in the present paper: the Late Closure preference leads to a misanalysis in (18a), despite the opportunity for Strongly active processing to override it. Moreover, based on our own results, we believe that the control information in 18a would have aided the recovery from the garden path, by promoting *the woman* as the main clause subject. However, as Pynte & Colonna's experiments were not designed to test this issue, it is not possible to verify whether control information had an ameliorating effect on garden path recovery.[4]

Now we address the question of why dependency formation based on control information has a weak effect on structural disambiguation while a similar structural ambiguity appears to have been resolved in filler-gap sentences such as (5) and (6) during the early parsing stages. In fact, previous studies have reported similar parsing differences between the two types of long distance dependencies. For example, using a cross modal paradigm, Swinney, Ford, Frauenfelder and Bresnan (1988) examined the processing of filler-gap dependencies as shown in (19). In this experiment, subjects were asked to make a lexical decision to a visually presented target while experimental sentences were presented auditorily. Targets were either an associate of one of the three NPs (*policeman*, *boy* or *crowd* in (19)) or an unrelated noun, and they were presented at one of the probing positions numbered as 1, 2 and 3.

(19) The policeman saw the boy$_i$ that the crowd at the party $_1$ accused __ $_{i\ 2}$ of the $_3$ crime.

The results showed an immediate priming effect of filler, *boy,* at the gap position (position 2). In contrast, dependencies based on control information have been found to elicit delayed activation. Using the same experimental method as in Swinney et al. (1988), Osterhout & Nicol (1988) examined the processing of sentences with PRO as in (20). The results showed that the priming

[44] Pynte and Colonna (2002) did include an experiment that demonstrated garden paths as the result of Late Closure, without the involvement of control information (their Experiment 3). However, they did not compare such conditions with conditions like (18a,b) (where control information is involved) in a single experiment.

effect of the antecedent of PRO, *dentist*, was not significant until the probing position 4, 1000 ms after the supposed PRO position.

(20) The actress invited the dentist$_i$ from the new medical center PRO$_{i\ 1}$ to go to the pa$_2$rty at the $_3$ mayor's $_4$ house $_5$.

Taken together, these results suggest that while the activation of filler in a filler-gap dependency is immediate, activation of an antecedent for PRO in a control construction can be delayed.

Thus, overall, results from previous studies suggest that while control information is used on-line sentence processing, the effect may be slightly delayed compared to that in a filler-gap dependency. One possible reason for this difference is that control information is a relatively soft parsing constraint that might be more vulnerable to semantics or pragmatics than to structural relations. In fact, various types of control have been accounted for in terms of semantic relations between sentential elements rather than syntax (Culicover & Jackendoff, 2001, 2006; cf. Hornstein, 1999). The nominal control construction in Experiment 1 is such an example in which controller for PRO is determined based on the thematic roles of a given control nominal. The adjunct control construction used in Experiment 2, on the other hand, could be a good candidate to be analyzed in terms of syntax (Rosenbaum, 1967) as in the examples used in that experiment, the main clause subject is always the antecedent for PRO, making the interpretation of PRO straightforward. However, even in the case of adjunct control, or closely related constructions, the referent of PRO can depend on the connective of the adjunct clause, as we can see in the following examples:

(21) (a) $John_i$ blamed $Bill_j$ after PRO_i making the mistake.

(b) $John_i$ blamed $Bill_j$ for PRO_j making the mistake.

In (a), as in the examples in our Experiment 2, the temporal connective *after* requires PRO to refer to the main clause subject *John*, while in (b), the connective *for* constrains PRO to refer to the recipient argument of *blame*, which in this case is the object of the main clause, *Bill.* Moreover, there are still other types of control construction whose interpretation could be better dealt with in semantics. For example, in (22) *John*, *Sarah*, *John and Sarah* or even an arbitrary referent could be assigned to PRO as its controller.

(22) $John_i$ talked to $Sarah_j$ about PRO taking better care of $himself_i$/$herself_j$/$themselves_{i+j}$/$oneself_{arbitrary}$.

Thus, the observation here is that, in contrast to other types of dependencies such as *wh*-dependencies, the interpretation of control is often based on semantic relations between various sentential elements (Jackendoff & Culicover, 2003), and requires the use of lexically specific information, such as that encoded in a connective or control nominal. We conjecture that these may be the conditions under which Strongly active processing fails to apply, leading to a delay in the use of predictive dependency information in ambiguity resolution. However, further research is clearly required on a range of dependency types before firm conclusions can be made.

The idea that strongly active processing may occur with wh-dependencies, but not with control dependencies could be interpreted to be consistent with Garden Path theory (Frazier, 1987; Frazier & Clifton, 1996), according to which initial syntactic ambiguity resolution is assumed to be carried out by a structure-building module that has no access to non-structural information. As wh-dependencies are structurally licensed, the theory allows for such dependencies to be processed in a strongly active way, as the Active Filler strategy can be prioritized over Late Closure within the structure-building module. However, as we have discussed above, the types of control dependencies that we examined in the present paper rely on lexical and semantic information, and so the computation of such dependencies may lie outside the remit of the structure-building module. If this is so, it would make sense for Late Closure (a structural principle) to take priority in the initial ambiguity resolution, while control information is used in the later recovery process.

Although the explanation that we have assumed so far is consistent with a modular distinction between structural and non-structural information, the results could also be explained by more interactive models, which assume that all relevant information can influence initial syntactic ambiguity resolution. One such model is the race-based theory of syntactic ambiguity resolution (Traxler, Pickering & Clifton, 1998, van Gompel, Pickering & Traxler, 2001; van Gompel et al, 2005). According to this theory, the initial choice of analysis can be affected by control information, as well as Late closure, but the analysis that is actually chosen can vary across trials, with a distribution that reflects the overall preference. In a race-based model, the results of our Experiments 1 and 2 could reflect a mixture of two types of trials: in some trials, strongly predictive processing occurred (leading to an absence of a garden path effect for the recipient control conditions) and in others, it failed to occur (leading to a garden path effect in the recipient control conditions that was of comparable size to that in the giver control conditions). This is certainly a possible account of our data, but such an account would need to explain why control information acts as a weak constraint on predictive dependency formation, even when it is obligatory, or at least very strongly preferred (as in the adjunct control conditions in Experiment 2), while previous studies suggest that the information relevant to wh-dependencies has a stronger influence. Similar remarks apply to other interactive models, such as the Constraint satisfaction model (see e.g, Macdonald, Pearlmutter & Seidenberg, 1994).

Conclusion

As we mentioned in the introduction to this paper, recent research has uncovered a wealth of evidence that human sentence comprehension uses predictive processes, but has paid less attention to the limits of prediction. In the present study, we asked whether the use of control information in predictive dependency formation would be given priority over a well-known structural preference, Late Closure. In two experiments we showed that active dependency formation based on control information does not prevent a garden path effect due to Late closure during the early parsing stages. Our evidence does, however, suggest that this information can be used to help readers recover from the misanalysis during the reanalysis. This contrasts with the processing of filler-gap dependencies in which parsing heuristics such as Active filler strategy could disambiguate local structural ambiguity (Cai et al., 2013). Thus, compared to the Active filler strategy in wh-dependency formation, control information poses only a weaker processing constraint. This could be because the interpretation of the control construction is more vulnerable to semantic or pragmatic relations of sentential elements. Taken together, the results suggest that active dependency formation is less likely to interact with structural disambiguation during the early parsing stages when active search is based on information that is not structural as well.

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Tables

Table 1 Means (and standard errors), aggregated by participants, for first pass, go-past and total times in milliseconds in Experiment 1

	Region2 *refusal*	Region3 *to wash*	Region4 *the kids*	Region5 *came*	Region6 *over to the house*
First pass (msec)					
Ambiguous giver	292 (15)	324 (18)	382 (20)	317 (13)	725 (42)
Ambiguous recipient	385 (28)	346 (19)	400 (19)	300 (10)	743 (52)
Unambiguous giver	295 (13)	340 (21)	365 (21)	272 (11)	765 (44)
Unambig. recipient	359 (24)	327 (18)	344 (19)	302 (14)	766 (44)
Go-past (msec)					
Ambiguous giver	346 (20)	388 (22)	507 (33)	539 (36)	2386 (191)
Ambiguous recipient	480 (34)	429 (25)	503 (25)	453 (26)	2544 (161)
Unambiguous giver	378 (28)	434 (29)	388 (24)	321 (18)	2168 (156)
Unambig. recipient	443 (34)	478 (29)	427 (33)	371 (24)	2112 (163)
Total time (msec)					
Ambiguous giver	548 (31)	656 (32)	759 (49)	561 (35)	1204 (74)
Ambiguous recipient	744 (44)	686 (29)	758 (40)	540 (26)	1224 (63)
Unambiguous giver	561 (29)	525 (35)	554 (31)	423 (19)	1213 (71)
Unambig. recipient	687 (37)	545 (33)	562 (30)	473 (27)	1126 (75)
Regressions out (%)					
Ambiguous giver	11.3 (2.1)	11.1 (2.1)	18.0 (3.0)	25.0 (2.8)	81.1 (3.7)
Ambiguous recipient	14.1 (2.5)	13.9 (2.3)	15.0 (2.2)	20.4 (2.9)	81.8 (3.4)
Unambiguous giver	13.5 (2.8)	16.5 (2.6)	3.9 (1.2)	9.0 (2.1)	81.5 (3.6)
Unambig. recipient	12.1 (2.7)	21.2 (2.7)	8.8 (1.8)	11.6 (1.9)	81.3 (3.6)

Table 2. Linear mixed effect model results for Experiment 1. Coefficients, standard errors and t-values are reported for the main effects of control type and ambiguity, as well as the interaction of these two factors. The ‘‘Slope” column indicates whether the random slope parameter corresponding to the effect was included in the model for participants (p) or items (i). An asterisk indicates that the effect is significant at α= 0.05 (using the |t| > 2 criterion).). Results for Region 2 are based on a model that also included Length and Log frequency of the control noun, as described in the text.

		First pass				*Go-past*				*Total time*			
		Coeff.	*SE*	*t*	Slope	Coeff.	*SE*	*t*	Slope	Coeff.	*SE*	*t*	Slope
	Intercept	322	21	15.29*		400	26	15.09*		617	37	16.87*	
R2	control	-38	29	-1.32	(p,i)	-28	32	-0.9	(p,i)	-44	45	-0.99	(i)
“refusal”	ambiguity	-13	14	-0.97	(i)	-4	19	-0.19	(p)	-22	23	-0.98	
	Ctrl x Amb	30	26	1.12	(i)	53	37	1.46		65	44	1.47	
R3	Intercept	336	16	21.44*		434	24	18.03*		603	32	18.83*	
“to	control	-4	11	-0.36	-	-44	18	-2.38*	-	-24	20	-1.21	-
wash”	ambiguity	-0.1	18	-0.01	(p)	48	27	1.77	(p,i)	-136	31	-4.38*	(p)
	Ctrl x Amb	34	21	1.61	-	-11	37	-0.30	-	0.6	40	0.02	-
R4	Intercept	373	20	18.94*		458	26	18*		661	39	16.96*	
“the	control	1.02	11	0.09	-	-19	19	-0.97	-	-0.7	21	-0.03	-
kids”	ambiguity	-36	12	-3.10*	-	-97	24	-4.09*	(i)	-199	26	-7.62*	(p)
	Ctrl x Amb	43	23	1.85	-	-38	38	-0.99	-	-5	42	-0.12	-
	Intercept	295	11	25.79*		425	20	20.70*		490	33	14.79*	
R5	control	-9	9	-1.02	-	9	33	0.27	-	-14	19	-0.74	-
“came”	ambiguity	-20	9	-2.28*	(i)	-147	35	-4.13*	-	-108	23	-4.74*	(i)
	Ctrl x Amb	-44	18	-2.5*	-	-144	69	-2.08*	(i)	-81	37	-2.17*	-
	Intercept	751	42	17.97*		2299	184	12.46*		1189	79	15*	
R6	control	-11	28	-0.39	-	-40	77	-0.52	-	36	34	1.05	-
“over...”	ambiguity	28	50	0.57	(p,i)	-333	78	-4.28*	-	-53	34	-1.56	-
	Ctrl x Amb	19	55	0.35	-	206	155	1.33	-	111	68	1.64	-

Table 3. Generalized Linear mixed effects results for First Pass Regressions out in Experiment 1. Coefficients (in log-odds units), standard errors, Z-values and p-values are reported for the main effects of control type and ambiguity, as well as the interaction of these two factors. The ‘‘Slope” column indicates whether the random slope parameter corresponding to the effect was included in the model for participants (p) or items (i). Asterisks indicate significance level: p<.001***; p < .01**; p < .05*; p < .1+

		First Pass Regressions Out				
		Coefficient	*SE*	*Z*	*p*	*Slope*
R2	(Intercept)	-2.11	0.17	-12.42	<.001***	
“refusal”	Control	-0.09	0.20	-0.42	.67	-
	Ambiguity	0.01	0.20	0.03	.97	-
	Ctrl x Ambig	0.46	0.40	1.14	.25	-
R3	(Intercept)	-1.99	0.18	-11.33	<.001***	
“to	Control	-0.50	0.26	-1.95	.051+	(i)
wash”	Ambiguity	0.56	0.19	2.91	.004**	-
	Ctrl x Ambig	-0.09	0.39	-0.25	.81	-
R4	(Intercept)	-2.33	0.16	-14.17	<.001***	
“the	Control	-0.32	0.24	-1.33	.18	-
kids”	Ambiguity	-1.17	0.24	-4.91	<.001***	-
	Ctrl x Ambig	-1.08	0.48	-2.28	.02*	-
R5	(Intercept)	-1.73	0.13	-13.14	<.001***	
“came”	Control	-0.12	0.20	-0.61	.55	-
	Ambiguity	-0.97	0.20	-4.73	<.001***	-
	Ctrl x Ambig	-0.75	0.41	-1.83	.067+	-
R6	(Intercept)	1.95	0.26	7.53	<.001***	
“over...”	Control	0.07	0.18	0.35	.72	-
	Ambiguity	-0.03	0.18	-0.17	.87	-
	Ctrl x Ambig	0.07	0.36	0.21	.83	-

Table 4. Mean sentence ratings (and standard errors) for the Experiment 2 norming study

	Giver-control	Recipient-control	Adjunct-control
Animate	3.06 (0.17)	3.03 (0.16)	3.55 (0.20)
Inanimate	3.00 (0.17)	2.71 (0.14)	2.06 (0.14)

Table 5. Means (and standard errors), aggregated by participants, for First-pass, Go-past and Total time, for Experiment 2

	Region 3 *to wash*	Region 4 *the kids*	Region 5 *came*	Region 6 *over to the house*
First Pass (msec)				
Ambiguous giver	314 (12)	367 (19)	312 (15)	642 (41)
Ambiguous adjunct	288 (11)	359 (17)	297 (12)	697 (37)
Unambiguous giver	302 (14)	319 (15)	295 (14)	731 (35)
Unambiguous adjunct	294 (13)	313 (15)	271 (12)	713 (37)
Go Past (msec)				
Ambiguous giver	374 (22)	464 (23)	551 (59)	2250 (145)
Ambiguous adjunct	358 (19)	476 (24)	417 (33)	2013 (116)
Unambiguous giver	397 (38)	399 (29)	345 (22)	1961 (115)
Unambiguous adjunct	380 (21)	369 (19)	315 (18)	1988 (144)
Total Time (msec)				
Ambiguous giver	652 (47)	750 (42)	562 (35)	1088 (55)
Ambiguous adjunct	585 (34)	689 (36)	471 (20)	1111 (57)
Unambiguous giver	498 (29)	543 (31)	443 (19)	1059 (44)
Unambiguous adjunct	537 (32)	514 (31)	431 (23)	1117 (61)
Regressions Out (%)				
Ambiguous giver	10.2 (2.0)	18.2 (2.8)	24.6 (3.4)	74.7 (3.6)
Ambiguous adjunct	12.4 (2.1)	18.9 (3.3)	18.3 (2.8)	76.0 (3.2)
Unambiguous giver	14.6 (3.0)	10.3 (2.2)	7.0 (1.5)	76.4 (3.9)
Unambiguous adjunct	17.9 (2.6)	9.2 (1.9)	7.9 (1.9)	77.5 (3.6)

Table 6. Linear Mixed Effects model results for Experiment 3. Coefficients, standard errors and t-values are reported for the main effects of control type and ambiguity, as well as for the interaction of these factors. The "Slope" column indicates whether the random slope parameter corresponding to the effect was included in the model for participants (p) or items (i). An asterisk indicates that the effect was significant at α = .05 (using the |t| > 2 criterion).

		First Pass				*Go-Past*				*Total Time*			
		Coeff.	*SE*	*t*	Slope	Coeff.	*SE*	*t*	Slope	Coeff	*SE*	*t*	Slope
R3	(Intercept)	299	11	26.84*		377	21	17.59*		568	39	14.72*	
	Control	17	8	1.99	-	14	25	0.54	(p)	14	20	0.71	-
	Ambiguity	-2	8	-0.19	-	23	21	1.11	(p)	-101	20	-4.98*	-
	Ctrl x Ambig	-15	17	-0.91	-	2	32	0.05	-	-110	41	-2.70*	-
R4	(Intercept)	339	16	21.55*		426	22	19.08*		624	36	17.56*	
	Control	7	10	0.68	-	8	16	0.49	-	46	21	2.18*	-
	Ambiguity	-46	10	-4.82*	-	-87	16	-5.42*	-	-191	21	-9.10*	-
	Ctrl x Ambig	0	19	0.01	-	43	32	1.33	-	-31	42	-0.75	-
R5	(Intercept)	291	12	23.95*		412	31	13.19*		470	26	18.19*	
	Control	21	9	2.30*	-	90	36	2.47*	(p,i)	48	23	2.09*	(p,i)
	Ambiguity	-22	9	-2.44*	-	-158	47	-3.38*	(p,i)	-76	22	-3.44*	(p,i)
	Ctrl x Ambig	14	18	0.78	-	-114	63	-1.80	(i)	-76	37	-2.03*	(p)
R6	(Intercept)	696	34	20.22*		2053	143	14.32*		1094	63	17.36*	
	Control	-19	24	-0.77	-	105	90	1.17	(i)	-41	29	-1.42	-
	Ambig	52	24	2.16*	-	-156	86	-1.81	(i)	-11	29	-0.37	-
	Ctrl x Ambig	73	48	1.51	-	-266	141	-1.89	-	-34	58	-0.59	-

Table 7. Generalized Linear Mixed Effects results for First Pass Regressions Out in Expweirment 3. Coefficients (in log-odds units), standard errors, Z-values and p-values are reported for the main effects of control type and ambiguity, as well as for the interaction of these two factors. The "Slope" column indicates whether the random slope parameter corresponding to the effect was included in the model for participants (p) or items (i). Asterisks indicate significance level: p<.001***; p<.01**; p<.05*; p<.1+

		First Pass Regressions Out				
		Estimate	*SE*	*Z*	*p*	*Slope*
R3	(Intercept)	-1.99	0.14	-13.86	<.001***	
	Control	-0.26	0.18	-1.45	0.15	(i)
	Ambiguity	0.43	0.18	2.35	0.019*	
	Ctrl x Ambig	-0.05	0.36	-0.13	0.9	
R4	(Intercept)	-2.09	0.17	-12.21	<.001***	
	Control	0.04	0.18	0.22	0.82	
	Ambiguity	-0.83	0.19	-4.47	<.001***	(p)
	Ctrl x Ambig	0.18	0.37	0.47	0.64	
R5	(Intercept)	-2.08	0.17	-12.04	<.001***	
	Control	0.17	0.22	0.8	0.43	
	Ambiguity	-1.3	0.22	-5.92	<.001***	
	Ctrl x Ambig	-0.62	0.44	-1.4	0.16	
R6	(Intercept)	1.53	0.24	6.3	<.001***	
	Control	-0.08	0.15	-0.52	0.6	(i)
	Ambiguity	0.12	0.15	0.75	0.45	
	Ctrl x Ambig	0.03	0.31	0.1	0.92	

Figure Captions:

Figure 1: Go-past reading times at the disambiguating verb position in Experiment 1.

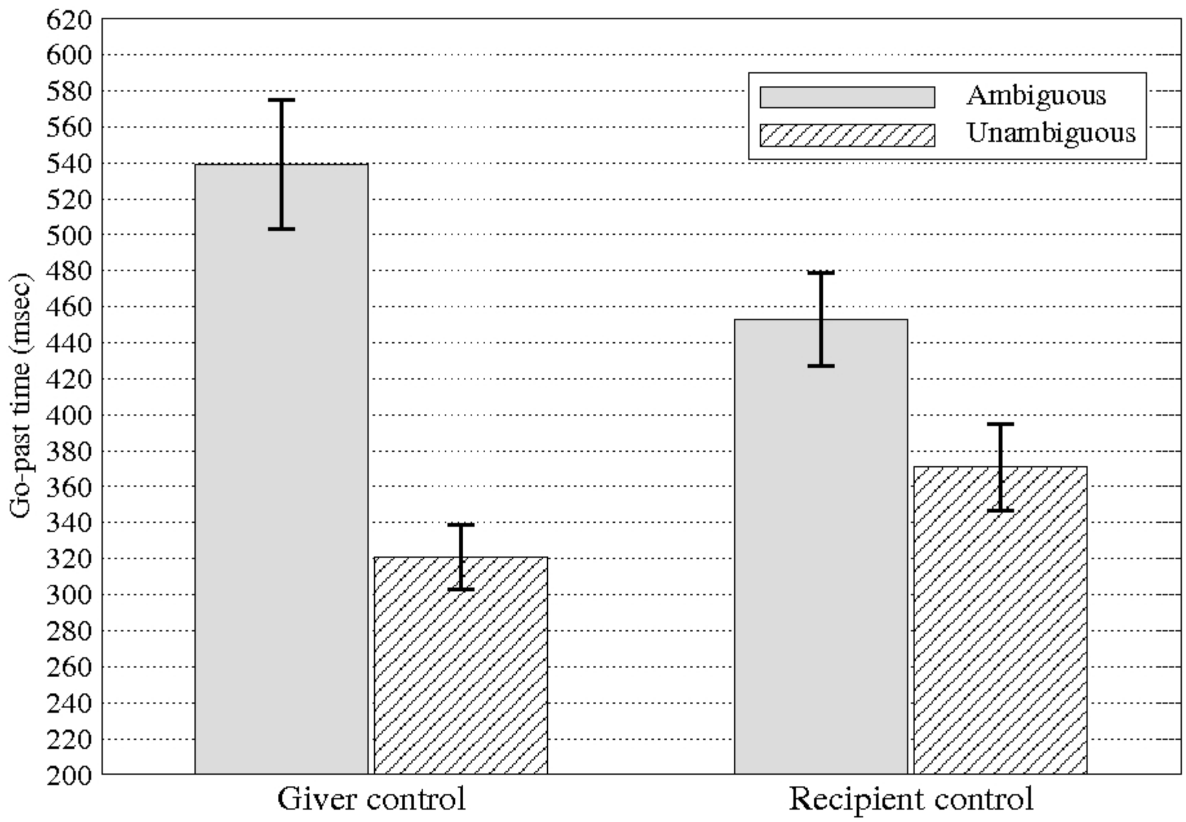

Figure 2: Total reading times at the disambiguating verb position in Experiment 1.

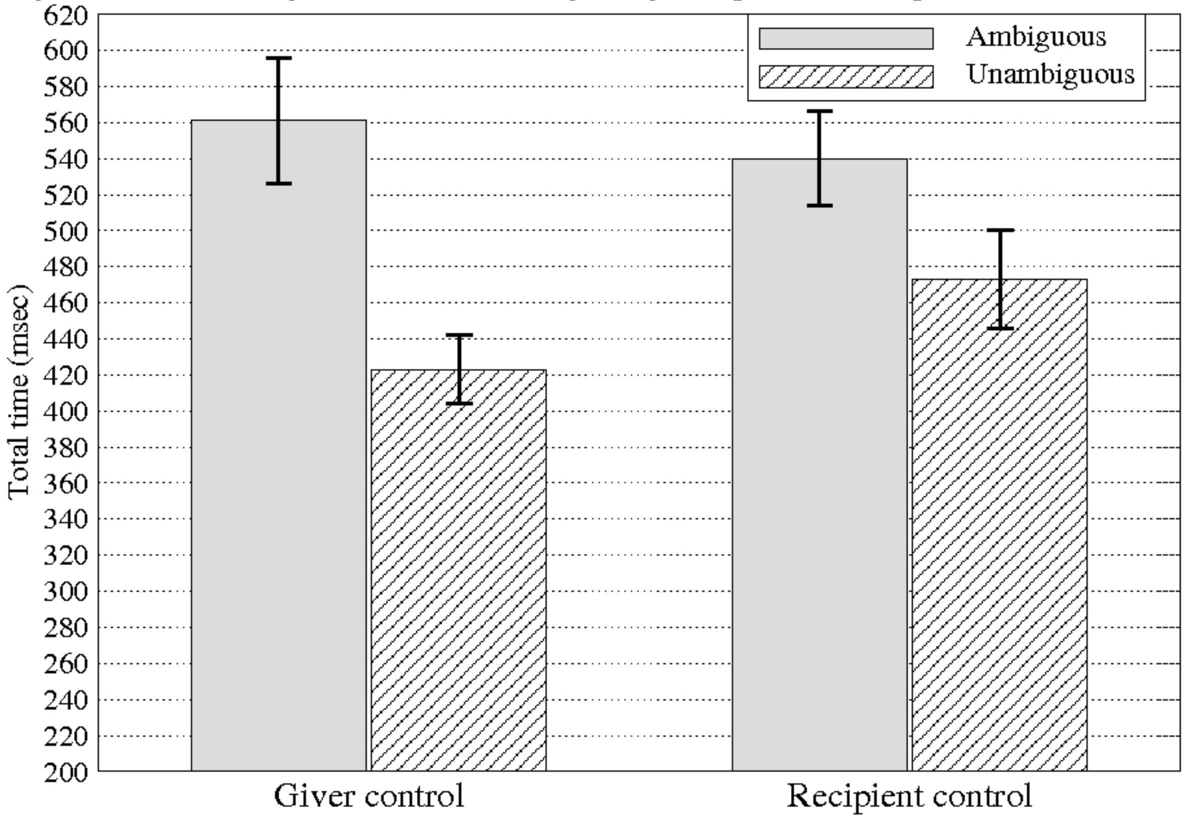

Figure 3: Go-past reading times at the disambiguating verb position in Experiment 2.

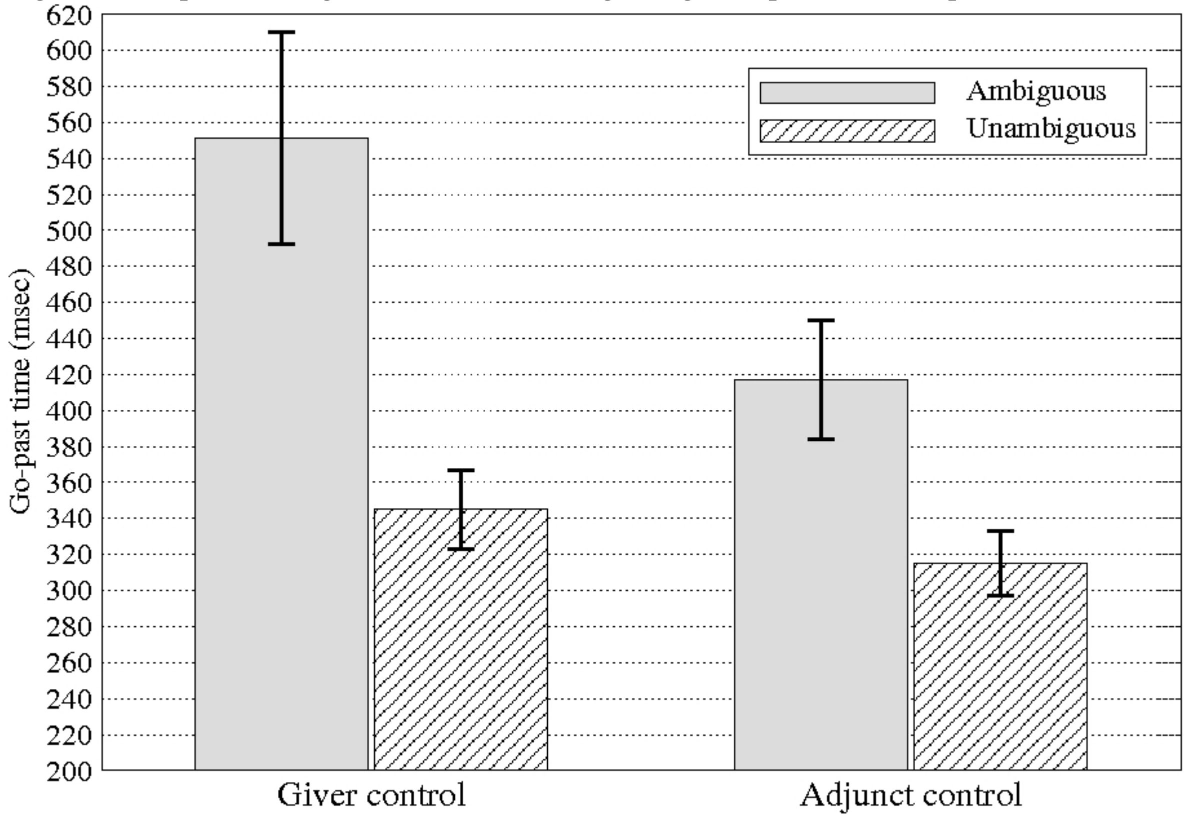

Figure 4: Total reading times at the disambiguating verb position in Experiment 2.