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Kleene'ing up your attitude: Against universal presupposition projection*

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Abstract It is often assumed that presuppositions in the scope of attitude predicates, a case of presupposition projection that is empirically unexplored, must be satisfied at every world that the attitude predicate quantifies over. This paper presents experimental evidence that such cases of presupposition projection can instead be modeled within a trivalent semantic framework, specifically using the Strong Kleene logic. The experimental results pose challenges for standard accounts based on a Universal Projection hypothesis, which fail to predict key aspects of the data. Instead, the findings align with theories in which presupposition failures can participate in compositional interpretation. Further, we find what we call Strawsonian squeamishness, increased variance in participants' responses when faced with presupposition failure.

Keywords: presupposition projection, quantification, Strong Kleene, attitude predicates, local accommodation, experimental semantics

1 Introduction

To give an account of presupposition projection is to be able to predict what the presupposition of a complex sentence is from the presupposition of its parts. A long tradition of research has addressed this problem as part of the compositional derivation of a sentence's meaning, such that at every application of a logical operator to an interpretable linguistic form, the meaning of the operator determines what the presupposition of the resulting expression is. An example is given in (1) for English

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Although the pun in the title relies on the fact that Stephen C. Kleene's name is more often than not pronounced as [ˈklini], Wikipedia reports [ˈkleɪni] as the correct pronunciation.

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not, where presuppositions are traditionally characterized in terms of definedness conditions.

(1) $[\![\text{not } A]\!]$ is defined only if $[\![A]\!]$ is defined. If defined, $[\![\text{not } A]\!]$ is true if and only if $[\![A]\!]$ is false.

A common intuition is that logical operators with similar meanings should lead to similar patterns of presupposition projection. Descriptively, operators have often been divided into groups according to their behavior with respect to projection, as in the case of holes, filters, and plugs for Karttunen (1973). The discussion about what the exact grouping should be, and why the groupings are the way they are, has been oscillating between trying to be more explanatory by giving unified accounts, and accepting to stipulate the projective behavior on an operator-by-operator basis.

In this paper, we pursue the hypothesis that the behavior of a logical operator with respect to presupposition projection does not have to be stipulated lexically, but follows from the logical contribution of the operator and a single logic that can deal with truth-value gaps. Our focus will be on presupposition projection from attitude predicates, which we will assume are quantificational operators. We will try and show that a Strong Kleene trivalent logic is a good approximation of such an underlying natural language logic for these cases, just as has been proposed for other quantificational environments (George 2010, Fox 2013). Our hypothesis can thus be stated as in (2).

(2) Generalized Strong Kleene hypothesis:
The logic that predicts presupposition projection is Strong Kleene for all domains of quantification.

We pursue this hypothesis experimentally, offering — to our knowledge — the first controlled empirical investigation of presupposition projection from attitude predicates. Our study makes two contributions: a theoretical one, and a secondary, methodological one. On the theoretical side, we show that presupposition projection from the attitude predicate *be certain* follows the trivalent Strong Kleene logic rather than projecting universally, extending to the modal domain a logic that had previously been motivated only for quantifiers over individuals; relatedly, soft and hard presupposition triggers do not differ in their projection profiles in this environment. On the methodological side, we show that using distributional models, we can interpret the dispersion of participants' responses in truth-value judgment tasks as Strawsonian squeamishness and thus as a reliable diagnostic for presupposition failure, supporting the Variance hypothesis of Thalmann (2025).

The paper is structured as follows. Section 2 presents the predictions of a generalized Strong Kleene logic for presupposition projection out of the scope of quantifiers

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over individuals and that of attitude predicates, comparing it to the alternative approach of Generalized Universal Projection that has been the undisputed standard for attitude predicates. Section 3 focuses on the factors that have been proposed as drivers of variation in the presupposition projection experiments, such as the type of the presupposition trigger and local accommodation. Section 4 presents our experimental investigation of presupposition projection out of the scope of the attitude predicate *be certain*, which supports our hypothesis in (2) and is in conflict with approaches positing universal projection; it is also here that we introduce, as a secondary methodological contribution, a way of diagnosing presupposition failure through data variance. Section 5 concludes.

2 Presupposition projection and quantificational environments

2.1 Quantifiers over individuals

Research on presupposition projection out of the scope of quantifiers (for a review, see Schwarz 2019: §3.1.1) has been affected by a data problem: there is no broad consensus about what empirical picture a theory of projection should derive for quantifiers over individuals. Consider the sentence in (3), where the presupposition trigger *stop* takes a universally quantified subject. In this case, it is generally agreed upon that for the sentence to be true, it must be true that all of the students used to smoke.[1]

(3) All of the students λx [x stopped smoking.]
$\rightsquigarrow$ All of the students used to smoke.

A different inference might be drawn in cases like (4a) and (4b). In particular, some authors report that such sentences may be judged as true even in contexts where the presupposition is not satisfied universally, that is, where not all of the students used to smoke (see Karttunen & Peters 1979, Beaver 2001, among others). We will turn to the experimental findings in this regard shortly.

(4) a. Not all of the students λx [x stopped smoking.]
$\not\rightsquigarrow$ All of the students used to smoke.
b. Some of the students λx [x stopped smoking.]
$\not\rightsquigarrow$ All of the students used to smoke.

1 One view of presupposition projection put forth in the literature is based on the notion of at-issueness: see Simons et al. 2010, Tonhauser, Beaver & Degen 2018 and related research. To our understanding, this approach fails to make any predictions for presupposition projection in quantified environments. For this reason, we will not consider it in this paper.

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One possible way to approach these cases was first spelled out by Heim (1983). According to this proposal, the semantic computation has presuppositions project with universal quantificational force by default. All the sentences above, provided that real quantificational expressions are involved, should then presuppose that all the students used to smoke. We will refer to this hypothesis as the Universal Projection hypothesis, given in (5) for quantifying determiners.

(5) Universal Projection hypothesis (for quantifying determiners):
Given a quantifying determiner $\mathscr{Q}$ and properties A, B_C such that for all x, $B_C(x)$ is defined only if $C(x)$ is true, the formula $\mathscr{Q}(A)(B_C)$ is defined only if for all x such that $A(x)$ is true, $C(x)$ is true.

On top of this default rule for presupposition projection, other mechanisms may apply that lead to weaker presuppositions, potentially addressing the intuitions reported in (4). On the one hand, some indefinite expressions are not really quantificational according to the view expressed by Heim (1983) and much subsequent work. On the other hand, natural language semantics might be equipped with alternative ways of interpreting certain presuppositional expressions, like local accommodation, which we come back to in Section 3. However, these strategies are given as possibilities to derive potential readings that these sentences might have according to some; the actual desiderata for a theory of presupposition projection from quantified expressions remain unclear.

A different approach to the same cases is adopted by Fox (2013), building on work by George (2010). Fox argues that a system based on the trivalent Strong Kleene logic allows for an adequate model of presupposition projection out of quantifiers over individuals, like the cases above. Importantly, Strong Kleene does not exactly predict presupposition projection, but rather truth conditions for a quantified formula, given that all predicates are assumed to be total functions to a set of three truth-values $\{\mathrm{T}, \#, \mathrm{F}\}$.[2] The predictions of Strong Kleene for quantified formulas are given schematically in (6) and (7), and the presupposition of a formula is the disjunction of its truth and falsity conditions (Fox 2013).

(6) Strong Kleene projection out of a universal quantifier:
The truth-value of a formula $\forall x \in \mathscr{D},\ P(x)$ is:

a. T if $P(x) = \mathrm{T}$ for all $x \in \mathscr{D}$;

b. F if there is an $x \in \mathscr{D}$ such that $P(x) = \mathrm{F}$;

2 In this sense, what we here call *projection* is an epiphenomenon of truth-conditional meaning computation, and terms like *entailment* and *presupposition* capture different aspects of the same system. A sentence S presupposes p if both S and *not* S entail p (Hajičová 1984, Partee 1996). Determining presupposition projection amounts to calculating in what situations a sentence evaluates to # given the meaning of that sentence's parts.

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c. # otherwise (if there is an $x \in \mathscr{D}$ such that $P(x) = \#$ and no $x \in \mathscr{D}$ such that $P(x) = \mathrm{F}$).

(7) Strong Kleene projection out of an existential quantifier:
The truth-value of a formula $\exists x \in \mathscr{D},\ P(x)$ is:

a. T if there is an $x \in \mathscr{D}$ such that $P(x) = \mathrm{T}$;

b. F if $P(x) = \mathrm{F}$ for all $x \in \mathscr{D}$;

c. # otherwise (if there is an $x \in \mathscr{D}$ such that $P(x) = \#$ and no $x \in \mathscr{D}$ such that $P(x) = \mathrm{T}$).

According to this view, presupposition projection cannot simply be called existential or universal. Rather, the presupposition of the quantifier's nuclear scope has to be satisfied to different degrees for the entire sentence to be true and for it to be false. In particular, for universal quantification in (6), the predicate needs to be true of all the elements of the domain of quantification for the sentence to be true, but it only needs to be false of a single element for the sentence to be false overall. Because of this asymmetry, a different disjunct of the presupposition is verified depending on whether the sentence is asserted or negated. For a negated sentence, the disjunct with existential quantification associated with the falsity conditions must be true, while a sentence without negation makes the disjunct with universal quantification true. This is in line with the intuitions reported in (3) and (4). The valid inferences in the case of universal quantification are schematized in (8), where A_B is a predicate such that $A_B(x) \neq \#$ only if $B(x)$ is true, similar to the notation in (5).

(8) a. If $\forall x \in \mathscr{D},\ A_B(x)$ is true, it follows that $\forall x \in \mathscr{D},\ B(x)$ is true;

b. If $\neg\forall x \in \mathscr{D},\ A_B(x)$ is true, it only follows that $\exists x \in \mathscr{D},\ B(x)$ is true.

The schemas given in (8) predict that it is logically valid to infer that the presupposition holds universally in (3), under *all of the students*, but not in (4a), under *not all of the students*. However, similar to the approach adopted by Heim (1983), Fox (2013) assumes that mechanisms of presupposition weakening and presupposition strengthening can apply in certain cases, and additional differences can arise from the different nature of some presupposition triggers. Some of these options will be discussed in Section 3.

The Universal Projection hypothesis and Strong Kleene are two theoretical possibilities to model presupposition projection out of quantified environments. Their predictions have been discussed in the literature with respect to quantifiers over individuals, like *all of the students*. However, these predictions have generally not come without qualification, such as the potential availability of additional mechanisms of presupposition weakening and strengthening, and because of the frequent uncertainty about what the empirical desiderata, or good predictions, of a theory of

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presupposition projection are. In the next section, we will move on from quantifiers over individuals and focus on the predictions of these two approaches in the domain of attitude reports.

2.2 What happened to attitudes?

The research on presupposition projection out of quantificational environments has centered on quantifiers over individuals, neglecting modal/intensional operators. A contributing factor to this narrow focus is certainly that the debate about whether and which modal operators should be analyzed as quantifiers is not settled.[3] Furthermore, presupposition projection out of modal environments has been neglected from the empirical point of view, to our knowledge. Seminal work by Heim (1992), grounded in dynamic semantics, postulates that predicates like *think* should presuppose that their complement be defined at every accessible world they consider. This predicts the validity of the inference in (9).

(9) Aditi thinks that Skye stopped smoking.

$\rightsquigarrow$ Aditi thinks that Skye used to smoke.

Throughout this paper, we will assume that at least some attitude predicates can receive adequate treatment with a quantificational analysis along the lines of Hintikka 1969, leaving to future research how to give an account of our findings in other frameworks, if one is to be preferred. If a quantificational analysis of *think* is adopted, such that the verb introduces universal quantification over a set of doxastically accessible worlds, definedness and truth conditions following Heim (1992) can be given as in (10), where $\textsc{dox}_a^w$ is the set of doxastically accessible worlds for an individual a at a world w.

(10) $[\![a \text{ thinks } p]\!]^w$ is defined only if $[\![p]\!]^{w'}$ is defined for all $w' \in \textsc{dox}_a^w$. If defined, $[\![a \text{ thinks } p]\!]^w$ is true if and only if $[\![p]\!]^{w'}$ is true for all $w' \in \textsc{dox}_a^w$.

We will say that a presupposition that projects according to the rule in (10) projects universally out of the attitude predicate. This schema for presupposition projection is entirely parallel to the one given in (5) for quantifiers over individuals, replacing the restrictor of a quantificational determiner with a set of accessible worlds that the attitude verb quantifies over. In Heim 1992, every attitude predicate is assumed to behave in a way similar to (10) regarding presupposition projection,

3 See for example Kratzer 2006, Moulton 2009, 2015 for the idea that some attitude predicates may not be quantificational. Additionally, see Lassiter 2017, Santorio & Romoli 2017, Koev 2019, among others, for the view that some attitude and epistemic predicates should come with a degree argument that expresses a probability.

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the difference between predicates being what set of accessible worlds is quantified over.[4]

A qualification is due at this point. According to the interpretation in (10), *think* does not behave like a hole for presupposition projection, that is, a presupposition in its complement does not become, unaltered, a presupposition of the complex sentence. Other work, like that by Geurts (1999), instead assumes that attitude predicates do behave like holes for presupposition projection on the basis of inferences like (11).

(11) Aditi thinks that Skye stopped smoking.

⇝ Skye used to smoke.

Although we agree that the inference in (11) is a reasonable one in out-of-the-blue contexts, we side with much previous work (Heim 1992, Sudo 2014, Thalmann & Matticchio 2025) in assuming that it is also cancelable, unlike the inference in (9). A difference in the possibility to be canceled between the two inferences is highlighted by the contrast in (12). A report of Aditi's thinking that Skye stopped smoking is felicitous if Aditi believes that Skye used to smoke even if they actually did not. It is not felicitous, instead, if Skye indeed used to smoke but Aditi does not know.

(12) a. Aditi mistakenly believes that Skye used to smoke, and she thinks that they stopped.

b. #Aditi doesn't know that Skye used to smoke, and she thinks that they stopped.

Regardless of the analysis that the inference in (11) may receive, it has to be acknowledged that the inference in (9) is not cancelable and is tied to the presupposition of the sentence via the meaning of the presuppositional lexical item *stop*. Our experiment will only test the uncancelable inference in (9) and leave the truth of the inference in (11) unaddressed. So even though participants may draw inferences like (9) additionally, these will not impact the experimental judgments, as our control conditions will check.

Among those approaches that do not treat attitude predicates as holes for presupposition projection, the claim that projection out of attitudes has the profile in (10) has not been questioned as a desideratum. More recent treatments of presuppositions, like those by Schlenker (2009) and Mandelkern (2024), are still designed to meet the intuition that a presupposition triggered in the scope of an attitude predicate has

4 According to Heim, some predicates may consider different sets of accessible worlds for their definedness and their truth conditions, as in the case of preferential predicates like *want*. Definedness conditions are nonetheless expressed in terms of universal quantification. In this paper, we will focus on a predicate, *be certain*, that does not have a preferential meaning.

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to be satisfied universally with respect to a set of worlds. We would like to compare this hypothesis with the predictions that a modal extension of a Strong Kleene logic makes given a quantificational analysis of attitude predicates.

To do so, we consider the attitude predicate *be certain*, instead of the more prototypical *think* or *believe*. We consider *be certain* to be one of the best candidates to approximate a universal quantifier in the domain of attitude predicates. Just like a universal quantifier, *be certain* validates the equivalence in (13) with conjunction, as illustrated by (14). The equivalence in (13) characterizes universal quantifiers and distinguishes them, for example, from existential quantifiers.

(13) $\forall x,\ P(x) \wedge \forall x,\ Q(x) \iff \forall x,\ (P(x) \wedge Q(x))$

(14) *Equivalently:*

a. Aditi is certain that Skye smokes, and she's certain that they drink.

b. Aditi is certain that Skye both smokes and drinks.

Since an approach based on Strong Kleene places a certain amount of emphasis on a difference between asserted and negated sentences, the behavior of the quantifier under negation plays a critical role. Unlike *think* or *believe*, the predicate *be certain* is not a neg-raiser: when embedded under negation, it does not report that the embedded proposition is false, and instead licenses the indirect scalar implicature that it might be true. As is familiar, the opposite behavior is observed with the neg-raising predicate *think*.[5]

(15) a. Aditi is not certain that Skye smokes.

$\not\rightsquigarrow$ Aditi is certain that Skye doesn't smoke.

$\rightsquigarrow$ Aditi considers it possible that Skye smokes.

b. Aditi doesn't think that Skye smokes.

$\rightsquigarrow$ Aditi thinks that Skye doesn't smoke.

$\not\rightsquigarrow$ Aditi considers it possible that Skye smokes.

A similar indirect scalar implicature is observed with the universal quantifier over individuals *all* under negation (Horn 1972, Gazdar 1979, Chierchia 2004, 2013, among others), exemplified in (16), which mirrors the existential inference we see with *be certain* in (15a). That these inferences are implicatures can be seen from

5 As an additional test for neg-raising (Gajewski 2007), it is possible to observe that strong negative polarity items, like *in weeks*, are not licensed in the complement of *not be certain*, but they are in the complement of *not think*:

(i) a. *Aditi is not certain that Skye has smoked in weeks.

b. Aditi doesn't think that Skye has smoked in weeks.

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their cancelability in contexts like (17).[6] We remain agnostic as to how indirect scalar implicatures are derived.

(16) Skye did not smoke all of the cigarettes.

$\rightsquigarrow$ Skye smoked some of the cigarettes.

(17) a. A: Did Skye smoke all of the cigarettes?
B: They didn't smoke all of the cigarettes; in fact/maybe, they didn't smoke any.

b. A: Is Aditi certain that Skye smokes?
B: She isn't certain that Skye smokes; in fact/maybe, she's certain that they don't.

The predictions of a Strong Kleene logic for a modal predicate that can be analyzed as a universal quantifier are given in (18). We want to stress that our hypothesis is not that these predictions are correct for predicates like *think* and *believe*, because neg-raising might affect the falsity and third-value-ness conditions of these predicates. Instead, they are predictions for presupposition projection out of the complement of *be certain*, which we hope to have shown behaves like a universal quantifier. We illustrate these predictions by giving the truth conditions for the example in (19), which embeds the presupposition trigger *stop*.

(18) Strong Kleene projection out of a universal modal:
The truth-value of a formula $\forall w \in \mathscr{W},\ p(w)$ is:

a. T if $p(w) = \text{T}$ for all $w \in \mathscr{W}$;
b. F if there is a $w \in \mathscr{W}$ such that $p(w) = \text{F}$;
c. # otherwise (if there is a $w \in \mathscr{W}$ such that $p(w) = \#$ and no $w \in \mathscr{W}$ such that $p(w) = \text{F}$).

(19) Aditi is certain that Skye stopped smoking. *(Strong Kleene)*

a. T if Aditi is certain that Skye used to smoke and stopped;
b. F if Aditi considers it possible that Skye used to smoke and did not stop;
c. # if Aditi considers it possible that Skye has never smoked in the past, but she is certain that if Skye used to smoke, they stopped.

6 Our assessment differs from the one by Uegaki (2020: 918), who suggests that this indirect scalar implicature is a presupposition. Yet, presuppositions cannot be canceled in the same way, as illustrated with the example below:

(i) A: Did Skye stop smoking?
B: #They didn't stop smoking; in fact/maybe, they've never smoked.

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The predictions of a Strong Kleene logic for presupposition projection out of the complement of *be certain* differ from the predictions of a Universal Projection hypothesis for the truth-values F and #. In particular, Universal Projection requires that the presupposition of the complement be universally met in the accessible worlds for the attitude report to be false. A weaker, merely existential condition is imposed by Strong Kleene, as stated in (19b). For the sake of completeness, we report in (20) the truth conditions predicted by the Universal Projection hypothesis for the same example. The difference between these two sets of predictions will be the focus of our experiment, presented in Section 4.

(20) Aditi is certain that Skye stopped smoking. *(Universal Projection)*

a. T if Aditi is certain that Skye used to smoke and stopped;

b. F if Aditi is certain that Skye used to smoke and considers it possible that they did not stop;

c. # if Aditi considers it possible that Skye has never smoked in the past.

3 Local accommodation and the soft-hard distinction

We will now discuss certain factors that the previous literature has proposed as drivers of variation in the judgments about the presupposition of quantified sentences. As we have tried to make clear above, one main issue is that there is no agreement about which presuppositions a good theory should predict for the core cases.

As we discussed in the previous section, proposals about presupposition projection out of quantificational environments often come equipped with mechanisms to weaken the predicted presupposition in certain cases. One notable strategy to weaken the presupposition of a sentence is the application of local accommodation, an operation that turns presupposition failures into the classical truth-value F. Local accommodation can be written as an operator that turns trivalent propositions into bivalent ones (see also Beaver & Krahmer 2001), as in (21), but its syntactic reality is not the focus of this paper.

(21) $[\![\mathscr{A}]\!] = \lambda p_{\langle s,t\rangle}.\ \lambda w_s.\ \begin{cases} \mathrm{T} & \text{if } p(w) = \mathrm{T} \\ \mathrm{F} & \text{if } p(w) = \# \text{ or } p(w) = \mathrm{F} \end{cases}$

A factor we need to introduce at this point is the distinction between *soft* and *hard* presupposition triggers. According to the literature (see for example the trivalent account in Fox 2013), local accommodation can apply and weaken the presupposition of certain sentences if they contain a soft presupposition trigger, being instead (mostly) unavailable if the presupposition trigger is hard. This being the case, we now attempt to clarify how it can be established whether a presupposition trigger

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is hard or soft. Although different diagnostics have been proposed to differentiate between these two classes, what we believe is the most widely adopted one uses contexts of explicit ignorance and conditional constructions (Abusch 2002, 2010, Abrusán 2011, Romoli 2015, Abrusán 2016). In a context where the speaker asserts ignorance with respect to its presupposition, a soft trigger embedded in a conditional antecedent does not lead to infelicity, whereas a hard trigger does. We illustrate this contrast in (22) with the soft trigger *stop* and the hard trigger *again*.[7]

(22) a. I don't know whether Skye ever smoked, but if they stopped smoking, Aditi will be surprised.

b. #I don't know whether Skye ever smoked, but if they smoked again, Aditi will be surprised.

If local accommodation is available, a sentence containing a soft presupposition trigger can sometimes receive a classical truth-value even if the presupposition is not satisfied in the context, provided that the presupposition is understood as being locally accommodated. In particular, a sentence can be judged true instead of truth-value-less if local accommodation happens within the scope of negation. The availability of local accommodation and its optionality have been taken to be possible drivers of speakers' variation in the acceptability of sentences containing presupposition triggers (Sudo et al. 2012, Fox 2013). On the assumption that local accommodation is not an obligatory operation (see also Beaver & Krahmer 2001, Schlenker 2008), one might expect variability in the judgments of sentences containing soft triggers in presupposition failure scenarios. That is, if participants make use of local accommodation, but do so inconsistently — allowing for presupposition failure to remain in some cases but not in others — this should lead to larger amounts of variability in acceptability judgments. Hard triggers, on the other hand, would be predicted to show less variability instead because local accommodation is unavailable across the board, and their presuppositions project without ambiguity.

To start investigating how this issue interacts with quantificational environments, we would need a sound classification of which triggers are hard and which are soft. However, the studies summarized in Schwarz 2019: §3.3.1, as well as Göbel & Bui

7 There are other differences between soft and hard triggers that have been experimentally verified. For instance, soft triggers are more felicitous than hard triggers in response to questions that make the presupposition at-issue. Below, we show this with experimental material from Chen, Thalmann & Antomo 2022, with the soft trigger *win* and the hard trigger *again*:

(i) A: Did the duck participate in the competition?
B: The duck won the competition.

(ii) A: Did the panda score a goal for the first time yesterday?
B: #The panda scored a goal again yesterday.

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2026 more recently, report that speakers allow for locally accommodated readings in ways that do not align neatly with the standard classifications of trigger status. Whether local accommodation is available for hard triggers has also been contested on methodological grounds: Siegel & Schwarz (2023) attempt to test this with the hard trigger *also* in explicit-ignorance contexts, as in (22), but their items could make other interpretations available because of how they are constructed.[8] It thus remains unclear whether their results establish what they are taken to establish.

In addition to the different availability of local accommodation for soft and hard triggers, some judgments provided by Charlow (2009) complicate matters further. According to Charlow's intuitions, soft and hard presupposition triggers are associated with different projection patterns altogether when embedded in quantificational environments.[9] These judgments can be illustrated with the examples in (23). The presuppositions of hard triggers, like *also*, are claimed to project universally in all cases, including when the trigger is embedded under an existential quantifier. Instead, this is not necessarily the case if the presupposition trigger is soft, as in the case of *stop*. Charlow states that the absence of universal projection in (23a) cannot be the result of local accommodation if such a sentence is still judged to presuppose something — whereas local accommodation as defined in (21) would predict absence of presuppositions altogether.

(23) *According to Charlow (2009):*

a. Some of these 10 students stopped smoking.
   $\not\rightsquigarrow$ All of these 10 students used to smoke.

b. Some of these 10 students smoked again.
   $\rightsquigarrow$ All of these 10 students smoked in the past.

These judgments, although provided without much empirical support, muddy the waters since they suggest that the hardness of a presupposition trigger can affect projection out of a quantificational environment in a way that is orthogonal to local accommodation: this approach effectively mandates different projection algorithms for the two classes of triggers.

8 Rather than the explicit-ignorance assertion, Siegel & Schwarz (2023) use an epistemic modal embedded under a perception verb as in (i), which may instead license a modal subordination interpretation that obviates the need for local accommodation altogether (cf. Roberts 1989):

(i) This could be wrong, but I heard that Paul might have ice cream; if he also has chocolate syrup, we could have sundaes for dessert.

9 Although Fox (2013) cites Charlow (2009) in support of the idea that some judgment variability may come from the soft-hard presupposition distinction, the predictions of the two approaches remain quite different. For example, Fox does not predict the presupposition to project universally in (23b).

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Given these previous claims and our discussion thereof, we will introduce both soft and hard presupposition triggers in our items to see if differences can be observed due to the possibility of applying local accommodation or to idiosyncrasies in the projection pattern of trigger classes. Despite the substantial tension between theory and empirical evidence, we want to note that the experiment we will present in this paper is not primarily designed to adjudicate between accounts of the soft-hard distinction. Rather, since both a difference in local accommodation availability and a difference in projection profiles from quantificational environments have been suggested to arise between trigger types, we include both a soft and a hard trigger in our materials to rule out the possibility that our results are attributable to trigger type rather than to the mechanisms driving projection in the relevant environment. In other words, trigger type will serve as a kind of safeguard in our design rather than as a primary factor of interest. Although with a different focus, the discussion of the experimental results in Section 4 will nonetheless shed some light on the issue of projection strength.

4 Experiment: Quantifiers over worlds

4.1 Choosing an experimental task

The experiment we designed for our study differs from what previous literature did to investigate presupposition projection from quantifiers. To justify our choice and motivate the need for an experimental investigation along the lines we present below, we will review some previous experimental literature and highlight some limitations that we tried to overcome with our design. We would like to highlight two main points: one is related to the goal of our study, which is to test the predictions of two hypotheses against each other, and the other is about the experimental task that is best suited to do so. We will avoid giving our experiment an exploratory flavor, and we believe that using a truth-value judgment task is the best way to compare the predictions of the Universal Projection and the Strong Kleene hypothesis.

One of the first experimental investigations of presupposition projection out of quantificational environments is due to Chemla (2009). This work, which has a fundamentally exploratory character, focuses on the variability of judgments that may arise if different quantifiers are compared. The clearest result obtained by Chemla is that participants tend to draw universal inferences about the presupposition of sentences where the presupposition trigger is embedded under the negative quantifier *none of these 10 students*. This result is certainly compatible with both the Universal Projection hypothesis and the predictions of Strong Kleene. For our study, we would like to move beyond exploration and find cases that can discriminate between the two approaches.

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Another important feature of the experimental investigation by Chemla (2009) is the use of an inference task, namely a task where participants are presented with a sentence and asked to judge the validity of drawing a certain inference from that sentence. Chemla spends some words to justify treating presuppositions as inferences and investigating them through the observation of the inferential process associated with a sentence that carries a presupposition. This process is also known as global accommodation (Lewis 1979; see also von Fintel 2008, among others). However, more recent literature has started converging on the view that for a given sentence, global accommodation does not always return an inference identical to its presupposition (Fox 2013, Mandelkern & Rothschild 2018, Thalmann & Matticchio 2025), and the process of inferring a context can be affected by other pragmatic and extra-linguistic factors that can lead a speaker to accommodate the most likely context, as opposed to the minimal amount of information necessary to accept a sentence as felicitous.

Most notably, the discussion by Chemla (2009) about his control items on scalar implicatures points in the same direction. Consider the stimuli in (24a), which only differ with respect to the choice of the determiner — *none* or *most*. Neither of these sentences has (24b) as a valid inference. Nonetheless, participants more easily accepted (24b) after being presented with the *most* version of (24a) than its *none* version. This can result from participants finding the context described by (24b) as more likely or plausible if they know that most of the students failed all of their exams. This behavior shows that factors that do not belong to core semantics can contribute to a participant's drawing an inference, and it is hard or impossible to make participants only draw inferences related to the linguistic domain — not to mention the hypothesis — that we are investigating.

(24) a. *Stimuli:*
   i. Among these 20 students, none failed all of their exams.
   ii. Among these 20 students, most failed all of their exams.
   b. *Inference to be judged:*
   Each of these 20 students failed some of their exams.

In addition to differences between quantifiers, variation in the judgments has been claimed to come from the speakers that provide them. In an experiment investigating interspeaker variation in the distribution of universal projection, Sudo et al. (2012) tested three quantified environments embedding the presupposition trigger *both*. The critical items used in their study are presented in (25).

(25) a. Some of these three triangles have the same color as both of the circles in their own cell.

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b. None of these three circles have the same color as both of the squares in their own cell.

c. Do any of these three squares have the same color as both of the triangles in their own cell?

As is typical in the covered box paradigm, participants were presented with a visible and a covered picture for each critical item and were asked to choose the one the sentence referred to. In each of the critical cases, the visible image failed to satisfy the hypothetical universal presupposition of the sentence. Under the logic of the covered box paradigm, this setup is designed to differentiate participants who derive a universal inference (expected to choose the covered image) from those who do not (expected to choose the visible one).

Sudo et al. (2012) find that participants' choices, while not uniform, give rise to an implicational hierarchy about the different embedding quantifiers. A participant who chooses the covered box for *some* also has a universal presupposition with the negative existential quantifier *none* and the existential polar question. If a participant instead does not derive a universal inference for *some*, they may still derive one for other environments.[10]

While the study aims at capturing speaker-level variability, it faces some limitations in its design. Most notably, only a single item was used for each quantifier condition. This makes it difficult to evaluate the reliability of participant-level inferences, as item-level variation is neither estimable nor controlled for. That is, without sufficiently many items per condition, measurement error cannot be estimated or mitigated. This concern is further underscored by the fact that only 32% of the participants responded correctly to the set of unambiguous filler items. However, even granting reliability, the limited number of critical items complicates the generalization of the findings beyond the specific linguistic material tested (cf. Clark 1973).

Another and more recent experimental investigation was pursued by Wang & Buccola (2025). The critical material in their experiment consists of sentences containing presupposition triggers in the scope of three different quantifiers, all presented at the same time together with a single common context. Their context manipulated the extent to which the presuppositional requirement of the sentence was met, considering uncontroversial presupposition failure, existential, and universal satisfaction.

Participants were tasked with judging the acceptability of Mandarin Chinese context-sentence pairs on a Likert scale labeled at the endpoints to indicate natural-

10 Note that the result that some people did not derive a universal presupposition for (25a) is in conflict with the intuitions presented by Charlow (2009), on the assumption that *both* is a hard presupposition trigger.

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ness. Assuming that the tested sentences were all syntactically acceptable, we would in principle want to distinguish sentences by their truth-value.

In this design, a classically true sentence is expected to fall at the very top of the naturalness scale, but with false and undefined items it is unclear whether we expect them to occupy similar or distinct regions on the scale. If there is an expectation of difference, there is no straightforward way to determine which should be more natural than the other. Consider for example one of the sentences with universal quantification and the context provided in (26): the Universal Projection and the Strong Kleene hypothesis do not make different predictions about whether this sentence is true in the context, which it is not, but rather about whether the sentence is false or undefined. In a design where false and undefined sentences are not clearly distinguished, evaluating the diverging predictions of the two approaches is not straightforwardly possible.

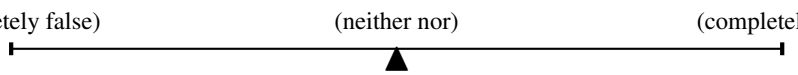
(26) CONTEXT: *One boy was late without regretting it, and the other two were not late.*
Each of the three boys regretted being late.

This distinction is instead what our experimental design will focus on and test, in the realm of attitude reports, where we pursued a targeted design that directly pits specific competing hypotheses against each other. A truth-value judgment task that allows for the expression of intuitions of truth, falsity, and a third category in between is best suited to assess this distinction, together with the introduction of negation as a manipulation, as we detail below. This choice allows us to directly contrast the Universal Projection and the Strong Kleene hypothesis rather than leaving theory comparison to a post hoc interpretation of exploratory results.

Additionally, we do not design an experiment to look for interspeaker variation, as we believe this goal is premature and presently unjustified, and we instead try to see if a stronger result can be obtained, one where repeated measures over items and over participants can reveal a pattern that conforms to either the Universal Projection or the Strong Kleene hypothesis.

Since the differences between the two accounts amount to differences in truth-value, we used a continuous slider, shown in (27), to express truth-value intuitions with fine-grained distinctions. For a version of this task without a slider, and in particular the labels we chose, see Križ & Chemla 2015, which established that this method allows for the detection of truth-value gaps. Thalmann (2025) used the same experimental paradigm with a continuous slider and found a replication of the results about homogeneity violations for all the conditions that his experiment shared with Križ & Chemla's, which suggests that the modifications of the method did not affect its original sensitivity to truth-value gaps.

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(27) 

4.2 Squeamishness and the Variance hypothesis

The task just motivated also affords a second, methodological opportunity, which constitutes the secondary contribution of this paper: alongside the truth-value judgments themselves, the way those judgments are distributed can be used as an independent diagnostic of presupposition failure. We develop this point here before turning to the design. It is generally assumed that presuppositions are conditions that a context must meet for an utterance to be felicitous. What we neglected to consider in the discussion so far is how interlocutors and experimental participants react in response to encountering utterances that are asserted in a context that fails to meet its presuppositional requirements. While one option would be to assume that presupposition failure triggers a similar reaction to encountering an ungrammatical sentence, such as strong and immediate intuitions of unacceptability, Strawson (1964) considers a different option. For him, an interlocutor confronted with presupposition failure experiences what he calls squeamishness, a brief moment of hesitation and uncertainty as to how the ongoing conversation is to proceed. In this sense, true and false sentences, which do not elicit squeamishness, should be easier to judge than undefined ones.[11]

In an exploration of whether squeamishness might be detectable experimentally, Thalmann (2025) finds support for what he calls the Variance hypothesis, the assumption that — at least in truth-value experiments — Strawsonian squeamishness finds its expression in standard deviation effects.[12] In his experiments on the pre-

11 Being a total logic, Strong Kleene is not itself equipped with a way to make squeamishness the consequence of encountering truth-value gaps, which formally do not exist. This is different from binary logics where presupposition failure results in semantic uninterpretability, which may be identified as the trigger for squeamishness. For Strong Kleene, by contrast, Thalmann (2025) argues that the independently needed pragmatic principle called Stalnaker's Bridge could be held responsible for squeamishness. Since Stalnaker's Bridge denies the status of being assertable to utterances that assign # — rather than T or F — to some contextual possibilities, squeamishness in this logic may be considered the result of encountering an unassertable utterance. The source of squeamishness in a Strong-Kleene-based approach would be pragmatic rather than semantic.

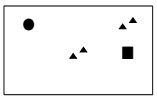
(i) Stalnaker's Bridge (Fox 2013, von Fintel 2008)
A declarative sentence S is assertable given a context set C only if for all $w \in C$, $[\![S]\!](w) \neq \#$.

12 Thalmann (2025) only presents evidence for such effects with the slider-based truth-value judgments we will employ here. To our knowledge, it is an open question whether such effects are detectable with other experimental designs like the more commonplace acceptability judgments. We merely note that Sun, Schmidt & Henry (2025) suggest that only scales with a sufficiently large number of levels may lead to detectable effects in terms of spread. In other words, an acceptability scale from 1 to 7 may be too coarse to investigate the Variance hypothesis.

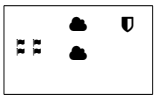
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suppositional import of the definite determiner *the*, where participants were tasked with giving truth-value judgments, he finds that conditions exhibiting presupposition failure come with judgment distributions that are more spread out than conditions that participants judge as true or false. In other words, participants unconsciously responded to truth-value gaps with more erratic, less clustered response behavior, which was detectable statistically as inflated standard deviation scores. Finding squeamish rating behavior for the gappy conditions in his experiment, Thalmann argues that inflated indices of dispersion may be used to detect intuitions of presupposition failure in addition to the standard truth-value judgments. Below, we illustrate two sample conditions that lead to squeamish responses in his second experiment: first, a violation of the so-called homogeneity inference associated with definite plurals in (28a), and second, a violation of the uniqueness presupposition for singular definites in (28b).

(28) a. Die Dreiecke stehen links neben dem Quadrat.
the triangles stand left next.to the square
'The triangles stand left of the square.'



b. Die Wolke steht links neben dem Schild.
the cloud stands left next.to the shield
'The cloud stands left of the shield.'



With the focus of our experiment on the truth-value of attitude reports and the projective behavior of presupposition triggers in their scope, we will attempt a replication of Thalmann's findings for presupposition projection from attitudes. That is, we will test whether the impact that presupposition failure had on participants' response behavior generalizes beyond the empirical domain under investigation in Thalmann 2025.

It is worth clarifying what would and would not count as evidence against the Variance hypothesis. The most direct refutation would be a presupposition failure condition with low variance, where participants converge on a stable judgment despite the presuppositional requirements being unmet. This should be distinguished from cases where local accommodation removes the failure: if participants locally accommodate a presupposition and subsequently give a confident judgment, the failure is no longer present, and squeamishness is not expected. More subtly, the shape of the rating distribution matters. Squeamishness, as Strawson (1964) conceives of it, reflects uncertainty about how to proceed, which should manifest as a relatively uniform spread of responses rather than a bimodal distribution that is indicative of a systematic disagreement between two groups of individually confident participants. A variance inflation driven by such disagreements would be a different phenomenon

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entirely, and the slider paradigm we employ is well suited to distinguishing between these cases. Finally, variance hikes in a presupposition failure condition without a corresponding effect on mean truth-value judgments would undermine the diagnostic value of the measure, at least as far as our experimental method is concerned, since the two are intended to converge on the same underlying phenomenon. We do not, however, take the Variance hypothesis to entail that presupposition failure is the only possible source of variance inflation. The more modest claim is that truth-value indeterminacy reliably produces it, of which presupposition failure is one instance. At the same time, we will be able to use the Variance hypothesis to test whether the predictions of the two hypotheses are in line with speakers' intuitions.

4.3 Design and materials

To compare the Universal Projection and the Strong Kleene hypotheses, we embedded presupposition triggers under *be certain* and manipulated the context. This manipulation was carried out both verbally and visually to make evaluating attitude reports less demanding. We additionally manipulated the presence of negation in the embedding clause: given that negation is a hole for presuppositions, (1), the judgment of a sentence suffering from presupposition failure should be unaffected by negation. We used *be certain* rather than the more prototypical *think* because *think* is a neg-raiser (Section 2.2) and might independently introduce a truth-value gap. Finally, we included the soft trigger *stop* and hard trigger *again* to assess whether speakers differentiate between trigger types when it comes to projection (cf. Charlow 2009, Fox 2013).[13]

The experiment was set up as in (29) and carried out in German via PCIbex (Zehr & Schwarz 2022).[14] Altogether, the experiment included 48 experimental items: 24 with *again* and 24 with *stop*.

(29) 2×4 design; 48 items

a. NEGATION: without vs. with matrix negation (within items and participants)

b. SCENARIO: `true` vs. `false` vs. `undefined` vs. `critical` (within items and participants)

c. Presupposition TRIGGER as a between-item factor: *again* vs. *stop* (between items, 24 each)

Participants were asked to judge the truth of an attitude report in a context (the SCENARIO manipulation) using the slider in (27). For the between-item factor

13 The experimental stimuli, the results, and the analysis code can be found at https://github.com/mkthalmann/supplementary-materials/tree/master/kleeneing-up/.

14 A demo version of the experiment can be found at https://farm.pcibex.net/r/paIsAk/.

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TRIGGER, each presupposition trigger had two lexical realizations: *aufhören* and *aufgeben* for 'stop', and *wieder* and *nochmal* for 'again'. Each experimental trial consisted of a SCENARIO portion followed by a critical utterance, which for each item only varied according to the NEGATION and TRIGGER manipulations.

In each item, a character expresses their doxastic state about a past and a present situation regarding another character via direct quotation. In an image presented alongside that quotation designed to redundantly encode that same doxastic state, past and present situations are represented by emojis that were connected by an arrow. The partitioned cartoon thought bubble containing different emoji states represents what the character considers possible.[15] In (30), we list a sample item (translated from German) with *again*; the images corresponding to the different scenarios are shown in Figure 1. In (31), we show an item with *stop*, the accompanying images being shown in Figure 2.[16] The German versions of these two sample items are given in (41) and (42) in Appendix A. Participants were made aware that the text and the picture conveyed the same information about the context.

(30) Peter is (not) certain that Jan canoed again.

`true` Peter: "I'm certain that Jan canoed last time, and I'm certain that Jan canoed this time."

`false` Peter: "I'm certain that Jan canoed last time, but I have no idea if this time, Jan canoed or not."

`undefined` Peter: "I have no idea if last time, Jan canoed or not, but I am certain that Jan canoed this time."

`critical` Peter: "I have no idea if last time, Jan canoed or not, and I have no idea if this time, Jan canoed or not."

(31) Markus is (not) certain that Sonja stopped drinking wine.

`true` Markus: "I'm certain that Sonja drank wine in the past, and I'm certain that Sonja doesn't drink wine now."

`false` Markus: "I'm certain that Sonja drank wine in the past, but I have no idea if Sonja drinks wine now."

`undefined` Markus: "I have no idea if Sonja drank wine in the past, but I am certain that Sonja doesn't drink wine now."

`critical` Markus: "I have no idea if Sonja drank wine in the past, and I have no idea if Sonja drinks wine now."

15 See Pearson & Dery 2013 for the use of cartoon speech/thought bubbles in their experimental investigation of dream reports.

16 The `true` and `false` scenarios also control for the inference compatible with treating *be certain* like a hole, (11): if participants judge the `true` scenario as true and the `false` scenario as false, the potential additional inference should be independent of the experimental judgments.

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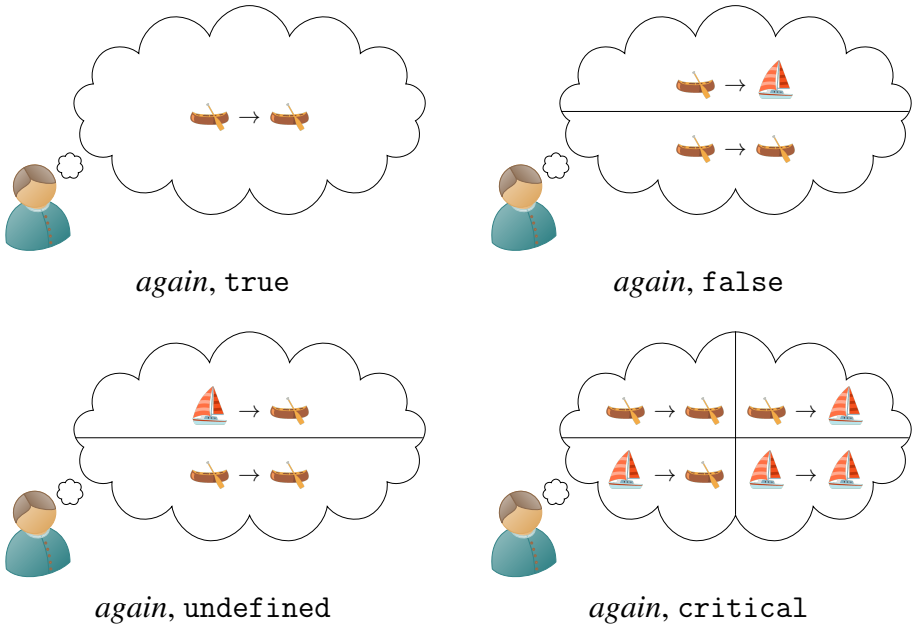


Figure 1 Visual scenarios for "Peter is (not) certain that Jan canoed again".

Due to the lexical differences between the two kinds of presupposition trigger, the visual material is slightly different for the two. For the *stop* items, the two potential components were a symbol and its crossed-out version, the latter indicating that the situation did not obtain. With the *again* items, we either had a symbol that represented the action mentioned in the item or a different, unrelated symbol. This latter symbol was used to indicate that an alternative to the mentioned action was carried out instead of the mentioned action. Since the controls were rated as expected for both *stop* and *again* items, as we will show, we do not believe that this difference between the two triggers in the pictorial representation of the context had any adverse effects.

As is well known, participants tend to dislike contexts in which an indirect scalar implicature is violated (see Chierchia, Fox & Spector 2012; for experimental evidence, see Bill et al. 2016, Thalmann & Panizza 2019). Since the negated experimental items all trigger one such implicature, which amounts to a possibility statement along the lines discussed in Section 2.2, we made sure that this implicature is satisfied in all of our scenarios. Consider for example the item in (30) and the pictures in Figure 1: in each of the SCENARIO manipulations, Peter considers it possible that Jan canoed last time and this time. This way, the ratings in the condition with matrix negation are not affected by the presence of this additional inference.

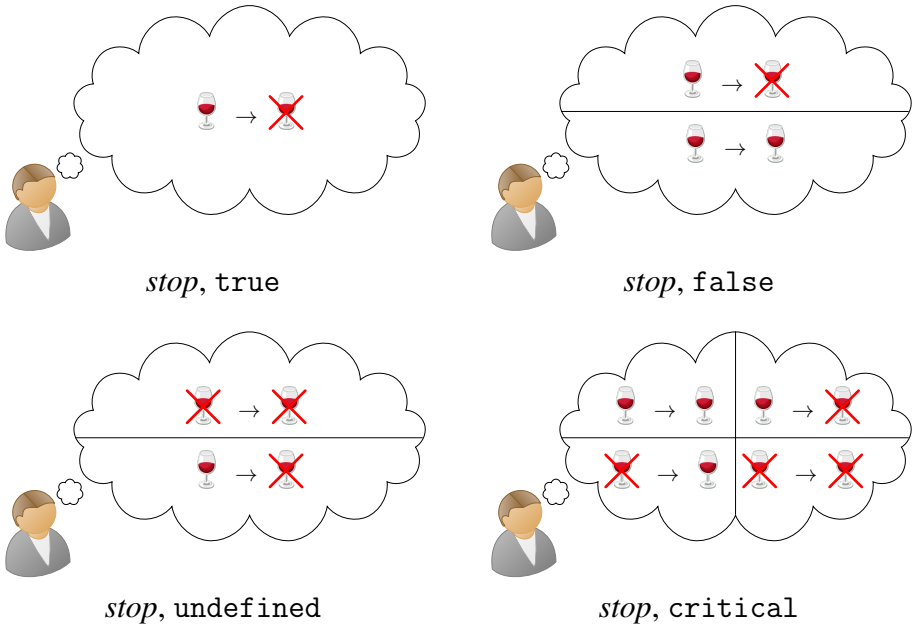


Figure 2 Visual scenarios for "Markus is (not) certain that Sonja stopped drinking wine".

4.4 Participants and procedure

We tested 34 participants (mean age 23.7 ± 6.2, 27 self-identified as female). All participants reported being native speakers of German. Participants received seven euros in exchange for their participation in the experiment, which lasted around 30 minutes.

At the beginning of the experiment, participants were informed that they would be seeing utterances by a speaker and then what was supposed to be a summary of that utterance. The experimental task consisted of evaluating the extent to which the report was true or not using the scale in (27). Before the beginning of the trials, participants were shown the characters whose beliefs would be summarized and whose faces would be included in the visual materials, reported in Figure 3.

In the next step, participants were familiarized with the meaning of the arrows and the clouds in the visual material with the help of seven warm-up trials, which included instructions on how the visual context corresponded to the verbal context. Just like with the experimental trials, the warm-up items showed the image at the top, above the verbal context, which in turn was displayed above the bold-faced critical item, whose truth-value was to be judged using the scale at the bottom of the screen. Of the warm-up items, none involved presupposition violations, and none

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included the presupposition triggers participants would see in the actual experiment. An example of a warm-up item is given in (32), translated from German.

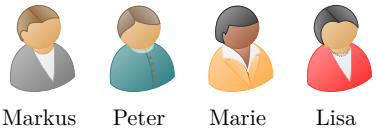


Figure 3 Attitude holder illustration from the instruction portion of the experiment.

(32) Susanne: "I'm certain that Markus didn't do archery in the past, and I'm certain that Markus does archery nowadays."
Susanne is certain that Markus started doing archery.

Upon completing the warm-up trials, participants moved on to the main part of the experiment, where they were shown the critical items in random order. Each trial also included a repetition of the experimental task at the top of the screen, phrased as in (33). Figure 4 shows what participants saw with the item in (31) in the `critical` scenario in the original German. After half of the experimental trials, participants were given the option for a break.

(33) How do you evaluate the bolded statement at the bottom as a report of [attitude holder]'s thoughts?

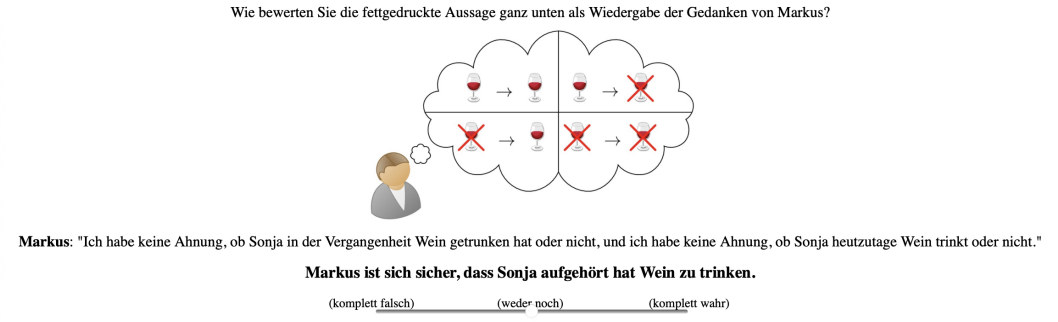


Figure 4 Screenshot of the experimental procedure.

Because we wanted to control for the possibility that participants would rate conditions differently depending on whether they relied on the verbal context or the

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visual material, we included a short question at the end of the experiment asking participants to indicate whether they had relied on the verbal context, the visual material, or both when making their judgments. As a response, 24 said they had relied on the verbal material, two on the visual context, and eight on both. The results were not affected by this factor, so we do not include it in the analysis report.

4.5 Hypotheses

The `true` and `false` scenarios served as control conditions. The `undefined` scenario similarly served as a control, but for an uncontroversially gappy judgment. Contrary to the `true` and `false` scenarios, the `undefined` scenario was expected not to be affected by the presence of matrix negation, leading to a scale-medial overall judgment in both cases.

In the `critical` condition, the doxastic state of the character allows the possibility that the presupposition is not met, and also that the presupposition is met and the embedded proposition is false. If presuppositions project universally out of attitude predicates, as in Heim 1992, the possibility that the presupposition is not met should make the sentence undefined. If, on the other hand, truth-values are calculated via Strong Kleene as in (19), the possibility that the embedded proposition is false should make the sentence false, and its negation should be true. Additionally, if local accommodation is available in this context with soft triggers, as hypothesized by Fox (2013), the results for *stop*, but not for *again*, should display a larger tendency to depart from judgments of intermediate truth, even in the `undefined` scenario. These predictions are summarized in Figure 5.

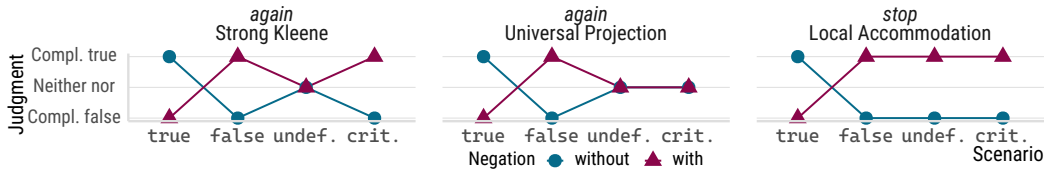


Figure 5 Predictions for the two approaches and the possibility for interpretations with local accommodation for *stop*.

To the extent that his intuitions generalize to quantifiers over worlds, Charlow (2009) predicts that hard triggers should project universally out of attitude contexts, while soft triggers should not (see again (23)). If this is the case, we expect that the *again* items should be judged as undefined in the `critical` scenario, as expected under the Universal Projection hypothesis, while the *stop* items, without negation,

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should be judged false in the same scenarios, as in the Strong Kleene or the Local Accommodation hypothesis in Figure 5. Hence, each of the two triggers should display its own projection profile.

Lastly, the Variance hypothesis from Thalmann 2025 predicts that conditions judged as involving presupposition failure should elicit larger standard deviations than true or false conditions because of squeamishness (Strawson 1964). For our case, we expect that presupposition failure conditions — those where negation does not impact mean judgments — should come with larger standard deviations compared to classically true or false conditions. This hypothesis requires us to consider not just average ratings but also their spread — a pressure that shapes the data analysis below.

4.6 Data analysis

One goal of this experiment is to test the Variance hypothesis, which makes a prediction about the ratings' spread: presupposition failure should produce more scattered ratings. The statistical models standardly used in experimental semantics cannot test this prediction because they assume homoscedasticity — meaning that every condition should have roughly the same amount of spread around its mean. That assumption is exactly what the Variance hypothesis denies, so we cannot adhere to it here. We therefore use a distributional model, that is, a model that estimates, for each condition, not only an average rating (the location) but also a separate amount of spread (the scale, i.e., the standard deviation). This lets the spread itself become a result we can test, rather than an assumption baked into the analysis. A useful feature of these models is that the location and scale parts are independent: they can be given different predictors.

With this in mind, we fit a Bayesian distributional linear mixed model using the `brm` function from the `brms` package (Bürkner 2021) in R 4.5.1 (R Core Team 2026).[17] The dependent variable ranged between 1 and 100 but was scaled to range between -2 (completely false) and 2 (completely true) prior to the analysis to aid interpretability. The model formula in (34) has two parts, corresponding to the two questions we introduced for the data analysis: one part predicting where ratings fall (location, the means) and one part predicting how spread out they are (scale, the standard deviations). Readers not concerned with the implementation details can safely skip to the results in Section 4.7; the essential point is that means and spreads are modeled separately.

17 The model had 8 chains with 10,000 iterations each, of which 5,000 were warm-up samples that do not represent actual posterior samples.

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(34) a. $Y \sim \text{TRIGGER} * \text{SCENARIO} * \text{NEGATION}$ *(location)*
$+ (1 + \text{SCENARIO} * \text{NEGATION} * \text{TRIGGER} \mid \text{PARTICIPANT})$
$+ (1 + \text{SCENARIO} * \text{NEGATION} \mid \text{ITEM})$

b. $\log \sigma \sim \text{NEGATION} * \text{SCENARIO} + \text{TRIGGER}$ *(scale)*

For the location estimates, we included all manipulations from (29) as well as their interactions as fixed effects. The random effects structure was maximal in the sense of Barr et al. (2013).[18] As for modeling the standard deviation portion of the model in (34b), we went with a slightly reduced set of coefficients involving no random effects structure to mitigate fit issues. As (34b) shows, standard deviations are modeled on the log scale owing to the fact that it is not possible for a condition to display negative dispersion. All predictors were sum-coded.

Bayesian modeling requires the setting of priors for the estimated parameters. For our case, the priors (except those for random effects, which were not modified from the default) were $N(0,x)$, with x ranging between .25 (for the log standard deviation) and 1 (for the location slopes). For the intercept and the slopes in the location submodel, we also added lower and upper bounds to the priors to reflect the coding of the bounded dependent variable at -2 (*completely false*) and 2 (*completely true*).

In addition to the model predictions, we will use a Bayes Factor analysis for Regions of Practical Equivalence (ROPE) (for an introduction, see Kruschke 2018) for hypothesis testing. A ROPE is a user-defined interval around a null value (e.g., zero) that represents effect sizes too small to be considered meaningful in practice, for instance, as we do here, declaring that any difference that falls within the interval of 0 ± 0.1 standard deviations is negligible. ROPEs thus allow us to treat certain small differences as practically zero, which avoids the difficulty of having to interpret statistically reliable but practically meaningless effects. Recall that, contrary to Null Hypothesis Significance Testing frameworks, in Bayesian statistics tests can yield positive evidence for equivalence (Gelman et al. 2013, Kruschke 2015). In the analysis we pursue here, support for an equivalence hypothesis then amounts to an effect that falls inside the ROPE.

Rather than compare models with or without a certain parameter, here we generalize this procedure to test for contrasts between parameter settings; for example, comparing the ratings for the `undefined` scenario with and without negation. A (log) Bayes Factor of 0 indicates that the data provide no preference between the competing hypotheses (viz., that two conditions lead to different and that they lead to equivalent ratings), meaning the posterior odds are equal to the prior odds. A positive (log) Bayes Factor indicates evidence in favor of the alternative hypothesis

18 This means that the model allowed every participant and every item to have their own version of each effect.

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(i.e., a difference where the effect in question falls outside the ROPE), whereas a negative value indicates evidence in favor of the null or equivalence hypothesis. For example, a log Bayes Factor of 2 would indicate that data are $e^2 = 7.39$ times as likely under the alternative hypothesis.

We use weakly informative priors centered on the null, following standard recommendations for conservative Bayesian modeling (e.g., Gelman et al. 2013: chapter 2). These priors express skepticism about effects and require the data to provide evidence against the null. A consequence of this choice, however, is that Bayes Factors are somewhat biased toward the equivalence hypothesis: a null-centered prior is already well-positioned to predict null-like data, so the equivalence model receives a predictive advantage that is partly independent of the data. We therefore do not rely on Bayes Factors alone, and also examine posterior marginal effects. These are model-adjusted condition means of the relevant parameters which can be interpreted for both location and scale estimates in our distributional model. Since these marginal means are not point estimates but full posterior distributions, comparing them involves assessing overlaps to judge whether the observed difference is meaningful in theoretical terms. To facilitate these decisions, we employ two types of credibility intervals, detailed below. Fortunately, our results are reasonably clear, so these decisions are not ambiguous.

The Bayes Factors were computed using the `bayestestR` package (Makowski, Ben-Shachar & Lüdecke 2019); for the posterior marginal effects we used the `emmeans` package (Lenth 2024).

4.7 Results

We excluded 4.35% of the responses due to the submission times. First, all observations where submission times exceeded 60 s were excluded, and then all observations whose submission times exceeded the new mean ± 3 SD.

The descriptive results are shown in Figure 6, where the raw data are plotted. Note in particular the almost uniform distribution of the ratings along the scale in the `undefined` scenarios. Compared to the other conditions, this confirms the need for a model that does not assume homoscedasticity (equal spread across conditions), thus justifying the distributional model we use here.

Because our model is Bayesian, every estimate it produces is a distribution of plausible values rather than a single point. In the figures that follow, we summarize each such distribution with both a curve and a bar under it. A Highest Density Interval (HDI) is drawn as the blue portion of the shaded area under the curve: it contains the most probable values — 95% of the highest density mass. On the other hand, an Equal-Tailed Interval (ETI) is drawn as a horizontal bar of varying thickness, indicating the central 50, 90, and 95% of the same distribution. When a

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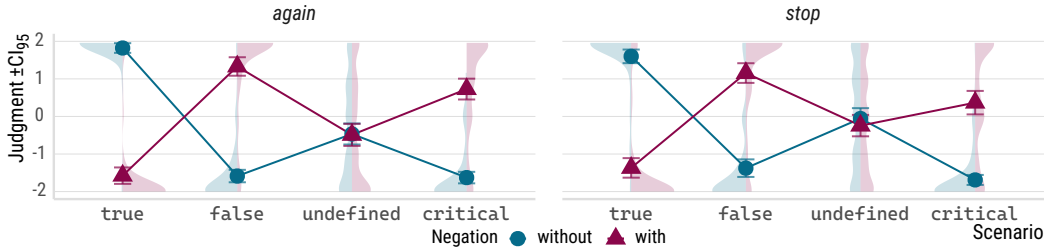


Figure 6 Results for the experiment. The shaded areas indicate the raw rating distributions; the (standard) confidence intervals are calculated on the whole data set and involve no by-participant or by-item clustering.

condition's intervals sit far away from another condition's intervals, the data clearly support the presence of an effect or a difference. The reader can therefore judge most of our claims by eye, simply by asking whether the relevant intervals overlap.

The coefficients for this model, as well as its summary, are shown in Figures 11 and 12 and Table 1 in Appendix B. Rather than interpret the model parameters directly, we will discuss the posterior marginal means, that is, the model's population-level estimate for each condition.

In Figure 7, we see the posterior marginal estimates for the location portion of the model: each curve corresponds to the truth-value ratings in a specific condition. The `true` and `false` conditions for both triggers show the expected judgments — toward the edges of the scale — and inverting effects of negation. For the `undefined` controls, we find scale-medial judgments and, as predicted for a truth-value gap, no effect of negation. By contrast, the `critical` scenario is affected by the negation manipulation, which elicits a change in truth-value judgments that is comparable to the `false` scenarios. The TRIGGER manipulation had little effect, with all contrasts we just described being present for both *again* and *stop*.

In Figure 8, we show the outcome of the Bayes Factors for the ROPEs (Regions of Practical Equivalence; see Section 4.6). As we stated above, a negative Bayes Factor indicates that we have evidence that the compared conditions have (practically) equivalent estimates. In other words, the detected difference falls inside the ROPE. Importantly, the analysis indicates that NEGATION has an effect for the `true`, `false`, and `critical` scenarios, but not for `undefined`. While the `critical` scenario aligns with the `false` one when no negation is present, the ROPEs do indicate a contrast between the two scenarios with matrix negation.

Turning from the location estimates to the scale parameters, we see in Figure 9 that the `true` and `false` scenarios show small standard deviations, which increase

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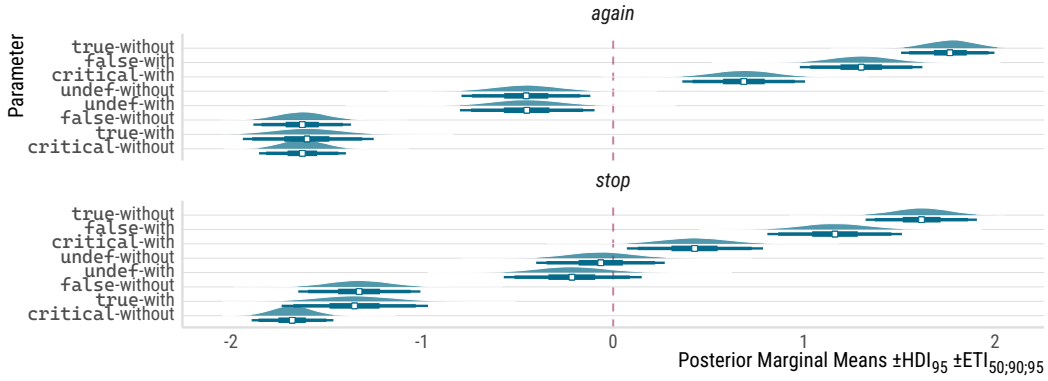


Figure 7 Posterior marginal means for the location parameters. Highest Density Intervals (HDIs) show as distributions the most probable values of a condition, containing a set percentage of the posterior with highest density. Equal-Tailed Intervals (ETIs) are the horizontal bars and show the central mass of the posterior with equal probability in each tail.

with negation. The `undefined` scenario, by contrast, shows large standard deviations regardless of the presence of matrix negation in the critical items. The `critical` scenario patterns with `true` and `false` without negation, but dispersion increases when negation is present. Figure 10 shows the Bayes Factors for the ROPEs for the scale parameters. These substantiate the results from the posterior marginal estimates.[19]

4.8 General discussion

Methodological validation. In our experiment, we tested projection patterns for soft and hard presupposition triggers from the attitude predicate *be certain*,

19 An anonymous reviewer wonders whether the larger standard deviations might be a mere consequence of the scale-medial position of the neither-nor category. This would predict elevated spread across the board, but we do not observe this. For a bounded scale, the maximum achievable standard deviation given a mean μ is $\sigma_{\max} = \sqrt{(\mu - \texttt{lower_bound})(\texttt{upper_bound} - \mu)}$ (Bhatia & Davis 2000). The `false` scenario without negation has a mean near the scale endpoint ($\hat{\mu} \approx -1.5$), giving it a theoretical ceiling of $\sigma_{\max} \approx 1.3$ — sufficient to accommodate the largest standard deviation we observe anywhere in the data (≈ 1.25, in the `undefined` conditions) — yet its observed standard deviation is only approximately 0.5. The elevated spread we observe for presupposition failure conditions is therefore specific to these conditions and cannot be dismissed as an artifact of scale construction; we attribute it to the squeamishness in participants' intuitions under presupposition failure.

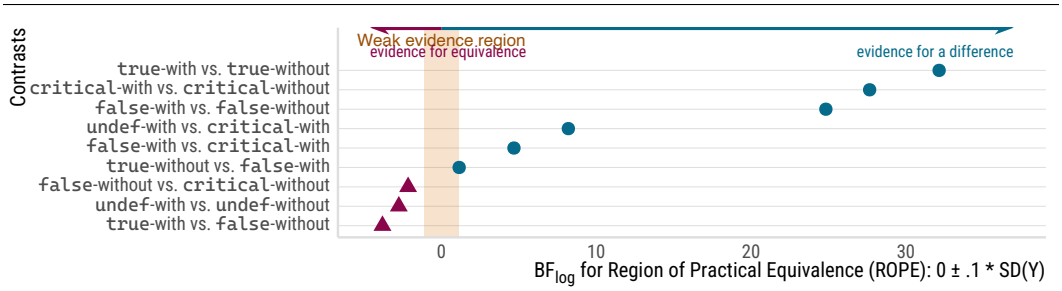


Figure 8 Bayes Factors for the location comparisons. Red triangles indicate that the posterior distribution of the difference between the two conditions is contained in the ROPE, that is, that the data do not provide evidence for a difference. Blue circles indicate that the posterior distribution of the difference between the two conditions is outside the ROPE, that is, that the data provide evidence for a difference.

manipulating both the context via belief states and matrix negation. The main goal was to determine what the experimentally founded desiderata for a logic of presupposition projection from attitude predicates are. To achieve this, we employed a continuous truth-value judgment task using the truth-value labels suggested by Križ & Chemla (2015), as first done by Thalmann (2025).

As a prerequisite for the following discussion, we need to establish that the experiment was successful at detecting truth-values in attitude reports, which are commonly regarded as a complicated domain for experiments, and which have so far not been investigated experimentally from this perspective. For the present case, all of our six control conditions, the `true`, `false`, and `undefined` scenario for both levels of NEGATION, show the expected results. Where both accounts predict truth/falsity, conditions are judged at the extremes of the continuous truth-value scale and show the predicted judgment inversion when the items are negated. The presupposition failure scenario, by contrast, elicited scale-medial condition means and the absence of an effect from matrix negation; this is expected from an operator that behaves like a hole for presuppositions, as defined in (1). From this, we conclude that our continuous truth-value judgment task with doubly encoded context manipulations is well suited for the investigation of presuppositions with attitude reports. With the paradigm thus validated, we now turn to the theoretical question that motivated the experiment, the projection profile of presuppositions embedded under *be certain*, before returning at the end of this section to the methodological payoff for the Variance hypothesis.

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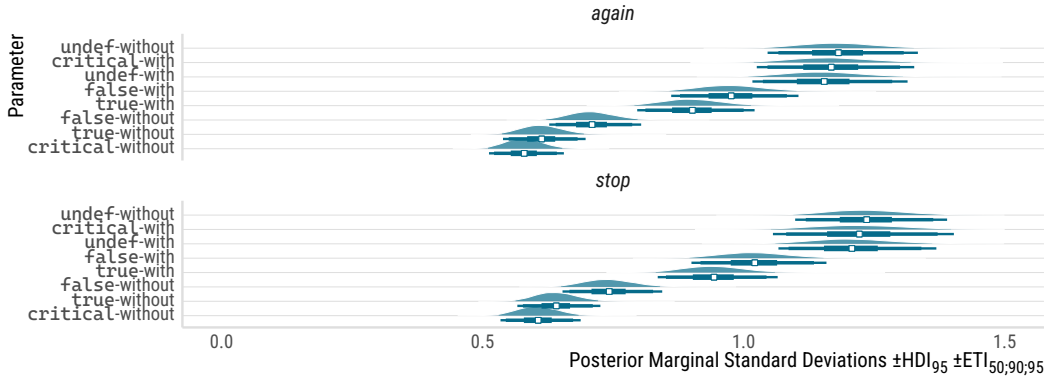


Figure 9 Posterior marginal means for the back-transformed scale parameters.

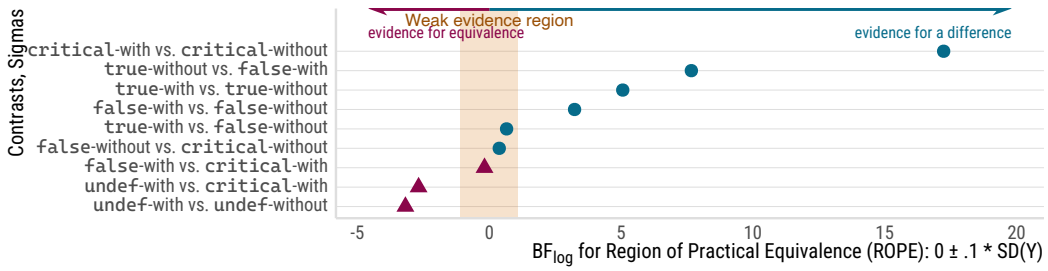


Figure 10 Bayes Factors for the scale comparisons.

The critical scenario. The `critical` condition was included in our experiment to tease apart the predictions of two approaches to presupposition projection from quantified environments. The standard assumption is that presuppositions project universally from attitude predicates (Heim 1992). By contrast, Strong Kleene logic predicts a kind of asymmetric projection pattern for universal quantifiers. For a sentence to be true, the presupposition must be met for all elements of the domain; for an intuition of falsity, one element in the domain yielding false is sufficient, see (6). While Strong Kleene has been hypothesized to derive projection for quantifiers over individuals in Fox 2013, it has not been considered as an option for attitude predicates before, to our knowledge. Our `critical` scenario describes a context with two crucial features: on the one hand, there are some doxastic alternatives where the presupposition is not met, and some where it is; on the other hand, the embedded proposition is false in some of the doxastic alternatives where the presupposition is met. The first feature led to a prediction of presupposition failure under the Universal

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Projection hypothesis, while the second feature led to a prediction of falsity under Strong Kleene. In our results for this scenario, we find not only judgments of falsity but also an effect of matrix negation, neither of which is expected with intuitions of presupposition failure, but which align with the predictions of Strong Kleene. Finally, we find that the presence of matrix negation leads to larger standard deviations in the `critical` scenario, which, in analogy with the false controls, is expected under the Strong Kleene but not under the Universal Projection hypothesis.

However, we also find two unexpected patterns, both in the presence of matrix negation: a contrast between the truth-value judgments for the `critical` and `false` scenarios and additionally, a larger amount of uncertainty in the `critical` scenario. We would like to suggest two ways of explaining these unexpected contrasts between `critical` and `false` scenarios with negation, though both inevitably remain speculative.

The first option concerns the complexity of the SCENARIO manipulation. The `critical` scenario with negation is the most complex condition in the experiment: it involves the highest number of partitions in the character's thought bubble and requires extra computational cost because of negation — which we know inflates standard deviations (see again Figure 12). It is possible that the additional increases in uncertainty we observe in this condition are the result of participants' cognitive load due to this uniquely complex condition.

The second option we consider for the unexplained results in the negated `critical` scenario has to do with differences in information density between the `false` controls and the `critical` scenario. In our design, participants are fully informed about the character's doxastic state, but the report in the `critical` condition with negation might be underinformative, even if true. To see why, consider the item in (35) together with its relevant entailment and indirect scalar implicature.

(35) Peter is not certain that Jan canoed again.

a. *Entails:* Peter considers it possible that Jan did not canoe again.

b. *Implicates:* Peter considers it possible that Jan canoed again.

Taken together, the entailment and the implicature of the sentence address only two out of four of Peter's doxastic possibilities that feature in the `critical` scenario — see Figure 1 for the images and (36) for the verbal contexts. The `false` scenario with negation, which Strong Kleene predicts parity with, shows these two doxastic possibilities and none besides.[20]

(36) `false` Peter: "I'm certain that Jan canoed last time, but I have no idea if this time, Jan canoed or not."

20 We discuss both the explanation in terms of cognitive load and in terms of comparison with the `false` condition in more detail in Appendix C.

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`critical` Peter: "I have no idea if last time, Jan canoed or not, and I have no idea if this time, Jan canoed or not."

From the perspective of an experimental participant, the items in the `critical` scenario might have felt like worse reports of the character's attitude simply via the comparisons with the `false` scenario.[21] On this view, the contrast between the two scenarios in terms of scale and location is caused by this underinformativity rather than differences in truth-value intuitions.

Despite the fact that the explanations we entertained above do not offer a conclusive answer to the unexpected patterns, we believe that the overall interpretation of the `critical` scenario as not triggering intuitions of presupposition failure still stands. We found the predicted contrast between the negated and the nonnegated versions, a difference that is hard to explain on the assumption of presupposition failure and the absence of such a contrast in the `undefined` scenario. Similarly, the effect of negation on the standard deviations is hard to capture with the background that our undefined controls showed no such impact. In sum, we believe that the results for the `critical` scenario are compatible with the Strong Kleene hypothesis, but hard to reconcile with the Universal Projection hypothesis.

Trigger type. According to our results, the two presupposition triggers *again* and *stop* are associated with the same projection profile, such that no effect of negation is detected in the `undefined` scenario, but an effect of negation is revealed in the `critical` scenario. As we discussed above, this profile is tough to explain as a result of universal projection, which means that the presupposition of neither of our triggers projects universally. Although it has been suggested by Abrusán (2016: 192) that *stop* might not be a soft trigger — an isolated claim, to our knowledge — *again*, alongside additive particles *too* and *also*, is a prototypical hard presupposition trigger (Abusch 2002, 2010, Romoli 2015). The behavior of *too* and *again* as hard triggers is also experimentally observed by Thalmann & Matticchio (2026). The results of our experiment do not provide any support for a difference between soft and hard triggers along the lines of Charlow's (2009) hypothesis: the presupposition of *again* does not project universally, just like the presupposition triggered by *stop*.

Local accommodation. Soft and hard presupposition triggers are also taken to differ with respect to their ability for local accommodation (Abusch 2010, Thalmann &

21 In a helpful discussion about this general point, Clemens Mayr suggested that there might be a competition with embedding a polar interrogative, exemplified in (i), which is in general a felicitous option when *certain* is under negation (Mayr 2019). To explore this option, we would need a theory of embedded interrogatives grounded in trivalent logic.

(i) Peter is not certain whether Jan canoed again.

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Matticchio 2026; but see Göbel & Schwarz 2023). In (22), repeated below, we saw that the soft trigger *stop* is acceptable in explicit ignorance contexts, while the hard trigger *again* leads to the intuitions of infelicity indicative of presupposition failure.

(22) a. I don't know whether Skye ever smoked, but if they stopped smoking, Aditi will be surprised.

b. #I don't know whether Skye ever smoked, but if they smoked again, Aditi will be surprised.

However, given our experiment, the predictions of Strong Kleene for presupposition projection seem to be correct for both soft and hard triggers, without the need for the assumption of local accommodation. After all, we found that there was no tendency for the soft trigger *stop* to be interpreted as merely false in the `undefined` scenario, which would be expected if local accommodation were available. Instead, we found the impact of the TRIGGER manipulation to be minor overall, with no reliable differences between the two levels of the between-item factor. These results suggest that participants do not make use of local accommodation, but it is unclear what this unavailability is due to, especially against the background of contrasts like (22). Below, we consider two possibilities, one task-related, the other in connection with potential syntactic and semantic restrictions on local accommodation.

At first blush, locally accommodating a presupposition in our `undefined` items seems pragmatically desirable. On the assumption of Stalnaker's Bridge (see Footnote 11), according to which an utterance is only assertable if it receives a classical truth-value in the context, we expect a certain pressure for an interpretation that does not lead to a violation of that principle. That is, we expect participants to make use of local accommodation to avoid presupposition failure when the option arises, namely when a soft trigger occurs in a context that does not support its presupposition. This is illustrated below for (37) where local accommodation returns the truth-value T instead of # in the presence of presupposition failure, with $\mathscr{A}$ defined as in (21).

(37) Markus is NOT certain that $\mathscr{A}$ [λw. Sonja stopped drinking wine in w]

a. F if Markus is certain that Sonja used to drink wine and stopped.

b. T otherwise.

However, maybe the nature of our task interfered with the pragmatic pressure we normally expect. In a conversation, presupposition failure is generally taken to engender a breakdown. In our trivalent truth-value judgment task, instead, an alternative perspective on presupposition failure opens up. Since the provided scale features a response option that is nonpenalized with respect to the true and false options, it seems possible that participants felt no pressure to locally accommodate soft presuppositions in the `undefined` scenario simply because the rules of the

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experiment did not offer any advantage to these interpretations. This appears even more plausible on the assumption that local accommodation is a last resort operation. From this perspective, the absence of local accommodation in our items is not surprising, but it suggests the question of whether local accommodation would be detectable in a task with quantifiers over individuals. After all, Fox (2013) argues that local accommodation is necessary to derive the projection patterns for presuppositions embedded under nominal quantifiers. If the analytical option we have been entertaining here is on the right track, we would expect that in a minimal variant of our experiment with quantifiers over individuals, we would likewise find no evidence for local accommodation. We leave exploring this question for future work.

Another, rather more radical speculation about the absence of judgments indicating local accommodation with our items is that local accommodation is simply not available in the syntactic position indicated in (37), right above the complement clause. Our results are compatible with a view where local accommodation is not freely available, as often assumed, but restricted to certain syntactically or semantically determined environments. An important observation in the literature is that local accommodation is reportedly possible in the antecedent, but not in the consequent of a conditional statement (Singh 2014). A minimal contrast that illustrates this observation is shown in (38), where the antecedent allows for local accommodation, while the consequent appears to be much more marked, despite the use of the soft trigger *stop* in both cases.

(38) CONTEXT: *Peter is in a pool of people who routinely take part in market research. Currently, the agency is working on an anti-smoking ad.*

a. I don't know whether Peter ever smoked, but if he stopped smoking, the ad will have served its purpose.

b. #I don't know whether Peter ever smoked, but if the ad has served its purpose, he stopped smoking.

It is then natural to expect that local accommodation is not available in unembedded clauses. If it were, it would be hard to explain what could block it from applying in the consequent of a conditional, which is syntactically a main clause. The literature often mentions cases of "local accommodation" under negation (Heim 1983, Bruno 2017), but these cases seem to us to be a separate phenomenon, specific to negative operators and more similar to metalinguistic negation (Horn 1985, 1989; see also Beaver & Krahmer 2001).[22] Crucially, this type of phenomenon is not

22 In support of this distinction is the observation by Homer (2008) for French that a soft presupposition that is locally accommodated in a conditional antecedent can stop anti-licensing negative polarity items, whereas the same does not happen under negation.

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sensitive to the cut between soft and hard presupposition triggers, as the example in (39) is perfectly acceptable with contrastive intonation on *again* (see also Jackendoff 1972, Büring 2003, Beaver & Clark 2008). This interpretation of negation, which presumably requires specific contextual support, is evidently not adopted by our participants during the experiment.

(39) Jan didn't canoe AGAIN yesterday; he canoed for the first time yesterday!

One could instead wonder why local accommodation does not apply to the complement of *be certain*, as suggested in (37). Under the hypothesis that attitude predicates and conditionals share a common semantic core as modal universal quantifiers, it would be surprising if local accommodation could apply in the complement of *be certain*, given that it cannot apply to the consequent of a conditional. The impossibility to apply local accommodation in the scope of a modal operator is parallel between the two cases, as illustrated in (40).

(40) a. Conditional report, C being the set of worlds compatible with the context:
$* \forall w' \in C,\ \big([\lambda w.\ \textit{if}\ldots\ \textit{in}\ w](w') \rightarrow \mathscr{A}\ [\lambda w.\ \textit{then}\ldots\ \textit{in}\ w](w')\big)$

b. Attitude report, $\text{DOX}_{\text{M}}^{w_0}$ being the set of Markus's doxastic alternatives in w_0:
$* \forall w',\ \big([\lambda w.\ w \in \text{DOX}_{\text{M}}^{w_0}](w') \rightarrow \mathscr{A}\ [\lambda w.\ \textit{that}\ldots\ \textit{in}\ w](w')\big)$

Although it remains at the level of a speculation, this option could add to our understanding of distributional properties of local accommodation. The facts revealed by our experiment remain the same, though: the results of our task can be explained by a Strong Kleene hypothesis for presupposition projection without the aid of a presupposition weakening mechanism like local accommodation.

The Variance hypothesis. We close the discussion by returning to the secondary, methodological contribution of the paper, which the results also bear on. Recall that Thalmann (2025) finds that intuitions of presupposition failure in this paradigm have measurable effects on participants' response behavior, albeit with the presuppositions of the definite article. The Variance hypothesis Thalmann advances posits that the characteristic sensation of Strawsonian squeamishness following presupposition failure is detectable experimentally via increases in standard deviations. In line with this prediction, we find that our presupposition failure controls cause almost erratic-seeming response behavior in our participants.[23] But not only did the `undefined`

23 That the `undefined` scenario shows a widely dispersed rating distribution is expected under the Variance hypothesis, but the distribution we found is an extreme form of excessive dispersion, even under the assumption of squeamishness. The uncertainty is so substantial that the response behavior in this scenario approaches a uniform distribution across the provided scale. Interestingly, in Thalmann

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scenario elicit large amounts of variance around the condition mean; the addition of negation, which in other scenarios led to increases in variability, had no effect on the standard deviations for the `undefined` scenario. Since our scale estimates mirror the truth-value-driven effect found in Thalmann 2025, we have further evidence that the method was successful, in addition to finding additional support for the Variance hypothesis and the diagnosis of presuppositions via squeamishness operationalized through standard deviations.

5 Conclusions

This paper argued that presupposition projection from attitude predicates is best captured by a Strong Kleene logic for presuppositions in modal environments. The experimental evidence collected is instead hardly compatible with a hypothesis where presuppositions project universally from attitude predicates.

The standard approach has been to assume on the basis of introspective data alone that presuppositions project universally (Heim 1992), meaning that the presuppositions triggered in the complement of an attitude predicate have to be satisfied at every world the attitude quantifies over. In the pursuit of a common logic driving projective behavior from quantified environments, we conjectured that Strong Kleene, considered accurate by George (2010) and Fox (2013) for projection from quantifiers over individuals, could be generalized to explain presupposition projection in modal environments as well. To our knowledge, this is the first study providing evidence in this direction and suggesting that a unified mechanism for presupposition projection is within reach.

Our experimental results for the attitude predicate *be certain* conform to the prediction of Strong Kleene because in the critical scenario designed to discriminate between hypotheses, adding negation has an effect on the truth-value of the sentence — unlike what would be expected from the presupposition failure that the Universal Projection hypothesis predicts. There is no need to invoke additional mechanisms like local accommodation to explain our data (a necessity assumed in Fox 2013). Moreover, our results are not easily compatible with the idea that soft and hard triggers have distinct projection profiles, at least in the domain of quantifiers over worlds, as an interpretation of Charlow 2009 could suggest. Still, more research is needed into other attitude predicates, as well as nondoxastic and nonuniversal modals, to see whether what looks like a promising candidate logic

2025, the raw ratings for the truth-value gap conditions looked much more normal than they do here. Investigating the extent to which quantitative differences between squeamishness-indicating standard deviations are informative will have to be left for future work.

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for presupposition projection now can be maintained in the face of a more complete empirical, experimentally grounded picture.[24]

Beyond the theoretical contribution, our results also speak to the methodology of experimentally investigating presupposition. Rather than treating the dispersion of the experimental data as a nuisance variable indicative of mere noise or trying to explain it as the result of interspeaker variation, we adopted the Variance hypothesis for presupposition failure from Thalmann 2025. Finding that participants' responses are more spread out in scenarios with presupposition failure, we argued that the dispersion of responses is a reflection of the uncertainty, or squeamishness in the sense of Strawson (1964), that unmet presuppositional requirements engender in speakers. Diagnosing this required fitting distributional models that estimate both location and scale, without assuming homogeneity of variance across conditions: models sensitive only to central tendency would have missed the effect entirely. In this way, we were able to use what is often discarded as noise to diagnose intuitions of penalized truth-values. Future work will hopefully determine whether inflated (and negation-invariant) standard deviations are a reliable indicator of presupposition failure in other experimental paradigms, too.

24 A helpful example is the case of neg-raising predicates, like *think*, discussed in Section 2.2. On top of a fundamental Strong-Kleene-like logic for presupposition projection, additional semantic or pragmatic complexity should be taken into consideration. We can hypothesize that the truth conditions of a *think*-report must take neg-raising into account. The results can be schematized as in (i), the intuitions being illustrated in (ii).

(i) Projection out of a neg-raiser:
The truth-value of $\mathsf{think}_{\mathscr{W}}\ p$ is: *($\mathscr{W}$ being the set of doxastic alternatives)*

a. T if $p(w) = \text{T}$ for all $w \in \mathscr{W}$;

b. F if $p(w) = \text{F}$ for all $w \in \mathscr{W}$;

c. # otherwise (if there are $w_1, w_2 \in \mathscr{W}$ such that $p(w_1) = \text{T}$ and $p(w_2) = \text{F}$ (excluded middle violation), or if there is a $w \in \mathscr{W}$ such that $p(w) = \#$).

(ii) Aditi thinks that Skye stopped smoking.

a. T if Aditi thinks that Skye used to smoke and stopped;

b. F if Aditi thinks that Skye used to smoke and didn't stop;

c. # if Aditi thinks that Skye might have used to smoke and stopped but also that Skye might have used to smoke and not stopped; or if Aditi thinks that Skye might not have used to smoke.

These predictions, specific to *think*, match the intuitions reported by Heim (1992) and the Universal Projection hypothesis, but they are explained as the result of neg-raising. They do not undermine the hypothesis that Strong Kleene is a good candidate to model the natural language logic for presuppositions and, more generally, the hypothesis that a logic that manipulates nonclassical truth-values is needed.

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A German sample items

(41) Peter ist sich sicher, dass Jan nochmal Kanu gefahren ist.
Peter is REFL certain that Jan again canoe driven is

a. Ich bin mir sicher, dass Jan letztes Mal Kanu gefahren ist, und ich
I am REFL certain that Jan last time canoe driven is and I
bin mir sicher, dass Jan dieses Mal Kanu gefahren ist.
am REFL certain that Jan this time canoe driven is

b. Ich bin mir sicher, dass Jan letztes Mal Kanu gefahren ist, aber ich
I am REFL certain that Jan last time canoe driven is but I
habe keine Ahnung, ob Jan dieses Mal Kanu gefahren ist oder
have no clue whether Jan this time canoe driven is or
nicht.
not

c. Ich habe keine Ahnung, ob Jan letztes Mal Kanu gefahren ist oder
I have no clue whether Jan last time canoe driven is or
nicht, aber ich bin mir sicher, dass Jan dieses Mal Kanu gefahren ist.
not but I am REFL certain that Jan this time canoe driven is

d. Ich habe keine Ahnung, ob Jan letztes Mal Kanu gefahren ist
I have no clue whether Jan last time canoe driven is
oder nicht, und ich habe keine Ahnung, ob Jan dieses Mal Kanu
or not and I have no clue whether Jan this time canoe
gefahren ist oder nicht.
driven is or not

(42) Markus ist sich sicher, dass Sonja aufgehört hat Wein zu trinken.
Markus is REFL certain that Sonja stopped has wine to drink

a. Ich bin mir sicher, dass Sonja in der Vergangenheit Wein getrunken
I am REFL certain that Sonja in the past wine drunk
hat, und ich bin mir sicher, dass Sonja heutzutage keinen Wein trinkt.
has and I am REFL certain that Sonja nowadays no wine drinks

b. Ich bin mir sicher, dass Sonja in der Vergangenheit Wein getrunken
I am REFL certain that Sonja in the past wine drunk
hat, aber ich habe keine Ahnung, ob Sonja heutzutage Wein trinkt
has but I have no clue whether Sonja nowadays wine drinks
oder nicht.
or not

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c. Ich habe keine Ahnung, ob Sonja in der Vergangenheit Wein
I have no clue whether Sonja in the past wine
getrunken hat oder nicht, aber ich bin mir sicher, dass Sonja
drunk has or not but I am REFL certain that Sonja
heutzutage keinen Wein trinkt.
nowadays no wine drinks

d. Ich habe keine Ahnung, ob Sonja in der Vergangenheit Wein
I have no clue whether Sonja in the past wine
getrunken hat oder nicht, und ich habe keine Ahnung, ob Sonja
drunk has or not and I have no clue whether Sonja
heutzutage Wein trinkt oder nicht.
nowadays wine drinks or not

B In-depth model coefficients and summary

Figure 11 suggests that the manipulation of the type of presupposition trigger did not have a meaningful effect, given that the main effect of TRIGGER and almost all its interactions have intervals overlapping zero; the two small exceptions (the interactions with `undefined` and `critical`) are negligible in magnitude and do not alter the pattern, as the marginal means discussed in Section 4.7 confirm.

The scale submodel coefficients in Figure 12 reveal that dispersion in participants' ratings varies systematically across conditions. As a first point of reference, the `undefined` scenario shows high dispersion regardless of negation, consistent with it serving as an uncontroversial presupposition-failure baseline; the negative NEGATION$_{\text{GM}}$:`undef`$_{\text{GM}}$ interaction reflects a modest dampening of the combined upward effect that negation and the undefined scenario each independently have on dispersion, but `undef`-with remains among the most dispersed conditions in absolute terms, see Figure 9.

The effect for `critical`-without is the lowest dispersion of any condition (Figure 9), while the positive NEGATION$_{\text{GM}}$:`critical`$_{\text{GM}}$ coefficient indicates that negation in the `critical` scenario is substantially more dispersed than the grand mean.

Finally, the *stop*$_{\text{GM}}$ coefficient is near zero, confirming that trigger type does not independently modulate rating variability, a point further supported by the near-identical dispersion profiles across the *again* and *stop* panels in Figure 9.

Table 1 again displays the coefficients of both submodels and additionally provides some convergence indicators.

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Parameter	Median (±ETI)	$\hat{R}$	Bulk ESS	Tail ESS
Intercept	−0.22 (−0.30, −0.13)	1	18965	26013
σ_Intercept	−0.11 (−0.15, −0.07)	1	17678	25792
NEGATION	0.21 (0.11, 0.31)	1	20912	23437
`false`	0.09 (−0.01, 0.19)	1	31200	29038
`undef`	−0.08 (−0.20, 0.04)	1	32298	30232
`crit`	−0.33 (−0.43, −0.24)	1	37265	31722
stop	0.04 (−0.04, 0.11)	1	26188	26990
NEGATION:`false`	1.15 (0.94, 1.35)	1	20244	24526
NEGATION:`undef`	−0.25 (−0.42, −0.07)	1	21952	27166
NEGATION:`crit`	0.90 (0.75, 1.04)	1	26200	29092
NEGATION:*stop*	−0.02 (−0.08, 0.04)	1	39426	30180
`false`:*stop*	0.00 (−0.08, 0.09)	1	43608	30566
`undef`:*stop*	0.12 (0.01, 0.23)	1	39388	29582
`crit`:*stop*	−0.11 (−0.22, −0.01)	1	32643	29595
NEGATION:`false`:*stop*	−0.08 (−0.20, 0.03)	1	27816	29044
NEGATION:`undef`:*stop*	−0.01 (−0.13, 0.10)	1	36678	29406
NEGATION:`crit`:*stop*	−0.03 (−0.13, 0.08)	1	31857	29861
σ_NEGATION	0.17 (0.13, 0.21)	1	36975	31346
σ_`false`	−0.05 (−0.13, 0.02)	1	28481	30984
σ_`undef`	0.28 (0.21, 0.36)	1	24991	29953
σ_`crit`	−0.07 (−0.14, 0.01)	1	19420	29475
σ_*stop*	0.02 (−0.02, 0.06)	1	38132	31818
σ_NEGATION:`false`	−0.01 (−0.09, 0.06)	1	33779	30658
σ_NEGATION:`undef`	−0.18 (−0.25, −0.11)	1	33522	31284
σ_NEGATION:`crit`	0.18 (0.10, 0.25)	1	28436	31138

Table 1 Model summary for the so-called fixed effects. Note that the standard deviation parameters, prefixed with σ_, are on the log scale.

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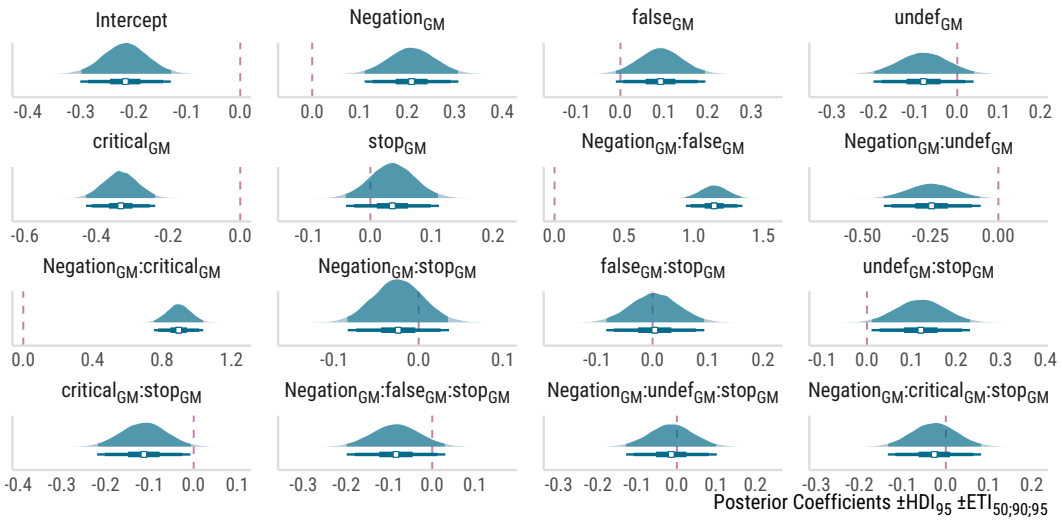


Figure 11 Model coefficients: location parameters. Coefficients are subscripted with GM (for 'grand mean') to reflect the sum coding of the predictors, which results in a comparison of the effect in question to the grand mean rather than, say, some reference category.

C Submission times and trial order

To evaluate two potential explanations for the unexpected pattern in the `critical`-with-negation condition, we examined submission times and trial order effects.

Our submission times are defined as the time elapsed until moving to the next trial rather than time to slider positioning. They reflect an imprecise measure of processing effort, and their reliability is further limited by the online setting of the experiment. With these caveats in mind, Figure 13 shows that sentences with negation were associated with somewhat longer submission times across conditions, consistent with the well-established finding in the psycholinguistic literature that negation imposes additional processing costs. However, the `critical` scenario with negation does not stand out relative to the other negated conditions, which would be expected if cognitive load specific to that condition were driving the unexpected judgment pattern. We therefore cannot confidently attribute the effect to cognitive load, though we note that a coarser measure of this kind may lack the sensitivity to detect such differences even if they exist.

Figure 14 shows judgment patterns broken down by trial quarter. The `critical` scenario with negation appears stable across quarters, with no visible trend suggesting that participants' responses in that condition were shaped by accumulating

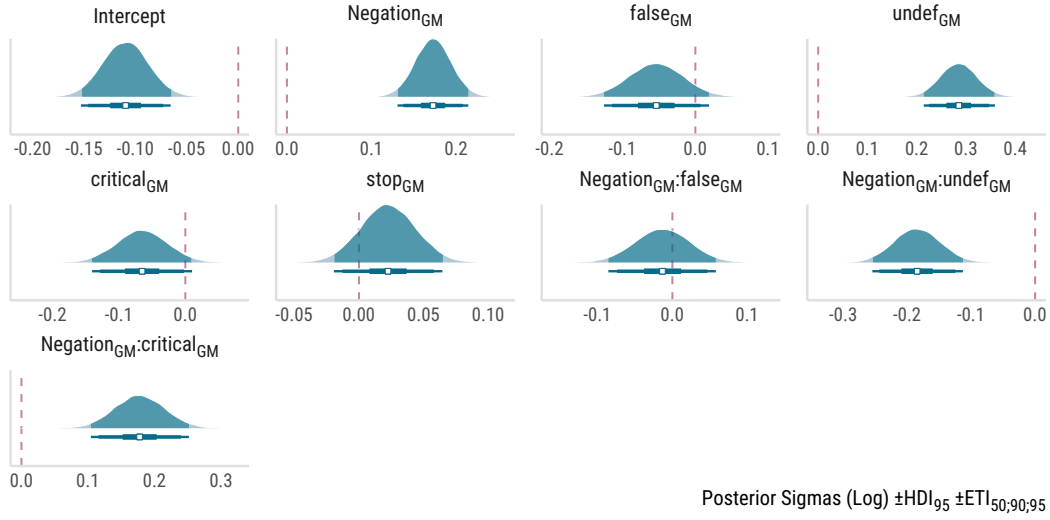


Figure 12 Model coefficients: scale parameters (modeled on the log scale). Coefficients are subscripted with GM (for 'grand mean') to reflect the sum coding of the predictors.

exposure to the `false` scenario over the course of the experiment. A trial-order explanation therefore receives little support from the data.

In sum, neither explanation can be confirmed or ruled out definitively, and the source of the unexpected pattern in the `critical` scenario with negation remains an open question for future research.

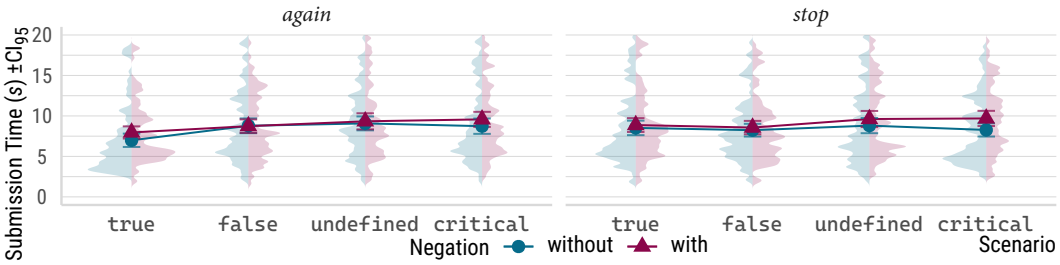


Figure 13 Submission time results.

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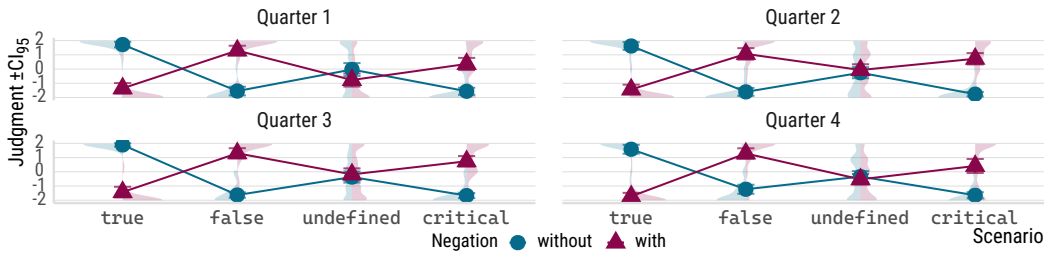


Figure 14 Experimental results divided by experimental quarter.

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