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Priming quantifier scope: Reexamining the evidence against scope inversion

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In a study of quantifier-scope priming, Chemla and Bott (2015) found evidence suggesting that, while representations of quantifiers' relative scope can be primed, a scope inversion operation cannot. We identify a confound in their materials. In Experiment 1, we replicate their finding with this confound intact. In Experiment 2, we remove the confound and find that all priming disappears. This confound demonstrates how structural priming paradigms can be sensitive to many dimensions of similarity, pointing to a need for task-specific controls. We conclude that the prior study does not provide evidence concerning the priming of either relative scope representations or operations. While priming of scope representations has been independently found in other paradigms, the jury is still out on Chemla and Bott's more novel finding – the absence of priming of a scope inversion operation.

Keywords: quantifier raising; scope; inversion; structural priming; logical form

1 Introduction

When we think, we systematically combine concepts to create complex thoughts. When we speak, we systematically combine words to create complex sentences. Since words express concepts and sentences express thoughts, one simple hypothesis is that the syntactic rules for combining words correspond one-to-one to the semantic rules for combining concepts, so that each natural language sentence expresses a unique thought. Sentences that are semantically ambiguous – cases where the same sentence, under a single syntactic and lexical analysis, can express two different thoughts – pose a significant challenge to this hypothesis. They are therefore an excellent tool for studying how the rules of syntax and the rules of semantics might differ. One systematic type of semantic ambiguity arises for doubly-quantified sentences such as:

(1) There is a circle above every star.

This sentence could mean either that there is a single circle, perched above all of the stars, or that, for each star, there is some circle above it, potentially a different one in each case (see Figure 1). Since a speaker surely knows which of these meanings they want to convey, they must be forming a representation that is unambiguous, unlike the English sentence. To account for this, many theories (e.g. Hornstein 1984; May 1985; Heim & Kratzer 1998) posit a level of representation distinct from the surface form of the sentence, typically called *Logical Form* (LF), which are explicit about the semantic relations between words in a sentence. The two LFs corresponding to each meaning of (1) can be glossed in predicate logic as:

(2) a. $\forall x[\text{Star}(x)) \rightarrow \exists y[\text{Circle}(y) \wedge \text{above}(y,x)]]$
For every *x*, if *x* is a star, then there exists a *y*, such that *y* is a circle and *y* is above *x*.

b. $\exists y[\text{Circle}(y) \wedge \forall x[\text{Star}(x) \rightarrow \text{above}(y,x)]]$
There exists a *y*, such that *y* is a circle, and for all *x*, if *x* is a star, then *y* is above *x*.

Sentences like (1) are called "scopally ambiguous" because the two readings differ in terms of which quantifier takes scope over the other. In (2a), the universal quantifier *every* takes wide scope –for every star, that star is above a (potentially different) circle. We refer to this as a *universal-wide* or U-wide reading. It is also the *inverse scope* reading, since the order of the quantifier at LF is the inverse of their linear order in the original sentence (1). (2b) is the interpretation where there is a circle which all the stars are above. This LF has the existential quantifier, *a*, taking wide scope, and we call it the *existential-wide* or E-wide reading.

How are different LFs constructed from one ambiguous sentence? Although there are diverse theories addressing this question, they all propose that when a quantifier is in object position, there are interpretive mechanisms that can assign it semantic wide scope, thereby deriving the inverse scope reading. Recent empirical studies have looked for evidence of such an inversion operation using priming paradigms. These experiments first elicit one unambiguous LF representation (such as 2a or 2b) for a scopally ambiguous sentence (such as 1), and test whether participants are more likely to arrive at a similar LF for another sentence, like:

(3) Every square is below a triangle.

There are two possible patterns of priming. If getting the U-wide reading of (1) makes it easier to get the *U-wide* reading of (3), this would suggest that what can be primed is the U-wide LF representation. If, on the other hand, the U-wide reading of (1) makes the *E-wide* reading of (3) more available, then it would suggest that what is primed is an inversion operation that picks out the second quantifier in both sentences. Either pattern of priming would require there to be a representation or operation that abstracts away from all the other differences between (1) and (3), such as the different word orders and the different nouns being quantified, but still encodes the relative scope between the universal and existential quantifiers.

Using picture stimuli with juxtapositions of arbitrary symbols, Chemla & Bott (2015; henceforth, C&B) tested sentences containing *every* and *a*, and found that U-wide LFs prime subsequent U-wide LFs. They fully varied the order of the two quantifier words and found that the same scopal relation was primed in all cases – U-wide primes elicited more U-wide target readings than E-wide primes did, independently of the quantifiers' order in the two sentences. These findings suggest that only LF representations can be primed. Had there been any priming of a scope inversion operation, U-wide primes of *a-every* sentences would have increased the availability of E-wide readings of *every-a* sentences.

However, before we can conclude that a scope inversion operation cannot be primed, there is an alternative explanation of C&B's results that must be investigated. The effects in this study could be the result of the priming of different strategies for completing the task rather than the priming of different LFs. As shown in the upper panel of Figure 1 (cf. Experiment 1), there are clear similarities between all the U-wide pictures and a different set of similarities between all the E-wide pictures. These similarities support a very simple strategy: a) for U-wide primes scan the picture from side-to-side, ensuring that the two central rows of elements are uniform (e.g. in the case of (1), a row of stars and a row of circles); b) for E-wide primes, scan the single central column of elements up-and-down, ensuring that it is uniform up to the last element (three shapes of the same kind in a column, and a different shape on the end). Once they realize this strategy leads to the correct choice, participants may prefer it to the alternative that requires more time and careful attention to each element (i.e. checking each star and circle in relation to each other).

Importantly, these strategies work regardless of whether the quantifier order is U-E or E-U, whether the predicate in the sentence is *above* or *below* or, and whether the symbols are circles or hearts. Once participants have identified that these strategies lead to correct responses on prime trials, they no longer need to attend to these sentences in order to breeze through the task, giving correct responses. If these strategies themselves are primed, then participants would be more likely to use the same strategy on the target trial as they had on the prime trial. For example, if they were primed to scan side-to-side for two rows on a U-wide prime, they would be more likely to choose the target picture that is amenable to the same scanning strategy – also U-wide. Critically, this hypothesis accounts for the observed pattern of results: greater U-wide responding on targets following U-wide than E-wide primes, regardless of quantifier word order, or whether the prime and target have the same predicate.

C&B argue against the possibility of priming based on visual similarity in their paradigm based on (a) the absence of visual priming effects in a prior study of LF priming (Raffray & Pickering, 2010), and (b) the existence of priming effects across different predicates in their own data. Neither argument is conclusive. While Raffray and Pickering had a control condition for picture priming in their task, their stimuli were not comparable to C&B's. They used pictures of people and objects in 12 different, asymmetrical agent-theme relations, described with transitive verbs, while C&B used arbitrary symbols arranged in two spatial configurations (one central column or two central rows) and two antonymic predicates (*above* and *below*). The lack of picture priming in Raffray and Pickering's paradigm therefore cannot rule out picture priming in C&B's. Nor can picture priming be ruled out by the presence of a priming effect across the predicates *above* and *below*, since the same picture configurations are used for both predicates within a given LF (see Figure 1). Testing the effect of visual priming requires a control condition.

The present study provides this control. In Experiment 1, we first replicate C&B, with the predicates *above* and *below* in both prime and target, just as in the original study. This experiment differs from C&B's only in that we have eliminated one of their sentence configurations – U-NEG sentences – which they found failed to prime anything. In Experiment 2, we change the predicates in the prime to: *to the left of*, *to the right of*, or *next to*, changing the pictures in the prime to match. We use the same target sentences and pictures as in the previous study (*above* and *below*). We now find no priming effect, suggesting that C&B's results are indeed tied to visual similarity between primes and targets.

2 Experiment 1

2.1 Methods

2.1.1 Participants

Using 80 participants, C&B found significant priming when averaging across different quantifier word orders, but not in each order separately, suggesting that more participants may be needed to find these relatively small effects. For this reason, and because we wanted to provide a strong test of the replicability of their finding, we doubled the number of participants. We recruited 175 participants on Amazon Mechanical Turk, and following C&B's criteria, excluded 25 of them for failing more than 10% of filler trials.

2.1.2 Materials

Sentences. Experimental sentences were constructed according to one of two frames:

- U-E sentences: Every [shape 1] is [predicate] a [shape 2]
- E-U sentences: There is a [shape 1] [predicate] every [shape 2]
- Filler sentences: Every [shape] is [color]

The shapes were hearts, squares, triangles, stars, diamonds or circles. The predicates were *above* and *below*. Examples are shown in Figure 1 (upper panel). There were four lists of stimuli, with participants randomly assigned to a list. Each list was obtained by randomly inserting shapes into the appropriate sentence frame (with shape 1 and 2 always differing from each other). Within a list, trials were administered in random order to each participant using the Ibex platform created by Alex Drummond (http://spellout.net/ibexfarm/).

Images. For each sentence, we constructed three types of images: a foil (F) consistent with none of the interpretations, an image consistent only with the U-wide interpretation, and an image consistent only with the E-wide interpretation. Prime trials paired a sentence with its foil image and either a correct U-wide or E-wide image. The choice of the correct image thus forced one of the two interpretations. Target trials paired a sentence with a correct U-wide and a correct E-wide image. Participants thus chose the image

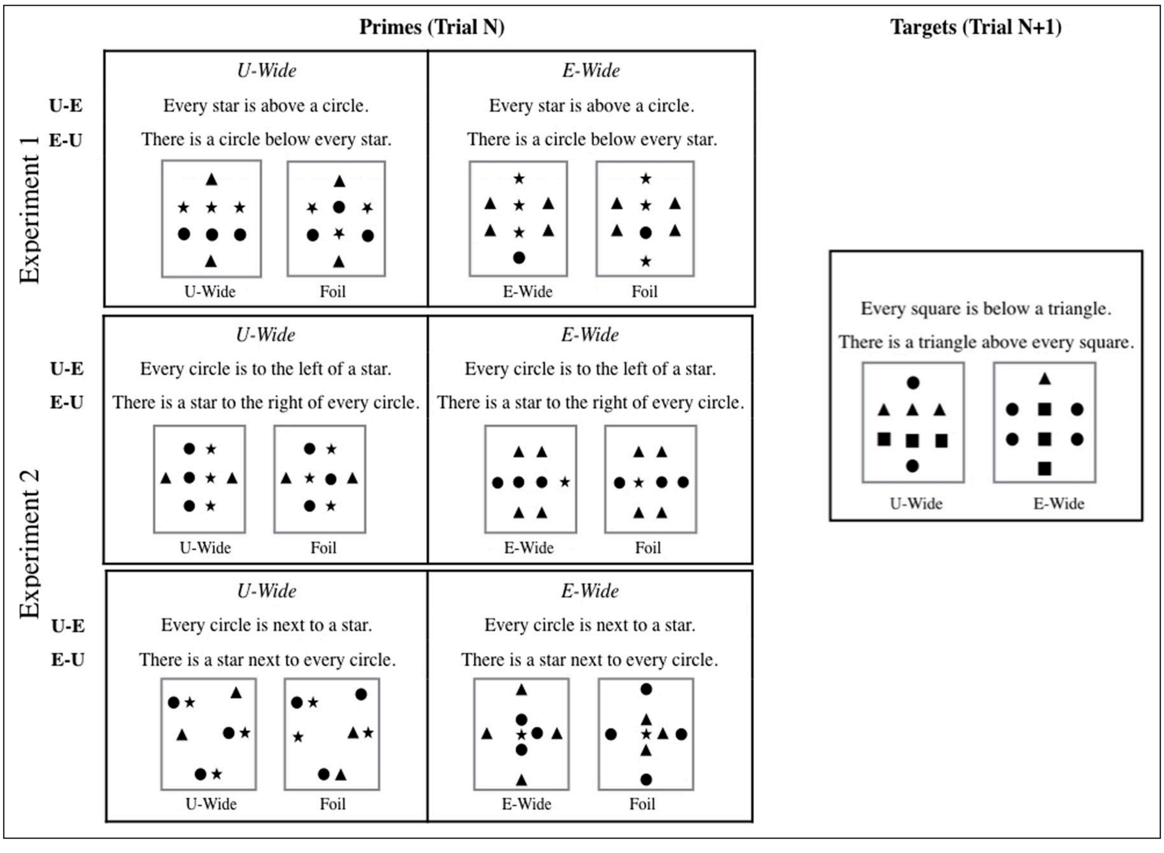

Figure 1: Sample prime and target trials in Experiment 1 (upper) and Experiment 2 (lower). Experiment 1 has the predicates *above* and *below*, replicating Chemla & Bott (2015). Experiment 2 used two types of predicates in the primes: LEFT/RIGHT (top) and *next to* (bottom). Participants read one sentence (either U-E, on the top row in each trial, or E-U, on the lower row) and had to choose which of the two pictures matched that sentence. Among the prime trials, examples of U-wide primes are on the left and of E-wide primes are on the right. The right-hand picture in each pair of prime trials shows the foil, or incorrect choice. The left-hand picture shows correct prime choices (either U- or E-wide, depending on the type of prime). On target trials, the left-hand picture shows the U-wide choice and the right-hand pictures shows the E-wide choice corresponding to the example sentence.

corresponding to their preferred interpretation. For filler trials, just one of the two images made the sentence true.

2.1.3 Procedure and Design

Experimental trials were presented in a prime-target pair. Primes consisted of one of two quantifier word orders (U-E or E-U), one of two predicates (*above* or *below*), and presented a correct picture consistent with one of two interpretations (U-wide or E-wide). Targets similarly contained one of two word orders (U-E or E-U). As in C&B, the predicates in target trials were randomly and independently chosen from *above* or *below*. Thus, Experiment 1 manipulated four within-subjects factors with two levels each: Word Order in Primes (U-E or E-U); Predicate in Primes (*above* or *below*); Word Order in Targets (U-E or E-U); and Prime Scope (U-wide or E-wide). A complete experimental set therefore consisted of $2 \times 2 \times 2 \times 2 = 16$ prime-target pairs. We further counterbalanced the position of the correct image in prime trials and the corresponding image in target trials (left or right) to obtain 4 trials for each of the 8 experimental pairs (32 pairs). There were also 64 filler trials, randomly interspersed between prime-target pairs, making $(64 * 2) + 64 = 192$ trials.

2.2 Results

Following C&B, we discarded responses on target trials if participants had answered the preceding prime incorrectly. This was rare (E-U primes: 1.67% U-E primes: 1%). We examined whether participants chose the U-wide picture on target trials as a factor of the other variables we manipulated. Because participants' choice on each trial was binary, we analyzed the data using a logit mixed-effects model (Jaeger 2008) implemented in the R programming language, using the lme4 package, (Bates et al. 2015).[1] Data and analyses for both experiments are available online on the Open Science Framework (https://osf.io/n7q3x/) and in supplementary materials.

We started with an omnibus model, including all of the variables which C&B had investigated separately: Prime Scope (U-wide vs. E-wide), Target Word Order (U-E vs. E-U), and Word Order Match (whether the Word Order in the target Matched or Mismatched the prime), as well as their interactions.[2] Figure 2 shows these results. Critically, like C&B, we found a significant main effect of Prime Scope ($\chi2(1) = 79.79$, $p < 0.0001$; U-wide primes increase the probability of U-wide responses on subsequent target trials). Unlike C&B, we also found a significant interaction of Prime Scope and Word Order Match ($\chi2(1) = 27.82$, $p < 0.0001$), reflecting a bigger priming effect on Matching targets. Simple effects revealed separately significant effects of Prime Scope for Matching ($\chi2(1) = 101$, $p < 0.0001$) and Mismatching targets ($\chi2(1) = 6.5$, $p = 0.01$). Thus, although priming was greater when word order matched between prime and target, a priming effect was present in both cases.

Although there was no interaction between Target Word Order and Word Order Match, we also looked at the effects of Prime Scope and Target Word Order separately in Matching-Order and Mismatching-Order trials, as C&B had done. In both cases, we included Prime Scope, Target Word Order, and their interaction as predictors. In Matching-Order trials, we found a significant effect of Prime Scope ($\chi2(1) = 103$, $p < 0.0001$; more U-wide responding after U-wide primes) and of Target Word Order ($\chi2(1) = 83.4$, $p < 0.0001$), with no interaction. Simple effect analyses showed a significant priming effect for U-E targets ($\chi2(1) = 40.3.338$,

[1] For each effect of interest, we report p-values from Type II Wald $\chi2$ tests on each factor (i.e. its significance after the inclusion of all other factors, except for higher order interactions involving that factor). We pursue the same analysis strategy as C&B, starting from a model with a maximal random effects structure (Barr, Levy, Scheepers & Tily 2013). We differ from C&B in reducing maximal models according to the reduction procedures recommended by Bates, Kliegl, Vasishth & Baayen (2015).

[2] In addition, there was a significant main effect of Word Order Match ($\chi2(1) = 15.17$, $p < 0.0001$; higher rates of U-wide responses in Mismatching than Matching cases) and a significant main effect of Target Word Order ($\chi2(1) = 167.44$, $p < 0.0001$; more U-wide responses for E-U than U-E targets).

$p < 0.0001$) and for E-U targets ($\chi2(1) = 67.9$, $p < 0.0001$). In Mismatching-Order trials, we again found a significant effect of Prime Scope ($\chi2(1) = 6.57$, $p = 0.01$) and of Target Word Order ($\chi2(1) = 86.1$, $p < 0.0001$), with no interaction. Simple effect analyses again showed a significant priming effect for U-E targets ($\chi2(1) = 4.4$, $p = 0.037$) and a marginal effect for E-U targets ($\chi2(1) = 2.74$, $p = 0.09$). Overall, priming was present regardless of the order of quantifiers in either prime or target.

Finally, we also looked at priming within and between the two predicates, *above* and *below*. In a model including the variables of Prime Scope and Predicate Match (Between- vs. Within-Predicate), we find a main effect of Prime Scope ($\chi2(1) = 72.8$ $p < 0.0001$), and a main effect of Predicate Match ($\chi2(1) = 8.93$, $p = 0.002$), but no interaction ($\chi2(1) = 0.01$, $p = 0.89$). Thus, the priming effect does not depend on predicate overlap. Figure 3 shows these results.

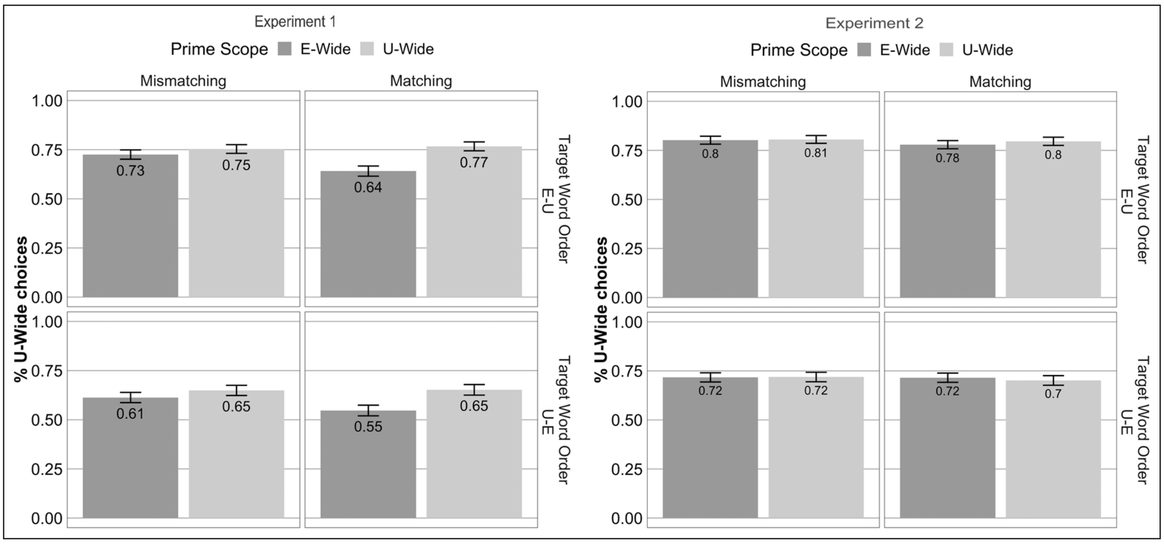

Figure 2: Target responses in Experiments 1 and 2. Mean percentage of U-Wide choices in targets for both prime configurations (E-Wide, U-Wide). Columns break down targets by Quantifier Word Order Match between prime and target (Mismatching, Matching). Rows show Target Quantifier Word Order (E-U, U-E). Error bars indicate +/–1 standard error.

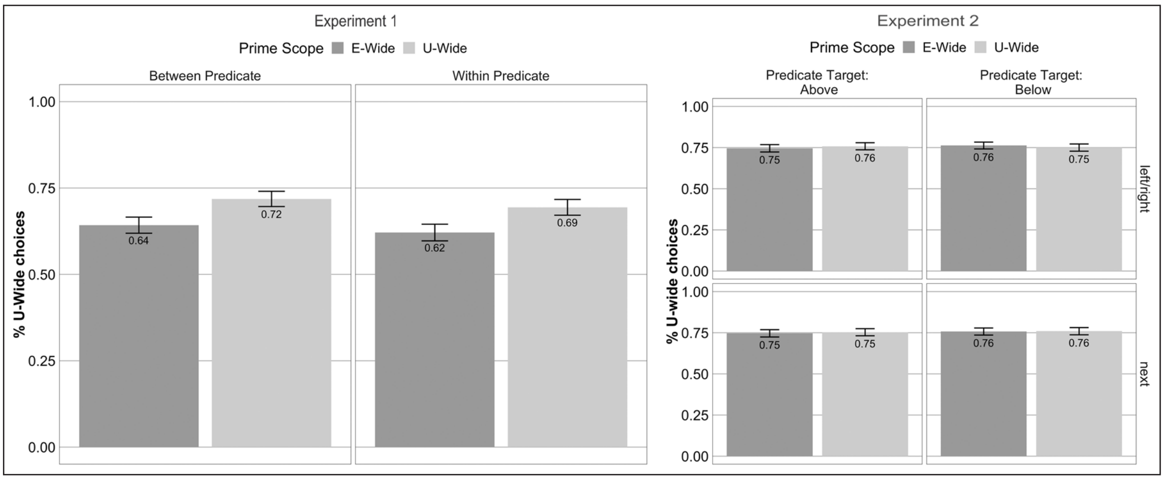

Figure 3: The left panel shows the effect of Prime Scope (U-wide vs. E-wide) in Experiment 1, broken down by within- vs. between-predicate. The right panel shows the effect of priming in Experiment 2, broken down by predicate in the prime (rows: *to the right*/*left of* and *next to*) and the target (columns: *above* and *below*). The Y-axis shows the percent of subsequent target trials where participants chose a U-wide picture. Error bars indicate +/–1 standard error.

2.3 Discussion

Using identical stimuli to C&B, we replicate their major findings. We find that U-wide prime trials increase the likelihood of U-wide target choices, both when the two sentences match in the word order of the quantifiers (U-E or E-U), and when they mismatch. This is consistent with the priming of LF representations across sentences. Unlike C&B, we also found a stronger priming effect for Matching- than Mismatching-Order prime-target pairs. For example, participants seeing a U-E target would be more likely to get an E-wide reading if it followed an E-wide U-E prime than an E-wide E-U prime. This is consistent with the additional priming of a scope inversion operation, which assigns the object quantifier wide scope, and so pushes toward an E-wide reading of U-E sentences, but a U-wide reading of E-U sentences.

However, all of these results are also consistent with a priming effect based on visual similarity. The finding that U-wide primes make U-wide targets more likely across all word orders is consistent with priming based *only* on picture similarity. The fact that this priming effect is stronger in Matching-Order prime-target pairs complicates the story a bit, but does not eliminate the possibility that priming is linked to a picture checking strategy. For example, it's possible that similarity of the sentences – by virtue of similar quantifier word orders – could affect the likelihood of rE-Using the same checking strategy on the target trial: use of both similar sentences *and* similar pictures may increase the likelihood of checking both prime and target pictures in the same way, increasing priming when the order of the prime and target match.

3 Experiment 2

To look at the contribution of the spatial checking strategy to the priming effect, we conducted two different manipulations of the prime trials. In one half of the primes, we used the preposition *next to*, corresponding to U-wide and E-wide pictures quite dissimilar to the target pictures (see Figure 1, bottom row). Whatever checking strategy participants used for these primes wouldn't readily apply to the targets, so that if such strategies drove the priming effect in Experiment 1, we would expect no priming effect here. In the other half of the primes, we used the prepositions *to the right/left of*. As Figure 1 shows, this reverses part of the pattern of picture similarity. Where U-wide *above/below* primes in Experiment 1 were more similar to U-wide *above/below* targets, U-wide *to the right/left of* primes share some visual similarity with U-wide *above/below* targets (objects are still arranged in pairs), but also with E-wide *above/below* targets (objects lined up vertically rather than horizontally). We reasoned that if the direction in which participants scanned the symbols was the sole driver of the priming effect in Experiment 1 (*scan horizontally* for U-wide pictures; *scan vertically* for E-wide pictures), we would find just the reverse pattern of priming here (e.g. U-wide *right/left* primes should prime E-wide readings in *above/below* targets). Alternatively, priming could require that both the major components of the checking strategy be the same: that you scan in the same direction for the same arrangement of elements (*two uniform rows* for U-wide; *one column* for E-wide). Since the arrangement of elements did not change on *right/left* primes, but the direction of scan did, the same exact strategy could not be applied across the prime and target trials. Thus, on this hypothesis we would expect no priming for *right/left* prime trials.

Of course, if the priming effect found by C&B, and replicated in Experiment 1, is due to the priming of LF representations, then the specific predicates should not matter. Just as we found equally robust priming across *above* and *below* in Experiment 1, we should find the same priming effect from *right/left* or *next to* in the prime to *above/below* in the target.

3.1 Methods

3.1.1 Participants

Since we were interested in the possible *absence* of a priming effect in this experiment, we wanted to increase the number of participants and our power to find an effect. We recruited 276 participants who were not in Experiment 1. Forty of these were excluded because they failed more than 10% of filler trials.

3.1.2 Materials

The only change from Experiment 1 was that the prime predicates were now *to the right/left of* and *next to*, with the prime pictures changing correspondingly (see Figure 1). To avoid lengthening the experiment, we did not increase the number of trials, so that there were half as many primes with each predicate as there were with *above/below* in Experiment 1.

3.2 Results and Discussion

Unlike Experiment 1, we found no effect of Prime Scope ($\chi2(1) = 0.16$, $p = 0.68$), and no significant interactions between any of the variables.[3] We also found no effect of Prime Scope in any of the prime-target configurations, running separate analyses for each combination of Target Word Order (U-E and E-U) and Word Order Match (Matching vs. Mismatching) (all $ps > 0.05$).[4]

A series of posthoc Bayesian analyses confirmed that for all these models there is moderate to strong evidence for the null hypothesis (Bayes Factors > 3).[5] To check that the effect of Prime Scope was significantly different between experiments, we built a model with Experiment (1 vs. 2), Prime Scope, and their interaction as predictors. As expected, we found a significant interaction of Prime Scope by Experiment ($\chi2(1) = 35.83, p < 0.0001$).

The absence of an effect of Prime Scope in Experiment 2 is not expected if priming is at the level of LF. As Figure 3 shows, we also find no interaction between Prime Scope and the predicates *to the right/left of* and *next to*. If participants in Experiment 2 had been primed to scan in the same direction in the target trials as on the prime trials (horizontally or vertically), regardless of whether the symbols were arranged in one line or two, we should have seen a reverse priming effect from *right/left* primes to *above/below* targets, but no priming on the *next to* primes. In combination with a real scopal priming effect,

[3] As in Experiment 1, we discarded target responses following inaccurate primes, corresponding to 3.4% of targets following E-U primes and 2.5% after U-E primes.

[4] As in Experiment 1, we first looked at an omnibus model containing Prime Scope (U-wide vs. E-wide), Target Word Order (U-E vs. E-U), and Word Order Match (Matching vs. Mismatching) and their interactions. We found significant main effects of Word Order Match ($\chi2(1) = 7.64$, $p = 0.005$; more U-wide responses after mismatching than matching primes) and Target Word Order (more U-wide responses on E-U than U-E targets; $\chi2(1) = 266.36, p < 0.0001$), but no interactions between these variables and the primary variable of interest, Prime Scope. Thus, across both experiments, we find a general preference for U-wide interpretations in both *every-a* and *a-every* constructions. Although this may seem surprising for *a-every* sentences, since it conflicts with the general assumption that linear scope should be easier to derive, it is consistent with many findings about the general preference of *every* to take wide scope (Kurtzman & MacDonald 1993; AnderBois, Brasoveanu & Henderson 2012; Feiman & Snedeker 2016). More surprising is the finding, in both experiments, that the U-wide preference is greater for *a-every* than *every-a* sentences, and also greater when quantifier order mismatch than match. We know of no theoretical account that predicts these findings.

[5] Mixed-effects logit models were fitted with STAN using the brms library. The prior distribution for all parameters was Student's t distribution with mean 0, a scale of 2.5 and 4 degrees of freedom. We followed common prior choice recommendations (Gelman et al. 2008). 10,000 samples of the likelihood of each data point were drawn after 10,000 burn-in iterations from four chains, for a total of 40,000 samples. Models were evaluated by computing the Bayes Factor (BF) of the null hypothesis (H0) over another, alternative hypothesis (H1). This was done using the "bayes_factor" function of the brms library. The BF will indicate the extent to which the data supports H0 over H1: the higher the outcome, the stronger the evidence for H0 over for H1. We computed three BFs, obtained from comparing the null hypothesis to an alternative hypothesis: (1) evidence for no effect of Prime Scope (BF = 56.3); (2) evidence for no interaction of Prime Scope × Word Order (BF = 21.6); and (3) evidence for no interaction of Prime Scope × Target Word Order (BF = 4.43). We thank an anonymous reviewer for suggesting this approach to our results.

priming of scanning direction could have produced a null effect for *right/left* primes and a priming effect for *next to* primes. Instead, there was no priming effect for *either right/left* or *next to* primes, and no difference between them. Once there is any difference between the checking strategies that apply on primes and targets, the effect reported by C&B, and replicated in Experiment 1, disappears.

4 Conclusion

The present experiments substantially change the interpretation of Chemla & Bott's (2015) findings. Experiment 1 replicates C&B, finding that U-wide primes make a U-wide interpretation of a target more likely regardless of whether the universal quantifier comes first or second in either sentence and regardless of whether the prime and target match on this dimension. However, Experiment 2 reveals that this effect is not actually about scopal priming, but depends crucially on the ability to use the *exact same* checking strategy for the prime and target sentences. Changing the predicate and pictures in the prime in a way that forces any change in participants' checking strategy is enough to eliminate the priming effect.

There is one alternative explanation that warrants further consideration: perhaps Experiment 1 reflects real scopal priming, but it requires that the same predicate appear in prime and target sentences. This kind of interaction between structural and lexical factors is common: syntactic priming effects are often larger when primes and targets share lexical content, an effect known as the "lexical boost" (Cleland & Pickering 2003; Arai et al. 2007; Ledoux et al. 2007; Tooley et al. 2019). Several findings, however, argue against this explanation for the present results.

First, Experiment 1 and C&B's original findings show that exact lexical overlap is not necessary for priming. Not only is there a priming effect from *above* primes to *below* targets and vice versa, the effect is no smaller than when prime and target use the same predicate. This leaves open the possibility that priming depends not on exact match, but on similarity between predicates, with *above* and *below* sufficiently similar to each other, but not to *right/left*.

Previous findings, however, suggest that predicate similarity does not affect scopal priming at all. First, C&B's original data show that predicate similarity is not sufficient for priming within their paradigm. In another manipulation, C&B had included sentences with *every* and *not*, and color predicates in both prime and target (e.g. *Every square is not blue*). The corresponding pictures showed shapes that were laid out the same way for both types of prime and target pictures, so that the checking strategy was based on the shapes' color rather than their spatial layout. Strategy priming was not possible because both choices on target trials showed 6 identical objects, in two differently-colored rows. Despite the predicates' semantic similarity, C&B failed to find a priming effect within U-NEG sentences, let alone from these sentences to either U-E or E-U sentences. We know of no theoretical reason why U-NEG sentences alone should be impervious to scopal priming. Rather, these results suggest that spatially-based checking strategies are the only locus of priming in this paradigm.

Finally, scopal priming without predicate similarity has been found in another task. Feiman & Snedeker (2016) used more naturalistic sentences (e.g. *Every boy climbed a tree*) and manipulated whether the verbs in the prime and target were the same (e.g. *climb* in both) or different (e.g. *climb* vs. *see*; their Experiment 3). Not only did they find a priming effect in both cases,[6] but the effect was no smaller when the verbs were different, suggesting

[6] Feiman and Snedeker only used U-E word order for both primes and targets, constituting a subset of C&B's trials. Still, we found no priming effect in Experiment 2 in any one of the word order combinations, including the subset from U-E primes to U-E targets.

that there is no lexical boost for scopal priming.[7] Critically, Feiman and Snedeker were able to rule out the possibility that priming in their task was based on visual similarity or checking strategies by showing that the priming effect disappeared if the prime and target mismatched on the quantifier (*each* vs. *every* vs. *all*), while the pictures stayed the same. Why wasn't the priming effect in Feiman and Snedeker (2016) based in checking strategies? We suspect that participants didn't need to rely on them. The events depicted agent and patient relationships that could be easily interpreted (Hafri et al. 2018) and mapped onto an E-wide or U-wide reading (see Figure 4). As a result, these pictures did not require participants to check relations between depicted objects one by one. In addition, the picture choices within a prime trial mismatched on the identity of the subject or object noun, which meant participants had to pay attention to the sentence at least enough to know which nouns to check for. Just which differences between the stimuli are critical is not clear, but Feiman and Snedeker's results show that genuine and verb-independent scopal priming effects can arise with a different stimulus set.

In sum, if the priming effects found by C&B and replicated in Experiment 1 do reflect scopal priming contingent on high similarity between predicates, the effect is subject to a set of ad hoc conditions not predicted by any theoretical account. The effect exists for sentences with *every* and *a* using pictures of arbitrary symbols, but not for sentences with *every* and *not* using arbitrary symbols, or for sentences with *every* and *a* using more naturalistic sentences and pictured events. On the other hand, if C&B's paradigm primes participants to deploy the same checking strategy on target as on prime trials, these data follow. The absence of priming in our Experiment 2 follows because the checking strategies no longer match, and the absence of priming in C&B's U-NEG condition follows because the spatial layouts for both readings are identical, so sentence-independent checking strategies cannot choose between them.

Our findings have consequences for future studies of priming. They suggest that the type of stimuli matter more than previously supposed. Complex arrangements of arbitrary symbols may lead participants to rely on simple visual heuristics that bypass linguistic processing.

Figure 4: An example of pictures depicting a U-wide (left) and an E-wide (right) reading of the sentence, *Every kid climbed a tree*, used in both Raffray & Pickering (2010) and Feiman & Snedeker (2016).

[7] Why there is no lexical boost in scopal priming is an open question. In syntactic priming, the lexical boost depends on repetition of a specific verb lemma, which may encode associated syntactic constraints (see Pickering & Branigan 1998; Branigan & Pickering 2017). For relative quantifier scope, it may be quantifiers that impose constraints on possible structures instead of verbs (AnderBois, Brasoveanu & Henderson 2012). Indeed, although scopal priming does not vary depending on similarity between verbs, it requires similarity between quantifiers (Feiman & Snedeker 2016).

In some cases, this will produce no apparent priming effect, leading to the conclusion that the relevant linguistic variables simply "do not prime" (as, for example, in C&B's conclusion about scope inversion). In other cases, these strategies can become the locus of priming, and if confounded with linguistically relevant variables, lead to erroneous theoretical conclusions. At the same time, our findings provide reasons for optimism. Priming based on task-specific strategies appears highly sensitive to even small mismatches between prime and target stimuli, so even relatively minor variations across stimuli may be sufficient to eliminate this locus of priming. This requires that experimentalists match control conditions closely to the task, either by changing only the variable of interest (keeping visual and other dimensions of similarity the same) and showing that priming disappears, or else by varying the similarity of other theoretically irrelevant features and showing that priming is unaffected. In this paper, we took the second approach, and showed that priming disappeared.

Our findings also bear on linguistic theory. Chemla & Bott (2015) offered evidence that, while relative quantifier scope representations can be primed, there is no priming of scope inversion across sentences with different quantifiers in object position. If true, this could challenge theories of LF construction, most of which posit a single inversion operation ranging across all quantifiers. Here we have undermined the evidential basis for this claim. We manipulated elements of the task – the pictures and the predicates – that should not have affected the assignment of quantifier scope. Instead, the entire priming pattern disappeared, suggesting that the original pattern did not provide reliable evidence either for the presence of representation priming, or conversely, the absence of inversion priming. While the jury is still out, the question of whether scope inversion can be primed remains important. Priming of inversion across all quantifiers would lend support to quantifier raising and type shifting approaches, while priming within only some groups of quantifiers, would lend support to approaches that emphasize the quantifier word's role in its scope assignment (e.g. Beghelli & Stowell 1997; Steedman 2012).

Abbreviations

LF = Logical Form; U = Universal; E = Existential; C&B = Chemla & Bott; NEG = Negation.

Supplementary Files

Supplementary files. Data and R Analysis script for both experiments, available as supplementary files and at the Open Science Framework. DOI: https://doi.org/10.17605/OSF.IO/N7Q3X

Competing Interests

The authors have no competing interests to declare.

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