

Priming local accommodation of hard triggers in disjunction*

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1. Introduction

While the traditional and theoretically most parsimonious view considers presuppositions as a uniform phenomenon, observations about potential differences between triggers have been present since early on, and have become a major focus of interest in recent work. Two classes of presupposition triggers are now commonly distinguished (Karttunen 1971, Zeevat 1992, Abusch 2002, Abbott 2006, Charlow 2009, Abrusán 2016, a.o). Following terminology in Abusch (2002), these are *soft* triggers and *hard* triggers.

A key point of differentiation between triggers concerns their ability to be suspended: soft triggers allow for suspension rather easily, whereas hard triggers do not. Abusch illustrates suspension with the soft triggers *start* and *stop* with the example in (1):

(1) John either started smoking or he stopped smoking.

If the standard presuppositions of both triggers obtained globally in (1), then the overall presupposition would be contradictory: that John both didn't use to and did use to smoke, based on the respective contributions of *start* and *stop*. In actual fact, (1) intuitively does not presuppose anything about John's previous smoking habits, indicating the presuppositions are suspended. A second illustration of suspension with soft triggers comes from contexts that explicitly state ignorance of the presupposition. In such contexts, soft triggers may still be used under operators from whose scope their presuppositions normally project, e.g., epistemic modals:

(2) John has been moody lately. I'm not sure whether he's ever been a smoker.
But based on his behavior, it is possible that he just stopped smoking.

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In (2) the speaker is explicitly agnostic as to whether John used to smoke, and even so, the use of *stop* under *possible* does not give rise to a clash. The felicity of (2) again is indicative of presupposition suspension.

The suspension behavior of soft triggers clearly contrasts with that of hard triggers such as *too* and *again*. For one, Abusch observed that suspension is unavailable when *too* is used instead of a soft trigger in a sentence parallel to (1):

- (3) # After the first meeting, John will either attend the second meeting too, or he will miss the second meeting too.

The first occurrence of *too* presupposes that John attended the first meeting, while the second occurrence presupposes that he missed it. This results in infelicity based on the contradictory presuppositions, suggesting that the presuppositions are not eligible for suspension. Similarly, the hard trigger *again* is not felicitous in an explicit ignorance context:

- (4) John looks very sick. I don't know whether he's had scurvy before, but I hear having scurvy twice can make you look this way.
So it is possible that John has scurvy again.

In (4), the speaker states agnosticism about whether John had scurvy before, but, even so, the presupposition of *again* projects from its embedding under *possible*. This leads to an unavoidable clash with the speaker's stated ignorance. Suspension with hard triggers seems difficult, if not impossible, to obtain.

These and similar considerations give rise to a fundamental question: do soft and hard triggers derive from *different mechanisms*? Abusch (2002) argued that they do (see also Simons et al. 2010, Abrusán 2011, Romoli 2011, 2014). By this line of thinking, only hard triggers *lexically encode* presuppositions. The use of soft triggers may give rise to presuppositions – or presupposition-like inferences – via *pragmatic processes*. Different authors propose different pragmatic implementations (e.g., based on reasoning about alternative expressions for Abusch 2002, Romoli 2011, 2014, and assessing the contextual salience of part of the entailment for Simons et al. 2010, Abrusán 2011).

This bifurcation in triggering mechanism naturally aligns with differences in suspension. With soft triggers, suspension amounts to the failure of a pragmatic process to derive a presupposition-like inference. Pragmatic processes are inherently variable, dependent on contextual considerations. In contrast, suspending the presuppositions of a hard trigger must involve a special mechanism: the presupposition is present, qua lexically encoded constraints, and must be canceled via some operation, such as Heim's (1983) local accommodation. Impossibility of suspension is the null hypothesis: the grammar may not in fact supply such operations.

In this paper, our aim is to re-evaluate the strength of evidence for a categorical distinction between triggers by testing experimentally whether suspension is really impossible with hard triggers. Specifically, we ask: can the presupposition of *again* be suspended under the right experimental conditions? We use cumulative priming to increase the availability of potential latent interpretations.

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Our investigation proceeds in two steps, corresponding to two experiments. Although the presuppositions of hard triggers project by default, our first experiment establishes the existence of some non-default reading where the presupposition does not project from a particular embedded context (disjunction). Moreover, our first experiment establishes priming as a useful mechanism to reveal non-default readings. The second experiment investigates the nature of these latent readings more closely, again employing a priming design. Though priming doesn't prove as successful as in the first experiment, the results nevertheless suggest that the latent readings observed in both experiments at least in part are due to an interpretation where the presupposed content contributes locally, rather than globally, as would result from a mechanism like local accommodation.

Consequently, our study calls into question the view that hard and soft triggers are categorically distinct in nature, to the extent that such a distinction is based on the claim that availability of presupposition suspension, and in particular local readings, would be limited to soft triggers. The difference in relative availability of suspension between the two types of triggers seems to be a *gradient* one. In this way, the results of our study place further constraints on theories of presupposition: a satisfactory theory should allow for local readings with soft and hard triggers, while at the same time explaining why these are less available for hard triggers.

2. Presupposition suspension in disjunction

We conducted two experiments to establish whether speakers can suspend the presupposition of *again* when embedded in disjunctive environments, as in (5):

- (5) On Wednesday, John either went to the orchard again, or he went to the movies.
- a. orchard < Wed \wedge [orchard-Wed \vee movies-Wed]
 - b. [orchard < Wed \wedge orchard-Wed] \vee movies-Wed

In principle, (5) can receive at least two different interpretations, depending on whether the presupposition of *again* projects as in (5a) or is interpreted locally in the first disjunct as in (5b). The interpretation in (5a), where the presupposition projects, is clearly the default intuitive reading: it requires that John went to the orchard before Wednesday in order for (5) to be accepted as felicitously true (in addition to John going either to the orchard or to the movies *on* Wednesday). But under the interpretation in (5b) where the presupposition is interpreted locally, this need not be the case: since the presupposition of *again* is interpreted *within* the first disjunct, the whole disjunction can be accepted as felicitously true as soon as the second disjunct (that John went to the movies on Wednesday) is true, even if he never went to the orchard prior to Wednesday.

Our experiments test for the availability of readings parallel to the one in (5b): if the presupposition of *again* is not suspendable and always projects (5a), as has been argued to be the case for hard triggers, the sentence should be judged inappropriate in a situation where John didn't go to the orchard before Wednesday, regardless of whether or not the second disjunct is true. On the contrary, if the presupposition of *again* can be suspended (5b),

it should be possible to accept (5) as an appropriate description of a situation where John never went to the orchard before Wednesday as long as he ended up going to the movies on Wednesday. Taking into account the observations from the literature that presupposition suspension is not easily available for hard triggers, if it is at all, both of our experiments utilized cumulative priming to bring out potential latent readings like (5b). The reasoning is that by first exposing speakers to situations that would require suspension in order to provide a response on a given trial, we can expect them to then be more likely to access readings like (5b) even in more neutral situations, under the hypothesis that suspension with *again* is available in the first place.

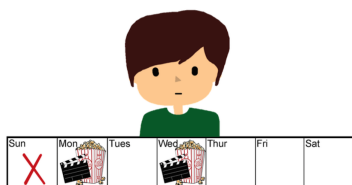
2.1 Experiment 1: priming latent readings

Experiment 1 shows that the presupposition of *again* does not necessarily project globally. The results are consistent with a local reading, but could also be explained in terms of other potential latent readings (see discussion in section 2.1.6 below). The main contribution of Exp. 1 is to establish that non-global readings of *again* exist.

2.1.1 Design

Our experiments used a picture selection task with a ‘covered box’ (Huang et al. 2013). Participants were invited to play a guessing game of sorts, where they would take the role of a detective trying to identify suspects based on their schedules for a certain week, as iconcially depicted in two calendar strip pictures (Schwarz 2015). Using the information provided by sentences like (5) as their only clue, they had to indicate which of two individuals’ reported schedules was most likely to be a match. Importantly, one of the reported schedules (the *covered picture*) didn’t provide fully explicit information about the nature of the associated suspect’s activities during the week. (6) provides an illustration of a pair of reported schedules as shown in the experiment:

(6) a. *Visible picture*



b. *Covered picture (Ps?)*



While the *visible picture* (6a) presents a suspect that went to the movies on Wednesday as well as prior to that day, the *covered picture* (6b) depicts a suspect that engaged in activities on Monday and Wednesday whose nature was not identified (as indicated by question marks). Crucially, with regard to the sentence in (5), the Monday activity depicted in the visible picture makes this schedule a bad match under a reading where the presupposition projects as in (5a), as it depicts a suspect that did NOT go to the orchard prior to Wednesday. In order to prevent participants from accommodating potential events prior to

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the depicted week that would result in the presupposition being met, (5) was preceded by a context sentence, e.g., *Henry came to town for the first time on Monday*. Information about arrival timing was also reflected in the pictures, where cross marks indicated days where the suspects had not yet arrived in town. The covered picture (**Ps?**) on the other hand is *compatible* with a projection reading to the extent that, for all we know, the depicted suspect might very well have gone to the orchard prior to Wednesday and either to the orchard for a second time or to the movies on Wednesday. If only reading (5a) is available, (6b) should then be the only viable match.

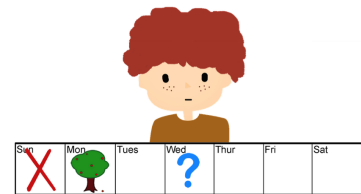
This design allows us to investigate the existence of latent readings by looking at the rate of visible-picture choices given these two options. Only if participants are able to access a reading where the presupposition fails to project, e.g., because it is locally interpreted as in (5b), do we expect any choices of the visible picture, since the non-presuppositional disjunct holds true of it, while the presupposition of the other disjunct does not. Observing participants choosing the visible picture over the covered picture, despite the violation of a global presupposition in the visible picture, would clearly indicate that (5) can be associated with readings other than the default (5a), thus challenging the view that the presuppositions of hard triggers like *again* always project.

Given the abundant, if cursory, evidence that suspension readings for triggers like *again* are not easily available, to say the least, our design comes with the risk of subjects consistently choosing the covered picture, since it's at least compatible with the preferred reading in (5a). To promote the availability of a suspension reading, we utilized a priming approach that confronted participants with a picture choice where neither option was compatible with the global presupposition in (5a), by pairing targets like the one in (6a) with a covered picture version like the one in (7a):

(7) a. Covered picture (**Ps-**)



b. Covered picture (**Ps+**)



In this version of the covered picture, the suspect is explicitly depicted as not going to the orchard prior to Wednesday (as in the visible picture), while the nature of the Wednesday activity remains unknown. Thus, both the (partially) covered and the visible pictures were incompatible with the presupposition that John went to the orchard before Wednesday. Consequently, whichever picture participants wound up choosing, their choice would be incompatible with the projection reading in (5a). To the extent that a suspension reading like (5b) was available to participants at all, we then expected them to fall back on this to align their final choice (expected to be the visible picture, given the explicit match with the non-presuppositional disjunct) with a reading of the sentence. We hypothesized that after exposure to several such trials, latent readings would become salient enough that participants would accept the same visible picture even in at least some trials where the

covered picture is compatible with a projection reading in (5a), as in the **Ps?** condition. In addition to the covered pictures already discussed, we also included versions that explicitly supported a projection reading (**Ps+**), as illustrated in (7b).

2.1.2 Materials

The three conditions defined by these variants of the covered picture were presented in different blocks and interspersed with control and filler trials. Two groups of participants were defined by different block orders, which crucially varied whether trials that paired the critical visible picture with the **Ps?** competitors followed ones with **Ps-** or **Ps+** competitors:

(8) *Block Orders for Critical Trial Competitor Types*

| | Block 1 | Block 2 | Block 3 |
|---------------|------------|------------|------------|
| Primed Group | Ps- | Ps? | Ps+ |
| Control Group | Ps+ | Ps? | Ps- |

Each block contained 6 critical trials (visible picture incompatible with a projection reading) and 6 control trials (visible picture supporting a projection reading) as well as 4 trials of each of 3 types of filler conditions. All fillers had target pictures compatible with a projection reading as in (5a), and half of them made the *presuppositional* disjunct true. The first type of fillers had competitor pictures that also matched a projection reading, but that didn't match the context sentence: the cross marks indicated a wrong day of arrival. The second type of fillers had competitor pictures depicting an activity on the mentioned day that didn't match any disjunct. The last type of fillers had competitor pictures that were less informative than the target pictures, parallel to the critical items: the target pictures explicitly matched the description, whereas the competitor pictures depicted question marks on Wednesdays. As a result, each block contained 24 test trials, for a grand total of $24 * 3 = 72$ trials per participant. The trials were identified by the mentioned and depicted activities (orchard, pool, aquarium, ...) across which the three test and three control conditions were distributed in a latin-square fashion, thus creating 6 subgroups of participants.

In addition to disjunctions like (5), we also tested their inverse order variants with another 6 subgroups of participants:

(9) On Wednesday, John either went to the movies, or he went to the orchard again.

2.1.3 Participants and Procedure

A total of 175 undergraduate students at the University of Pennsylvania were recruited to take the experiment online via Sona. The experiment was implemented on Ibex. An archived version of the experiment is available at <http://spellout.net/ibexexps/SchwarzLabArchive/OrAgainCB/experiment.html>. 85 participants saw disjunctions with *again* in the first disjunct, out of whom 44 participants were assigned to the **Ps-primed** group and 41 participants were assigned to the **Ps+ control** group. 90 partici-

pants saw disjunctions with *again* in the second disjunct, out of whom 47 participants were assigned to the **Ps- primed** group and 43 participants were assigned to the **Ps+ control** group. Participants first saw an initial screen with instructions introducing the task as a detective game, as explained earlier, and then saw 2 practice trials to become familiar with the task. Each trial began with a display of the context sentence, advancing to the crucial disjunctive sentence after pressing the space bar. The picture choices were displayed upon yet another press of the space bar, upon which a picture had to be chosen by either pressing 'F' or 'J', as indicated above the pictures.

2.1.4 Predictions

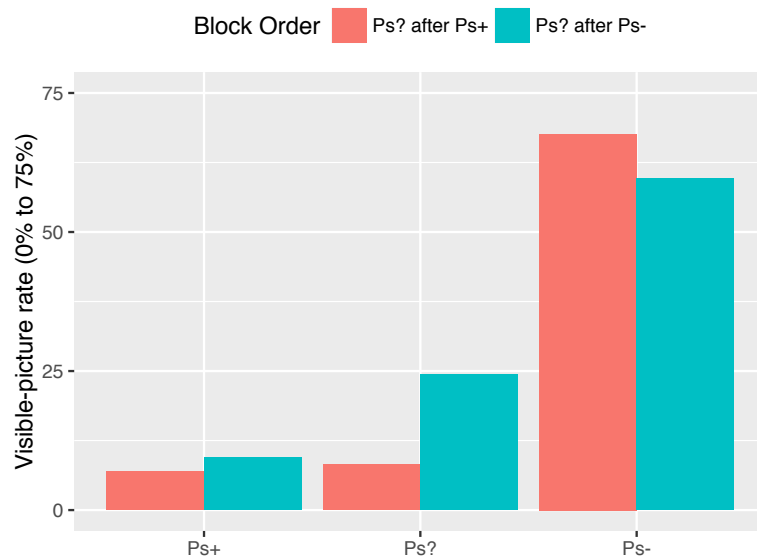
Our null hypothesis was that disjunctions like (5) only allow for projecting readings like (5a). Our alternative hypothesis was that disjunctions like (5) additionally allowed for suspension readings like (5b). The null hypothesis predicts no effect of the priming manipulation and that the rate of visible picture choices in the **Ps?** trials should not be different in the **Ps+** control group and the **Ps-** primed group. The alternative hypothesis predicts an effect of the priming manipulation in the direction of more visible picture responses to the **Ps?** trials in the **Ps-** primed group than in the **Ps+** group.

2.1.5 Results

We excluded 33 participants based on low accuracy ($< 75\%$) on the second type of fillers.¹ The data reported below collapses the results from the remaining 142 participants for both disjuncts orders. Disjunct order only appeared to incur small drops in visible-picture rates in **Ps?** and **Ps-**, and showed no significant effect or interaction in the models that we fitted. The figure in (10) reports the mean rates of visible picture choices in the critical trials determined by the *Competitor Types* described above ($Ps+$, $Ps?$ and $Ps-$) for both *Block Orders* (primed: **Ps? after Ps-** in red; and unprimed: **Ps? after Ps+** in blue).

For purposes of data analysis, we focused on trials with the the **Ps+** vs. **Ps?** competitors to test whether priming with **Ps-** had an affect on the latter. We ran a logistic regression model in R (version 3.1.2) using the function *glmer* from the package *lme4* (version version 1.1 – 11). Our model predicted the choice of visible vs. covered picture in the critical trials, as a function of two centered parameters: *Competitor Type* ($Ps+ = -1$ vs $Ps? = 1$) and *Block Order* (unprimed: $Ps? after Ps+ = -1$ vs primed: $Ps? after Ps- = 1$). Our model tested for main effects as well as an interaction between the aforementioned parameters, and included a random intercept and a random slope for *Competitor Type* per participant as well as a random intercept and a non-correlated random slope for *Block Order* per item. More complex random structures would either not converge or result in over-fitting. By fitting the most parsimonious model, we follow the recommendation of (Bates et al. 2015).

¹Accuracy was low on fillers of type 1, where the target minimally differed from the competitor picture in the depicted day of arrival. Though only target pictures were explicit matches in fillers of type 3, the competitor pictures ultimately constituted implicit matches. Only fillers of type 2 had clearly identifiable, explicit, single matches, and therefore provided the most conservative basis for accuracy removal.

(10) *Rates of visible-picture choices by Block Order and by Condition for the 142 accurate participants*

The statistical results revealed a significant interaction ($p < .05$, $\beta = 0.4452$, $SE = 0.1816$),² as well as corresponding significant main effects of *Competitor Type* ($p < .05$, $\beta = 0.8205$, $SE = 0.3827$) and *Block Order* ($p < .05$, $\beta = 0.5273$, $SE = 0.2403$).

2.1.6 Discussion

The results clearly refute the null-hypothesis, based on the assumption that only the projection reading in (5a) is available, i.e., that disjunctive sentences containing the presupposition trigger *again* uniformly impose a global requirement that its presupposition is met, regardless of which disjunct makes the sentence true in a given situation. While that reading does seem to be the pre-dominant one, as indicated by the fact that participants based their responses on it in the **Ps?** condition after only having been exposed to **Ps+** trials, this default can be overridden once they are forced to consider other potential interpretations via prior exposure to **Ps-** trials. The statistical interaction indicates that this priming setup had a significant impact, and led to an increase in target acceptances, which can only be explained in terms of a reading of the sentence that does not impose such a global constraint.

However, the results from this experiment do not necessarily establish that when accepting the visible picture in critical trials, participants were doing so based on a reading where the presupposition featured as part of the disjunct containing *again* (5b). This is because two other candidate interpretations of the sentence are compatible with the critical visible picture:

²Treatment models including **Ps-** as a third level indicate significant effects in line with those results, though including *Disjunct-Order* as a parameter makes the interaction only marginally significant ($0.05 < p < 0.1$) when looking at it separately for each disjunct order.

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- (11) a. $(\neg \text{movies-Wd} \rightarrow \text{orchard} < \text{Wd})$ *Conditional Ps*
& $(\text{movies-Wd} \vee \text{orchard-Wd})$
b. $(\text{movies-Wd} \vee \text{orchard-Wd})$ *Ps cancelled*

Many (though by no means all) theories of presupposition projection assume that the semantically encoded presupposition in disjunctions is merely a conditional one (11a), i.e. that the presupposition of a given disjunct only needs to hold if the other disjunct is false (though this conditional presupposition may get strengthened, to deal with issues relating to the proviso problem). If such a reading involving a conditional presupposition were latently available, an alternative interpretation of the priming effect arises, as the **Ps**-priming might make this conditional presupposition more salient. Yet another option that is in principle possible is that the presupposition of *again* can be cancelled altogether, as in (11b), which again would make the critical visible picture a good match, and priming could be manipulating the availability of presupposition cancellation. Note that these options are not mutually incompatible, i.e., in principle, all of these readings could be at play and have their salience increased by our **Ps**-priming trials.

2.2 Experiment 2: Priming local readings

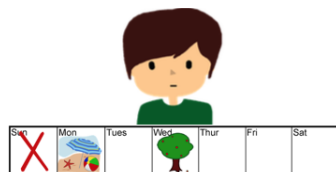
Our second experiment used a similar priming setup combined with an additional sentence variation using *Neither-Nor* disjunctions to assess just what latent readings are at play in Experiment 1, in particular whether the reading in (5b), where the presupposition of *again* contributes locally to the disjunct the trigger appears in, is at all available.

2.2.1 Design & Materials

As before, we used a block design, and the first block came directly from Experiment 1, either in the **Ps**- or the **Ps?** variant. The second block included the critical variation in (12), paired with a picture as in (13). Given the lack of an effect of disjunct order in Experiment 1, only sentences with *again* in the second disjunct (where, generally speaking, conditional presuppositions might be most likely to arise in disjunctions) were included.

- (12) On Wednesday, John neither went to the movies nor did he go to the orchard again.

(13)



Given the (implicit) presence of negation in the *Neither-Nor* variant, the different readings now pattern differently, so that the equivalent of the local reading in (5b), namely (14b), is

singled out in being the only one compatible with the situation depicted in (13), where the character did go to the orchard on Wednesday but had not done so before.

- (14) a. $\boxed{\text{orchard} < \text{Wd}} \ \& \ \neg(\text{movies} - \text{Wd} \vee \text{orchard} - \text{Wd})$ *Global Ps: ✗*
- b. $\neg(\text{movies} - \text{Wd} \vee (\boxed{\text{orchard} < \text{Wd}} \ \& \ \text{orchard} - \text{Wd}))$ *Local Ps: ✓*
- c. $(\neg \text{movies} - \text{Wd} \rightarrow \text{orchard} < \text{Wd})$
 $\boxed{\& \ \neg(\text{movies} - \text{Wd} \vee \text{orchard} - \text{Wd})}$ *Conditional Ps: ✗*
- d. $\boxed{\neg(\text{movies} - \text{Wd} \vee \text{orchard} - \text{Wd})}$ *Ps cancelled: ✗*

Consequently, acceptance of this kind of picture would be a clear indication of the existence of a local reading. Given the additional component of negation, assessing the sentences in the second block may well be more taxing. In particular, it is intuitively difficult to reconcile the negation introduced by *Neither-Nor* with the fact that in (13), the activity mentioned in the presuppositional disjunct (going to the orchard) is instantiated on Wednesday. And indeed, preliminary testing suggested that when given any alternative choice with question marks in relevant calendar slots, participants strongly prefer these. However, if only confronted with the picture in (13) in light of the sentence at hand, it becomes intuitively easier to acknowledge that there is a sense in which the picture matches the sentence. We therefore modified the task in the second block slightly, utilizing a simple truth-value judgment task without any competitor images.

An additional false control condition provided a baseline comparison to assess whether a local interpretation is at all present, by manipulating the picture so that the presupposition was met (e.g., where the shown character went to the orchard on Monday), but the assertion was false (e.g., because he did go to the movies on Wednesday).

2.2.2 Participants & Procedure

We recruited 122 participants through Prolific.ac, split into groups for the different priming conditions and counterbalancing across items, as in Experiment 1. The participants read the same instructions as before. Trials once again proceeded step-by-step, with participants pressing the space bar to reveal the next bit of information. The second block contained 6 critical and 6 control items, as well as fillers parallel to those used in Experiment 1. Between the two blocks, participants saw a brief message informing them of the slight change in task. An archived version of the experiment is available at <http://spellout.net/ibexexps/SchwarzLabArchive/PrimingOrNeither/experiment.html>.

2.2.3 Predictions

As in Experiment 1, the null hypothesis was that local readings are not available for the sentences that we tested. The alternative hypothesis was that local accommodation readings are at least latently available, and that they can be primed by prior exposure to similar

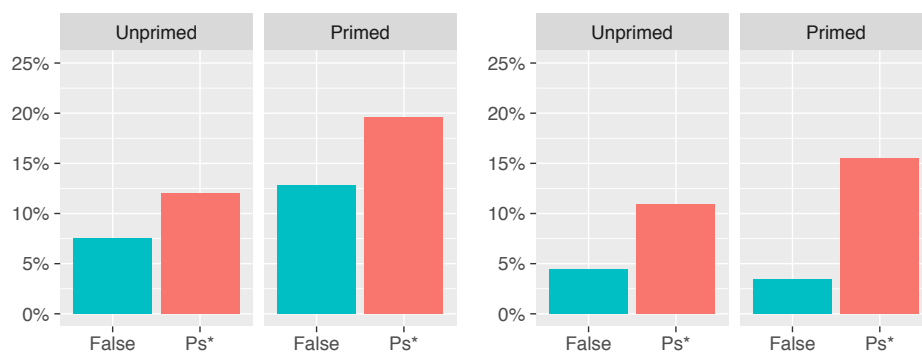
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readings in simple disjunctions. The null hypothesis predicts no effect of the type of *Either-Or* trials seen in the CB task (**Ps?** vs. **Ps-**) in the first block on the judgments given in the second block for the *Neither-Nor* sentences in the critical condition in the TVJT task. The alternative hypothesis, by assuming that the effect observed in the first experiment resulted from priming local readings, predicts that the rate of *True* responses to the critical trials in the second block should be higher for participants who saw **Ps-** trials in the first block than for participants who saw **Ps?** trials in the first block.

2.2.4 Results

As in Experiment 1, the key question of interest was whether participants' behavior in the critical trials of the second block would be affected by the type of trials they were exposed to in the initial block. We measured overall accuracy on fillers in the second (*Neither-Nor*) block and found that it was already high on 'true' fillers (around 90%) before exclusion but low on 'false' fillers. We proceeded to analyses both on the unfiltered data and on data filtered by overall accuracy on fillers from the second block. We excluded 18 participants based on low accuracy (< 75%) on those fillers.³ The rate of 'True' responses for the critical (**Ps***) and false control pictures is shown in (15), split by priming group. The figure on the left represents unfiltered data. The figure on the right represents responses from participants with high accuracy both on 'true' and 'false' fillers.

(15) Rates of "True" responses in the critical and false control conditions by Priming group. Left: all 122 participants; Right: 104 accurate participants



In order to assess the statistical significance of the observed effects, we conducted logistic mixed-effect model analyses using the *glmer* function in the *lme4* R-package. We focused on two types of contrasts: we compared the rate of 'true' responses in the critical condition either i. with control true conditions (within items) or ii. with filler false conditions (between items) to test whether priming with **Ps-** *Either-Or* items had an affect on the critical *Neither-Nor* items only. The logistic regressions models that we ran predicted

³The role of the fillers in the *Neither-Nor* block differed from the fillers from Experiment 1. The four readings we considered for the *Neither-Nor* sentences in (14) all predicted the same response on each filler: using those fillers as a basis for exclusion was therefore a way to look at whether local readings were indeed available and elicited by priming once we exclude unexplained interpretations and/or task-specific strategies.

‘true’ responses as a function of two treatment parameters: *Condition* (baseline: critical) and *Priming* (baseline: unprimed). Our models tested for main effects as well as an interaction between the aforementioned parameters, and included a random intercept per participant, as well as a random intercept per item in the first type of models (within items) or a random slope for *Condition* per participant in the second type of models (between items). More complex random structures would either not converge or result in over-fitting. We ran those models both on the whole set of data and on a subset of the data, filtered by accuracy.

The model contrasting the critical conditions with the control true conditions in the whole data set revealed a significant simple effect of priming ($p < 0.01$, $\beta = 1.4696$, $SE = 0.5272$) as well as a significant interaction ($p < 0.001$, $\beta = -2.4041$, $SE = 0.6497$). Models using the filtered subset of data failed to show the same indications of significance, as did models contrasting the control condition with the ‘false’ fillers (regardless of which data set was used). The latter models revealed that the rate of ‘true’ responses was significantly higher in the critical condition than in the ‘false’ filler condition in both priming groups (all p 's < 0.05).

2.2.5 Discussion

Given the relation of the candidate readings of *Neither-Nor* sentences to the critical pictures, any reliable presence of ‘true’-judgments provides a clear indication of the existence of a reading where the presupposition of *again* is interpreted locally. And indeed, we find significantly more ‘true’-judgments here than in a false filler condition (where the presupposition is met but the assertion is false), in both groups of subject primed with **Ps+** *Either-Or* blocks and those with preceding control blocks (**Ps?**). This suggests that a local interpretation indeed exists for *again* in *Neither-Nor* sentences, though it is clearly not the preferred interpretation, given the relatively low rates of ‘True’ judgments at about 10-15%.

The predicted priming effect was not supported by the data consistently, apart from the interaction and simple effect in the comparison to true controls. This lack of evidence for priming might be attributable to a number of factors. First, we may lack sufficient statistical power after accuracy filtering. Second, the priming manipulation may have been less effective, given the switch in sentences and tasks across blocks; furthermore, the control **Ps?** block already exhibited higher acceptance rates for the critical visible picture than in Experiment 1, at about 35%, resulting in some amount of priming of local readings even in the control group. Additional questions arise with regards to the apparent increase in ‘true’-judgments for the False Control items when looking at the unfiltered data. It is possible that the priming block decreased accuracy overall or made additional readings we have failed to consider more salient.⁴ Further work will need to address these possibilities.

But with these caveats in place, we should still appreciate the descriptive trends in the expected direction, as well as the overall contribution of the present results in the broader theoretical context. While a consistently significant interaction would have provided the clearest evidence that local readings exist for both *Neither-Nor* and *Either-Or* sentences, as

⁴One candidate for such a reading would be a *duplicated* local reading, whereby the *Neither-Nor* sentences would convey that neither activity occurred *both on and before* the mentioned day.

that would have rendered the most straightforward explanation of priming across sentence types, we still have a combination of interesting and important findings, which together at least suggest that local readings exist for both cases. Experiment 2 clearly establishes the existence of local readings for *Neither-Nor* sentences, while Experiment 1 establishes priming of non-global readings for *Either-Or*. It seems conceptually unlikely that local readings would only exist for the former, since that would require a highly construction-specific mechanism. Furthermore, if local readings are latently available, it seems unlikely that they couldn't be primed. However, future work is needed to establish the combination of the conclusions drawn here more conclusively in empirical terms.

3. General Discussion

One of the key arguments in the literature for distinguishing different types of presupposition triggers has been based on observations in relation to the availability of any type of presupposition suspension, e.g., in the form of local readings under embedding operators. Abusch's soft triggers have been noted to allow the relevant readings quite readily, whereas hard triggers such as *again* do not. On a number of related proposals, presuppositions associated with the relevant expressions should be seen as distinct types of inferences, which are introduced by a distinct mechanism. In particular, one prominent class of proposals assumes that only hard triggers involve lexically hard-wired presuppositions, whereas the relevant inferences introduced by soft triggers are derived pragmatically in one form or another. In sum, contrasts in the availability of presupposition suspension have been used to support the case for a categorical distinction between different types of triggers.

In the present set of experiments, we aimed to assess to what extent there is a categorical distinction in the availability of local readings for the presupposition of the hard trigger *again*. Using picture matching tasks with a covered box as well as a sentence-picture truth value judgment task, we gained experimental insights into the latent availability of local readings for *again* in disjunctions. Experiment 1 established that when primed with picture choices that were altogether incompatible with global readings of the presupposition, participants became more likely to respond based on a non-global reading, even if the set of options did not force such a response. In other words, we were able to increase the availability of non-global readings through block-priming. Unfortunately, in simple *Either-or* disjunctions, the relevant responses do not conclusively settle just what reading participants are basing their responses on, as the critical picture was also compatible with a conditional presupposition or a complete cancellation of the presuppositional inference. However, turning to *Neither-Nor* disjunctions, we were able to home in on responses that are unambiguously indicative of a local interpretation of the presupposition, i.e., a reading where the content introduced by *again* (e.g., that Henry went to the orchard before Wednesday) is crucially contributing to the disjunct the trigger appears in. The results from Experiment 2 revealed that such an interpretation is indeed available, if to a limited extent.

In the larger context of the question of how and whether to differentiate different classes of presupposition triggers, these results point to a more gradual distinction in the availability of local readings, which in turn calls into question attempts to construct a categorical distinction between types of triggers in theoretical terms. To the extent that there indeed is

such a gradual distinction (since we don't have data on soft triggers in the present context, our results don't speak to that question directly), something still needs to be explained, of course. However, there may be a number of independent or orthogonal factors that could be called upon to account for such differences, while maintaining an overall homogeneous perspective on presupposition triggers. One particularly relevant extension of the present line of work would be to try to prime local readings across triggers, as this could provide evidence in favor of there being just one type of mechanism that underlies the relevant interpretations in both cases.

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