Neglect zero: evidence from priming across constructions

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Abstract

Recent studies use semantic structural priming to show that various cases of linguistic strengthening happen through a common mechanism: generation of implicatures through alternative-based (scalar) reasoning. In this paper, we used priming to investigate another group of cases, where strengthening is postulated to follow from the tendency to systematically neglect structures that verify a sentence by virtue of an empty configuration (neglect-zero): empty-set quantifiers (*at most/fewer than*) and disjunction under a universal quantifier. We report data indicating semantic priming between these two structures, but not between them and scalar *some*. We propose that 1. there is a common mechanism in use for strengthening constructions postulated to follow from the neglect-zero tendency, and that 2. this mechanism is different from the one involved in alternative-based reasoning.

Keywords: Priming; Semantics; Implicatures; Neglect-zero; Alternatives; Empty set quantifiers.

Introduction

As famously noted by Grice (1975), the interpretation of natural language sentences in everyday conversation may systematically deviate from the literal meaning prescribed by classical logic. Pragmatic principles are employed in conversation that lead to strengthened interpretations compatible with a more restricted set of situations. Hence, the sentences in (1) have a literal (weak) interpretation and a strong interpretation resulting from pragmatic strengthening.

(1) a. Some of the squares are black. UPP
$\rightsquigarrow$ Not all of the squares are black.

b. Fewer than three/At most two squares are white.
$\rightsquigarrow$ There are some white squares. ESQ

c. Each square is black or white. DIS
$\rightsquigarrow$ There is a black square and a white square.

Different mechanisms have been proposed to explain these interpretations. In seminal work, Horn (1984) proposed that meanings are strengthened through a mechanism that negates potential alternative utterances. The canonical example of strengthening via alternative-based reasoning are scalar implicatures as in (1-a). On the standard view, the implicature arises when the stronger *all*-alternative to a *some*-expression is negated. Several alternative-based theories postulate that a similar explanation applies to (1-b) and (1-c), for instance, involving the grammatical operator (EXH), which systematically negates relevant alternatives (e.g. Crnič, Chemla, & Fox, 2015; Bar-Lev & Fox, 2023).

Recently, Aloni (2022) argued that another kind of strengthening occurs due to a cognitive bias called *neglect-zero*. She observes that, to avoid cognitive complexity, people systematically tend to neglect structures that verify a sentence by virtue of an empty configuration (*zero-models*; cf. also O. Bott, Schlotterbeck, & Klein, 2019). Aloni (2022) hypothesizes that strengthened interpretations of (1-b) and (1-c) occur due to a human tendency to prefer concrete representations over abstract ones and from a general difficulty of engaging with empty configurations.

For illustration, a model where all squares are black (■,■,■) is a zero-model for (1-b) and (1-c). Sentence (1-b) is literally true in this context since $0 < 3$; sentence (1-c) is true as well since every square is black and hence black or white. Thus, in this model, both sentences are verified by virtue of an empty set of white squares. However, neglect-zero theories postulate that zero models are systematically neglected in the interpretations of sentences and cause trouble with verification (see O. Bott et al., 2019; Aloni, 2022; Aloni & van Ormondt, 2023; and cf. also Nieder, 2016, for motivation from domain-general cognition).

This tendency can be understood as a reasoning-simplification bias, which allows for faster processing of sentences at the cost of making a mistake in cases where we encounter (rare) models which, due to their emptiness, are more difficult to process and fall out of the heuristic (Johnson-Laird, 1983; Kahneman, 2011). Aloni (2022) argues that this bias can be suspended if more precise communication is needed. This can happen either locally to process a single sentence or globally for the entire conversation, e.g. in contexts of mathematical discourse.

Both alternative-based and neglect-zero theories received empirical support, as their predictions regarding the interpretations of various expressions are realized in human reasoning.[1] L. Bott and Chemla (2016) used structural priming to demonstrate that some constructions share (part of) mental representation involving the alternative-based mechanism. Using a similar methodology, we investigated constructions that are predicted to share *neglect-zero* mechanisms and tested whether this mechanism is distinct from the alternative-based one. More specifically, we tested the predictions of alternative-based and neglect-zero-based accounts regarding

[1] See, e.g., L. Bott and Noveck (2004), Chemla and Bott (2014), Marty, Romoli, Sudo, and Breheny (2021), Ramotowska, Marty, Romoli, Sudo, and Breheny (2022) and Degano et al. (2025).

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relations between interpretation choices for (1-a), (1-b) and (1-c) through between-construction structural priming. In particular, we tested whether the frequency of strengthening in (1-b) changes after participants were primed to suspend one of the other strengthenings in (1).

Structural priming in semantics and pragmatics

Structural priming relies on the assumption that interpretative representations and processes remain active across experimental trials (Pickering & Ferreira, 2008). Hence, if a participant uses a mechanism to derive an interpretation of a sentence in a prime trial, they are more likely to use it again in the subsequent target trial. If a shared mechanism is used to strengthen the meaning of expressions in (1), priming a weak interpretation of one sentence/structure in a prime trial should result in a more frequent choice of a weak interpretation of another one in a target trial.

Raffray and Pickering (2010) demonstrated priming of quantifier scope relations in doubly-quantified sentences and Feiman and Snedeker (2016) extended this across different quantifiers if their meaning is similar enough. L. Bott and Chemla (2016) showed that various constructions hypothesized to be interpreted with an alternative-based strengthening mechanism can prime each other (across construction) and hence provide evidence that they share an underlying mechanism of interpretation. The study involved inferences like (1-a), but also the interpretation of bare numerals (*two* $\rightsquigarrow$ *exactly two*) and ad hoc implicatures. Marty, Romoli, Sudo, and Breheny (2024) further showed that priming in these cases takes not only a local (trial-to-trial) effect but may *spill over* the whole experiment influencing the overall distribution of interpretation choices in target trials independently from the prime trial which precedes the target. Meyer and Feiman (2021) included Free Choice inferences into the design and found no across-construction priming between FC and scalar *some* or bare numerals, suggesting that a non-alternative-based theory is needed to explain FC. Such result can be explained if FC is a neglect-zero related phenomenon derived analogically to (1-c) as proposed by Aloni (2022). This paper employs the priming methodology to investigate whether there is evidence for a common mechanism underlying the phenomena explained by the neglect-zero bias.[2]

The present study was designed in such a way that each comparison involves priming across constructions. This is important because it is still unclear under which circumstances priming can be observed and whether it extends beyond the particular construction under investigation. Even when attempting to prime weak interpretations of *fewer than three* through *at most two* below, as in (1-b), quantifiers were systematically varied between prime and target trials employing different types of determiners (superlative vs. comparative; see Feiman & Snedeker, 2016 for discussion on (the lack of) across-quantifier priming regarding relative scope). Furthermore, the picture stimuli used in the prime trials in the present study are maximally comparable in terms of visual features and strategies they may give rise to (Feiman et al., 2020; Husband & Patson, 2021). Since it is still an open question under which conditions across-construction priming can be observed, our study is also methodologically relevant.

[2] In line with the results from the mentioned studies, we tacitly assume that semantic priming targets the interpretational mechanisms rather than the resulting representations (but see Feiman, Maldonado, & Snedeker, 2020, for discussion wrt. priming of quantifier scope).

Hypotheses and general predictions

Our study focuses on two inferences, (1-b) and (1-c), which, to our knowledge, were not previously investigated in a priming paradigm. We include (1-a) to test the prediction of alternative-based approaches (e.g. Crnič et al., 2015) that these three inferences share an alternative-based strengthening mechanism. Our hypothesis is that while (1-b) and (1-c) arise from the same mechanism, the tendency to neglect empty configurations (zero-models), (1-a) has a source in a different strategy, i.e., in alternative-based strengthening. If priming is found across all constructions, the alternative-based theory will be confirmed. If priming is only found across (1-b) and (1-c) but not between them and (1-a), the *neglect-zero* theory will receive empirical support.

Experiment

We ran a priming experiment based on a specific version of a picture-matching task called the *better-picture* paradigm, following the aforementioned studies. In this task, participants were exposed to two types of trials. In the first type, participants saw a sentence and two open cards presented together on one screen. In the second type, they saw a sentence and a covered card (called *better-picture*) presented alongside an open card. Participants were told that only one of the cards would match in each trial, and they were instructed to choose that one. The covered card should only be chosen if the open picture did not match the sentence. The priming was introduced by a prime trial followed by a target trial. In the target trials, they saw the open card and *better-picture* and a sentence with the empty-set quantifier *fewer than three*. In the prime trials, participants saw two open cards and a sentence that varied across sub-experiments. We ran four sub-experiments: three sub-experiments with three different prime trials (distributive inferences (DIS), upper-bounded scalar inferences (UPP), and non-empty-scope strengthening in superlative quantifiers (ESQ)) and one baseline experiment (BAS), in which primes were replaced by filler items.

The dependent variable was the proportion of *better picture* choices in target trials. The choice of *better picture* indicated that participants chose the strengthened interpretation, and the choice of the open picture indicated that they chose the literal meaning (see 1). Crucially, we predicted that the interpretation choice would depend on the preceding prime. After priming participants toward the weak interpretation, we expected a higher proportion of *open picture* choices.

We will first describe the DIS sub-experiment, including materials and the general procedure, and afterward go

through differences to the other three sub-experiments. In the next section, we will discuss all results together.

Methods

Participants We recruited 440 (self-declared) native speakers of English (246 females, average age=39.8) through Prolific.com pre-screened for primary, first and earliest language in life: English and located in the UK, USA, Ireland or Australia. The participants were paid £3 for their effort and gave informed consent for participation. The research was preregistered and approved by Ethics Committee of the Faculty of Humanities of the University of Amsterdam (FGW-2371).[3] We took the meta-analysis on syntactic priming by Mahowald, James, Futrell, and Gibson (2016) into consideration when determining the sample size for the present study.

DIS-ESQ sub-experiment (N=80): design and materials In the DIS-sub-experiment, in prime trials, we used sentences containing disjunction under a universal quantifier like (1-c). The experimental design contained two types of prime trials: the critical prime where a *zero-model* had to be chosen because the second picture falsified the sentence (e.g. the critical prime for the DIS sub-experiment in Table 1; combined with the pictures 1 and 2 in Figure 1) and one where a *non-zero-model* had to be chosen (see example in Table 1 combined with the same two pictures). The pictures always showed two types of geometrical shapes (chosen from square, triangle, circle, cross, and heart), one in the upper half and one in the lower half of the picture. The elements of one of these sets were homogeneous with respect to their color, and the other set had mixed colors, containing one element with a different color.[4] Prime trials were followed by a target trial showing an open and a covered card in which the open card showed a zero-model (e.g. the sentence in the target condition in Table 1; combined with Picture 3). The target trial showed a sentence with *fewer than three* (in all sub-experiments). It probed for zero model acceptance by showing a zero model (e.g. Picture 3) alongside a covered card which participants should only choose if they would think the open card did not match the sentence. After one type of prime in a given item, a zero model with heterogeneous colors had to be chosen in the target and after the other one the homogeneous set had to be chosen, which was counterbalanced between items. To balance out potential effects of visual features of the pictures, each item (consisting of 2 prime-target pairs) was paired with another item in such a way that the same pictures were used but played opposite roles. This was achieved by changing the geometrical shapes and color terms mentioned in the sentence. This means that in the first item of the pair the first card (e.g. Picture 1) had to be chosen once as a zero model and once as a non-zero model and in the second item the opposite card (e.g. Picture 2) had to be chosen once as a zero and once as a non-zero model. Since both pictures had to be chosen in both roles, their overall visual features were completely identical. Moreover, we ensured in this way that all the colors mentioned in the sentences were present on every verifying card accompanying them. The motivation for this was to exclude the possibility of strategic effects or effects based on visual similarity as best as possible. Items were distributed across lists using a Latin square in such a way that each participant saw only one of the primes in a given pair of items, i.e. each participant saw one-fourth of the entire stimuli set.

In addition to prime and target trials, the experiment included fillers, where the sentence presented was unambiguous (e.g., *Exactly three squares are black*). Half of the fillers involved two open-card trials and half were *better picture* trials. On average, half of these covered-card filler trials required participants to choose the covered card, and in the other half, the open card was the correct choice. The sub-experiments differed only in the prime trials. The fillers and targets remained constant in position across sub-experiments.

DIS-ESQ sub-experiment: procedure The experiment began with a short training (8 filler trials) where participants received feedback, making sure that they understood the task. If they answered correctly, the word 'Correct' briefly appeared on the screen. Otherwise, 'Incorrect' appeared, and participants had to wait 3 seconds before continuing. Next, participants moved to the main part of the experiment. In the main part of the experiment, each participant saw 48 quintuples of trials (including one prime-target sequence) in the following order: $3\times$filler $\rightarrow$ prime $\rightarrow$ target. Hence, each participant saw 240 trials. An experimental session took on average 22.2 min. The participants were randomly assigned to one of the four conditions (sub-experiments).

UPP-ESQ sub-experiment (N=80): design and materials In the UPP-sub-experiment empty-set choices for the ESQ *fewer than three* were primed using sentences with scalar *some* (1-a). In the critical primes, participants had to choose a picture where all objects satisfied the property, violating the scalar inference and forcing the participants to choose the weak interpretation. In the control primes, some, but not all, objects satisfied the property, allowing for strengthening (see examples in Table 1). The target trials and fillers, the visual stimuli, the generation of the experimental lists as well as the procedure were identical as in the DIS- sub-experiment. Note that the same pictures had to be chosen once in the critical and once in the control primes.

ESQ-ESQ sub-experiment (N=80 +120 in replication): design, materials and procedure In ESQ-sub-experiment empty-set choices for the comparative empty-set quantifier *fewer than three* were primed using empty-set choices in prime trials with the superlative empty-set quantifier *at most two*. In critical primes, the participants had to choose a picture which satisfied the sentence by virtue of an empty set of relevant objects, forcing a weak interpretation choice as

[3] The data, statistical analysis and stimuli are available here.

[4] We used color blind friendly palettes of colors using the following combinations: (blue, brown, grey, orange), (black, blue, orange, purple), (brown, green, pink, yellow).

Table 1: Experimental conditions and example items.

sub-experiment	Condition	Sentence	Pictures
DIS	critical-prime	*Each of the hearts is orange or purple.*	1 and 2
DIS	control-prime	*Each of the squares is black or orange.*	1 and 2
UPP	critical-prime	*Some of the hearts are purple.*	1 and 2
UPP	control-prime	*Some of the squares are black.*	1 and 2
ESQ	critical-prime	*At most two of the hearts are black.*	1 and 2
ESQ	control-prime	*At most two of the squares are orange.*	1 and 2*
all subexp	target	*Fewer than three of the hearts are purple.*	? and 3
BAS	control-prime	*Exactly three squares are black.*	1 and 2

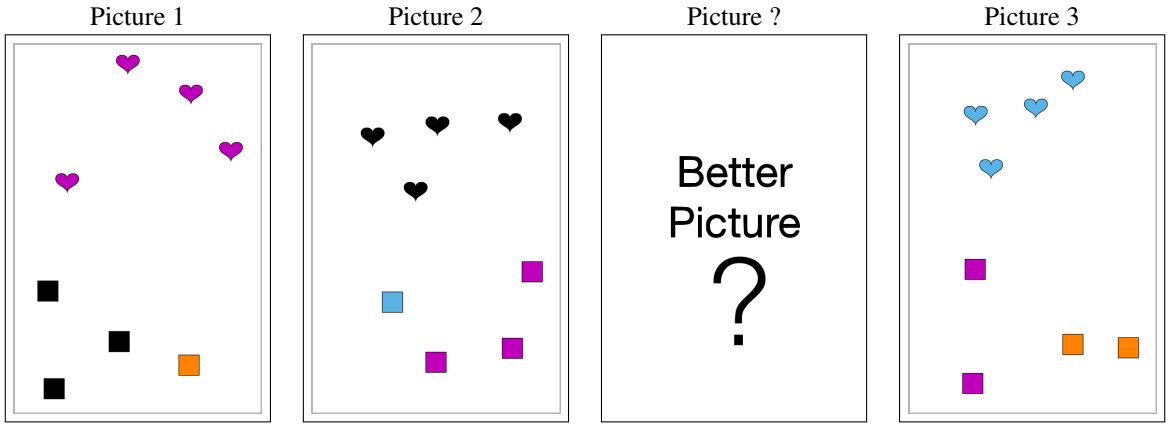
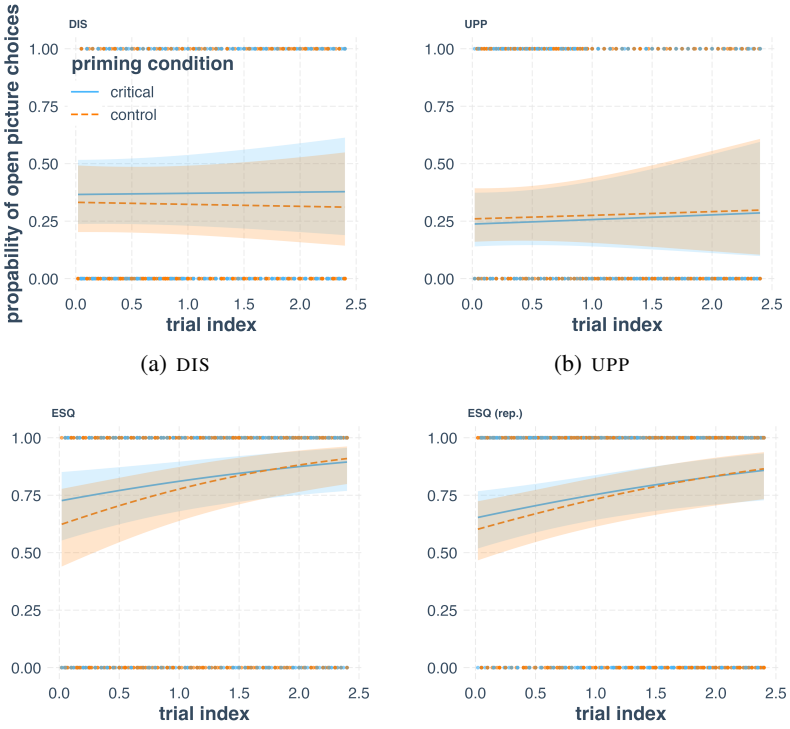


Figure 1: Examples of pictures used in the experimental trials for example sentences in Table 1. Prime trials included Pictures 1 and 2 and target trials Picture ? and Picture 3. In Picture 2* (not shown) the purple squares were replaced with orange ones.

in Table 1. In control trials, at least one object was present, hence the sentence was non-emptily supported by the picture allowing for a strengthened interpretation. Again, the target trials and fillers, the generation of the experimental lists as well as the procedure were identical as in the DIS-sub-experiment, and thus also as in the UPP-sub-experiment. In this sub-experiment the visual stimuli differed slightly. This was necessary because of the monotonicity properties of the quantifiers and the way the pictures were set up wrt. the control primes. To give a concrete example, in the second picture that was paired with the control-prime sentence shown in Table 1, the three purple squares were replaced by orange ones. Otherwise, Picture 1 would have verified the strong interpretation of the quantifier and Picture 2 the weak reading. This would have violated the experimental design because only one picture in each trial was allowed to be a true model of the sentence. To still balance out potential effects of superficial visual features, we recycled these replacement pictures from the second item in a very similar item pair further down in the list of experimental items. This way differences in visual features were minimized and, moreover, exactly the same set of visual stimuli were used in the two priming conditions over the entire experiment.

BAS sub-experiment (N=80), design, materials and procedure We follow Feiman and Snedeker (2016) in including a separate baseline experiment in which sentences in prime trials were replaced by unambiguous sentences similar to fillers (see Table 1), but everything else, including the pictures in prime trials, the other fillers, the target trials and the trial order remained the same. The purpose of the baseline was twofold. Firstly, it was used to establish a baseline rate of open-picture (weak interpretation) choices in target trials. Secondly, it was used to distinguish priming from general adaptation effects due to repeated exposure to the covered-picture target trials.

Statistical analysis We excluded participants who achieved less than 75% accuracy on the fillers. We also excluded prime-target pairs if the prime in the pair was judged incorrectly. For the remaining trials, we analyzed the proportion of open-picture choices in target trials after correct choices on prime trials in each sub-experiment separately using logit mixed-effects models. These models included the PRIMING CONDITION (levels: *critical prime* vs. *control prime*), an index of the TRIAL POSITION within the experiment as well as their interaction as fixed effects and a random intercept and random slope of trial number for participants as random effects. The random effects structure was determined using model comparisons. Random effects were included if they



(a) DIS (b) UPP

(c) ESQ (d) ESQ repl.

Figure 2: Regression lines from logit mixed-effects models for each sub-experiment (DIS, UPP, ESQ and its replication), tracking the change of the probability of an open-picture choice over the course of the experiment.

improved model fit. Since our main hypothesis concerned the effect of PRIMING CONDITION on the target trial directly following a given prime trial, we simplified models by removing effects involving the TRIAL POSITION if they were not significant, as determined by model comparisons between the more complex and the simplified model. As we tested a directed hypothesis, i.e. more acceptance of zero models after *critical primes*, we performed one-tailed hypothesis tests for this specific effect. In addition, we computed an, otherwise identical, analysis comparing the control condition to the baseline condition. We planned to replicate any given sub-experiment, if we found a marginal priming effect (following Feiman & Snedeker, 2016). This happened in the ESQ-sub-experiment (see results below) where we ran a replication with 120 participants in addition to the 80 participants from the first run.

Results

The participants understood the task well: the average error rate on filler items in each experiment did not exceed 16.1% (range across sub-experiments: $14.3 - 16.1\%$). In total, 47 participants were excluded because of low accuracy (by sub-experiment: BAS: 3, DIS: 10, UPP: 8, ESQ: 13, and replication of ESQ: 13). In the baseline experiment, the rate of open-picture, i.e. zero model, choices in the target trials was 49% overall (and no change over the course of the experiment: $\chi^2(1) = .68, p = .41$). Zero model acceptance in targets was lower than in the baseline in the DIS (44%) and UPP-sub-experiments (38.7%), whereas it was substantially higher in the ESQ-sub-experiment (64.7%; replication: 60.5%). This difference was significant for the ESQ-sub-experiment ($\beta = . - 1.24, z = -2.36, p = .019$) and its replication ($\beta = -0.954, z = -2.21, p = 0.027$) as well as for the UPP-sub-experiment ($\beta = .933, z = 2.02, p = .044$) but not for the DIS-sub-experiment ($\beta = .305, z = .645, p = .519$). The accuracy on prime trials was 97.2% (*critical*: 98.7%, *control*: 95.8%) in the DIS, 98.1% (*critical*: 99%, *control*: 97.4%) in the UPP, 84.4% (*critical*: 83.2%, *control*: 85.6%) in the ESQ and 81.6% (*critical*: 81.3%, *control*: 82%) in the replication of the ESQ-sub-experiment.

Figure 2 shows regression lines from full logit mixed-effects models for each sub-experiment, tracking the change of the probability of open picture choice throughout the experiment. In the ESQ- and UPP-sub-experiments, we observed an increase in the probability of zero model acceptance in the target trials across the experiment. In addition, the acceptance of zero models was numerically higher after critical than after control conditions, in line with trial-to-trial priming, in the DIS- (2% difference overall) and ESQ-sub-experiments (including its replication; 4% and 0.9% difference, respectively). In the UPP-sub-experiment there was, by contrast, no indication of priming (0.2% difference in the opposite direction).

In the DIS-sub-experiment the logit mixed-effects model analysis revealed a significant effect of PRIMING (one-tailed test: *critical* vs. *control condition*: $\beta = -.207, z = -1.84, p = .033$) after removing the non-significant interaction between the TRIAL POSITION and PRIMING CONDITION ($\chi^2(1) = .268, p = .605$) as well as the effect of the TRIAL POSITION ($\chi^2(1) = .011, p = .918$) from the model. In the UPP-sub-ex-

periment, neither the interaction between the PRIMING CONDITION and TRIAL POSITION ($\chi^2(1) = 0, p = 1$) nor the effect of the TRIAL POSITION ($\chi^2(1) = .11, p = .74$), nor the effect of the PRIMING CONDITION ($\beta = .063, z = .566, p = 1$) was significant. In the ESQ-sub-experiment, there was a significant effect of the TRIAL POSITION ($\beta = .613, z = 3.53, p < .001$) and a marginal effect of the PRIMING CONDITION (one-tailed test: $\beta = -.187, z = -1.52, p = .065$) after removing a non-significant interaction of these two factors ($\chi^2(1) = 2.2, p = .138$) from the model. Because the critical effect of the PRIMING CONDITION was marginal, we replicated the ESQ-sub-experiment. The replication yielded qualitatively similar results (TRIAL POSITION × PRIMING CONDITION: $\chi^2(1) = .114, p = .735$; TRIAL INDEX: $\chi^2(1) = 9.51, p = .002$), but the effect of PRIMING CONDITION was not significant ($\beta = -.092, z = -.929, p = .353$).

Discussion

We found a significant trial-to-trial priming effect for the DIS-sub-experiment, a marginal effect in the ESQ-sub-experiment, and no priming for UPP. Since the effect in ESQ was marginal, we ran a replication, which yielded no trial-to-trial priming effect. Moreover, we discovered a higher (compared to the baseline) average acceptance rate, as well as an adaptation effect in the acceptance of zero models in ESQ and lower average acceptance rate without an adaptation effect in UPP. No significant difference from baseline in average acceptance and no adaptation effect was found for DIS.

Since participants can be primed to compute weaker interpretations of sentences like (1-b) by a forced computation of weaker interpretations of (1-c), our data support the idea of a common underlying strengthening mechanism in these cases. The lack of such effect when they are primed to suspend scalar inferences is consistent with the idea that the mechanisms for (1-a) and (1-b) are different. This is exactly the pattern predicted by *neglect-zero* based theories, which therefore also gain support.

However, the absence of reliable trial-to-trial priming in the ESQ-experiment, where quantifiers with the same truth conditions are used in both primes and targets, requires additional explanation. First, we observe that unlike in the other sub-experiments, we found a global effect, i.e. in both priming conditions, the acceptance of zero models in target trials was higher than in the baseline; the acceptance rate further improved with the duration of the experiment staying barely below the ceiling in the last trials of the experiment. Moreover, the average acceptance rate of primes was relatively low, making the priming attempt less successful. Thus, the lack of difference between priming conditions can be explained by the global effect covering such difference (cf. Jaeger and Snider (2013) for a discussion of global vs. local syntactic priming). This possibility is compatible with the predictions of Aloni (2022) that both local and global suspension of neglect-zero is possible; in the latter case, we expect what we found: no difference between conditions but a global (possibly gradient) improvement in accepting zero models.

Such a global effect is missing from DIS. This may be because the difference between sentences and verification procedures of primes and targets in this sub-experiment is more considerable. Hence, a need to suspend the bias for the entire discourse is less apparent and results only in an unconscious local suspension in target trials due to a still active mechanism after the prime trial.[5] In the UPP-sub-experiment, we found a lower average acceptance rate of the target items. This can be because the literal meaning of the prime sentence asks to check for the existence of the object (finding *some x* satisfying the property). If this mechanism remains active for the target trial, where the existence of the objects is not satisfied, we would expect the observed lower acceptance rate. Critically, this means that we found the priming of the literal meaning of *some* and not its strengthened interpretation.[6]

Conclusions

Across-construction priming between (1-b) and (1-c), indicates that, as postulated by *neglect-zero* based theories, there is a common underlying mechanism between these sentences, for which the choice of the weaker (literal) interpretation requires engaging with zero models: verification by an empty configuration. On the other hand, we found no priming between the scalar *some* (1-a) and (1-b), which may point to a different mechanism being involved in the interpretation choice for these structures, going against the predictions of some alternative-based accounts (cf. Bott., Schlotterbeck, Klochowicz, Ramotowska, & Aloni, to appear, for converging evidence using a different method).

Our results contribute to the discussion about the possibility of across-construction priming, since we found a trial-to-trial effect between very different constructions in the DIS-ESQ sub-experiment (cf. Feiman & Snedeker, 2016; Feiman et al., 2020; Husband & Patson, 2021).

We plan to replicate the ESQ experiment using comparatives as both prime and target. If similarity-induced global suspension drives global adaptation in the absence of trial-to-trial priming in ESQ, the same pattern is to be expected there (contra strengthened trial-to-trial syntactic priming in case of lexical identity, see Mahowald et al., 2016). Further evidence can be provided by a reversed-priming setup, which would prime participants to neglect zero. There we expect to find a trial-to-trial priming effect. We plan to investigate whether the priming relation between (1-c) and (1-b) is bidirectional and whether the interpretation of FC can be primed by *neglect-zero* based structures like (1-c) and (1-b), given the lack of priming by alternative-based structures as shown by Meyer and Feiman (2021).

[5] As a possible confound, one of the reviewers mentioned the availability of the inverse scope reading of DIS (i.e., Each heart is black or [Each heart is] blue.). This reading is, by design, discouraged by the control-primes, as it is false in both pictures. Moreover, the critical prime is also a zero model for the inverse scope reading (Aloni & van Ormondt, 2023). Hence, the inverse scope reading, if derived, also requires zero-model-based reasoning.

[6] We thank Mora Maldonado for this observation.

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