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The Role of Verb Repetition in Cumulative Structural Priming in Comprehension

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Recently processed syntactic information is likely to play a fundamental role in online sentence comprehension. For example, there is now a good deal of evidence that the processing of a syntactic structure (the target) is facilitated if the same structure was processed on the immediately preceding trial (the prime), a phenomenon known as structural priming. However, compared with structural priming in production, structural priming in comprehension remains relatively understudied. We investigate an aspect of structural priming in comprehension that is comparatively well understood in production but has received little attention in comprehension: the cumulative effect of structural primes on subsequently processed sentences. We further ask whether this effect is modulated by lexical overlap between preceding primes and the target. In 3 self-paced reading experiments, we find that structural priming effects in comprehension are cumulative and of similar magnitude both with and without lexical overlap. We discuss the relevance of our results to questions about the relationship between recent experience and online language processing.

Keywords: psycholinguistics, structural priming, adaptation, sentence processing

Supplemental materials: http://dx.doi.org/10.1037/xlm0000236.supp

When speakers have the choice between near meaning-equivalent syntactic structures (e.g., "The clown gave the boy a balloon" vs. "The clown gave a balloon to the boy"), the probability of producing either of these structures increases after exposure to a prime sentence with the same structure (e.g., speakers are more likely to produce the first example above if they just produced a sentence like "Tom threw the dog a bone"; Bock, 1986). This phenomenon, known as *structural priming*, has played an important role in research on language production (for reviews and theoretical proposals, see Chang, Dell, & Bock, 2006; Jaeger & Snider, 2013; Pickering & Garrod, 2013; Reitter, Keller, & Moore, 2011). Structural priming in production has also been influential as a tool to probe the nature of linguistic representations (for a review, see Pickering & Ferreira, 2008).

Compared with production, structural priming in syntactic comprehension has received considerably little attention. In sentence comprehension, *structural priming* refers to the facilitated comprehension of a structure following exposure to that structure. In a comprehensive review of the field, Pickering and Ferreira (2008) commented on the striking sparsity of studies that investigated structural priming in comprehension in ways parallel to production. Since then, the field has seen a flurry of studies that have followed the call for action. Recent work on priming has shown, for example, that garden-path effects—in which the underlined material in temporarily ambiguous relative clause (RC) sentences such as (1a) takes longer to read than an unambiguous RC baseline (1b) or a much more frequently occurring temporarily ambiguous main verb (MV; 1c)—are reduced when sentences such as (1a) are read immediately following a sentence with the same structure, such as (2) (Ledoux, Traxler, & Swaab, 2007; Noppeney & Price, 2004; Tooley, Traxler, & Swaab, 2009; Traxler & Tooley, 2008).

1. The soldiers . . .

   a. . . . warned about the dangers *conducted the midnight raid*. (Ambiguous RC)

   b. . . . who were warned about the dangers *conducted the midnight* raid. (Unambiguous RC)

   c. . . . warned about the dangers *before the midnight* raid. (MV)

2. The workers warned about the wages decided to file complaints.

Similar trial-to-trial facilitation effects have been observed for a number of different syntactic structures, e.g., sentences with modifier-goal ambiguities such as (3), in which "in the box" is parsed as either a modifier of or goal-prepositional phrase for "peanuts," depending on the prime [Traxler, 2008]; priming of early closure readings in sentences such as (4), in which "the school" is parsed as a subject noun phrase (NP) rather than the

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object of "left" [Noppeney & Price, 2004; Traxler, 2014]; and attachment ambiguities, in which subjects can be primed to attach the prepositional phrase in (5) to "the man or the girl" [Branigan, Pickering, & McLean, 2005]).

3. The vendor tossed the peanuts in the box into the crowd during the game.

4. After the headmaster had left the school deteriorated rapidly.

5. The man saw the girl with the telescope.

Structural priming in comprehension has now been demonstrated for both reading (most of the work cited above, e.g., Ledoux et al., 2007; Tooley et al., 2009; Traxler, 2008) and auditory sentence processing (Arai & Mazuka, 2014; Arai, van Gompel, & Scheepers, 2007; Kamide, 2012; Scheepers & Crocker, 2004; Thothathiri & Snedeker, 2008a, 2008b). The latter studies have also provided evidence that the facilitatory effect of trial-by-trial structural priming stems at least partly from increased expectations for the prime structure during processing of the target sentence (see also Fine & Jaeger, 2013).

Still, much remains to be understood about structural priming in comprehension. Here, we focus on the relation between trial-to-trial effects of exposure to a prime structure and the cumulative effects on comprehension across many primes. Little is known about the latter, although it has played an important role in research on production. Below, we begin by introducing cumulative structural priming and its relevance to accounts of structural priming. Then, we introduce the property of cumulative structural priming we focus on in this article: the role of verb overlap between prime and target. As we discuss below, verb overlap has figured prominently in research on trial-to-trial structural priming in both production and comprehension, because it has been taken to speak to the nature of the mechanisms involved in structural priming (for an overview, see Hartsuiker, Bernolet, Schoonbaert, Speybroeck, & Vanderelst, 2008).

Trial-to-Trial Versus Cumulative Structural Priming

Research on structural priming in production has found that exposure to multiple primes has a cumulative effect (e.g., Bock, Dell, Chang, & Onishi, 2007; Bock & Griffin, 2000; Branigan, Pickering, Stewart, & McLean, 2000; Hartsuiker et al., 2008). For example, the more often participants were exposed to double object (or prepositional dative) primes during an exposure phase, the more likely they were to produce that structure in a sentence completion task following the exposure phase (Kaschak, 2007).

The finding that priming is cumulative has played an important role in distinguishing between accounts of syntactic priming in production. Cumulative effects are not expected if priming is purely a result of transient activation boosts (e.g., Branigan, Pickering, & Cleland, 1999). Cumulativity has thus been taken to argue that structural priming in production is at least in part as a result of learning (Jaeger & Snider, 2013; Kaschak, Kutta, & Coyle, 2014; Kaschak, Kutta, & Jones, 2011; Kaschak, Kutta, & Schatschneider, 2011).

If structural priming in production and comprehension rely on the exact same mechanisms (as hypothesized by, e.g., Chang et al., 2006, and Tooley & Bock, 2014), we would therefore expect similar cumulative priming effects in syntactic comprehension (for a review of arguments for such comparative work, see Pickering & Ferreira, 2008; Tooley & Traxler, 2010). In particular, if structural priming in comprehension is a side effect of implicit learning during language processing (Chang et al., 2006; Fine & Jaeger, 2013; Jaeger & Snider, 2013; Luka & Barsalou, 2005), rather than transient activation (Pickering, Branigan, & McLean, 2002), cumulativity is expected for comprehension, too.

A few studies have begun to address this question (Fine, Jaeger, Farmer, & Qian, 2013; Kaschak & Glenberg, 2004; Long & Prat, 2008; Wells, Christiansen, Race, Acheson, & MacDonald, 2009). In a seminal article, Wells et al. (2009) showed that repeated exposure to object-extracted RCs can, over the course of several days, diminish the processing cost associated with this famously difficult-to-process structure, highlighting the role of linguistic experience in sentence comprehension. In a similar vein, Long and Prat (2008) exposed participants to temporarily ambiguous RCs, as in (1), above, in which "warned about the dangers" can be temporarily interpreted as the matrix verb of the sentence.

Sentences such as (1a) are known to elicit garden-path effects at the disambiguation point ("conducted"; e.g., Ferreira & Henderson, 1990). The magnitude of the garden path effect can be ameliorated when the matrix subject biases toward the intended RC reading rather than against it (like "The evidence" vs. "The witness," respectively, in [6]; McRae, Ferretti, & Amyote, 1997; McRae, Spivey-Knowlton, & Tanenhaus, 1998; Pearlmutter & MacDonald, 1995; Tabossi, Spivey-Knowlton, McRae, & Tanenhaus, 1994).

6. The {evidence, witness} examined by the lawyer was unreliable.

Long and Prat (2008) found that, after multiple days of exposure to sentences in which animate subjects ("the witness") always occurred with the a priori expected MV continuation, and inanimate subjects ("the evidence") always occurred with RCs, subjects who did not initially exploit the plausibility information (i.e., low-span readers) came to benefit from this lexical cue.

The studies by Wells et al. (2009) and Long and Prat (2008) demonstrate that long-term training through massive exposure to syntactic structures that are otherwise hard to process can facilitate processing of these structures. These works leave open, however, whether similar cumulative facilitation effects can be observed on a shorter time scale, for instance, within an experiment. Such *rapid* cumulativity has been observed for structural priming in production (Jaeger & Snider, 2013; Kaschak et al., 2014; Kaschak et al., 2011).

Evidence that cumulative facilitation of a priori difficult structures can occur after similarly brief exposure comes from more recent studies (Craycraft, 2014; Farmer, Fine, Yan, Cheimariou, & Jaeger, 2014; Fine et al., 2013; Fine, Qian, Jaeger, & Jacobs, 2010; Kaschak & Glenberg, 2004). Fine et al. (2013) used materials similar to those used by Long and Prat (2008) to investigate whether repeated exposure to RCs within a single experimental session would lead to cumulative reductions in the processing cost associated with that structure. This is indeed what was observed: After exposure to around 25 temporarily ambiguous stimuli of the type shown above in (1) (a total of 36 critical items were presented), subjects exhibited no detectable garden path effect (see also Fine et al., 2010, for another type of temporary ambiguity). Parallel to evidence from trial-by-trial structural priming, this cumulative effect of exposure within a single experimental session has also been found to be reflected in anticipatory eye movements (Kamide, 2012), suggesting that it is at least in part a result of adaptation of expectations about the relative frequency of the primed structure (for a proposal along these lines as well as further evidence in favor of it, see Fine et al., 2013).

Here, we have two goals. If confirmed, evidence of rapid cumulative structural priming would suggest that structural priming in comprehension is cumulative at time scales similar to those observed for structural priming in production. Our first goal, therefore, is to replicate these effects. Second, we aim to contribute to a better understanding of how these cumulative effects relate to trial-to-trial priming effects in syntactic comprehension (e.g., Branigan et al., 2005; Noppeney & Price, 2004; Thothathiri & Snedeker, 2008a; Tooley et al., 2009; Traxler, 2008; Traxler & Tooley, 2008). To this end, we investigate the role of verb overlap between the prime and target (or, in the case of cumulative priming, *verb repetition* across critical items), which has been the target of many recent investigations of trial-to-trial priming in syntactic comprehension, but has not previously been investigated in cumulative structural priming in comprehension.[1]

Verb Repetition in Structural Priming

The emerging body of work on cumulative structural priming (none of the studies cited above use this term) has largely focused on questions about implicit learning—whether experience with a structure improves subsequent processing (Long & Prat, 2008; Wells et al., 2009), whether experiment-specific distributional information can be extracted from the input (Fine et al., 2013; Kamide, 2012), and whether mere exposure can lead subjects to acquire entirely novel grammatical constructions (Kaschak & Glenberg, 2004; Fraundorf & Jaeger, 2016). By asking whether cumulative structural priming is sensitive to verb repetition, the current study provides an initial step toward unifying previous work on cumulative priming, on the one hand, and previous work on trial-to-trial structural priming—in which the effect of verb repetition has been a central focus—on the other.

Some previous studies suggest that priming in comprehension occurs only when prime and target sentences share the same verb (Arai et al., 2007; Ledoux et al., 2007; Tooley et al., 2009; Traxler & Pickering, 2005; Traxler & Tooley, 2008), whereas others suggest that priming can occur with or without repeated verbs (Arai & Mazuka, 2014; Fine et al., 2013, 2010; C. S. Kim, Carbary, & Tanenhaus, 2014; Thothathiri & Snedeker, 2008b; Traxler, 2008). Still other studies suggest that although structural priming in comprehension does not *require* repeated verbs across prime and target, priming is *stronger* when this situation holds (Tooley & Bock, 2014; Traxler, 2014; Traxler, Tooley, & Pickering, 2014). This magnification of priming effects in the presence of verb overlap between the prime and target is known as the "lexical boost" effect, and was first observed in language production (e.g., Hartsuiker et al., 2008; Pickering & Branigan, 1998).

The relationship between trial-to-trial and cumulative priming in comprehension, and whether they are sensitive to the same representational information, is a pertinent question in light of recent formal models of priming in production suggesting that an adequate model of structural priming likely needs to include a rapidly decaying component (captured by "transient activation" in some models [cf. Branigan et al., 1999; Pickering & Branigan, 1998; Pickering & Garrod, 2004], or by declarative memory in others [Chang et al., 2006]) and a longer-lasting, error-sensitive implicit learning mechanism (Chang et al., 2006; Jaeger & Snider, 2013; for explicit discussions of the need for two mechanisms, see Chang et al., 2006; Hartsuiker et al., 2008; Reitter et al., 2011).

In contrast to dual-mechanism accounts of priming, some researchers have suggested that the short-lived lexical boost may also follow from more general principles of learning or expectation adaptation. Specifically, Jaeger and Snider (2013) suggest that the rapidly decaying lexical boost observed in production priming may reflect the fact that content words follow a "bursty" distribution in spontaneously produced spoken and written language (Heller, Pierrehumbert, & Rapp, 2010; Katz, 1996): Rather than occurring with a uniform probability across linguistic environments, words tend to occur in "bursts" of high local probability. In comprehension, listeners may therefore expect verbs (and their associated structures) to occur in "clusters" (for some direct evidence in support of this, see Myslin & Levy, 2015), which would lead to a rapidly decaying lexical boost. However, the same reasoning may also lead to the prediction that verb repetition will have longer-lasting effects, assuming that subjects (implicitly) reason that the *entire experimental environment* constitutes a "cluster," within which verb-structure pairs are likely to be repeated. The goal of the current study is not to adjudicate between dual-mechanism and single-mechanism expectation adaptation accounts of priming (nor do our experiments constitute an adequate test for distinguishing these two very broad frameworks). We mention these divergent perspectives here simply to highlight the rich array of previous work that is relevant to our current question. We return to these theoretical questions in the General Discussion.

In the current set of experiments, if verb repetition was present in an experiment, items with repeated verbs were separated by, on average, roughly 20 intervening sentences. Therefore, if dual-mechanism models are correct, and structural priming is mediated by the same mechanism in comprehension and production, then the lexical boost should rapidly decay in comprehension as well, and cumulative structural priming should not be influenced by verb repetition. We have shown in our previous work that cumulative structural priming for such materials can be observed without verb overlap (Fine et al., 2013), but it remains an open question whether this effect interacts with verb repetition.

The Current Study

In three self-paced reading experiments, we examine the effect of repeated verbs on cumulative structural priming. Subjects read ambiguous and unambiguous sentences like (1a) and (1b), repeated below as (7).

7. The experienced soldiers/. . .

[1] What we refer to as verb overlap or verb repetition is also sometimes referred to as lexical overlap, though the focus is usually on verbs, or, more generally, the head of the phrase being primed.

a. . . . warned about the dangers/ *conducted the midnight/* raid. (Ambiguous RC)

b. . . . who were/warned about the dangers/ *conducted the midnight/* raid. (Unambiguous RC)

Sentences like (7a) are temporarily ambiguous because "warned about the dangers" can be parsed either as the MV phrase of the sentence, or as an RC modifying "soldiers." Previous work has shown that reading times (RTs) during the disambiguating region (i.e., the point in the sentence in which the MV reading is ruled out in [7a], italicized above) are significantly higher for temporarily ambiguous RC sentences relative to unambiguous RC sentences like (7b). We will refer to this difference as the *ambiguity effect* (also known as the garden-path effect, discussed above, cf. Frazier, 1987).

In the experiments reported in this article, the ambiguity was always resolved as an RC. Therefore, cumulative experience in the experimental environment should lead to cumulative priming, which we quantify as the incremental and cumulative reduction in the ambiguity effect.[2] In Experiments 1 and 2, we present subjects with sentences like (7), and measure the change in the ambiguity effect as subjects progress through the experiment. In Experiment 1, verbs were systematically repeated across the experiment; in Experiment 2, a different verb was used in each critical sentence. In Experiment 3, we completely eliminated repetition of all content words, including verbs, nouns, adjectives, and adverbs. We quantitatively compare the cumulative priming effect across experiments to ask what effect verb repetition has on cumulative structural priming in comprehension.

Experiment 1: Cumulative Structural Priming With Verb Repetition

Method

Subjects. Eighty-eight subjects were recruited via Amazon's crowdsourcing platform, Mechanical Turk. Only subjects with U.S. IP addresses were allowed to participate. Subjects were self-reported native speakers of English, and only subjects with at least a 95% approval rating from previous jobs were included. There is, by now, a wealth of evidence suggesting that psycholinguistic experiments administered over the web replicate results obtained in lab-based experiments, even with online measures such as self-paced reading (e.g., Demberg, 2013; Enochson & Culbertson, 2015; Keller, Gunasekharan, Mayo, & Corley, 2009; Munro et al., 2010).

Materials. Critical items were constructed from sentence pairs like (7a) and (7b). Eight different verbs giving rise to the MV/RC ambiguity (*watched*, *washed*, *taught*, *served*, *called*, *warned*, *dropped*, *pushed*) were repeated five times to yield 40 critical items with different lexicalizations apart from the verbs (i.e., different NPs, adjectives). Ambiguity was counterbalanced across two experimental lists.

In addition, each list contained the same 80 fillers. Filler sentences featured a variety of syntactic structures and, crucially, were constructed so as not to include verbs that give rise to the MV/RC ambiguity (e.g., "All the undergraduates in the class had trouble keeping up; The foreign delegates arrived at the embassy surrounded by security guards"). All critical items and fillers, for both experiments, are provided in Appendix A of the online supplemental materials.

Items and fillers were presented in the exact same pseudorandom order in both lists. The pseudorandom order adhered to the constraint that two critical items not occur on consecutive trials, that the same condition not be repeated more than three times in a row, and that the experiment begin and end with filler trials. Moreover, these lists were constructed such that the eight verbs used to create the critical items were presented in five consecutive blocks, and no verb was repeated within a block. Thus, critical items containing the same verb were separated, on average, by eight critical items ($SD = 3$) or by an average of 23 critical and filler items ($SD = 8$). Next, the order of the two lists counterbalancing ambiguity was reversed, yielding two presentation orders. This step is important in that it decreases the correlation between item identity and presentation order. This is particularly crucial for experiments on cumulative priming, as it guarantees that effects of *amount of* cumulative exposure to a given syntactic structure is not confounded with the *identity* of the items that contained that structure.

Procedure. Subjects read sentences in a self-paced moving window display (Just, Carpenter, & Woolley, 1982). At the beginning of each trial, the sentence appeared on the screen with all nonspace characters replaced by a dash. Subjects pressed the space bar using their dominant hand to view each consecutive word in the sentence. Durations between space bar presses were recorded. At each press of the space bar, the currently viewed word reverted to dashes as the next word was converted to letters. A yes–no comprehension question followed all experimental and filler sentences, with the correct answer to half of all comprehension questions being "yes." Subjects required, at most, 30 min to complete the experiment.

Results

Data coding and exclusions. Following common practice in the analysis of self-paced reading data, RTs less than 100 ms or greater than 2,000 ms were excluded. This resulted in .4% data loss.

Next, we excluded trials on which subjects answered comprehension questions incorrectly, resulting in an additional 7% data loss, averaging across critical items and fillers (within critical items only, there was 9% data loss, with 10% of questions for ambiguous items being answered incorrectly and 8% for unambiguous items being answered incorrectly; this difference was significant, replicating MacDonald, Just, & Carpenter 1992). For all experiments, we excluded subjects with a comprehension question accuracy rate below 80%. No subjects in Experiment 1 were affected by this criterion. After exclusions, the average by-participants comprehension accuracy was 93% ($SD = 3$). None of the results for any of the experiments presented here depend on the

[2] In the current study, we assume that both unambiguous and ambiguous RCs "count" as RCs to the subject, and that the cumulative effect of observing both is what leads to cumulative syntactic priming. It is possible that temporarily ambiguous RCs would lead to stronger cumulative priming effects (following an account along the lines of that proposed by Kaschak & Glenberg, 2004). We leave this question to future work.

exclusion of incorrectly answered items (after we summarize the RT results, we also present an analysis of comprehension question accuracy across all three experiments).

We then performed a residualization step intended to remove the effect of word length on log RTs.[3] Word length is a strong predictor of word-by-word RTs, and it is standard in analyzing RTs, to regress RTs onto word length and then to use the residuals of this model as a dependent measure (Ferreira & Clifton, 1986). We therefore regressed log RTs in the entire data set (excluding only practice trials) against word length (in characters) using linear mixed effects regression (Baayen, Davidson, & Bates, 2008). The model also included a by-subject random slope for word length. These random effects allow the model to discount mean differences in RTs across subjects as well as variable sensitivity to the effect of word length across subjects. The residuals of this model yield residual log RTs, which will serve as the dependent measure in all analyses reported throughout the manuscript.

Analysis. Although the sentences were read one word at a time, for the purposes of analysis, we followed standard practice and segmented sentences into regions indicated by the forward slashes in (7) above. We designate these regions the *subject*, the *relativizer* (only present in unambiguous sentences, e.g., "who were" in [7b]), the *ambiguous region*, the *disambiguating region*, and the *final word*. These regions are the same used by MacDonald et al. (1992).

We begin by plotting by-region mean residual log RTs (i.e., residual log RTs averaged across words within each region) in Figure 1). This figure serves to demonstrate that before looking for evidence of structural priming, we have replicated the ambiguity effects found in previous work. Consistent with previous research, Figure 1 shows larger RTs for ambiguous (dark, solid lines) relative to unambiguous (light, dashed lines) sentences. This effect surfaced in the ambiguous and disambiguating regions, and spilled over onto the final word of the sentence (cf. Table 1). This replicates MacDonald et al. (1992), who also found significant ambiguity effects in these three regions.

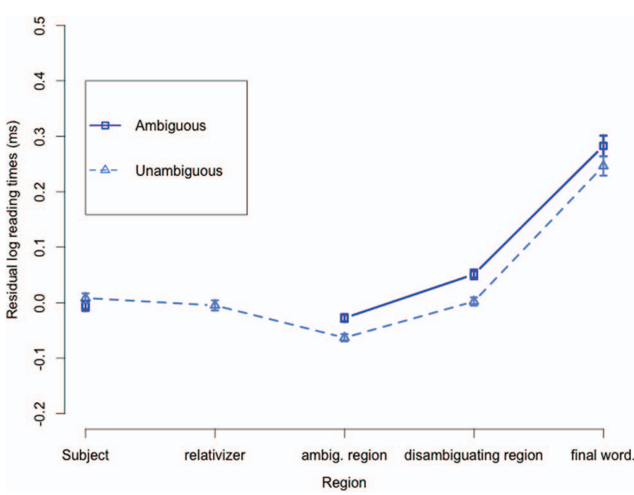

Figure 1. Mean residual log reading times at each sentence region in Experiment 1, plotted separately for ambiguous (dark, solid lines) and unambiguous (light, dashed lines) items. Error bars represent 95% confidence intervals on the means. See the online article for the color version of this figure.

Cumulative structural priming is predicted to surface as a change in the ambiguity effect over the course of the experiment. In the following, we thus focus on the disambiguating region, in which the ambiguity effect is observed (indeed, as shown in Table 1, the effects of interest only surfaced in this region, as predicted). We regressed mean residual log RTs during the disambiguating region onto ambiguity (ambiguous vs. unambiguous), item order (coded 1–40), and the interaction between the two. In order to control for task adaptation (i.e., an overall speed-up in RTs across all regions as a result of increasing familiarity with the self-paced reading paradigm), we included a main effect of log-transformed *stimulus order*. Stimulus order differs from item order in that it is an index of when an item was presented relative to both items and fillers. Stimulus order therefore captures how long (i.e., for how many trials) subjects have been doing the experiment, providing a measure of adaptation to the self-paced reading task; item order captures the number of RCs observed by subjects at a given point in the experiment, providing a measure of cumulative priming (specifically, the two-way interaction between ambiguity and item order). Here and in all experiments, all predictors were mean-centered to reduce collinearity with higher order interactions (remaining fixed effects correlation rs $< .2$, except for high collinearity between the main effects of item order and log stimulus order, $r = -.8$).[4] In this and all other analyses, the maximal random effects structure justified by the design was used (in Experiment 1, this corresponded to random intercepts for subject and item, as well as by-subject and by-item random slopes for ambiguity, item order, and their two-way interaction).

Results: RTs on the disambiguating region. There was a main effect of ambiguity: RTs during the disambiguating region were significantly slower in ambiguous sentences relative to unambiguous sentences ($\beta = .03$, standard error [SE] $= .003$, $p < .001$).[5] The main effect of log stimulus order also reached significance ($\beta = -.1$, $SE = .01$, $p < .001$), indicating that the disambiguating region was read faster with increasing exposure to the task. The main effect of item order did not reach significance ($\beta = -.002$, $SE = .001$ $p = .2$). Crucially, and as predicted, there was a significant two-way interaction between ambiguity and log item order ($\beta = -.001$, $SE = .0002$, $p < .001$): The ambiguity

[3] We log-transformed raw RTs because this transformation (a) allows RTs to more closely satisfy the assumption of normality, and (b) led to (mildly) better model fits than nontransformed RTs. The results reported in this article do not depend on this decision, though the noncritical ambiguity effect during the *ambiguous* region of Experiment 2 is significant when log-transformed RTs are used, but only marginally significant when nontransformed RTs are used.

[4] In the analyses for all three experiments, item order and log stimulus order have highly correlated effects (i.e., are highly collinear). We leave both in the models reported below because the two are theoretically distinct and often account for unique variance. Neither of the two effects is critical to our argument. By including stimulus order, we rule out the possibility that the critical effect of cumulative priming (i.e., a two-way interaction between ambiguity and item order) is driven by actual changes in subjects' expectations for the RC structure, rather than simply an overall decrease in RTs. In our previous work, we have discussed this issue in detail, ruling out this possibility both statistically and experimentally (see Fine et al., 2010, 2013). Collinearity between these two predictors does not affect the coefficients or standard errors of any other predictors.

[5] All significance levels are based on the t distribution, under the assumption that, for data sets of this size, t values with absolute values greater than 1.96 are significant at $\alpha = .05$.

Table 1
Coefficients and t Values for Each Predictor (Rows), at Each Sentence Region Columns), in Experiment 1

Predictor	Subject		Relativizer		Ambiguous region		Disambiguating region		Final word	
	β	*t*	β	*t*	β	*t*	β	*t*	β	*t*
Item order	.002	1.826	**−.003**	**−2.245**	−.002	−1.494	−.002	−1.846	**−.004**	**−2.247**
Log stimulus order	**−.130**	**−8.906**	**−.077**	**−4.512**	**−.108**	**−8.564**	**−.103**	**−7.215**	**−.078**	**−2.792**
Ambiguity (= ambiguous)	−.006	−1.511	NA	NA	**.019**	**6.103**	**.026**	**7.516**	**.021**	**3.210**
Ambiguity: Item order	−.001	−1.790	NA	NA	−.001	−.590	**−.001**	**−4.620**	**−.001**	**−1.898**

Note. For data sets of this size, *t* values with an absolute value of 1.96 or greater are significant at $p \leq .05$. Significant effects are in bold. The relativizer region was only present in unambiguous sentences; therefore, the effect of ambiguity cannot be measured at this region. We use NA to indicate this.

effect significantly decreased as subjects observed more and more RCs over the course of the experiment. To graphically demonstrate the effect of item order over and above stimulus order, we residualized raw RTs against both word length and stimulus order. Plotting these residual (log) RTs more clearly reveals the underlying cause of the change of the ambiguity effect—a diminished processing cost for ambiguous RCs relative to unambiguous RCs. This effect is visualized in Figure 2.

The two-way interaction between ambiguity and log item order was observed only during the disambiguating region and (marginally) during the final word, likely reflecting "spillover." The results of the model described above, fit separately to each sentence region, are summarized in Table 1.

Results: Comprehension question accuracy. A common concern in experiments like this (and, indeed, a concern raised by our reviewers) is that self-paced reading can be tedious. It is possible that subjects stop paying attention to the sentences they are reading in order to more quickly finish the task. This is sometimes thought to cause increasingly faster RTs, in particular, on difficult sentences such as ambiguous RCs. If this intuition is confirmed, this would constitute a confound. If the RT effects we reported above are indeed driven by decreasing attention, we should see decreases in the rate of correctly answered comprehension questions.

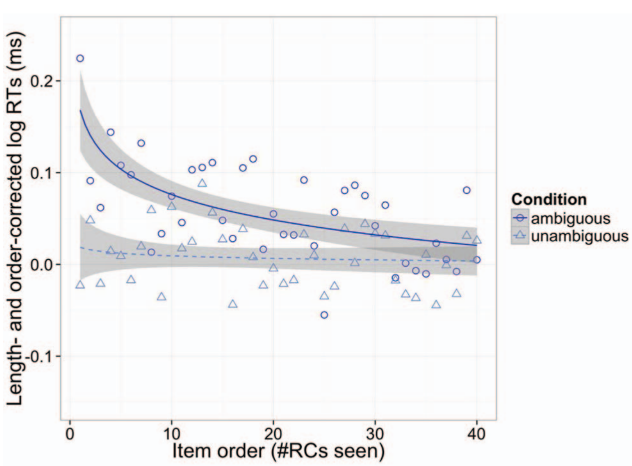

Figure 2. Change in residual log RTs from Experiment 1 during the disambiguating region for ambiguous (circles and dark, solid line) and unambiguous (triangles and light, dashed line) items as a function of item order. The plot reveals overall speed-ups in RTs as the experiment progresses, as well as a decrease in the ambiguity effect, that is, cumulative syntactic priming. RTs = reading times; RC = relative clause. See the online article for the color version of this figure.

To address this question, we used mixed-effects logistic regression to analyze comprehension question accuracy on critical trials. For ease of exposition, we present the analyses of comprehension question accuracy for all three experiments here. The comprehension question accuracy for ambiguous and unambiguous items in all three experiments is summarized in Table 2.

We regressed comprehension question accuracy onto ambiguity, item order, and the two-way interaction between these two predictors. The main effect of ambiguity went in the same direction for all three experiments (significant for Experiments 1 and 2): Subjects were more likely to incorrectly answer questions after ambiguous RCs. This replicates previous work (e.g., MacDonald et al., 1992) in suggesting that sentences with ambiguous RCs are indeed harder to process. The main effect of item order was not significant in Experiments 1 and 2, and was positive and marginally significant in Experiment 3 ($\beta = 0.02$, $SE = 0.008$, $p = .09$), suggesting that subjects in Experiment 3 became slightly *more* accurate, overall, as the experiment progressed. The crucial two-way interaction between ambiguity and item order was in the same direction in all three experiments (significant in Experiments 1 and 3, and marginally significant in Experiment 2, $p = .1$): Subjects became *better* at answering comprehension questions for ambiguous items relative to unambiguous items (confirmed by simple effect analyses, which found this improvement to be marginally significant for Experiment 1 and significant for Experiment 3); accuracy on unambiguous RCs remained unchanged throughout the experiments (all simple effect analyses, $p > .2$).

The analyses of comprehension question accuracy thus provide no evidence for decreasing attention as a result of boredom or fatigue. Rather, we see—in Experiments 1 and 3—an improvement in comprehension of ambiguous RCs, consistent with the idea that exposure facilitates processing.

Discussion

The results of Experiment 1 show that, as subjects read RC sentences, the processing advantage conferred by early disambiguating information ("who were," in the example above) on RTs during the later "disambiguating" region diminishes. We interpret this as evidence of cumulative structural priming. This finding replicates previous work on cumulative priming in syntactic comprehension (Farmer et al., 2014; Fine et al., 2013, 2010; Kamide, 2012; Kaschak & Glenberg, 2004; see also Long & Prat, 2008;

Table 2
Comprehension Question Accuracy Rates (After Subject Exclusions) for Ambiguous and Unambiguous Items in Experiments 1–3

Experiment	Ambiguous	Unambiguous
Experiment 1	10%	8%
Experiment 2	12%	8%
Experiment 3	10%	8%

Wells et al., 2009). In Experiment 2, we ask whether this effect depends on repeated verbs among the critical items.

Experiment 2: Cumulative Structural Priming Without Verb Repetition

Method

Experiment 2 involved exactly the same materials and procedure as Experiment 1, but without verb repetition. Eight of the critical items in this experiment were included in Experiment 1. The remaining 32 critical items were created by replacing the verb in one of the critical items from Experiment 1 with a near-synonym (e.g., changing "pushed" to "shoved" in "An impatient shopper shoved through the doors complained to the manager"). The critical items from Experiment 2 are included in Appendix A of the online supplemental materials.

The same 80 filler sentences were used here as in Experiment 1. Experiment 2 employed the exact same method to create four experimental lists (and two pseudorandom presentation orders) as Experiment 1, although items and fillers were not presented in the same exact order across Experiments 1 and 2.

Eighty subjects saw 40 critical items in one of the four possible lists. Subjects were recruited via Mechanical Turk according to the same procedure described for Experiment 1. Subjects required, at most, 30 min to complete the experiment.

Results

Data coding and exclusions. As in Experiment 1, RTs less than 100 ms or greater than 2,000 ms were excluded before computing residual log RTs. This resulted in 1% data loss. Next, we excluded trials on which subjects answered comprehension questions incorrectly (see Table 2). Two subjects with low accuracy rates on the comprehension questions (<80%) were excluded, leaving 78 subjects for the analyses reported below. After excluding these subjects, the average by-participants comprehension accuracy was 92% (SD = 3).

Analysis. By-region mean residual log RTs (i.e., averaging across words within each region) are plotted in Figure 3. The same broad patterns evident in Figure 1 are present in Figure 3, with significant ambiguity effects at the ambiguous, disambiguating, and final word regions, replicating both Experiment 1 and MacDonald et al. (1992) (cf. Table 3).

We then conducted the same analysis as in Experiment 1 to examine changes in the ambiguity effect over the course of the experiment. As in the analysis of the data from Experiment 1, we centered all predictors. Collinearity was generally low ($r < .2$), with the exception of high collinearity between the main effects of item order and log stimulus order (r = −.9). The maximal random effects structure justified by the design that would converge corresponded to random intercepts for subject and item, as well as by-subject and by-item random slopes for ambiguity, item order, and their interaction. We found main effects of ambiguity (β = .03, SE = .004, $p < .001$), item order (β = −.005, SE = .001, $p <$ .001), and log stimulus order (β = −.07, SE = .01, $p < .001$): As in Experiment 1, residual log RTs during the disambiguating region were overall higher for ambiguous relative to unambiguous items, and decreased with cumulative exposure to RC structures. Finally, there was a significant two-way interaction between ambiguity and log item order (β = −.001, SE = .0001, $p < .01$). We plot this effect in Figure 4, again using length- and stimulus-order-residualized log RTs.

The two-way interaction was observed in the disambiguating region. As in Experiment 1, the effect reached only marginal significance in the final word region (β = −.001, SE = .0004, p = .1). The results of this model, fit separately to the data from each sentence region, are summarized in Table 3.

Comparison of Experiments 1 and 2. Qualitatively speaking, Experiment 2 replicated Experiment 1: We observed significant main effects—of similar magnitudes—of ambiguity and item order, as well as a two-way interaction between these two variables, in both experiments. Our results so far suggest that verb repetition is not *required* for cumulative priming in syntactic comprehension. This echoes some previous trial-to-trial priming results (Arai & Mazuka, 2014; C. S. Kim et al., 2014; Thothathiri & Snedeker, 2008a, 2008b; Traxler, 2008). Next, we ask whether there is a *quantitative* difference in the priming effect across these experiments that is attributable to verb repetition, that is, a lexical boost effect in cumulative priming in comprehension.

First, we computed residual log RTs using the aggregated data from Experiments 1 and 2 in the manner described above (analyzing raw RTs returns identical results). This residualization step was

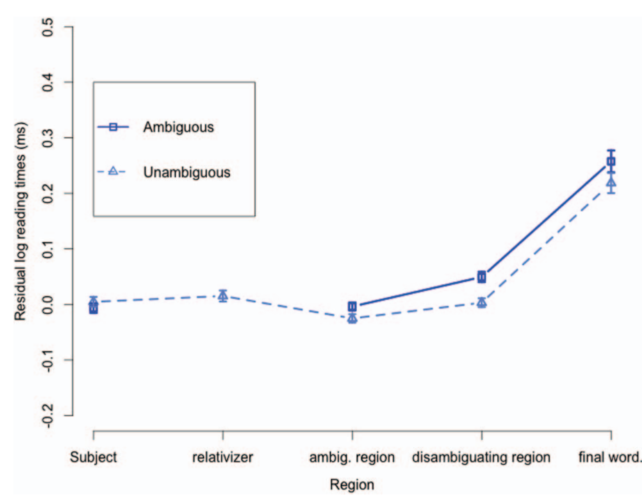

Figure 3. Mean residual log reading times at each sentence region in Experiment 2, plotted separately for ambiguous (dark, solid lines) and unambiguous (light, dashed lines) items. Error bars represent 95% confidence intervals on the means. See the online article for the color version of this figure.

Table 3
Coefficients and t Values for Each Predictor (Rows), at Each Sentence Region Columns), in Experiment 2

Predictor	Subject		Relativizer		Ambiguous region		Disambiguating region		Final word	
	β	*t*	β	*t*	β	*t*	β	*t*	β	*t*
Item order	−.002	−1.415	−.002	−1.612	**−.003**	**−2.698**	**−.005**	**−3.810**	**−.005**	**−2.757**
Log stimulus order	**−.078**	**−4.636**	**−.083**	**−3.658**	**−.085**	**−5.635**	**−.071**	**−5.185**	**−.083**	**−3.818**
Ambiguity (= ambiguous)	−.002	−.569	NA	NA	**.012**	**2.218**	**.026**	**5.848**	**.022**	**3.149**
Ambiguity−Item order	.001	1.068	NA	NA	.001	1.039	**−.001**	**−2.405**	−.001	−1.768

Note. For data sets of this size, *t* values with an absolute value of 1.96 or greater are significant at $p \leq .05$. Significant effects are in bold. The relativizer region was only present in unambiguous sentences; therefore, the effect of ambiguity cannot be measured at this region. We use NA to indicate this.

repeated on the aggregated data so that our analysis can detect experiment-specific effects (all effects replicate if RTs are residualized separately for each experiment and then combined). Next, we analyzed residual log RTs against the full factorial design of ambiguity, item order, and experiment (Experiment 1 or Experiment 2), as well as a main effect of log stimulus order. We included random intercepts for subjects and items, as well as by-subject and by-item random slopes for ambiguity, item order, and their two-way interaction).

Replicating the separate analyses of Experiments 1 and 2, the aggregate analysis of the disambiguation region revealed significant main effects of ambiguity ($\beta = .03$, $SE = .003$, $p < .001$), item order ($\beta = -.003$, $SE = .001$, $p < .01$), log stimulus order ($\beta = -.1$, $SE = .001$, $p < .001$), and the interaction between ambiguity and item order ($\beta = -.001$, $SE = .0001$, $p < .001$).

Most germane to the question of whether the effects observed across the two experiments differed in any way, neither the main effect of experiment nor any of the interactions in which it participated reached significance on the disambiguating region ($ps >$ .4). The results for all other sentence regions are summarized in Table 4.

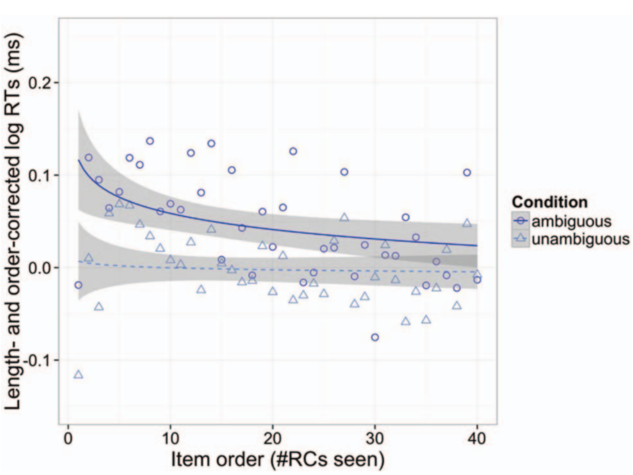

Figure 4. Change in residual log RTs from Experiment 2 during the disambiguating region for ambiguous (squares and dark, solid line) and unambiguous (triangles and light, dashed line) items as a function of item order. The plot reveals overall speed-ups in RTs as the experiment progresses, as well as a decrease in the ambiguity effect, that is, cumulative syntactic priming. RTs = reading times; RC = relative clause. See the online article for the color version of this figure.

With few exceptions, the results at all other sentence regions are as expected, with significant effects of stimulus order across all regions (and effects of item order at the disambiguating and final word regions), ambiguity effects at the ambiguous, disambiguating, and final word regions, and a two-way interaction between ambiguity and item order during the disambiguating region and the final word region.

Two effects were not expected. First, there was a main effect of experiment during the ambiguous region: during this region, residual log RTs were slightly lower for Experiment 1 than Experiment 2. This is somewhat surprising because the material during these regions was the same across experiments, except for the verb (the effect of experiment remains significant even when the verb is excluded from this region, though it is possible that such a lexical priming effect could spill over into the rest of the ambiguous region[6]). A second possible explanation for this effect is that the different semantic properties of the verbs across the two experiments generate different expectations during the ambiguous region, leading to differences in overall RTs in the ambiguous region.[7] There was also an unexpected effect of the three-way interaction during the subject region. We have no explanation for this effect.

Discussion

We manipulated verb repetition across Experiments 1 and 2 and found that the cumulative priming effect is of a quantitatively indistinguishable magnitude in both experiments. The results are consistent with the hypothesis that priming in comprehension is guided by the same mechanism as priming in production, and that this mechanism is comprised of a long-lasting implicit learning mechanism that is not sensitive to repeated lexical material and a short-lived advantage for repeated verbs, resulting from either declarative memory (Chang et al., 2006) or transient boosts in the activation of lemma information (Branigan et al., 1999; Malhotra, 2009).

[6] We thank an anonymous reviewer for this suggestion.

[7] It seems unlikely that the main effect of experiment is responsible for the lack of a significant three-way interaction at the disambiguating region, though it is possible that the different thematic relations, tied to different verbs, across the two experiments worked against, and thereby masked, an underlying difference in the adaptation effects across the two experiments.

Table 4
Coefficients (and t Values) for Each Predictor (Rows), at Each Sentence Region Columns), for the Combined Data From Experiments 1 and 2

Predictor	Subject		Relativizer		Ambiguous region		Disambiguating region		Final word	
	β	*t*	β	*t*	β	*t*	β	*t*	β	*t*
Experiment (= repeated)	.004	.622	.007	1.124	**−.013**	**−2.828**	.003	.482	.017	1.106
Item order	−.001	−.169	−.002	−1.555	**−.003**	**−2.957**	**−.003**	**−3.951**	**−.004**	**−3.266**
Log stimulus order	**−.997**	**−8.769**	**−.094**	**−6.599**	**−.096**	**−9.319**	**−.086**	**−8.310**	**−.085**	**−4.808**
Ambiguity (= ambiguous)	−.003	−1.472	NA	NA	**.015**	**5.082**	**.026**	**9.430**	**.022**	**4.579**
Experiment: Item order	.001	.586	−.001	−.190	.001	.435	.001	.454	.001	.902
Experiment: Ambiguity	−.002	−.698	NA	NA	.004	1.214	−.001	−.074	−.001	−.009
Ambiguity: Item order	−.001	−.756	NA	NA	.001	.626	**−.001**	**−5.048**	**−.001**	**−2.412**
Experiment: Ambiguity–Item order	**−.001**	**−2.007**	NA	NA	−.003	−1.207	−.001	−.785	−.001	−.128

Note. For data sets of this size, *t* values with an absolute value of 1.96 or greater are significant at $p \leq .05$. Significant effects are in bold. The relativizer region was only present in unambiguous sentences; therefore, the effect of ambiguity cannot be measured at this region. We use NA to indicate this.

Experiment 3: Cumulative Structural Priming Without Any Lexical Repetition

Method

Experiment 3 involved a modified version of the design and materials used in Experiments 1 and 2. Both Experiments 1 and 2 unintentionally contained repetition of lexical items apart from the verb (see Appendix A of the online supplemental materials). Twenty nonverb content words were repeated, on average, two times ($SD = 0.3$), averaging across all sentence regions. This affected 25 out of 40 items (that is, 60% of items contained one content word, e.g., "hot," that also occurred in another critical item). This nonverb lexical repetition was identical across Experiments 1 and 2. Such nonverb repetition across Experiments 1 and 2 is compatible with our goal to assess the effect of verb overlap on cumulative structural priming (most of previous work has focused on the role of verb overlap, or, more generally, lexical repetition of the *head* of the structure).

However, there is also some evidence from production that nonhead repetition can also affect structural priming. For instance, Cleland and Pickering (2003) find stronger priming effects in production when prime and target share a subject NP (see also Snider, 2008). Between our Experiments 1 and 2, seven out of 40 critical items contained subject NPs that overlapped lexically with the subject NPs of other items. It is thus possible that we failed to find a difference between Experiments 1 and 2 because repeated nouns, adjectives, and adverbs strengthened the priming effects in both experiments, making it harder to detect an effect of verb repetition. To address this possibility, care was taken in Experiment 3 to eliminate all repeated content words across critical and filler items. Additional motivation for Experiment 3 comes from the fact that the comparison of Experiments 1 and 2 yielded a null effect with regard to verb overlap. One must always exercise caution when interpreting null effects, and replicating the effect constitutes good practice as an initial step in interpreting it.

Experiment 3 employed the exact same procedure as Experiments 1 and 2 above. Eighty subjects participated in this study, and three were removed because of low (<80%) accuracy rates on the comprehension questions.

Results

Data coding and exclusions. We excluded trials with abnormally high (>2,000ms) and low (<100ms) RTs, as well as trials on which subjects answered comprehension questions incorrectly. After exclusions, mean by-subject accuracy on the comprehension questions was 93% ($SD = 3$), averaging across critical items and fillers.

Analysis. By-region mean residual log RTs (i.e., averaging across words within each region) are plotted in Figure 5. The same broad patterns evident in Figures 1 and 3 are present in Figure 5, with significant ambiguity effects at the ambiguous, disambiguating, and final word regions, replicating Experiments 1 and 2 and MacDonald et al. (1992) (cf. Table 5). One unexpected effect of ambiguity was observed during the subject region—with unambiguous items being read more *slowly*. We comment on this below.

We analyzed residual log RTs during the disambiguating region using the same modeling procedure employed in the analysis of the data from Experiments 1 and 2. The model included random intercepts for subjects and items, as well as by-subject and by-item random slopes for ambiguity, item order, and their interaction. This experiment replicated Experiments 1 and 2: There were significant main effects of ambiguity ($\beta = .03$, $SE = .004$ $p < .001$) and log stimulus order ($\beta = -.1$, $SE = .02$ $p < .001$). The main effect of item order did not reach significance ($\beta = .001$, $SE = .001$, $p = .6$). Crucially, we observed the predicted two-way interaction between ambiguity and item order ($\beta = -.001$, $SE = .0001$, $p < .001$). The two-way interaction is shown in Figure 6. Once again, we visualize the cumulative priming effect using length- and stimulus-order-residualized log RTs

We summarize the results of the regression model described above, fit separately to the data from each sentence region, in Table 5.

The two-way interaction was observed only during the disambiguating region, but not in the final word region (the latter was significant in Experiment 1 and marginally significant in Experiment 2). The ambiguity effect was observed at the ambiguous region, the disambiguating region, and the final word, replicating Experiments 1 and 2, as well as MacDonald et al. (1992). We also

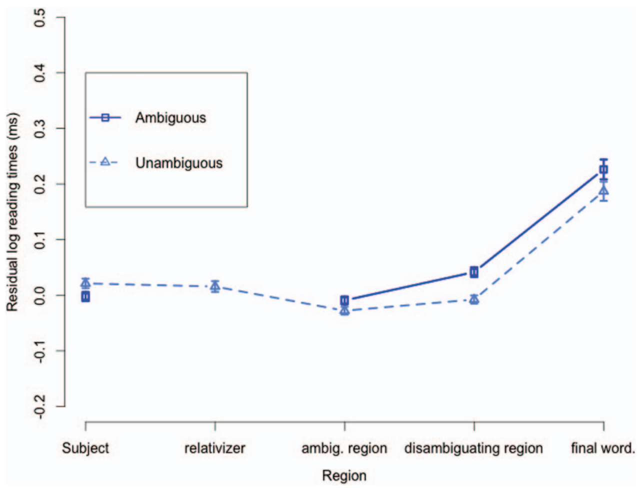

Figure 5. Mean residual log reading times at each sentence region in Experiment 3, plotted separately for ambiguous (dark, solid lines) and unambiguous (light, dashed lines) items. Error bars represent 95% confidence intervals on the means. See the online article for the color version of this figure.

observed an effect of log stimulus order across all regions, reflecting overall speedup in RTs over the course of the experiment.

In Experiment 3, we also observed an unexpected "ambiguity effect"—slightly greater RTs for *un*ambiguous sentences—during the subject region. This effect was surprising given that the material during the subject region was identical across ambiguous and unambiguous sentences, which might explain the small magnitude of the effect. We have no explanation for this effect.

Comparing Experiments 1 and 3. We next tested whether there were any differences in the cumulative structural priming effects observed across Experiments 1 and 3. As in the comparison of the data from Experiments 1 and 2, we began by computing residual log RTs using the aggregated data from Experiments 1 and 3. Next, we regressed residual log RTs onto the full factorial design of ambiguity, item order, and experiment (Experiment 1 or Experiment 2), as well as a main effect of log stimulus order. We included the maximal random effects structure justified by the design (i.e., random intercepts for subjects and items, as well as by-subject and by-item random slopes for ambiguity, item order, and their two-way interaction).

The results of this comparison echo the results of the comparison, above, of Experiments 1 and 2. We found—replicating the analyses of each of the separate experiments—a significant main effect of log stimulus order ($\beta = -.1$, $SE = .005$, $p < .001$), a significant main effect of ambiguity ($\beta = .03$, $SE = .005$, $p < .001$), and a significant two-way interaction between ambiguity and item order ($\beta = -.001$, $p < .001$). Moreover, the three-way interaction between ambiguity, item order, and experiment—which captures the quantitative difference in the magnitude of the cumulative structural priming effects across the two experiments—was not significant ($p = .5$). In other words, even after completely removing repeated lexical items from the materials used in Experiment 2, there is no evidence that cumulative syntactic priming in comprehension is any weaker than in Experiment 1, in which verbs were systematically repeated (and other content words were unsystematically repeated). The results of this model, fit separately to each sentence region, are summarized in Table 6.

Echoing the results of Experiments 1 and 3, we observed a main effect of log stimulus order across all sentence regions (with a significant main effect of item order only during the final word), as well as a significant main effect of ambiguity during the ambiguous, disambiguating, and final word regions. The two-way interaction between ambiguity and item order was also observed at the disambiguating region. As in the separate analysis of Experiment 3, we observed a significant main effect of ambiguity during the subject region, such that unambiguous items were read more slowly during this region. This effect was not observed during the subject region in the separate analysis of Experiment 1, though the two-way interaction between experiment and ambiguity during the subject region only approached significance ($\beta = .003$, $SE = .01$, $p = .19$).

Finally, replicating the comparison of Experiments 1 and 2 above, we found the same two, somewhat surprising effects. First, there was a main effect of experiment during the ambiguous region, such that RTs were slightly faster for Experiment 1 relative to Experiment 3 (again, the effect was driven by Experiment 1). As stated above, this effect may be attributable to different semantic properties of the verbs across the two experiments generating different expectations during the ambiguous region, leading to differences in overall RTs in the ambiguous region (recall that the same verbs were used in Experiments 2 and 3). Second, a hint of the unexpected three-way interaction between ambiguity, item order, and experiment—observed in the comparison of Experiments 1 and 2—was observed here (marginally significant at $p = .1$). That these two effects replicate to some degree is not surprising, as (a) both comparisons involve Experiment 1, and (b) Experiments 2 and 3 were mostly identical stimuli (the same differ-

Table 5
Coefficients and t Values for Each Predictor (Rows), at Each Sentence Region Columns), in Experiment 3

	Subject		Relativizer		Ambiguous region		Disambiguating region		Final word	
Predictor	β	*t*	β	*t*	β	*t*	β	*t*	β	*t*
Item order	.001	.667	−.001	−.957	−.001	−.041	.001	.719	−.003	−1.232
Log stimulus order	**−.086**	**−5.028**	**−.071**	**−3.375**	**−.111**	**−5.942**	**−.129**	**−6.985**	**−.102**	**−3.405**
Ambiguity (=ambiguous)	**−.012**	**−3.287**	NA	NA	**.099**	**2.185**	**.027**	**6.232**	**.021**	**4.114**
Ambiguity–Item order	.001	.283	NA	NA	−.001	−.177	**−.001**	**−3.327**	−.001	−.075

Note. For data sets of this size, *t* values with an absolute value of 1.96 or greater are significant at $p \leq .05$. Significant effects are in bold. The relativizer region was only present in unambiguous sentences; therefore, the effect of ambiguity cannot be measured at this region. We use NA to indicate this.

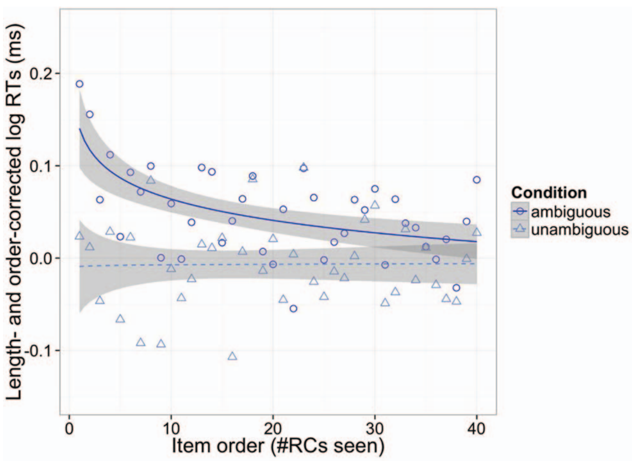

Figure 6. Change in residual log RTs from Experiment 3 during the disambiguating region for ambiguous (squares and dark, solid line) and unambiguous (triangles and light, dashed line) items as a function of item order. The plot reveals overall speed-ups in RTs as the experiment progresses, as well as a decrease in the ambiguity effect, that is, cumulative structural priming. RTs = reading times; RC = relative clause. See the online article for the color version of this figure.

ences were not observed when comparing Experiments 2 and 3, $p > .5$).

Discussion

In Experiment 3, we sought to replicate the cumulative structural priming effects observed in Experiments 1 and 2 after removing all repeated content words in the materials of Experiments 1 and 2. By replicating the crucial effects of Experiments 1 and 2—main effects of ambiguity and log stimulus order, as well as a two-way interaction between ambiguity and item order during the disambiguating region—the results of Experiment 3 provide further evidence for cumulative structural priming in comprehension that does not require verb repetition. In short, the results of Experiment 3—as well as the results of the comparison of Experiments 1 and 3—suggest that cumulative syntactic priming in comprehension does not depend on lexical repetition. In the General Discussion, we elaborate on the ramifications of our results.

General Discussion

This article set out to examine whether cumulative priming during syntactic comprehension is sensitive to verb repetition. In three self-paced reading experiments, we found clear evidence for cumulative structural priming, but no evidence that this effect depended on verb repetition. Specifically, independent of verb repetition, we found that the ambiguity effect for MV/RC-ambiguous sentences significantly diminished—and eventually was undone completely—as subjects accumulated experience with RCs.

Before discussing the theoretical implications of our results, two points bear brief emphasis. The first is that a failure to observe an effect of verb repetition is a null effect, and one must always exercise caution when interpreting such effects. That being said, it is important to note that several previous articles have claimed that there is *no* structural priming in comprehension without verb overlap (Arai et al., 2007; Ledoux et al., 2007; Tooley et al., 2009; Traxler & Pickering, 2005; Traxler & Tooley, 2008). Given that we observe strong cumulative priming, a “null effect” of a boost for verb overlap—when priming is still observed both with and without overlap—is informative. Second, results like those reported in this article suggest that traditional sentence processing experiments employing similar designs may be systematically underestimating the magnitude of garden-path and other effects (see Fine et al., 2013, and Jaeger, 2010, for further discussion).

The Lexical Boost and the Mechanism(s) Underlying Structural Priming

As we discussed in the introduction, the current study builds on previous work on cumulative priming in comprehension (Fine et al., 2013; Kamide, 2012; Kaschak & Glenberg, 2004; Long & Prat, 2008; Wells et al., 2009), and extends this work by examining cumulative priming along a dimension previously overlooked in that literature (verb repetition) that has been discussed extensively in the trial-to-trial priming literature.

Table 6
Coefficients and t Values for Each Predictor (Rows), at Each Sentence Region Columns), for the Combined Data From Experiments 1 and 3

	Subject		Relativizer		Ambiguous region		Disambiguating region		Final word	
Predictor	β	*t*	β	*t*	β	*t*	β	*t*	β	*t*
Experiment (= 1)	.006	.893	−.001	−1.838	**−.016**	**−3.596**	.003	.472	.029	1.887
Item order	.002	1.872	−.002	−1.576	−.001	−1.317	−.001	−1.275	**−.003**	**−2.170**
Log stimulus order	**−.110**	**−9.753**	**−.077**	**−4.519**	**−.103**	**−9.151**	**−.106**	**−9.267**	**−.091**	**−4.250**
Ambiguity (=ambiguous)	**−.009**	**−3.654**	NA	NA	**.014**	**5.346**	**.026**	**9.768**	**.022**	**5.162**
Experiment: Item order	.001	1.417	−.001	−1.509	−.001	−1.340	−.001	−1.625	−.001	−.399
Experiment: Ambiguity	−.003	−1.326	NA	NA	.005	1.717	−.001	−.290	−.001	−.049
Ambiguity: Item order	−.001	−1.119	NA	NA	−.001	−.553	**−.001**	**−4.692**	−.001	−1.469
Experiment: Ambiguity–Item order	.001	1.658	NA	NA	−.001	−.173	−.001	−.412	−.001	−1.296

Note. For data sets of this size, *t* values with an absolute value of 1.96 or greater are significant at $p \leq .05$. Significant effects are in bold. The relativizer region was only present in unambiguous sentences; therefore, the effect of ambiguity cannot be measured at this region. We use NA to indicate this.

Although there is some amount of equivocation with respect to this question in trial-to-trial priming in comprehension, the emerging consensus seems to be that structural priming does not require repeated verbs, but can be strengthened (or "boosted") by it (Tooley & Bock, 2014; Traxler, 2014; Traxler et al., 2014). In syntactic production, a much more substantial body of work on structural priming has also shown evidence for a lexical boost (Hartsuiker et al., 2008; Pickering & Branigan, 1998). Interestingly, in syntactic production, although priming effects for lexically independent syntactic structures seem to be long-lived, the lexical boost appears to rapidly decay (but see Coyle & Kaschak, 2008). This poses one of the central empirical challenges for models of priming in syntactic production: Transient activation accounts readily explain short-lived as well as lexically specific priming effects (e.g., Pickering & Branigan, 1998) but do not explain longer-lasting, learning-based effects. By contrast, implicit learning accounts readily explain long-lasting effects of abstract structural priming, but do not easily explain the lexical boost. In response to this challenge, current models of structural priming in production often propose a mechanism comprised of two components—one suitable to capturing short-lived changes in the processing of a given structure, and another capturing longer-lasting changes. For example, Reitter et al. (2011) proposed an ACT-R based model of priming in production, in which distributional patterns over syntactic structures are implicitly learned, and lemma-based (or verb-based) information is subject to rapid power-law decay. Chang et al. (2006) took a different tack and argued that long-lived effects of syntactic priming are driven by an error-driven implicit learning mechanism (implemented via the back-propagation learning algorithm in a connectionist architecture), and the lexical boost effect is mediated by explicit (declarative) memory for specific lexical items that is known to very rapidly decay (cf. Chang et al., 2006, p. 256, for discussion, as well as Chang, Janciauskas, & Fitz, 2012, for more recent work that develops this hypothesis).

Given the state of the field in structural priming in production, in order to understand whether priming in comprehension and production share a similar or identical underlying cause, it is crucial to fill in the empirical picture and ask whether the effect of lexical repetition behaves the same over short and longer time scales in comprehension, or whether the lexical boost in comprehension is similarly short-lived. The results of the current experiments, taken together with previous work showing evidence for the lexical boost in comprehension (Tooley & Bock, 2014; Traxler, 2014; Traxler et al., 2014), suggest that the lexical boost in structural priming in comprehension is short-lived, as has been shown in production (Hartsuiker et al., 2008).

On the face of it, then, our results seem to provide further support for the mechanistic parity between structural priming in comprehension and production, as previously argued by Chang et al., 2006 and Tooley & Bock, 2014. In the introduction, we briefly mentioned a different account of structural priming—related to recent work on expectation adaptation (Fine et al., 2013; Jaeger & Snider, 2013; Kleinschmidt & Jaeger, 2015)—that provides an alternative perspective on the effect of verb repetition in priming. Specifically, if comprehenders are sensitive to (a) the "bursty" distribution that lexical items (including verbs) follow in natural language, and (b) the correlation between verbs and syntactic structures (Trueswell, 1996), it is possible that comprehenders will expect specific verb-structure pairs to be repeated within a given linguistic environment, which would surface as a lexical boost effect in cumulative priming in comprehension. Although our results do not support this hypothesis, there are at least two plausible explanations as to why we may not have observed an effect of verb repetition that follow naturally from some recent work on expectation adaptation (Craycraft, 2014; Fine et al., 2013; Jaeger & Snider, 2013; Kleinschmidt & Jaeger, 2015; Myslín & Levy, n.d.). First, it is possible that the materials in our experiments were sufficiently thematically and semantically unrelated (as is the case in most lists of sentence processing stimuli) that subjects did not (implicitly) infer that specific verb-structure pairs were likely to be repeated over the course of the experiment. If this is correct, then experimental materials such as those that we used here could be modified in order to suggest the existence of a common underlying "topic" that might lead to longer-lasting effects of verb repetition. We leave this to future work.

Second, it is possible that the lack of a lexical boost effect in Experiment 1 (relative to Experiments 2 and 3) is a result of the fact that verbs in our experiments are not *informative* with respect to syntactic structure: In all experiments reported above, sentences with MV/RC ambiguities were *always* resolved toward the RC interpretation, regardless of the verb. Even if participants come in to our experiments with the expectation that verbs are informative about the syntactic structures that follow them (as the works cited above strongly suggest, especially Trueswell, 1996), they might quickly revise this expectation in light of the statistical properties of the experiment. Future work can address this question by testing whether verb-specific structural priming is observed in environments (experiments) in which verbs remain informative about the structures that follow them. For instance, if one set of MV/RC-ambiguous verbs always occurs with RCs, and another set always occurs with MVs. for the first portion of an experiment, then processing RCs in a subsequent portion of the experiment should be facilitated for sentences including those verbs that always occurred with RCs earlier in the experiment. Appendix B provides a preliminary test of this hypothesis.

Trial-to-Trial and Cumulative Structural Priming: Bridging the Gap

Partially independent of the question of whether the same mechanism underlies structural priming in comprehension and production, we believe the current results—along with other recent research on cumulative structural priming in comprehension—take an important step in bridging the gap between what we have referred to as trial-to-trial priming in comprehension, on the one hand, and cumulative priming in comprehension, on the other. The connection between trial-to-trial and cumulative priming effects has received little attention in previous work, even when it was arguably relevant (though see Fine et al., 2013, for discussion). Here, we highlight two questions in which the two paradigms can mutually inform each other.

One such question relates to the persistence of structural priming effects in comprehension. For example, in a recent article, Tooley, Swaab, Boudewyn, Zirnstein, and Traxler (2014) examined trial-to-trial priming in comprehension when multiple (manipulated to range from 0–3) unrelated sentences intervene between primes and targets with overlapping verbs. Tooley and colleagues found that structural priming persisted over intervening trials. This replicates earlier work by Fine et al. (2013; Experiment 1, cf. Figure 5), who found cumulative priming effects for the same structure (sentences with temporarily ambiguous RCs) in a paradigm in which RCs are separated, on average, by four intervening sentences (see also Fine et al., 2010, as well as the current results). The current results further suggest that persistent cumulative priming is also observed in the absence of verb overlap (see also Craycraft, 2014; Farmer et al., 2014; Fine et al., 2013).

Another area in which cumulative priming paradigms like the one employed here can inform research on trial-to-trial priming relates to the role of *cue informativity*. Recent studies on cumulative structural priming have found that comprehenders can exploit the informativity or reliability of, say, lexical or contextual cues to syntactic structure in order to learn the environment-specific statistics of syntactic constructions (Craycraft, 2014; Myslin & Levy, submitted; for related work in production, see Coyle & Kaschak, 2008). This seems to contrast with studies on trial-to-trial priming that have addressed related questions (S. Kim & Mauner, 2006; Traxler & Tooley, 2008). For example, Traxler and Tooley (2008) found that repeated subject NPs across prime-target pairs did not lead to significant priming, and interpret this lack of an effect as evidence against "strategic prediction" accounts of syntactic priming. One possibility, however, is that the effects of repeated subject NPs are too small to be observed in trial-to-trial priming, or that they only emerge after participants have *learned* that, in the current environment, the lexical identity of subject NPs is informative about upcoming syntactic structures. Indeed, recent evidence from a series of studies on cumulative structural priming in comprehension lends credence to the latter idea. Craycraft (2014) found that participants could both learn and exploit the fact that, in their experiment, subject NPs were correlated with specific syntactic structures. We consider this an interesting venue for future research. For example, it is possible to reanalyze studies on trial-to-trial priming using statistical procedures like those employed in the current study. This can shed light on how trial-to-trial priming changes throughout the course of the experiment (for examples, see Arai & Mazuka, 2014; Jaeger & Snider, 2013).

Conclusion

In conclusion, we found no difference in the magnitude of the strength of cumulative structural priming during comprehension depending on verb repetition. With respect to theoretical debates informing most previous work on syntactic priming, these results provide further support for the mechanistic parity of priming in comprehension and production, with longer-lived learning effects operating over abstract structural information and short-lived boosts in the activation of lexical-structural information. That being said, this study is likely only one of many steps that must be taken to fully understand the relationship between online processing and recent linguistic experience, and the mechanism or mechanisms that mediate this relationship.

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