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Framing Effects: Three Linguistic Accounts and Experimental Evidence

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1 Introduction

The term *framing effects* was coined in psychological research on judgment and decision making. Framing effects are a prime example of the phenomenon that altering a description can result in major consequences. A widely used decision problem in framing studies is the scenario of an imminent outbreak of a deadly disease expected to kill 600 people as introduced by Tversky & Kahneman (1981). The task of participants is to choose between two alternative programs to combat the disease, with one program having a sure outcome and the other one a risky outcome. The outcomes are presented either in terms of number of people who will survive the disease (see (1)) or in terms of number of people who will die of the disease (see (2)).

(1) If Program A is adopted, 200 people will be saved. (sure option)

If Program B is adopted, there is 1/3 probability that 600 people will be saved, and 2/3 probability that no people will be saved. (risky option)

(2) If Program A is adopted, 400 people will die. (sure option)

If Program B is adopted, there is 1/3 probability that nobody will die, and 2/3 probability that 600 people will die. (risky option)

The typical finding of studies employing the deadly-disease scenario is that the different framing of the options affects participants' choices. In the *survive*-frame condition, the sure option, i.e. Program A, is preferred, whereas it is dispreferred in the *die*-frame condition.

In the present paper, I will look at framing effects from a linguistic perspective. Section 2 gives a brief overview of framing effects. In Sections 3 to 5, I will consider three linguistic accounts of framing effects. For each of these accounts, I will report novel experimental findings from pilot studies. Section 6 concludes the paper.

2 Framing Effects

Framing effects are a robust finding; they have been demonstrated in numerous experimental studies for a variety of scenarios, frames, and experimental methods and tasks (for reviews see Levin et al., 1998; Teigen, 2015). For example, in a study by Duchon et al. (1989), participants were presented with a financial allocation scenario in which an R&D manager is confronted with the request of a project team for additional funding. The previous performance of the team is stated in terms of number of either successful projects or unsuccessful projects (see (3)).

(3) Of the projects undertaken by the team,

a. 30 of the last 50 have been successful.
b. 20 of the last 50 have been unsuccessful.

The frame manipulation was found to have an effect on participants' ratings of their tendency toward rejecting or agreeing to the team's funding request. Participants in the *successful*-frame condition indicated a stronger inclination to agree to the funding request than participants in the *unsuccessful*-frame condition.

In the taxonomy of Levin et al. (1998), the financial allocation judgment task (Duchon et al., 1989) is an example of *attribute framing*, whereas the deadly-disease decision task (e.g. Tversky & Kahneman, 1981) is an example of *risky choice framing*. In attribute framing studies, the frame manipulation involves an attribute that is predicated to an entity or eventuality either in a positive frame or in a negative frame. Experimental tasks vary from judging the given entity or eventuality on a particular dimension to making a bivariate decision. In risky-choice framing studies, participants have to choose between two options that are either positively framed or negatively framed. One of the two options involves a single, sure outcome (e.g. Program A in (1) and (2)), whereas the other option is "risky", in that it involves two mutually exclusive alternative outcomes (e.g. Program B in (1) and (2)). Experimental findings from valence rating studies suggest that risky-choice framing effects are not due to a contrast between the sure and risky option in a given frame but are rather due to the valence of the sure option. Valence ratings were found to differ between the differently framed variants of the sure option but not between the differently framed variants of the risky option (Peters & Levin, 2008; Kühberger & Gradl, 2013). In light of this finding, risky-choice framing can be viewed as a complex variant of attribute framing.

2.1 Framing Effects as a Matter of Debate

The first demonstration of a framing effect by Tversky & Kahneman (1981) caused a stir because the finding that ostensibly equivalent descriptions can lead to different decisions was viewed as evidence for irrationality in judgment and decision making, i.e. a violation of the principle of description invariance. The line of reasoning is a proof-by-arithmetic argument: If 200 out of 600 people will be saved then the remaining 400 people will die of the disease and vice versa.[1] In recent years, different objections have been raised against the notion that framing effects demonstrate irrational behaviour. However, the source of framing effects is still a matter of controversy. The following summary of prominent accounts of framing effects illustrates the range of accounts from the psychological literature.

Tversky & Kahneman (1981) interpreted their finding of a risky-choice framing effect in the framework of *prospect theory*, i.e. a descriptive account of decision behaviour, and explained framing effects in terms of different value functions for gains vs. losses ("losses loom larger than gains") and in terms of subjective probabilities. According to *fuzzy-trace theory* (Reyna & Brainerd, 1991), framing effects emerge because people base their judgments and choices on coarse "gist" representations of the descriptions that do not contain numerical information (e.g. If Program A is adopted, some will survive/some will die). The *information-leakage account* (e.g. McKenzie & Nelson, 2003) proposes that framing effects arise because differently framed descriptions convey diverging implicit information with regard to a reference point, i.e. a standard option or an expectation (e.g. If Program A is adopted, the number of lives to be saved is higher than with the reference point/the number of lives lost is higher than with the reference point). The *ambiguity hypothesis* (e.g. Kühberger, 1995) attributes the source of risky-choice framing effects to the incompleteness of the description of the sure option. It is

[1] Note that the expected values for the risky options are the same as those for the corresponding sure options, namely that 200 people will be saved and that 400 people will die.

assumed that the incomplete descriptions invite particular inferences as to the fate of the unmentioned people in a (e.g. If Program A is adopted, not only the mentioned 200 people will be saved but also some additional people/not only the mentioned 400 people will die but also some additional people).

None of these accounts can capture the full range of findings on framing effects. Moreover, all of the accounts involve ad hoc assumptions that are not independently motivated.

2.2 Framing Effects as Effects of Linguistic Variation

Framing effects have received only limited attention in the linguistic literature. Reversely, research on framing effects has largely neglected the role of language. Yet, the very basis of framing effects is linguistic variation. As such, framing effects constitute a stimulating, practical test case for the scope of semantic/pragmatic tools and concepts. In the following three sections, I will discuss three extant linguistic accounts of framing effects.

Holleman & Pander Maat (2009) attribute the source of framing effects to a difference in use conditions. They account for framing effects in terms of pragmatic inferences regarding the argumentative orientation of the speaker of a particularly framed description. In what follows, I will refer to their account with the term *argumentative-orientation account*.

Mandel (2014) attributes the source of framing effects to numeral interpretation. He assumes that numerals that are present in differently framed descriptions as employed in research on framing effects receive a lower-bound reading. For this reason, I will use the term *lower-bound reading account* to refer to Mandel's explanation of framing effects.

Geurts (2013) attributes the source of framing effects to a difference in counterfactual semantic alternatives. He proposes that evaluations of a particular state of affairs trigger the heuristic assumption that the order of its counterfactual alternatives on the given evaluative scale is aligned to their order on an entailment-based scale. To refer to Geurts' account, I will use the term *alignment-assumption account*.

3 The Argumentative-Orientation Account

According to the argumentative-orientation account proposed by Holleman & Pander Maat (2009), differently framed descriptions differ in their use conditions, depending on the communicators' intentions.[2] Speakers' frame selection is assumed to be guided by their argumentative orientation, i.e., by the direction of the conclusion the listeners are intended to draw from the utterance. Complementary, listeners make argumentative-orientation inferences. Based on the uttered frame, they infer the direction of the conclusions intended by the speaker. According to this account, framing effects arise because different frames trigger diverging argumentative-orientation inferences, for example the intended conclusion to agree to (*successful*-frame) or to reject (*unsuccessful*-frame) the funding request in the R&D financial allocation scenario.[3]

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[2] Holleman & Pander Maat narrow the scope of their account down to attribute framing. However, when considering the above-mentioned finding that risky-choice framing effects are not due to having to choose between a sure and a risky option, but are driven by the evaluation of the sure option (Peters & Levin 2008; Kühberger & Gradl, 2013), their account could likewise be applied to risky-choice framing.

[3] The argumentative-orientation account is similar in spirit to the information leakage account as it also explains framing effects in terms of implicit information. However, the two accounts crucially differ, in that the argumentative-orientation account focusses on the role of communicators' intentions, which are neglected in the information leakage account.

The argumentative-orientation account offers a genuine pragmatic explanation of framing effects, in terms of communicative mechanisms and defeasible, pragmatic inferences. To account for argumentative-orientation inferences, Holleman & Pander Maat (2009: 2209) propose that speakers and recipients adhere to the following heuristics:

> *Speaker's maxim*: when a situation lends itself to description in terms of a two-valued variable, profile the component carrying the value that best fits the direction of the conclusions one would prefer to be drawn from the utterance.
>
> *Recipient's corollary*: when a situation lends itself to description in terms of a two-valued variable, the component that is profiled indicates the direction of the conclusions the speaker would prefer to be drawn from the utterance.

In addition, Holleman & Pander Maat (2009) assume that frame selection and interpretation is affected by markedness differences between frames. They propose that the markedness of a frame is determined by polarity (positive, i.e. present = unmarked; negative, i.e. absent = marked) and goal salience in a given scenario (goal consistent = unmarked; goal inconsistent = marked).[4] With regard to the effect of markedness, they suppose that it modulates the strength of argumentative-orientation inferences, such that they are more pronounced with marked frames than with unmarked frames.

Holleman & Pander Maat tested their account in a series of experiments. The experiments included trials that addressed the proposed speaker's maxim, i.e. production trials, and trials that addressed the recipient's corollary, i.e. interpretation trials. In production trials, participants were presented with either one of two argumentative orientations (see (4)) and had to choose between two differently framed continuations (e.g., *full* vs. *empty*). In interpretation trials, participants were presented with either one of two frames (e.g., *full* vs. *empty*) and had to choose between two continuations that stated opposite argumentative orientations (see (5)).

(4) Moving our stuff into the new house is going well / progressing slowly.

(A) At night, the house is half full.
(B) At night, the house is half empty.

(5) Today I'm moving to my new house. In my former house there are still a number of boxes. The hallway there is half full / half empty.

(A) My moving goes swiftly.
(B) My moving is getting on slow.

Participants' choices were systematically affected by the given argumentative orientation and by the given frame, respectively, and exhibited the assumed markedness asymmetry. These findings are consistent with the notion that frame choice is governed by the speaker's argumentative orientation and with the converse notion, that a given frame conveys implicit information on the speaker's intended argumentative conclusion. However, this evidence for the argumentative-orientation account is equivocal. First, the findings stem from a forced choice task. Hence, it is questionable whether they are generalizable to natural conditions of language production and interpretation, i.e. whether people adhere to the speaker's maxim and the recipient's

[4] For example, in a filling scenario, *full* is unmarked, i.e. goal consistent, and *empty* is marked, i.e. goal inconsistent, whereas in an emptying scenario, *full* is marked, i.e. goal inconsistent, and *empty* is unmarked, i.e. goal consistent.

corollary when not being dished up with the possibility of alternative descriptions and of alternative argumentative conclusions, respectively.[5] Second, and moreover, the studies did not investigate framing effects on judgment and decision making and hence the findings are not telling with regard to whether or not argumentative-orientation inferences play a (causal) role in the emergence of effects of linguistic variation on judgments and decisions.

Argumentative-orientation inferences are conversational implicatures in the sense that they pertain to the speaker's intention. If it could be shown that framing effects do not arise when the framed information stems from an intentionless source, e.g. a statistics report, then this would provide clear support for the argumentative-orientation account. The goal of Experiment A was to gain insight in this regard. To this end, two conditions were contrasted: the framed information either stemmed from a human being or from an intentionless statistics report. A clear drawback of this methodological approach is that the findings are conclusive only with a particular pattern of results, i.e. an effect of framing when the source is a human being and no effect of framing when the source is a statistics report. The finding of a framing effect in both source conditions would be inconclusive. That is, such a finding could be interpreted as showing that it does not matter whether the source is intenionable, thereby casting doubts on the validity of the argumentative-orientation account. Yet, the finding would not provide evidence against the argumentative-orientation account, insofar as a framing effect in the condition with the intentionless source could be attributed to participants' conjectures with regard to the communicative intentions of the writer of the decision scenario. However, an experimental demonstration that framing effects do not arise with an intentionless source would provide clear evidence for a causal role of argumentative-orientation inferences in the emergence of framing effects. It is for this potential that I considered an experimental test still worth pursuing.

3.1 Experiment A

Experiment A was designed to investigate whether the intention facility of the source of framed information is a decisive factor for the occurrence of a framing effect. To this end, I employed a modified German version of Duchon et al.'s (1989) R&D financial allocation scenario with two source conditions. The source of the differently framed information (number of successful vs. unsuccessful projects) was either a human being or a statistics report (see (6)).

3.1.1 Method

All participants of the experiments reported in this paper were German native speakers and were recruited from the student population of Berlin and Potsdam. They gave informed consent for participation and participated in exchange for the chance to win € 25 in a raffle.

Sixty-eight students (18 to 37 years, $M = 24.25$; 53 female) participated in Experiment A. The data of two additional participants were excluded from the analyses because they were not native speakers of German.

The materials for the experimental trials comprised 2×2 versions of a decision problem. The decision problem was an adjusted German variant of the financial allocation problem as employed by Duchon et al. (1989). As in the original variant, the frame manipulation consisted in stating either the number of previously successful projects or the number of previously unsuccessful projects of the team that asks for additional funding. Orthogonally to these two versions, and different from the original variant, there were two additional versions that differed

[5] See Claus et al. (2019) for evidence that findings from a forced-choice task can differ substantially from findings from a more indirect method and may lead to divergent conclusions.

with regard to whether the framed information was portrayed as stemming from a human being or from a statistics report. The description of the financial allocation problem as it was presented to the participants is shown in (6), along with a translation to English.

Experiment A employed a 2×2 mixed design with the factors SOURCE (human being vs. statistics report) as group factor and FRAME (successful vs. unsuccessful) as repeated measures factor.[6] Participants were randomly assigned to either of the two conditions of the group factor SOURCE. All participants were first presented with the financial allocation problem in one of the two frames. After a series of distractor trials, to be outlined below, they were presented with the financial allocation problem in the other frame. The order in which the two frame conditions were given to the participants was randomly assigned to them. In both experimental trials, participants first had to make a categorical choice, i.e., whether they would agree with the request for additional funding or reject it. Then, they were presented with a 7-point rating scale and were asked to indicate the strength of their tendency towards agreeing or rejecting the funding request. The endpoints of the scale were labelled with "very strong inclination towards rejecting" (*sehr starke Tendenz zur Ablehnung*) and "very strong inclination towards agreeing" (*sehr starke Tendenz zur Zustimmung*).

(6) Stellen Sie sich die folgende Situation vor: Seit ein paar Wochen leiten Sie die Abteilung für Forschung und Entwicklung eines Technologiekonzerns. In der Abteilung arbeiten mehrere Forschungsteams an verschiedenen Projekten. Sie haben heute von einigen der Teams Anträge auf zusätzliche finanzielle Mittel erhalten. Eines der Teams bittet Sie um zusätzliche 50.000 Euro für ein laufendes Projekt. Alle für das Projekt ursprünglich bewilligten Mittel sind bereits verbraucht und das Projekt liegt hinter dem Zeitplan. Aber das Team glaubt, das Projekt noch erfolgreich abschließen zu können. Sie verfügen aktuell noch über ein sehr hohes Budget an Forschungsgeldern. Aber jede Ausgabe, die Sie jetzt machen, schränkt Ihre zukünftige finanzielle Flexibilität ein. Nach Ihrer Einschätzung der Lage ist es durchaus möglich, dass das Projekt ohne Erfolg abgeschlossen wird. Dann wären die zusätzlichen Mittel verloren. Wenn jedoch das Projekt mit Erfolg abgeschlossen wird, dann wäre das Geld gut angelegt.

'Imagine the following situation: For a few weeks now you have headed the research and development department of a technology company. In the department, there are several research teams that work on various projects. Today, you received requests for additional funding from some of the teams. One of the teams asks you for an additional 50,000 euros for an ongoing project. All funds originally approved for the project have already been used up and the project is behind schedule. However, the team believes that the project can still be successfully completed. Currently, you still have a very large budget for research funds. However, every expense you make now limits your future financial flexibility. According to your assessment of the situation, it is quite possible

[6] In all three experiments reported in this paper, the factor FRAME was manipulated within participants, with distractor tasks in between the presentation of the framing problems in the two description variants. The main reason for employing a within-subject design is a principled one: experimental conditions in between-subjects designs do not only differ in the experimental manipulation. This drawback of between-subjects designs is particularly relevant to the collection of subjective judgments (Birnbaum, 1999). In recent years, there is a trend towards employing within-subject designs in research on framing (e.g. Mandel, 2014; Chick et al., 2015). An objection against within-subjects designs in framing studies is that they increase task transparency and reduce framing effects. However, findings from a study by Aczel et al. (2018) on the impact of transparency on framing effects do not support this objection.

that the project will be completed without success. Then the additional funds would be lost. However, if the project is successfully completed, the money would be well spent.'

Human being: Sie erkundigen sich bei dem vorherigen Leiter der Forschungs-und-Entwicklungsabteilung nach dem Team, das die zusätzlichen Mittel beantragt hat. Er sagt Ihnen, dass …

'You ask the former head of the R&D department about the team that requested the additional funding. He tells you that ...'

Statistic report: Sie informieren sich in den Statistiken zu den Forschungsteams in Ihrer Abteilung über das Team, das die zusätzlichen Mittel beantragt hat. Darin lesen Sie, dass …

'You enquire the statistics report of the research teams in your department about the team that requested the additional funds. In it you read that …'

Successful: … 30 der letzten 50 Projekte des Teams erfolgreich waren.

'… 30 of the last 50 projects of the team have been successful.'

Unsuccessful: … 20 der letzten 50 Projekte des Teams erfolglos waren.

'… 20 of the last 50 projects of the team have been unsuccessful.'

In between the two experimental trials, participants completed a block of 20 distractor trials. The distractor block started with a sequence of eight trials in which participants were presented with evaluative statements unrelated to the financial allocation problem of the experimental trials. Participants' task in these distractor trials was to respond to questions pertaining to the evaluative statements. This was followed by a sequence of four different distractor trials in which participants were asked to produce three words that start or end with a given chain of two to three letters. After the word production trials, there was a second sequence of eight distractor trials with the task of responding to questions about given evaluative statements.

3.1.2 Results and Discussion

There were two dependent variables in Experiment A, the first one from the choice task, i.e. agreement with or rejection of the funding request, and the second one from the task to rate the strength of their tendency towards agreeing or rejecting. Table 1 shows the proportion of choices for the option to agree with the funding request. The median ratings of tendency strength are shown in Table 2.

Table 1. Percentages of agreement-option choices in the four conditions of Experiment A

	FRAME	
SOURCE	Successful	Unsuccessful
Human being	67.6%	51.4%
Statistics report	71.0%	58.1%

The choice data were analyzed by using a generalized linear mixed model with a binomial logit function and with participants as random factor. There were two fixed factors, SOURCE and FRAME, both with deviation coding (+.5, -.5). There was no main effect of SOURCE (b = -.40, SE = .75, z = -.54, p = .59) and a significant main effect of FRAME (b = 1.11, SE = .52, z = 2.13, p < .05). The interaction of both factors was not significant (b = .22, SE = .97, z = .23, p = .82).

Table 2. Median ratings in the four conditions of Experiment A. The rating levels range from 1 ('very strong inclination towards rejecting') to 7 ('very strong inclination towards agreeing')

	FRAME	
NUMERAL	Successful	Unsuccessful
Human being	5	4
Statistics report	5	4

The rating data were analysed by using a cumulative link mixed model for ordinal data (R package ordinal) with SOURCE and FRAME as fixed effects and with participants as random effect. For both fixed factors, deviation coding was used (+.5, -.5). The seven points on the rating scale were coded with natural numbers from 1 for 'very strong inclination towards rejecting' to 7 for 'very strong inclination towards agreeing'. The analysis yielded a main effect of FRAME (b = 1.37, SE = .37, z = 3.67, p < .001). The main effect of SOURCE (b = .25, SE = .58, z = .43, p = .67) and the interaction effect (b = .25, SE = .68, z = .36, p = .72) were not significant.

Participants' categorical choices as well as their tendency strength ratings were affected by the framing manipulation. Crucially, the framing effects were not modulated by whether the source of the framed information was a human being or an intentionless statistics report. As outlined above this finding is inconclusive with regard to the validity of the argumentative-orientation account. It can be explained within the framework of the account when assuming that the framing effect in the 'statistic report' condition is due to argumentative-orientation inferences regarding the intentions of the writer of the decision scenario.

3.2 Argumentative-Orientation Account: Interim Conclusion

According to the argumentative-orientation account as proposed by Holleman & Pander Maat (2009), differently framed descriptions are not information equivalent in that they differ in their use conditions. Holleman & Pander Maat attribute the source of framing effects to argumentative-orientation inferences, i.e. pragmatic inferences on communicative intentions.

The findings of Experiment A are inconclusive with regard to whether the intentionality of the source of framed information is decisive. For the time being, it holds that there is neither unequivocal evidence against nor experimental support for the argumentative-orientation account. To put it in other words, there is currently no empirical evidence for a causal link between argumentative-orientation inferences and framing effects.

Whereas the account is neutral on the experimental-evidence side, there are some theoretical concerns. First, that judgments and decisions are systematically affected by framing cannot be accounted for solely on the basis of argumentative-orientation inferences. The argumentative-orientation account involves the tacit, additional assumption that people adhere to the argumentative orientation inferred. It is very questionable whether this holds in general.

Second, the initiating condition for argumentative-orientation inferences, as stated in the verbalisation of the recipient's corollary is: *"*when a situation lends itself to description in terms of a two-valued variable" (Holleman & Pander Maat, 2009: 2209). Hence, argumentative-orientation inferences are assumed to be based on the contrast between the given frame and its alternative. This assumption bears the questionable implication that argumentative-orientation inferences involve the mental activation of the alternative frame (over and above activation due to semantic relatedness spreading).

Third, the assumptions on the relation between argumentative orientation and frame seem to be largely intuition-based and are not clearly spelled out. Fourth, the frames that are juxtaposed in framing studies typically differ in affective valence, with one frame carrying a positive valence and the other frame carrying a negative valence (e.g. *successful – unsuccessful, save – die*). Indeed, it is plausible that the choice of using a positively or negatively valenced frame systematically relates to the speaker's argumentative orientation, i.e. in terms of arguing for or against something. It is also not unlikely that positively or negatively connotated frames at times trigger inferences on the speaker's argumentative orientation. Yet, such inferences require linking up the given frame with a particular argumentative orientation. Obviously, it is the frame's valence that is the key to the linkage. Thus, a precondition of argumentative-orientation inferences from positively or negatively valenced frames is an evaluation of the framed information. In general, this precondition is usually met, considering that comprehenders routinely make affective evaluations. The issue now is that the evaluation by itself is also a possible source of framing effects. Hence, the extra assumption of argumentative-orientation inferences is dispensable – unless it could be shown that an account in terms of argumentative-orientation inferences has a wider explanatory coverage than an account in terms of affective evaluations.

Notwithstanding these concerns, the considerations underlying the argumentative-orientation account are of value for research on framing effects. The starting point of the account are the use conditions of differently framed information. The production side has received only limited attention in research on framing effects. One exception is the information leakage account (e.g. Sher & McKenzie, 2006). The novelty of the argumentative-orientation account is that it relates frame choice to communicative intentions and introduces pragmatic insights to framing research. Moreover, it points to possible markedness differences between frames. This issue has largely been neglected in framing research (see McKenzie & Nelson, 2003 for findings that could be interpreted in terms of a markedness asymmetry). Holleman & Pander Maat (2009) hypothesize that frames that are goal consistent in a given scenario are unmarked whereas goal inconsistent frames are marked. This conjecture is of relevance for research on framing effects, especially when considering that in the most frequently employed materials of framing studies, i.e. the deadly-disease decision problem and its variants, evaluative valence is confounded with goal consistency, i.e. the positively valenced frame is goal consistent and the negatively valenced frame is goal inconsistent.

4 The Lower-bound Reading Account

The description variants in framing studies typically contain numerical expressions. Whether or not two differently framed descriptions can be considered to be complementary descriptions of the same states of affairs crucially depends on the readings of the given numerals (Macdonald, 1986; Kühberger, 1995; Mandel, 2014). In his account of framing effects, Mandel (2014) assumes a tendency to assign a lower-bound reading to the numerals (at least *n*).

If true, the lower-bound reading account would offer a parsimonious explanation of framing effects. Consider, for example, the deadly-disease scenario. Under a lower-bound reading, the two description variants of the sure option (see (1) and (2)) are not equivalent with regard to their outcomes, as shown in (7).

(7) a. At least 200 will be saved ≡ At most 400 will die ≠ At least 400 will die

b. At least 400 will die ≡ At most 200 will be saved ≠ At least 200 will be saved

Moreover, and crucially, the *survive*-frame variant is, under a lower-bound reading, better than the *die*-frame variant given the human conviction that the more lives saved/the fewer lives lost the better. Hence, from the view of the lower-bound reading account, framing effects do not at

all demonstrate a violation of the principle of description invariance. If numerals are interpreted as indicating a lower bound, then two differently framed descriptions vary not only in the description.[7] For instance, in the deadly-disease decision problem, the outcomes of the sure option are not identical given a lower-bound reading of numerals in the two frame variants. The number of lives saved is possibly higher and the number of lives lost is lower in the *survive*-frame variant than in the *die*-frame variant. Thus, when considering that the goal of disease intervention programs is to save lives, i.e. prevent the loss of lives, the preference for the sure option in the *survive*-frame variant may not be evidence for irrationality in judgment and decision making but rather for rational behavior.

Mandel (2014) tested his lower-bound reading account in a series of experiments and reports three findings in support of his account. In the following, I will first consider two of these findings. I will address the third finding in Section 4.3.

Mandel assessed participants' interpretation of the numerals in the descriptions of the deadly-disease intervention programs by employing a forced choice task with three interpretation alternatives: 'at least' (corresponding to a lower-bound reading of the numerals), 'at most' (corresponding to an upper-bound reading), and 'exactly' (corresponding to a precise, bilateral reading). Consistent with the lower-bound reading account, the majority of participants indicated a lower-bound reading ('at least': chosen by 64%; 'exactly': chosen by 30%; 'at most': chosen by 6%).[8] Furthermore, a framing effect was observed only for the subgroup of participants who indicated a lower-bound reading, i.e. there was no framing effect for the subgroup of participants who indicated a precise reading. However, these findings do not provide unequivocal support for the lower-bound reading account.

There are two specific methodological issues pertaining to the numeral-interpretation assessment in Mandel's experiment. First, it is not unlikely that round numbers such as 200 and 400 are interpreted as approximate estimates (e.g. Schindler & Yalch, 2006; Solt et al., 2017). However, the forced-choice task in Mandel's experiment did not include an alternative to indicate an approximate reading of the given numeral. That is, in case of an approximate reading, participants had to choose between the precise reading, lower-bound reading, and upper-bound reading. This is problematic per se but is even more an issue of concern when considering that it is unclear whether these choices were equally distributed across the three options available. Second, the numeral interpretation assessment immediately followed the task of choosing between the sure and the risky option. Thus, the just-made choice might have influenced participants' indication of the numeral interpretation, for instance, in terms of a justification of the choice. To gain insight into the relevance of these two specific methodological issues, I conducted Experiment B.

4.1 Experiment B

The goal of Experiment B was to investigate participants' interpretation of the numerals in the deadly-disease scenario and its relation to the occurrence of framing effects with a modified version of Mandel's (2014) numeral-interpretation assessment method. The forced choice task of the interpretation assessment in Experiment B included a fourth interpretation alternative,

[7] Note that the lower-bound reading account is a variant of the ambiguity hypothesis.

[8] According to Mandel (2014: 1193), this finding fits well with the Neo-Gricean view that numerals have a lower-bound semantics (e.g. Horn, 1972; Levinson, 1983). However, the finding can also be captured by concurrent views, including the notions that numerals are ambiguous between a bilateral and lower-bound semantics (Geurts, 2006), that the semantics of numerals is underspecified (Carston, 1998), or that their semantics is bilateral (e.g. Breheny, 2008; Kennedy, 2015). All accounts agree that numerals can receive a lower-bound reading. They differ in whether the lower-bound meaning is semantic or pragmatic in nature.

i.e. 'approximately', in addition to the three alternatives in Mandel's study, 'at least', 'exactly', and 'at most'. Moreover, the numeral-interpretation assessment did not immediately follow the decision task but was separated by distractor trials.

4.1.1 Method

Seventy-six students (18 to 39 years, M = 23.53; 58 female) participated in Experiment B. The data of one additional participant were excluded from the analyses because they were not a native speaker of German.

The materials for the experimental trials consisted in two differently framed variants of a modified German version of the deadly-disease decision problem as shown in (8). In the *survive*-frame variant, the outcomes of the two programs to combat the disease were framed in terms of the expected number of people to survive the disease and in the *die*-frame variant, the outcomes were framed in terms of the expected number of people to die of the disease.

(8) Die Gesundheitsbehörde einer Kleinstadt bereitet sich auf den Ausbruch einer hochansteckenden Krankheit vor, durch die voraussichtlich 600 Menschen getötet werden. Es gibt eine Vielzahl von unterschiedlichen Programmen zur Bekämpfung der Krankheit, zwischen denen sich die Gesundheitsbehörde entscheiden muss – u.a. die folgenden beiden Programme.

'Imagine the following situation: The health authority of a small town is preparing for the outbreak of a highly contagious disease, which is expected to kill 600 people. There are a variety of different programs to combat the disease between which the health authority must decide – including the following two programs.'

Survive: Bei Anwendung von Programm A werden 200 Menschen überleben.

Bei Anwendung von Programm B werden mit einer Wahrscheinlichkeit von 1/3 600 Menschen überleben und mit einer Wahrscheinlichkeit von 2/3 wird niemand überleben.

'If Program A is adopted, 200 people will survive.

If Program B is adopted, there is a probability of 1/3 that 600 people will survive, and a probability of 2/3 that no one will survive.'

Die: Bei Anwendung von Programm A werden 400 Menschen sterben.

Bei Anwendung von Programm B wird mit einer Wahrscheinlichkeit von 1/3 niemand sterben und mit einer Wahrscheinlichkeit von 2/3 werden 600 Menschen sterben.

'If Program A is adopted, 400 people will die.

'If Program B is adopted, there is a probability of 1/3 that no one will die, and a probability of 2/3 that 600 people will die.'

Experiment B employed a one-factorial repeated-measures design with the factor FRAME (die vs. survive). Participants were first presented with the deadly-disease decision problem in either of the two frames. This was followed by twelve distractor trials in which participants' task was to respond to questions on evaluative statements unrelated to the deadly-disease decision problem.[9] After that, participants were again presented with the deadly-disease decision problem,

[9] These trials served as the experimental and filler trials of Experiment C that was conducted together with Experiment B.

this time in the other frame. The order of the two frame conditions was counterbalanced across participants. The second decision task was followed by two sequences, each consisting in a set of four distractor trials preceding the assessment of the interpretation of the numeral in the sure option of the deadly-disease decision problem in either of the two frame variants. In each of the eight distractor trials, participants were presented with a chain of two to three letters and were asked to produce four words that start or end, respectively, with the given chain.

In the two experimental decision-problem trials, participants first had to choose between the two programs to combat the disease, i.e. between the sure option (Program A in (8)) and the risky option (Program B in (8)). Subsequently, they were asked to indicate the strength of their preference for the chosen option on a 7-point rating scale, ranging from "no difference in preference between the two programs" (*kein Präferenzunterschied zwischen beiden Programmen*) to "very strong preference for Program X" (*sehr starke Präferenz für Programm X*), with X being instantiated with the identifier of the chosen program.

In the two numeral-interpretation trials, participants were again presented with the deadly-disease scenario and with the description of Program A (sure option) either in the *survive*-frame variant or in the *die*-frame variant. The order of the two frame conditions in the numeral-interpretation trials was the same as in the decision problem trials. The task of the participants was to indicate how they understood the wording *200 Menschen* ('200 people') or *400 Menschen* ('400 people'), respectively, by choosing between four alternative interpretations: genau *n* Menschen ('exactly *n* people'), mindestens *n* Menschen ('at least *n* people'), höchstens *n* Menschen ('at most *n* people'), ungefähr *n* Menschen ('approximately *n* people'), with *n* being instantiated with either *200* or *400*, depending on the respective frame variant of the sure option. There were four different orders in which the four interpretation alternatives were presented, such that each alternative had a different position in each order. Participants were randomly assigned to one of the four order conditions.

4.1.2 Results and Discussion

Table 3 shows the distribution of selected interpretation options in the numeral-interpretation assessment task. In both frame conditions, most participants chose the precise-reading option. The distribution in the *survive*-frame condition did not significantly differ from the distribution in the *die*-frame condition ($\chi^2(3, N = 76) = 3.17$, $p = .37$).

Table 3. Percentages of selected numeral-interpretation options in Experiment B

	Exactly *n*	At least *n*	At most *n*	Approximately *n*
Survive-frame	77.6%	7.9%	2.6%	11.8%
Die-frame	80.3%	3.9%	2.6%	13.2%
Overall	78.9%	5.9%	2.6%	12.5%

Table 4 shows the proportion of sure-option choices in the decision task. The choice data were analysed by generalized linear mixed modelling with a binomial logit function and with participants as random factor. The fixed factor was FRAME, with deviation coding (+.5, -.5). The overall analysis revealed a significant main effect of FRAME ($b = -.22$, $SE = .56$, $z = -3.85$, $p < .001$). For further analyses, the choice data were split into two subsets depending on the corresponding numeral-interpretation data, i.e. a subset with precise reading cases and a subset with non-precise reading cases (lower-bound, upper-bound or approximate reading). There was a significant main effect of FRAME in the analysis of the precise-reading data subset ($b = -1.96$, $SE = .56$, $z = -3.42$, $p < .001$) and in the analysis of the non-precise reading data subset ($b = -19.02$, $SE = 5.65$, $z = -3.37$, $p < .001$).

Table 4. Percentages of choices for the sure option in the decision task of Experiment B

	Overall	Precise reading	Non-precise reading
Survive-frame	68.4%	69.5%	64.7%
Die-frame	31.6%	31.1%	33.3%

The ratings of the strength of preference for the chosen option were recoded into a measure of weighted choice (cf. Mandel, 2014), ranging from -6 for 'very strong preference for the risky option' to 0 for 'no difference in preference between the two options' to 6 for 'very strong preference for the sure option'. The median choice weights are given in Table 5. The weighted-choice data were analysed with cumulative link mixed models for ordinal data with participants as random effect and FRAME as fixed effect (deviation coding: +.5, -.5). The main effect of FRAME was significant in the analysis of the overall data set ($b = 1.30$, $SE = .31$, $z = 4.20$, $p < .001$) and in the separate analyses of the precise-reading subset ($b = 1.31$, $SE = .35$, $z = 3.71$, $p < .001$) and the non-precise reading subset ($b = 2.17$, $SE = .97$, $z = 2.23$, $p < .05$).

Table 5. Median choice weights computed from the rating task of Experiment B. The choice weights range from -6 ('very strong preference for the risky option') to 0 ('no difference in preference between the two options') to 6 ('very strong preference for the sure option'), i.e. negative values indicate a preference for the risky option and positive values indicate a preference for the sure option

	Overall	Precise reading	Non-precise reading
Survive-frame	2	2	2
Die-frame	-1.5	-1	-2

The results of Experiment B do not replicate Mandel's (2014) findings. The vast majority of participants of Experiment B indicated a precise reading of the numerals rather than a lower-bound reading. Moreover, the precise reading did not involve a moderation of the framing effect. There were significant effects of the framing manipulation on participants' choices and preferences ratings not only for the whole data set but also for the subset that included only precise-reading cases. Thus, the results of Experiment B are in clear conflict with the core assumption of the lower-bound reading account, i.e. that framing effects are due to a non-precise, lower-bound interpretation of numerals.

4.2 Lower-bound Reading Account: Interim Conclusion

In the lower-bound-reading account (Mandel, 2014), framing effects are ascribed to a lower-bound numeral interpretation. Mandel's (2014) own findings are consistent with his account. The majority of participants indicated a lower-bound reading in a numeral interpretation assessment and a framing effect was observed only for the data subset of these participants. In contrast, the findings of Experiment B are inconsistent with the lower-bound reading account. While it is tempting to attribute the conflicting findings to the methodological difference in assessing the numeral interpretation, the current empirical basis is too shallow to draw such a general conclusion.

Mandel (2014) reports a further finding in support of his account. When a precise reading of the numerals in the sure option of the deadly-disease scenario was enforced by modifying them with *exactly* (… *exactly 200 people will be saved/exactly 400 people will die*), there was

no framing effect. However, there is a conflicting finding from a study in which a framing effect was observed when the numerals were likewise modified with *exactly* (Chick et al., 2016)[10].

Thus, findings regarding the validity of the lower-bound reading account so far are mixed. The supporting evidence from Mandel's study was not replicated in other studies. Rather, Experiment B and the study by Chick et al. (2016) yielded findings that are difficult to reconcile with the lower-bound reading account. What clearly speaks in favour of the account is its parsimony. It makes do with just one assumption, the lower-bound reading, which, however, is controversial.

5 The Alignment-Assumption Account

According to Geurts' (2013) account of framing effects, differently framed descriptions are not information equivalent in that they differ in their counterfactual alternatives, e.g. different numbers of lives saved (*survive* frame) or different numbers of lives lost (*die* frame). In a nutshell, Geurts (2013) assumes that people's judgments and decisions for given options are systematically related to their judgments and decisions for the counterfactual alternatives of the given options. He proposes that with qualitative evaluations, people act on a heuristic that he named *alignment assumption*.

Consider the case of a positive evaluation of the sure option in the *survive*-frame condition and the *die*-frame condition, as expressed by the evaluative statements in (9). (9a) and (9b) appear contradictory. Anyone who endorses (9a) cannot at the same time consistently endorse (9b).

(9) a. It's good that 200 people will survive.

b. It's good that 400 people will die.

I will outline below that and how the contradiction between (9a) and (9b) can be explained via the alignment assumption heuristic. Before, I will address other explanations of the contradiction that may appear simpler but, in fact, lead to a dead end.

One such dead-end explanation is that the contradiction between (9a) and (9b) is due to the decent fit of *good* and *survive* in (9a) and the poor fit of *good* and *die* in (9b). Consider (10) that differs from (9) only in the modification of the numerals with comparative quantifiers. Yet, (10a) and (10b) do not appear contradictory, suggesting that the fit between *good* and *survive* vs. *die* is not decisive.

(10) a. It's good that more than 200 people will survive.

b. It's good that fewer than 400 people will die.

A further dead-end explanation of the contradiction between (9a) and (9b) is that (9a) conforms with the human conviction that the more lives saved/the fewer lives lost the better whereas (9b) disconforms with that conviction. However, the contradiction holds independent of the beholder's conviction with regard to the valence of lives saved or lost. A nasty dictator might agree with (9b) but she cannot likewise agree with (9a).

The contradiction can also not be simply explained away with a lower-bound reading of the numerals in (9a) and (9b). To see this, consider Geurts' (2013) example of a plane crash

[10] The preliminary results of an ongoing experiment in German, in which I enforced a precise reading via modification with *genau* ('exactly'), replicate the finding by Chick et al. (2016) in exhibiting a framing effect, contra the lower-bound reading account.

and consider (11a) and (11b). The context of the first sentence enforces a precise reading of the numeral in the subsequent evaluative statement. Still, the evaluative statements in (11a) and (11b) appear contradictory. This is remarkable insofar as the two embedded propositions in the second sentence are descriptively equivalent in the given context and are embedded under the same evaluative predicate, *good*. Yet, the two evaluative statements appear to contradict each other. According to Geurts, this is how the problem of accounting for framing effects "translates" into a semantic puzzle. Hence, solving the semantic puzzle by explaining the contradiction will additionally yield a solution for the framing problem.

(11) a. Out of exactly 600 passengers, exactly 400 died. It's good that the remaining 200 passengers survived.

b. Out of exactly 600 passengers, exactly 200 survived. It's good that the remaining 400 passengers died.

Let's now see how Geurts explains the apparent contradiction between (9a)/(11a) and (9b)/(11b). The positive evaluation of a proposition implies that it ranks sufficiently highly on the qualitative scale of goodness that orders its alternatives. If these alternatives can also be ordered on a quantitative scale in terms of entailment-based strength, then the two scales are assumed to be aligned, as indicated by the formulation of the alignment assumption in (12).

(12) $\forall q \forall q'((q \in Alt(p) \wedge q' \in Alt(p)) \rightarrow (q > q' \rightarrow q \gg q'))$
In prose: For all alternatives of a proposition p being embedded under an evaluative predicate (e.g., *good*) it holds that if one alternative, q, is logically stronger (>) than another alternative, q', then q also ranks higher (≫) than q' on the relevant qualitative scale.

For the evaluative statements in (9), the alignment assumption implies the inferences in (13). The alignment-assumption inference for the *survive*-frame in (13a) can roughly be paraphrased as 'the more lives to be saved the better'. In contrast, the alignment-assumption inference for the *die*-frame in (13b) can roughly be paraphrased as 'the more lives to be lost the better'. These inferences are in clear contradiction with each other and this explains the contradiction of (9a)/(11a) and (9b)/(11b).[11]

(13) a. $n + 1$ will survive $\gg_{\text{better than}}$ n will survive

b. $n + 1$ will die $\gg_{\text{better than}}$ n will die

To account for framing effects, Geurts (2013) assumes that the choice task in framing studies involves an evaluation process that can be expected to be affected by the alignment assumption. The preference vs. dispreference for the sure option in the *survive*-frame vs. *die*-frame condition can then be attributed to that the alignment-assumption inference involved in a positive evaluation of the sure option conforms to human convictions for the *survive*-frame and disconforms with those for the *die*-frame. Conversely, the alignment-assumption inference involved in a negative evaluation of the sure option conforms to human convictions for the *die*-frame (the

[11] According to one of the reviewers, the contradiction between (11a) and (11b) disappears when *only* is added to (11b) as in (i). This can be accounted for within the framework of the alignment-assumption account when assuming that *only* is downward-entailing (e.g. von Fintel, 1999). It could also be explained in terms of an evaluative reading of *only*.

(i) It's good that only the remaining 400 passengers died.

A limitation of the alignment-assumption account is that it cannot explain effects of non-monotone numeral modifiers (e.g. *exactly, approximately*) because the alignment assumption is only applicable when the counterfactual alternatives can be ordered on an entailment-based scale.

more lives lost the worse) and disconforms with those for the *survive*-frame (the more lives saved the worse).

In a previous study (Claus, 2019), I experimentally tested two predictions that can be derived from the alignment-assumption account. Both predictions relate to the effect of upward- vs. downward entailing comparative quantifiers. Consider the variants of the descriptions of the sure options of the deadly-disease scenario in (14), in which the numerals are modified with the comparative modifiers *more than* and *fewer than*.

(14) a. If Program A is adopted, more than 200 people will survive.

b. If Program A is adopted, fewer than 400 people will die.

With the upward-entailing *more than* in the *survive*-frame description and the downward-entailing *fewer than* in the *die*-frame description, a positive evaluation of the sure option does for both frames involve alignment-assumption inferences (see (15)) that conform to human convictions (the more lives saved/the fewer lives lost the better). By the same token, the alignment-assumption inferences from a negative evaluation disconform for both frames with human convictions. Hence, the alignment-assumption account predicts that there should be no difference in valenced evaluations between (14a) and (14b). Moreover, it predicts that there should be no framing effect when the numerals in the sure options of the deadly-disease scenario are modified as in (14).

(15) a. more than $n + 1$ will survive $\gg_{\text{better than}}$ more than n will survive

b. fewer than n will die $\gg_{\text{better than}}$ fewer than $n + 1$ will die

The results of my experiments were consistent with the predictions. With modified numerals as in (14), there was neither an effect of the framing manipulation on participants' evaluative ratings of the sure option (rating-task experiment) nor was there a framing effect on participants' choices between the sure and risky option (choice-task experiment). In contrast, with standard descriptions of the sure options, i.e. with bare numerals, there was a significant effect of framing in the rating-task experiment and in the choice-task experiment. These findings provide initial experimental support for the alignment-assumption account. However, they leave open whether people actually act from the alignment assumption. The goal of Experiment C was to gain some insight in this regard.

5.1 Experiment C

In Experiment C, I explored whether people make the proposed alignment-assumption inferences in response to valenced evaluations. Participants were presented with positive and negative evaluations of fictitious states of affairs for which they had no prior knowledge, and then were asked questions on how the speaker of the evaluative statements would evaluate an alternative state of affairs. Examples are given in (16) and (17).

(16) Dr. Karrer says: It is good that 36 of the tested shampoos contained Burarlin.
Would Dr. Karrer find it better if more than 36 of the tested shampoos contained Burarlin?

(17) Dr. Karrer says: It is bad that 54 of the tested deodorants contained Pulontin.
Would Dr. Karrer find it worse if fewer than 54 of the tested deodorants contained Pulontin?

For the example in (16), the alignment-assumption inference from the evaluative assertion is: the more shampoos contain Burarlin the better. Accordingly, if people make alignment-assump-

tion inferences, then a *yes*-response to the question is expected. The alignment-assumption inference from the evaluative assertion (17) is: the more deodorants contain Pulontin the worse. Hence, for the question in (17), a *no*-response corresponds to alignment-assumption inferences.

5.1.1 Method

Experiment C was conducted together with Experiment B. Hence, the participants of Experiment C were the same as those of Experiment B.

The materials of Experiment C comprised the description of a scenario and a series of evaluative statements pertaining to states of affairs from the scenario along with questions relating to the evaluative statements. The scenario was a test laboratory in which twelve test series were conducted to examine whether or not cosmetic products contain particular substances (see (18)).

(18) In einem Prüflabor für Körperpflegemittel und Kosmetika wurden zwölf Testreihen zu verschiedenen Produkten, wie z.B. Duschgele und Wimperntuschen, durchgeführt. Dabei wurde überprüft, ob die getesteten Produkte bestimmte positive und bestimmte negative Substanzen enthalten. Die Testreihen wurden von Frau Dr. Karrer geleitet.

'In a test laboratory for toiletries and cosmetics, twelve test series were carried out for various products, such as shower gels and mascara. It was examined whether the tested products contain certain positive and certain negative substances. The test series were conducted by Dr. Karrer.'

There were twelve trials, eight experimental trials and four filler trials. Each trial started with the description of a result of one of the twelve test series. The description stated the number of tested products of a particular type and indicated how many of the products contained a particular substance. Each description referred to a different product type and a different substance. All substances were fictitious and had pseudo names. A sample description from the experimental trials is given in (19). In all eight experimental trials, the quantity of products that contained the respective substance was indicated with a bare numeral. In the four filler trials, the quantity was indicated with a modified numeral or with the quantifiers *kein-* ('no') or *all-* ('all').

(19) In Testreihe 7 wurden 68 Zahnpasten getestet, von denen 62 Norbuzin enthielten.

'In test series 7, 68 toothpastes were tested, 62 of which contained Norbuzin.'

Each description of a test result was followed by a comment of Dr. Karrer, the conductor of the test series. The comment was a valenced evaluation of the given test results. In the experimental trials, there were two versions of the evaluative statements. The test result was evaluated either as good or as bad. An example is shown in (20). In two filler trials, the test result was evaluated as good and in the remaining two filler trials, it was evaluated as bad.

(20) Dr. Karrer sagt: Es ist gut/schlecht, dass 62 der getesteten Zahnpasten Norbuzin enthielten.

'Dr. Karrer says: It is good/bad that 62 of the tested toothpastes contained Norbuzin.'

In each trial, the task of the participants was to respond to a question on how the speaker of the evaluative statement would evaluate a counterfactual test result. There were 2×2 versions of the question in the experimental trials that differed with regard to valence (better/worse) and with regard to the direction of the counterfactuality (more than/fewer than). A sample question with all four versions is shown in (21). In the filler trials, the question wording was fixed, with the combinations of *besser*/*schlechter* ('better/worse') × *mehr als n*/*weniger als n* ('more than n/fewer than n') being equally distributed across the four filler trials.

(21) Würde es Dr. Karrer besser/schlechter finden, wenn mehr/weniger als 62 der getesteten Zahnpasten Norbuzin enthielten?

'Would Dr. Karrer find it better/worse if more/fewer than 62 of the tested toothpastes contained Norbuzin?'

Participants had to indicate their answer to the questions by choosing one of three response options: *ja* ('yes'), *nein* ('no'), and *keine Vermutung* ('no guess').

The design of Experiment C was a 2×2×2 within-subject design with the factors COMMENT VALENCE (good vs. bad), QUESTION VALENCE (better vs. worse), and QUESTION DIRECTION (more vs. fewer). Participants were randomly assigned to eight groups. The eight conditions resulting from the 2×2×2 design were allotted to the eight items and participant groups according to the counterbalancing schema for complex within-subject designs suggested by Pollatsek & Well (1995: 793). Experimental and filler trials were presented to the participants in eight different mixed orders.

5.1.2 Results and Discussion

Table 6 shows the distribution of responses. For the analyses, *no-guess*-responses were pooled with either the *yes*-responses or the *no*-responses, depending on which of the two response options was inconsistent with the alignment-assumption inference in the respective condition. For example, *no-guess*-responses were pooled with *no*-responses in the condition 'COMMENT VALENCE = good, QUESTION VALENCE = better, QUESTION DIRECTION = more' and with *yes*-responses in the condition 'COMMENT VALENCE = good, QUESTION VALENCE = better, QUESTION DIRECTION = fewer'. The response data were analysed by generalized linear mixed modelling with a binomial logit function and with participants and items as random factors. There were three fixed factors, COMMENT VALENCE, QUESTION VALENCE and QUESTION DIRECTION, all with deviation coding (+.5, -.5). The main effects of COMMENT VALENCE (b = .25, SE = .31, z = .82, p = .41), QUESTION VALENCE (b = -.11, SE = .31, z = -.36, p = .72) and QUESTION DIRECTION (b = -.06, SE = .31, z = .20, p = .84) were not significant and neither were the two-way interactions of COMMENT VALENCE and QUESTION VALENCE (b = -.99, SE = .62, z = -1.59, p = .11) and of QUESTION VALENCE and QUESTION DIRECTION (b = -.15, SE = .62, z = -.24, p = .81). The interaction of COMMENT VALENCE and QUESTION VALENCE (b = -1.37, SE = .62, z = -2.20, p < .05) was significant as well as the three-way interaction (b = -19.75, SE = 1.32, z = -14.99, p < .001).

Table 6. Percentages of chosen responses in Experiment C as a function of COMMENT VALENCE, QUESTION VALENCE and QUESTION DIRECTION

			Yes	No	No guess
good	better	more	94.7%	5.3%	-
		fewer	5.3%	93.4%	1.3%
	worse	more	6.6%	93.4%	-
		fewer	84.2%	11.8%	3.9%
bad	better	more	3.9%	94.7%	1.3%
		fewer	93.4%	3.9%	2.6%
	worse	more	90.8%	6.6%	2.6%
		fewer	10.5%	85.5%	3.9%

To examine the three-way interaction, two separate analyses were conducted. For the data subset in the condition 'COMMENT VALENCE = good', there were no significant main effects (QUESTION VALENCE: b = -.61, SE = .45, z = -1.37, p = .17; QUESTION DIRECTION: b = -.63, SE = .45, z = -1.40, p = .16) and a significant interaction of QUESTION VALENCE and QUESTION DIRECTION (b = -10.09, SE = .97, z = -10.36, p < .001). The distribution of responses by QUESTION DIRECTION in the condition 'QUESTION VALENCE = better' significantly differed from the distribution in the condition 'QUESTION VALENCE = worse' ($\chi^2(1, N = 152) = 114.65, p < .001$). In the condition 'QUESTION VALENCE = better', the vast majority of participants indicated a *yes*-response, when the QUESTION DIRECTION was 'more' and a *no*-response when the QUESTION DIRECTION was 'fewer'. In the condition 'QUESTION VALENCE = worse', the response pattern was reversed.

The distribution of responses in the condition 'COMMENT VALENCE = bad' mirrors the distribution in the condition 'COMMENT VALENCE = good'. The analysis of the data subset in the condition 'COMMENT VALENCE = bad', also yielded no significant main effects (QUESTION VALENCE: b = .39, SE = .44, z = .91, p = .36; QUESTION DIRECTION: b = .75, SE = .43, z = 1.73, p = .08) and a significant interaction of QUESTION VALENCE and QUESTION DIRECTION (b = 9.97, SE = .97, z = 10.35, p < .001). The distribution of responses by QUESTION DIRECTION in the condition 'QUESTION VALENCE = better' significantly differed from the distribution in the condition 'QUESTION VALENCE = worse' ($\chi^2(1, N = 152) = 114.65, p < .001$). In the condition 'QUESTION VALENCE = better', the vast majority of participants indicated a *no*-response, when the QUESTION DIRECTION was 'more' and a *yes*-response when the QUESTION DIRECTION was 'fewer'. In the condition 'QUESTION VALENCE = worse', the response pattern was reversed.

The response pattern observed in Experiment C indicates that participants overwhelmingly agreed with propositions that correspond to alignment-assumption inferences from the given evaluative comments, and that they overwhelmingly disagreed with propositions that were in conflict with the inferences. This holds for comments with positive evaluations (*gut* ['good']) and for comments with negative evaluations (*schlecht* ['bad']). The results of Experiment C provide preliminary evidence for a central supposition of the alignment-assumption account, i.e. that people make alignment-assumption inferences.

5.2 Alignment-Assumption Account: Interim Conclusion

According to the alignment-assumption account (Geurts, 2013) differently framed descriptions have diverging semantic alternatives, e.g. different numbers of either lives to be saved or lives to be lost, or different numbers of either successful projects or unsuccessful projects. The difference in semantic alternatives comes into play when the descriptions are to be evaluated. Geurts proposes that the evaluation of a given proposition systematically reflects how its counterfactual, semantic alternatives would have been evaluated due to inferences following from the alignment-assumption heuristic.[12]

Experimental investigations of the alignment-assumption account are sparse. The findings of my previous experiments (Claus, 2019) on the effect of modification with upward-entailing vs. downward-entailing comparative quantifiers support predictions that were derived from the alignment-assumption account and that do not follow from any other account of framing effects. The findings of Experiment C indicate that people make alignment-assumption inferences. However, the findings stem from a question-response task. Hence, they do not allow for any

[12] Note that a difference in semantic alternatives does not necessarily come along with diverging evaluations. What matters is whether the alignment-assumption inferences for a given evaluation contradict each other.

conclusions regarding the spontaneity of alignment-assumption inferences. It remains to be investigated with methods that are more indirect whether the alignment-assumption is indeed an internalized heuristic.

6 Conclusion

Framing effects have been extensively investigated in psychological and economic research on judgement and decision making. However, their source is not yet settled. In this paper, I took a look at framing effects as effects of linguistic variation and discussed three linguistic accounts.

In the argumentative-orientation account (Holleman & Pander Maat, 2009), framing effects are assumed to emerge from pragmatic inferences on the intended argumentative conclusion as implied by a particularly framed description. There is no clear evidence for a clausal link between argumentative-orientation inferences and framing effects. Besides, there are theoretical issues that cast doubt on the argumentative-orientation account. However, while the account is less adequate in explaining framing effects, it may well prove of value in explaining frame choice, i.e. identifying the use conditions of framing.

In the lower-bound reading account (Mandel, 2014), framing effects are accounted for in terms of numeral interpretation. The findings of experimental tests of the assumption that framing effects are driven by a lower-bound reading of numerals are mixed. Hence, the validity of the lower-bound reading account is currently unsettled. Dismissing the lower-bound reading account at the present stage, would be ill-founded, especially when considering that it is the most parsimonious account of framing effects.

In the alignment-assumption account (Geurts. 2013), framing effects are assumed to be based on diverging evaluative judgments due to diverging sets of semantic alternatives for differently framed descriptions. The alignment-assumption account is backed up by experimental findings. Yet, studies directly relating to the account are rare. It remains to be seen whether it will stand the test in future studies.

In conclusion, it seems that the lower-bound reading account and the alignment-assumption account potentially open up feasible directions towards a valid, linguistic explanation of framing effects.[13] The two accounts represent distinct alternatives to explain framing effects that are not mutually exclusive. The difference between differently framed descriptions due to a lower-bound reading and the difference in counterfactual alternatives and alignment-assumption inferences may both contribute to the emergence of framing effects, either in combination or inter-individually.

Both accounts carry implications on effects of numeral modification, albeit with different underlying rationales. Consider, for instance, numeral modification with *at most*, for which both accounts predict a reversed framing effect in decision problems like the deadly-disease scenario, i.e. that the sure option is dispreferred in the positive frame and preferred in the negative frame. This prediction follows from the lower-bound reading account because *at most* is upper-

[13] Both accounts can readily explain that in studies employing fully explicated descriptions of the sure option (see (i)) no framing effect was found (e.g. Kühberger, 1995; Mandel, 2014). The differently framed variants of fully explicated descriptions do not differ in the variables considered decisive in the two accounts. They have identical outcomes in terms of number of lives saved and lives lost, and it holds for both description variants that a positive or negative evaluation of the outcome as a whole involves contradictory alignment-assumption inferences for the two conjuncts.

(i) If Program 1 is adopted, a. [200 people will be saved, and 400 people will not be saved].
b. [400 people will die, and 200 people will not die].

bounded, and it follows from the alignment-assumption account because *at most* is downward entailing. By a similar token, the finding of a framing effect when the numerals are modified with *exactly* (Chick et al., 2016) is not only challenging for the lower-bound reading account but also for the alignment-assumption account. It cannot be explained in terms of alignment-assumption inferences because *exactly* is non-monotone. As with *exactly*, neither the lower-bound reading account nor the alignment-assumption account predicts a framing effect for numeral modification with *approximately*.

It is an empirical task to validate the two accounts' implications regarding numeral modification. Note, however, that though both accounts ultimately explain framing effects in terms of diverging evaluations of differently framed descriptions, neither of them traces framing effects directly back to immediate appraisals of affective valence and/or goal consistency. What is lacking, from a theoretical perspective, is a parsimonious semantic-pragmatic account of the interpretation and evaluation of complex expressions, such as differently framed descriptions with bare or modified numerals. Investigating semantic and pragmatic aspects of appraisal as a source of framing effects may help to bridge the gap between psychological and linguistic research on framing effects.

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