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The Role of Non-Actuality Implicatures in Processing Elided Constituents

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Abstract

When an elided constituent and its antecedent do not match syntactically, the presence of a word implying the non-actuality of the state of affairs described in the antecedent seems to improve the example (*This information should be released but Gorbachev didn't.* vs *This information was released but Gorbachev didn't.*) We model this effect in terms of Non-Actuality Implicatures (NAIs) conveyed by non-epistemic modals like *should* and other words such as *want to* and *be eager to* that imply non-actuality. We report three studies. A rating and interpretation study showed that such implicatures are drawn and that they improve the acceptability of mismatch ellipsis examples. An interpretation study showed that adding a NAI trigger to ambiguous examples increases the likelihood of choosing an antecedent from the NAI clause. An eye movement study shows that a NAI trigger also speeds online reading of the ellipsis clause. By introducing alternatives (the desired state of affairs vs. the actual state of affairs), the NAI trigger introduces a potential Question Under Discussion (QUD). Processing an ellipsis clause is easier, the processor is more confident of its analysis, when the ellipsis clause comments on the QUD.

Keywords

ellipsis; syntax; semantics; implicatures; parallelism; sentence acceptability; question under discussion

The nature of the relationship between an elided phrase and its antecedent has been widely discussed in the linguistic literature (Fiengo & May, 1992, Merchant, 2001, Sag, 1976, Sag & Hankamer, 1984, Williams, 1977, and many others) and the psycholinguistic literature (Garnham & Oakhill, 1987, Garnham, Oakhill, & Crain,1998, Martin and McElree, 2008, Mauner, Tanenhaus & Carlson, 1995, Tanenhaus & Carlson, 1990, Yoshida, Dickey and Sturt, 2011, for example). The basic issue is whether the antecedent for an elided phrase is a syntactic object or a semantic object. If syntactic, then one expects the antecedent and elided constituent to match syntactically, apart from morphological features. However, if the antecedent is semantic, for example just a salient property (Hardt, 1993, Dalrymple et al., 1991), then there is no reason to expect syntactic matching between antecedent and elided phrase. In the context of this debate, the existence of ellipses with mismatching antecedents is of considerable theoretical interest.

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One argument for the antecedent being a syntactic object is that most mismatches in grammatical voice between an ellipsis site (indicated by Δ) and its antecedent are at best very degraded in acceptability. For example, VP ellipsis in (1b) seems dramatically worse than in (1a) because the elided VP does not match in voice with the only available antecedent.

(1) **a.** Fred kicked John and Bill did Δ too. (Δ = kick John)

b. # John was kicked and Bill did Δ too. (Δ = kick John)

If sentence (1b) is ungrammatical, then its unacceptability is not surprising.

However, there are attested examples of sentences that are relatively acceptable, despite having precisely the voice mismatch that makes (1b) so degraded (Dalrymple et al. 1991, Hardt 1993, 2005, Kehler 2000, 2002). Example (2), from Hardt (1993), is one much-cited example.

(2) This information coul been released by Gorbachov, but he chose not to Δ (Hardt 1993: 131)1

Δ = release this information.

Some have taken the existence of acceptable voice mismatches in ellipsis to refute the claim that a grammatical condition requires an elided constituent and its antecedent to match syntactically (see references above). But then some account must be given for why mismatch ellipsis is often very unacceptable. Another approach (Merchant, 2007, 2008) is to assume that Verb Phrase Ellipsis (VPE) involves a structure where voice is not specified and assume there is some other explanation for the unacceptability of (1b). Yet another approach, pursued here, is to assume the Recycling Hypothesis, the claim that the grammar does indeed require syntactic matching between antecedent and elided constituent but that sentences which violate the matching constraint may be repaired and even judged acceptable under certain conditions. Acceptability is enhanced when the reader or listener has a reason to expect that the mismatch may have resulted from a speaker error, when the speaker's intended message is apparent, and when the error is easy to repair because it involves only a small number of well-motivated or clearly evidenced syntactic operations, and the speaker's intent is clear (Arregui et al., 2006, Frazier & Clifton, 2011, Frazier, 2008). For instance, a mismatch between antecedent and elided constituent might result from the speaker misremembering a more marked or more complicated antecedent clause as a less complicated or less marked clause, such as misremembering a passive antecedent clause as an active. This predicts an asymmetry in the acceptability of mismatch ellipsis which has been confirmed experimentally (Arregui et al, 2006). In general, the proposed approach to mismatch ellipsis falls into the general domain of reversing predictable speech errors in comprehension. An example of reversing a predictable speech error that has been studied experimentally (Frazier & Clifton, 2011) is that of undoubling words that have been repeated or doubled, as when a reader interprets a sentence like *Many people often thought that you use whipped cream pie* as if it had only one of the two quantifiers *many* and *often* (quotation taken from a National Public Radio discussion of clowns and pie throwing).

There are several existing theories regarding the grammatical status of voice mismatches, but there is so far no satisfactory account of why some voice mismatches are relatively acceptable while others are so degraded. Kehler (2000, 2002) provided one elegant attempt

[1]The auxiliary *could* in this example contains an ambiguity between an ability modal and an epistemic modal, which complicates its analysis. Intuitions indicate that substituting the clear deontic modal *should* removes the ambiguity but results in a similar amelioration of the syntactic mismatch. The account to be offered in this paper applies to the original *could*, but applies more 'crisply' to the unambiguous *should*.

to account for voice mismatches using a general theory of discourse coherence relations that has been successful in other domains, including pronoun reference (Kehler et al., 2008, Rohde, Levy & Kehler, 2011). Kehler proposed that the pattern of acceptable mismatches reflects the discourse coherence relationships between antecedent and ellipsis clauses. When a clause containing an elided VP is in a *Cause-Effect* or *Contiguity* relationship with the clause containing the antecedent, Kehler claimed that the antecedence relationship is purely semantic and does not require syntactic identity. However, when the two clauses are in a *Resemblance* relation, there are syntactic constraints on identity between an elided VP and its antecedent (presumably because in order to establish the similarities or contrasts that underpin Resemblance relations it is often necessary to identify sub-clausal constituents). Example (3a) shows an attested voice mismatch that Kehler claims is acceptable due to the Cause-Effect relation between the antecedent and ellipsis clause. Kehler argued that example (3b) is not acceptable because the clauses are in a Resemblance relation, and therefore require syntactic parallelism.

(3) **a.** This problem was to have been looked into, but obviously nobody did Δ. (Kehler 2002: example (4) page 5)

Δ = look into the problem

b. # This problem was looked into by John, and Bob did Δ too. (Kehler 2002: example (5) page 5)

Δ = look into the problem

While the difference in acceptability between (3a) and (3b) is obvious, there is evidence that it should not be attributed to discourse coherence relations. Frazier and Clifton (2006) presented evidence counter to Kehler's theory of ellipsis, showing a penalty for a syntactic mismatch between antecedent and elided constitutent not only for Resemblance coherence relations but also for Causal coherence relations. They explicitly manipulated sentences that varied only in the connective used to conjoin clauses (*because, even though* vs. *just like*) and found that a causal connective did not increase comprehensibility judgments for ellipsis examples with voice mismatches compared to a resemblance connective (even when comprehensibility of voice-matching examples were equated). This directly disconfirms Kehler's (2002) coherence theory of ellipsis, which predicts mismatches are grammatical for examples with causative relations (though obviously it does not bear on the question of whether discourse coherence theory offers a correct account of processing anaphora.) Kim and Runner (in press) conducted a similar experiment with similar results when the antecedent was passive and the ellipsis was active (as in Frazier & Clifton, 2006). Kim and Runner did find that the unacceptability of passive elliptical clauses with an active antecedent was modestly reduced when the relation was Cause-Effect, compared to Resemblance, though crucially a significant mismatch penalty was observed for Cause-Effect relations. Kertz (2011) found that acceptability was reduced by the same amount for mismatching (compared to matching) elliptical sentences when the connective expressed a Cause-Effect relation as when it expressed a Resemblance relation (with no apparent effect on their non-elliptical counterparts).

Despite these findings, some property of the Cause-Effect sentences that Kehler examined (e.g., (3)) clearly reduced the need for syntactic parallelism. Kertz (2008) suggested that it was not their discourse relations, but instead, their information structure2, a factor that may

[2]Although Kertz (2008) has been interpreted as defending Kehler's position against the Frazier and Clifton (2006) criticisms, she actually proposed a third alternative: "The critical difference, I propose, between the cases of acceptable and unacceptable ellipses examined so far is not syntactic structure, and not discourse structure, but information structure: cases of unacceptable mismatch tend to focus the subject argument of the target [elliptical] clause, while cases of acceptable mismatch instead focus the auxiliary verb" (Kertz 2008, p 284).

have played a role in the difference that Kim and Runner found between active and passive ellipses. She claimed that when an argument of the elliptical phrase rather than its auxiliary verb contrasts with earlier material, structural parallelism between antecedent and elided constituent is particularly important. Kertz investigated sentences like those in (4) in a written rating study. She found that the penalty for having a syntactically non-parallel ("mismatch") antecedent was less when there was a presumed focus on the auxiliary *can* (4d) than when the focus was on the argument *poisonous plants* (4b).

(4)

a. Venomous snakes are easy to identify, and poisonous plants are as well. (Arg focus, Parallel)

b. It's easy to identify venomous snakes, and poisonous plants are as well. (Arg focus, Non- Parallel)

c. It's easy to identify venomous snakes, and most experienced hikers can. (Aux focus, Parallel)

d. Venomous snakes are easy to identify, and most experienced hikers can. (Aux focus, Non-Parallel)

Kertz (2008) proposed that the preference for syntactic parallelism is best explained not as a grammatical constraint but as an information structure constraint "enforcing the syntactic alignment of contrasting arguments" (p. 286). When it is arguments that contrast (*venomous snakes, poisonous plants*), parallelism will be especially important, more important than in sentences where the auxiliaries contrast (*is* and *can* in (4c, d)).

In the present paper, we assume that elliptical sentences with a syntactically nonparallel antecedent are fundamentally ungrammatical (see Arregui et al, 2006), and explore one property that systematically increases the acceptability of mismatch ellipsis. This property is a specific, previously unstudied, type of conversational implicature that could be responsible for the increase in acceptability of some examples of mismatching ellipsis, including examples like (3b). These implicatures, which we will call *Non-Actuality Implicatures* (henceforth NAIs), implicate3 that the state of affairs described does not hold in the actual world. Consider (5):

(5)

a. A party was planned for July 4th.

b. A party should have been planned for July 4th.

Semantically, sentence (5a), with an indicative tense, asserts that a party was planned. Because (5a) asserts that a party was planned for July 4th,, it would be a contradiction to follow (5a) with *but a party was not planned for July 4th*. By contrast, sentence (5b), which contains the modal *should*, doesn't have an actuality entailment and it could easily be followed by *but a party was not planned for July 4th* without resulting in a contradiction. Pragmatically, we assume, (5b) has the Non-Actuality Implicature that a party wasn't planned. Of course, as with all conversational implicatures, the implicature is cancellable, and the utterance could continue *John was supposed to go to Paris … and he did.*

We will investigate NAIs introduced by triggers such as *is supposed to, needs to, wants to*, and *is eager to*, and also modals such as *should* and *must* (see von Fintel, 2006 on the semantic analysis of modals). A heterogeneous set of modals and other constructions seem to trigger NAIs, including future oriented predicates like *want, is eager to*, and non-epistemic modals such as *needs, is supposed to*, and *should*. The relation between the future

[3]Our claim is that a nonactuality implicature is sufficient to improve the status of mismatch ellipsis, though a nonactuality entailment, as in some Experiment 2 items with *would/could/should have*, also does. In both cases, the idea is that introducing alternatives to the actual world suffices to introduce a potential question under discussion (even though the formal status of the alternatives differs.)

orientation of predicates like *want* and the fact that they give rise to NAI is unclear. Although we assume (in the General Discussion) that NAIs are not scalar, in fact this requires further investigation. In principle, statements containing an NAI trigger might compete with stronger statements asserting that the state of affairs in the complement of the trigger holds in the actual world, e.g., *John wants to be in Paris* might compete with *John is in Paris*. For present purposes, it does not actually matter whether NAIs arise through scalar reasoning, as a result of applying the principle of Quantity (Grice, 1975), or whether the NAI arises in some other way. In any event, we provide normative evidence that the expressions we use in the experiments to be reported do support an NAI.

We propose that an NAI will facilitate mismatch ellipsis because the NAI implicitly contrasts with the description of the actual state of affairs, and thereby implicitly focuses on contrast between the state of affairs described and the actual state of affairs. For example, in the sentence *John wanted to leave, and he did*., the verb *wanted* introduces an NAI and the implicit Question Under Discussion (QUD) '*Did John leave?*' (see below for discussion of the concept of QUD) the ellipsis clause should then comment on the QUD, as captured by the NAI hypothesis in (6) .

(6) NAI hypothesis: The presence of an NAI will set up an alternative which implicitly focuses the antecedent clause, and sets up a potential QUD, thereby motivating and guiding the processing of a later clause which comments on the QUD, especially when the antecedent of an ellipsis needs to be repaired.

The NAI hypothesis predicts that mismatch ellipsis will be more acceptable when there is an NAI, because the alternatives introduced by the NAI will implicitly focus the antecedent making the antecedent easier to identify and therefore repair. Further, confidence in the interpretation of the ellipsis clause will be high when the ellipsis clause can resolve the QUD (whether there is a form mismatch between antecedent and elided constituent or not). The NAI hypothesis also predicts a preference for choosing an antecedent that allows the elided clause to comment on a QUD, due to the salience of the antecedent (the implicit focus) and the desire for an utterance to comment on the QUD. If, as we expect, the mechanism underlying the effect of an NAI applies on-line, it should be possible to observe its effects during the normal reading of a sentence. After discussing the notion of a 'QUD,' we present experimental evidence relevant to each of these predictions.

Question under Discussion (QUD)

The central goal of the studies presented here is to provide evidence that NAIs are one source of facilitation in processing voice mismatches. At a more theoretical level, we propose that the effect of an NAI can be understood in terms of the role of focus in determining the implicit 'QUD' (QUD; Roberts, 1996, 2004, cf. Beaver & Clark, 2008) and constraining likely discourse continuations. Roberts' basic idea is that the discourse proceeds by continually raising and answering implicit questions. The possible answers to these questions are the proffered alternatives. If the question is accepted by the interlocutor, it becomes the current QUD.

The start of a discourse may be understood as posing the most general question "What is the way things are?" Or, in a well-studied type of example (Cooper, et al., 2000), a travel agent might ask an overt question "Where would you like to go?" The client might answer "Madrid. There will be two of us." In this way, the current QUD is answered and a new implicit QUD was introduced ("How many of you will be traveling?") In addition to overt questions and implicit questions identified from the content of their answers, implicit questions may be identified by the presence of a focus that introduces alternatives. In simple cases of focus such as *BILL left*, the focus on *Bill* presupposes that the QUD is the question *Who left?*, whose meaning is generally analyzed as a set of alternative answers of the form*:*

x left. In the case of an NAI, a non-actual state of affairs presents an alternative to the actual world. For example, in (7a), the antecedent *John wanted to go to Paris* implicates that John did not, in fact, go to Paris. No such implicature is present in (7b). Like explicit focus, we suggest that an NAI often introduces a QUD concerning the actuality/non-actuality of the state of affairs described, thereby setting up an expectation for the continuation shown which may be construed as an answer to the QUD. (For a discussion of the relation between the QUD approach and the coherence approach to discourse structuring, especially anaphora resolution, see Rohde, 2008).

(7) **a.** John wanted to go to Paris, … and he did. / but he didn't.

b. John went to Paris, … # and he did. / # but he didn't.

We propose that the QUD set up by an NAI is the fundamental source of the effects we explore. It is one of the factors, discussed earlier in connection with the Recycling Hypothesis, that enables or encourages a reader or listener to recycle a mismatching antecedent of an ellipsis. Quite simply, the predictability of an ellipsis clause that would comment on the QUD may serve to rescue a less-than-perfect ellipsis. We recognize that the constraint on the likely continuation of discourse introduced by the NAI/QUD, and the attendant expectations for (e.g.) confirmation or cancellation of the NAI, might also facilitate comprehension of non-ellipsis sentences or ellipsis examples with matching antecedents. However, our particular interest concerns the role of an NAI/QUD in increasing the acceptability of mismatch ellipses, since they, in contrast to non-ellipsis or matching-ellipsis sentences, need to be repaired or rescued.

Experiment 1a was designed to determine whether the NAI disproportionately increases the acceptability of sentences with syntactically mismatching antecedents of elided verb phrases compared to ones with matching antecedents, a prediction of our claim that ellipsis without a matching antecedent is technically ungrammatical (and on the present assumptions, must be rescued). Experiment 1b further tested the NAI hypothesis, along with the possibility that any modal increases the acceptability of a mismatching ellipsis whether it carries an NAI or not. Experiment 2 tested ellipsis with two permissible antecedents to determine if adding an NAI trigger to one possible antecedent clause increases the probability of choosing the antecedent from that clause. It should if the NAI introduces a QUD. Experiment 3 examined whether an NAI speeds reading of an ellipsis clause in a comprehension study without any need for judgments of acceptability, or a need to choose among possible interpretations of the sentence. This eye movement study is particularly important given that it has not previously been established whether the type of implicature investigated here, NAIs, are drawn online during the incremental processing of a sentence. Further, if our QUD-based approach is on the right track, Experiment 3 provides an initial test of whether the QUD influences language comprehension during the ongoing analysis of a sentence or discourse.

Experiment 1a

Experiment 1a was designed to address the question of whether there is facilitation due to the presence of an NAI in the antecedent clause, and whether such facilitation is limited to mismatching examples. To pursue this question, Experiment 1a examined the effect of NAIs both in examples with mismatching antecedents and examples with matching antecedents, as illustrated in (8).

(8) **a.** This information was released but Gorbachev didn't. (passive-active)

b. This information should be released but Gorbachev didn't. (NAI, passive-active)

c. Someone released this information but Gorbachev didn't. (active-active)

d. Someone should release this information but Gorbachev didn't. (NAI, active-active)

Methods

Materials—Twenty sentences like those in (8) were constructed. There were four forms of each sentence, representing the factorial combination of passive vs. active antecedent clause (mismatching vs. matching antecedent, since the ellipsis was always active (8a–b vs. 8c–d)) and absence vs. presence of an NAI-creating modal expression (*should be* or *is supposed to be*; 8a,c vs. 8b,d). A five-point acceptability rating scale was presented with each item, with 1 = “unacceptable,” 5 = “fully acceptable”. All items appear in Appendix 1. An additional 104 sentences of various forms (fillers and other experiments) were also written. Of these 104 sentences, 64 required a 5-point rating scale, and the remainder were queried with a two-choice interpretation question. A practice list of 6 sentences was used in Experiment 1a.

Validation norms—To ensure that the modal in items like (8b, d) actually did induce an NAI, we collected normative data on 48 University of Massachusetts undergraduates. A computer presented the initial clauses of the 20 passive modal sentences used in Experiment 1a, followed by a 2-choice interpretation question (e.g., a subject would see *This information should be released*. Followed by a question-answer line like: *Was the information already released? 1. It probably was. 2. It probably wasn't*). The subject was to press the number key corresponding to the ‘correct’ answer. These items were presented, randomly ordered, with a total of 90 other questionnaire items that served as fillers. The “probably was” answer was chosen on 11% of the trials (SE = 1%). Thus, the NAI appeared to be made approximately 89% of the time.

Subjects and Procedures—Forty-eight University of Massachusetts undergraduates, all native speakers of English and none who participated in the validation norming, were tested in individual half-hour sessions. They were instructed that they would see sentences and questions, one at a time, on a computer monitor, using locally-written software. A sentence and its question and rating scale or answers appeared together on the screen. When the question was "How acceptable was that sentence?" subjects were to press a number key 1–5 corresponding to their judgment of whether "it makes sense and is something you could easily imagine yourself or another native speaker of English saying or hearing and not noticing anything odd about it." They were encouraged to use the entire 5-point scale. When a sentence was followed by the question "What did that sentence mean?" accompanied by two numbered answers, they were to press the 1 or the 2 key to indicate their choice of the best paraphrase. Each participant saw six practice items followed by the test items in an individually-randomized order.

Results

The mean acceptability ratings appear in Table 1. The data were analyzed using linear mixed-effects models with two fixed effect factors (NAI absence vs. presence, syntactic match vs. mismatch) (Baayen, Davidson, & Bates, 2008). We used model comparison to determine whether random slopes for each factor and their interaction improved model fit, and report the simplest justified model. Treatment coding with the NAI absence, syntactic match as the baseline was used. Where random slopes were used, we interpret a t-value of absolute value 2 or greater as significant. The model parameters appear in Appendix 2. For Experiment 1a, the interaction between the two fixed effect factors was significant (t = 6.34). The effect of each of these factors was significant at the baseline (NAI presence

lowered ratings for the active-active match items; passive-active mismatch lowered ratings when NAI was absent = −3.72 and −13.55, respectively). It is clear that the NAI increased acceptability of the Passive-Active Mismatch cases, but decreased acceptability of the Match cases.

Discussion

The results of Experiment 1a indicate that an NAI improved the status of ellipsis examples but only those with a mismatching antecedent. The effect reversed for examples with a matching antecedent. Sentences with mismatching antecedents were judged as less acceptable than those with matching antecedents, even with an NAI. This pattern of results is expected if a mismatching antecedent must be repaired, as specified by the Recycling Hypothesis of Arregui et al. (2006), and if repairs are easiest and most likely to be carried out when there is good evidence of the speaker's intent, as there is when the ellipsis clause (the clause containing the elided constituent) comments on a/the QUD. When no repair is needed (in the match cases), the modal actually appeared to lower the acceptability of the NAI sentences. This may be due to the extra complexity introduced by the modal, or to the slightly-odd information structure of the parallel-NAI sentences (e.g., in *Someone should release this information but Gorbachev didn't*, there may be a clash between the tendency to put focus on the phrase-final *didn 't* and the tendency to put focus on the agent *Gorbachev* because of its contrast with the possibly-focused subject *Someone*).

Experiment 1b

Experiment 1b, like Experiment 1a, tested the central prediction of the NAI hypothesis that examples of ellipsis with mismatching antecedents would be judged more acceptable when they convey an NAI than when they do not. Twelve sentences like (9), which we will refer to as Type I items, were constructed. The first clause was passive, and the second was active (a syntactic mismatch on our assumptions). There were two versions of each example. The a-version did not convey an NAI. The corresponding b-version contained a modal phrase (*should be, must be, or needed to be*) conveying an NAI.

(9) **a.** This information was released but Gorbachev didn't.

b. This information needed to be released but Gorbachev didn't.

In addition to the Type I items like (9), Experiment 1b tested items like (10), Type II items. The Type I items present the sharpest manipulation of the presence of an NAI. However, this manipulation is confounded by two factors that could possibly affect acceptability. The NAI form of Type I items (9b) have a modal expression (*needed to be* in the example) that the non-NAI forms (9a) lack and the non-NAI forms of Type I items (9a) assert the actuality of the event described. The Type II items eliminate both of these confounds: A modal was included in both the non-NAI (10a) and the NAI (10b) forms of Type II items, but the modal used in the non-NAI form, *may*, does not imply either the actuality or non-actuality of its complement. It asserts the possibility of its complement. . The NAI forms of Type II items (10b) contained a modal that conveyed a non-actuality implicature, as in the NAI form of Type I (9b) sentences.

(10) **a.** The cookies may have been made, but the babysitter didn't.

b. The cookies needed to be made, but the babysitter didn't.

Methods

Materials—Twelve pairs of Type I sentences that contrasted in the absence or presence of a non-actuality implicating modal, as illustrated in (9), were constructed, together with 16 pairs of Type II sentences with a modal phrase that did not or did carry an actuality-relevant

implicature, as in (10). More Type II than Type I sentences were constructed because of our concern that any difference in the Type II sentences would be less robust, and we wanted to obtain a more precise estimate of any such difference. The five-point acceptability rating scale used in Experiment 1a was again used, with 1 = “unacceptable,” 5 = “fully acceptable”. All items appear in Appendix 1.

These 28 sentences were combined with 88 other sentences, filler items and items from unrelated experiments. Of these, 30 were presented with the 5-point acceptability rating scale used for the experimental items. The remaining 58 sentences were presented with two-choice interpretation questions. A six-item practice list was also made up, with three acceptability judgment and three interpretation items, using materials that were not related to any of the experimental items.

Validation Norms—As was done in Experiment 1a, the frequency of making the NAI was assessed for both the Type I and the Type II sentences of Experiment lb. Forty-eight 48 University of Massachusetts undergraduates were presented with the initial clauses of the 12 Type I and the 16 Type II sentences, followed by two-choice questions about whether or not the event mentioned in the first clause had probably already occurred (phrased the same way as in the Experiment 1a norms). A given subject saw all 12 of the Type I items in their modal form, and saw half of the Type II items with the non-NAI modal (e.g., (10a) and half with the NAI modal (10b). Assignment of items to conditions was counterbalanced over subjects, so that normative judgments were collected on both the NAI and the non-NAI forms of all the Type II items. The 28 items from this normative assessment were randomly intermixed with a total of 105 filler items and items from other experiments.

A “probably was” (non-NAI) answer was given to the Type I sentences 20% of the time (SE = 1.5%) and to the Type II sentences with an NAI modal 18% of the time (SE = 1.8%). Thus, subjects with a modal intended to support an NAI made an NAI over 80% of the time. In contrast, the “probably was” answer was given to the Type II sentences with a non-NAI modal 67% of the time (SE = 2.2%); the NAI was apparently made only 33% of the time to the modal sentences intended not to support an NAI. We continue to refer to the items with “may” as non-NAI items, but acknowledge that they may simply support an NAI much lower frequency or certainty than our NAI items.

Subjects and Procedures—Forty-eight University of Massachusetts undergraduates, all native speakers of English (as was the case in all experiments reported here), were tested in individual half-hour sessions. They were instructed as in Experiment 1a, and the procedures used in Experiment 1a were used in Experiment 1b (although the experiments were run at different times).

The six practice sentences and then the 116 test sentences were presented in an individually-randomized order. The sentence and the question appeared on the monitor screen at the same time, and remained on the screen until the subject pressed an appropriate number key. The computer recorded the subject's response (and decision time, although there were no interesting differences in times, so they are not reported here).

Results

The mean ratings appear in Table 2. The data were analyzed as a linear mixed model, with the presence vs. absence of NAI and Type I vs. Type II items as fixed effect factors and subject and item intercepts and slopes for NAI and Type as random factors (Baayen, Davidson, & Bates, 2008). Treatment coding was used, with the no-NAI, Type I condition as the baseline. The parameters of linear mixed models reported in this paper appear in Appendix 2. The interaction between the two fixed effect factors was significant ($t = -2.18$),

indicating that the apparent advantage conferred by an NAI was greater for Type I than Type II items. The primary linear mixed model indicated that the effect of NAI was significant at the baseline Type I items (t= 5.79), and an auxiliary linear mixed model fitted to just the Type II modals indicated that the NAI advantage was also significant for them (t = 3.55). Finally, Type II items received higher ratings than Type I items at the No-NAI baseline (t = 2.53).

Discussion

As expected according to the NAI hypothesis, the examples with an NAI were rated significantly more acceptable than their counterparts without an NAI. Further, the effect could not be (entirely) due to the absence of an actuality entailment since the Type II sentences, which lacked the actuality entailment but still contrasted in the presence or absence of an NAI, also showed a significant effect. We thus take the results as evidence for the NAI hypothesis.

Experiment 2

As noted in the introduction, by implicitly contrasting the actual world and the state of affairs described in the clause that contains it, an NAI may introduce a possible or implicit QUD. If a/the QUD concerns whether some state of affairs holds, it may make certain continuations pragmatically likely. This relation between the QUD and the preferred interpretation of ellipsis is spelled out in (11):

(11) Non-Actuality Implicature ambiguity resolution hypothesis:

- **a.** An NAI sets up alternatives involving a non-actual state (e.g., x wanting to be in Paris) and the actual state of affairs (e.g., x not being in Paris). These alternatives raise an issue and thus suggests an implicit question under discussion.
- **b.** An NAI often constrains the expected continuation of the discourse, since discourse should answer or comment on the question under discussion.

In general, material that is focused, and thus introduces alternatives, makes a better antecedent for an ellipsis than material that is not (Carlson et al., 2009; Frazier, Clifton, & Carlson, 2007). If NAIs facilitate antecedents through an implicit focusing mechanism (the introduction of alternatives, even if they are not alternatives in the sense of focus semantics, Rooth, 1992), then in sentences with two possible matching antecedents, the presence of an NAI in one clause should induce readers to choose the antecedent in that clause more often than they do without the NAI present. This expectation was tested in Experiment 2. We constructed two sentence mini-discourses like those in (12).

(12)

- **a.** The supervisor chastised a worker who brought production to a halt. Fred did recently.
- **b.** The supervisor chastised a worker who would have brought production to a halt. Fred did recently.

The first sentence of the experimental items always contained a relative clause modifying the object of the matrix clause in the first sentence. The second sentence contained an elided verb phrase which could take either the matrix VP (*chastised a worker who…*) or the relative clause VP (*brought production to a halt/would have brought production to a halt*) as its antecedent. The b-forms contained an NAI in the relative clause VP. We predicted that this NAI would increase the frequency with which interpretations of the ellipsis in the

second sentence took the relative clause VP (*brought production to a halt* in (12)) as its antecedent.

Methods

Materials—Twenty-two two-sentence discourses were constructed in two forms each, as illustrated in (12). The NAI (b) forms contained a modal which carried a non-actuality implicature; the modal *would have* appeared most frequently, but *could have*, *should have*, *wanted to*, *almost*, and *eager* were also used. See Appendix 1 for all experimental items.

Two forms of a 22-item written questionnaire were created, in each of which half of the 22 sentences appeared in the non-NAI (12a) form and half in the NAI (12b) form in a counterbalanced fashion. Each item was followed by a two-choice question, e.g., following (12),

___ Fred recently chastised a worker.

___ Fred recently brought production to a halt.

A single randomization was made of each form and printed.

Subjects and procedures—Thirty University of Massachusetts undergraduates completed the written questionnaire working individually (half receiving each form) and marking their interpretations on a copy of the printed form. They were instructed to read a sentence, form a quick intuitive impression of what it meant, and check the following alternative interpretation that came closest to their own interpretation.

Results

Fifty-three percent of the simple past tense, no-NAI, sentences (12a) received an interpretation in which the antecedent of the ellipsis was the embedded, relative clause VP (*Fred recently brought production to a halt*). In contrast, a greater percentage (62 percent) of the modal, NAI (12b) sentences received such an interpretation. The results were analyzed statistically using a mixed-effects logistic regression (Jaeger 2008). This analysis showed that the presence of NAI was a significant predictor in the choice of interpretation ($z = -2.55$, $p = .011$). Parameter estimates for the mixed-effects model are shown in Appendix 2. Mixed-effect analyses showed that while the log odds of an embedded choice for the simple past tense, no-NAI, sentences were not greater than 1 (i.e., 50% choice; $z = 0.60$, $p > .50$), they were greater than 1.0 for the modal, NAI, sentences ($z = 3.61$, $p < .001$).

Discussion

As predicted, there were significantly more relative clause antecedent choices in Experiment 2 for the elided VP when the relative clause conveyed an NAI than when it did not. As noted earlier, the implicit contrast between the state of affairs described in the sentence and the actual world may serve to introduce alternatives and thereby focus the clause carrying the NAI (12b). By hypothesis, these alternatives introduce a QUD, which is the question answered or commented on by the second sentence. If perceivers expect material following a clause with an NAI to answer an implicit question or explain why, say, the desired or required state of affairs described was not attained, this might result in a preference for the antecedent which introduced the NAI.

QUD Assuming that the presence of an NAI trigger introduces an implicit focus, by introducing alternatives, and that the alternatives influence discourse integration/ predictability might explain why the NAI tended to hurt matching-ellipsis examples like (8d) in Experiment 1a (e.g., *Someone should release this information but Gorbachev*

didn't.). Plausibly the most expected continuation of the first clause would be something like *but nobody did* (an instance of a contrast relation). Given that the speaker has concentrated on Gorbachev, implying that he was the person in a position to release the information or the person most responsible, intuitively it seems as if the conjunction *because* would have been a better choice than *but*.

Notice that our account of NAIs locates the basic effect in the antecedent clause and not primarily in the ellipsis clause (though an indirect effect in the ellipsis clause could come about due to parallelism). Clifton and Frazier's (2010) Experiment 2 results provide some support for this claim, showing that a nonactuality-entailment inducing modal in the antecedent of an ellipsis improved acceptability while one in the ellipsis itself did not. Intuitions about the effects of a negation may also support it. Consider (13) and (14). The negation in the first clause may have the effect of introducing alternatives and thus behaving much like the cases of NAI we have discussed. In (13) this means that the first clause negation might help the mismatching ellipsis whereas the presence of negation in the ellipsis clause as in (14) might not help as much.

(13) The garbage wasn't taken out but Sally will.

(14) The garbage was taken out but Sally didn't.

Experiment 3

Experiment 1a showed an NAI in the antecedent clause improves the status of mismatch ellipsis examples. Experiment 1b showed that the NAI effect cannot be attributed just to a penalty for examples without a modal or to a penalty in examples with an actuality entailment, since adding an NAI-triggering modal to an antecedent clause improves examples more than adding a nonNAI-triggering modal. Experiment 2 showed that adding an NAI (or a nonactuality entailment) to a clause also increases the probability of choosing an antecedent in that clause. Experiment 3 used eye tracking to determine whether NAIs are computed on-line.

It can be important to know whether a psycholinguistic effect is an on-line effect or is limited to situations in which readers are asked to reflect on the acceptability or interpretation of sentences. This is particularly true when the effects implicate inferential processing, which may not be tied as closely to particular portions of the sentence as lexical, syntactic and compositional semantic processes may be (see Sauerland & Yatsushiro, 2009 for various overviews of implicature processing). Experiment 3 examined the timing of the effect that a modal or the presence of an NAI has on the interpretation of a sentence, using the methodology of eye tracking during reading. If the enhanced acceptability of elliptical sentences with a mismatching antecedent when they contain an NAI is the result of processes that take place during normal sentence comprehension, then reading of such sentences may be facilitated compared to sentences without an NAI.

Methods

Materials—Twenty pairs of sentences similar to those used in Experiment 1 were constructed. Each sentence occurred in an actuality entailment form and an NAI form, and each had a neutral continuation so that the sentence did not end right at the ellipsis. An example appears in (15); all sentences appear in Appendix 1. The / marks indicate analysis regions, as indicated later. Half the items had *was* in the no-NAI form and half had *may have been*, to provide generality to both the Type I and the Type II forms studied in Experiment 1b.

(15) **a.** The information was released $/_1$ but Gorbachov didn't, $/_2$ even though $/_3$ lives were clearly at stake. $/_4$

b. The information needed to be released /$_1$ but Gorbachov didn't, /$_2$ even though /$_3$ lives were clearly at stake. /$_4$

These 20 sentences were combined with 68 sentences from other experiments plus 5 practice sentences. Simple comprehension questions were made up for approximately 1/3 of the items. The 20 experimental sentences were divided into two groups to be presented in a counterbalanced fashion, so that each subject saw 10 sentences in each of the versions illustrated in (15) and each sentence was tested equally often in each form across the experiment.

Subjects and procedures—Thirty members of the University of Massachusetts community were tested in sessions that typically lasted one-half hour. Each read sentences presented on a computer monitor and their eye movements were recorded by an EyeLink 1000 eyetracker (http://sr-research.com/) sampling the position of the right eye at a 500 Hz. rate. Materials were presented using EyeTrack software (http://www.psych.umass.edu/eyelab/), which also recorded time and location of fixations using functions provided by SR Research.

The operation of the eyetracker was explained to the subject, who was told to read the sentences to be seen at a normal, comfortable pace while comprehending them well enough to answer straightforward questions about them. Subjects were seated approximately 60cm from the computer screen. The equipment was then aligned with the subject's eye and a 3-point calibration was made. For all experimental and practice trials, approximately 3.5 characters subtended 1° of visual angle. The experimental session began with the 5 practice trials, followed by the 88 experimental trials in an individually-randomized order. On each trial, a box appeared at the center of the monitor and the subject fixated on it. Minor errors of alignment were automatically corrected, and if alignment was not acceptable, it was recalibrated. Then a box appeared at the position of the first character in the sentence to be shown next. When the subject fixated on this box, the sentence appeared in its entirety. The subject read the sentence, and upon finishing it, pressed a key on a keypad. On approximately 1/3 of the trials, a simple two-choice question about the sentence appeared, which the subject answered by pressing one of two keys. The next trial then began.

Results

The data about time and location of fixations were analyzed in the conventional manner, as described by Rayner et al. (1989) and Staub and Rayner (2007). Fixations under 80 ms and over 800 ms were eliminated, after assimilating short fixations to nearby normal-length fixations. Several measures of eye movements were taken: Experimental sentences were divided into analysis regions as indicated by the subscripted / symbols in (15). The critical regions were Region 2 (which included the ellipsis), Region 3 (the spillover region; the first word following Region 2 if 6 characters or longer, otherwise the first two words), and Region 4 (the remainder of the sentence). First fixation duration (the duration of the initial fixation in a region), gaze duration (the sum of all fixation durations in a region from first entering it to first leaving it), go-past or regression path duration (the sum of all fixation durations in a region from first entering it until first going past it to the right), and the percentage of trials on which a first-pass fixation in a region was followed by a regressive saccade to an earlier region.

The mean values of these measures appear in Table 3, tabulated by absence vs. presence of the NAI and by absence vs. presence of the *may* -type modals in the non-NAI conditions (labeled Type I vs. Type II in Experiment 1b). The data were initially analyzed using mixed-effects regression models with random intercept effects of Subject and Item, and fixed effects of NAI and the presence of the modal *may* as in Experiment 1a. Model comparison

indicated that adding the presence of *may* factor did not significantly improve model fit over a simpler model with only the NAI factor (Baayen, 2008), so we report the results of the simpler model. Except for the logistic regression analyses of % Regressions Out, we report t-values of greater than 2 or less than −2 as significant (Tables of the mixed-effects analysis parameters appear in Appendix 2).

Absence vs. presence of an NAI did not significantly affect any measure of reading time in Region 2, which included the ellipsis site (*didn't*) (all $t < 1.0$). Therefore, only effects on the spillover regions (Regions 3 and 4) will be discussed in detail. In Region 3, first fixations were significantly shorter for the NAI than the no-NAI condition ($t = -2.403$). This difference did not extend to the second line of text, Region 4 ($t < 1.0$). Gaze durations behaved in a similar fashion, being shorter for the NAI than the no-NAI condition in Region 3 ($t = -2.569$). Again, as with first fixation, this difference did not extend to the last region of the sentence. In fact, the implicature (NAI) condition was numerically slower for Region 4, although this difference was not significant ($t < 1.0$).

Go-past times differed only in Region 4, where the NAI times were longer than the no-NAI times ($t = -2.478$). Although the Go-past time differences were in the expected direction for Regions 2 and 3, the differences did not approach significance. The Go-past time difference in Region 4 largely reflected the differences in probability of a regression out of Region 4, which was greater for the no-NAI than the NAI condition ($z = -2.689$). The small differences in regression probability in regions 2 and 3 were nonsignificant.

Discussion

Experiment 3 demonstrated that readers are sensitive quickly if not immediately to the ameliorative effect of an NAI on the disruption caused by a nonparallel ellipsis while they are reading the clause containing the ellipsis. The effect of an NAI is thus not a result of some kind of deliberative evaluation of the sentence form. However, the beneficial effect of the NAI does not appear immediately on the offending region, but appears slightly later (on the end of the sentence-initial clause in the present materials). This contrasts with the typically immediate appearance of syntactic or severe semantic anomaly (see Clifton & Staub, 2008, for a review), but appears similar to the timing of effects of moderate semantic implausibility (Rayner et al., 2004) or aspectual coercion (Traxler, Pickering & McElree, 2002).

General Discussion

While it has been known for decades that some attested examples of mismatch ellipsis strike one intuitively as remarkably acceptable, the startling range of acceptability of mismatch ellipsis examples has remained a puzzle. Interesting accounts of why some examples are relatively acceptable, while others are downright terrible, have been advanced, but we find them less than fully convincing. The studies described here shed some new light on this matter. When an antecedent clause conveys an NAI, the acceptability of a mismatch ellipsis example improves markedly and reliably, as shown by Experiment 1a and 1b. These experiments showed that the presence of an NAI in the antecedent clause improved the rating of ellipsis examples with mismatching antecedents but did not improve an ellipsis example with a matching antecedent as much Showing that the benefit of an NAI was limited to or largest in examples of mismatch ellipsis fits with the claim that mismatch ellipsis is not fully well-formed -- a claim reinforced by the generally low ratings of the mismatch examples in our studies. Experiment 1b showed that sentences conveying an NAI were rated as more acceptable than sentences that did not, even when compared to sentences that contained the modal *may* (which conveyed neither an actuality entailment nor a non-actuality implicature), thus eliminating the hypothesis that the apparent facilitation of NAIs

is really in fact only a penalty for mismatch ellipsis in cases where the antecedent conveys an actuality entailment.

Examples with the modal *may* arguably involve auxiliary focus. Consequently, the results of Experiment 1b suggest that Kertz's (2008) observation that examples with auxiliary focus tolerate mismatches better than examples with argument focus will not suffice to account for our results. Although one might have thought that the addition of a modal simply increases the probability of computing auxiliary focus and thereby decreases the importance of parallelism, following Kertz's proposal, this approach would not account for the difference between the different types of modals tested in Experiment 1b where NAI triggering modals improved acceptability more than *may* did.

The fact that an antecedent was also chosen more often in cases of ambiguous ellipsis if the antecedent contained an NAI (Experiment 2) suggests that the NAI highlights the antecedent clause and its relation to a following clause that resolves the implicit question introduced by the NAI. We think this finding both supports the hypothesized role of NAIs in facilitating mismatch ellipsis, and also supports the contention that various sorts of alternatives introduce potential QUDs, including alternatives implied by the semantic denotation of a question (standardly analyzed as the set of its alternative answers), those implied by focus (as in the focus value of a constituent in focus semantics, Rooth, 1992) and alternatives implied by the contrast between the actual world and a non-actuality implicature..

Experiment 3 simply showed that reading of a mismatching ellipsis was disrupted less when its antecedent contained an NAI than when it did not. The effect was not immediate, appearing only on the phrases following the ellipsis itself, but suffices to show that an NAI does have its effect on on-line processing.

Our proposed account of these phenomena is that, in an NAI clause, the contrast that obtains between the state of affairs described and the actual world serves to implicitly focus the modality or polarity of the state of affairs described. For instance, the sentence *This information needs to be released* implies that the information has not been released and it implicitly focuses on the deontic modality (the obligation), not the description of the state of affairs under discussion (this information being released). Implicitly focusing on the modality or polarity of the state of affairs by introducing an alternative to the actual world may constrain the likely continuation of discourse by offering a potential QUD (Was the information released?). A reader or listener can expect a subsequent sentence in the discourse to answer or comment on this QUD. Given a sentence like *Josh should/wants to go to Paris*, one is very likely to continue with *and he will* or *but he won't*. By contrast, a sentence without an NAI such as *Josh went to Paris* seems open to a much larger range of likely continuations. By constraining likely continuations of the discourse, NAIs may help the listener to figure out the speaker's likely intent even if the input sentence is not fully grammatical.

Throughout the discussion the results of the experiments have been discussed primarily with respect to a syntactic approach to ellipsis requiring matching of an antecedent and elided constituent in the syntax (at Logical Form). One might wonder whether a semantic approach to ellipsis could fare as well as the syntactic account. We think not. On a semantic approach it is unclear why an NAI facilitates primarily mismatch ellipsis cases and not matching ellipsis (for other unexplained generalizations, such as why passive-active mismatches are better than active-passive ones, see Arregui et al., 2006). Consequently an approach where in general only a semantic antecedent is required for elided constituents does not seem as attractive or explanatory to us as a syntactic approach.

An alternative possible approach is suggested by Kertz's (2008) proposal that information structure (rather than syntactic or discourse structure) is the factor that affects the acceptability of nonparallel ellipses. It is possible that the effect of an NAI could be attributed to how it changes information structure (e.g., encouraging focus on the auxiliary of the ellipsis). , However, as mentioned earlier, it seems likely that Kertz's information structure hypothesis would apply equally well to both the NAI-inducing and the non-NAI inducing modals used in Experiment 1b, and thus would not account for the difference found there. In addition, in contrast to our current NAI/QUD based account, Kertz's account does not apply to the findings of Experiment 2. The preference for an NAI-inducing antecedent clause cannot plausibly be attributed to information-structure effects, since the clause in question appeared in a relative clause. One would expect the root clause of a sentence to carry the primary role in defining information structure.

Apart from the structure and processing of ellipsis, the present results are also interesting from the perspective of a theory of processing conversational implicatures. Scalar implicatures (e.g., interpreting *some* as 'some but not all' because if the speaker had intended the stronger 'all' statement s/he would have used *all*) have received considerable attention recently in the processing literature (e.g., Clifton & Dube, 2010; Geurts & Pouscoulous, 2009; Grodner et al., 2010; Huang & Snedeker, 2009). Other, non-scalar, implicatures have received less attention. It is interesting, we think, that a presumably non-scalar implicature like the NAI studied here is regularly computed, as shown by the norming data. It suggests that listeners and readers are very sensitive to the choice of words that convey modality. Further, NAIs are drawn on-line as part of normal reading, as shown by the eye movement experiment (Experiment 3), not just computed when metalinguistic judgments are tapped. Both of these findings are completely novel. While a full theory of the circumstances under which listeners or readers draw NAIs goes beyond the scope of the present paper, the current studies do show that a heterogeneous set of triggers give rise to NAIs, including particular modals (e.g., *should*), particular non-modal verbs (*want*), and even some adjectives (*eager*). The current results thus open up a new avenue of research in the area of conversational implicatures. An understanding of the role of NAIs is also likely to open up important areas of language comprehension. In addition to highlighting implicit mechanisms for identifying the QUD, NAIs raise interesting issues concerning how larger chunks of discourse are organized and concerning the link between the representation and use of world knowledge and the representation and processing of multiple perspectives and (false) beliefs. We take up these points in turn.

NAIs may help the reader to integrate the following clause into discourse if the NAI introduces a QUD and the ellipsis clause resolves or comments on that question. That implicature computation should be related to the current or potential QUD is not surprising. Theoretical (van Kupplevelt, 1995) and empirical studies (Zondervan, Meroni & Gualmini, 2008) suggest a strong connection between the two. In particular, a speaker's choice of one word rather than a competitor should be particularly careful and informative when it is precisely what is the issue under discussion. In the case of an NAI, the implicature may also define a goal state that provides a motivation for a protagonist's behavior. If some goal-state is *wanted, hoped for, needed, required*, etc. and it was mentioned by the speaker, then the goal-state (e.g., the target of the want or hope, what will be true if the wants or hopes are realized) may be important for organizing the discourse because the states of affairs described may be related to the protagonist's motivations, and to the goal-states and their (non-)attainment. So beyond constraining the likely continuation of the discourse in terms of the content of the next clause, we suspect that NAIs may play an important role in organizing larger chunks of discourse and thereby also be an important influence on which elaborative inferences are drawn during language comprehension.

Turning to the representation of non-actual states of affairs, there is little psycholinguistic work to draw on. One exception is Ferguson and Sanford (2008) (see also Warren, McConnell, & Rayner, 2008), who report three eye movement recording studies. Participants read sentences that contained a violation of the Real World (RW) or of a hypothetical Counterfactual World (CW). In a RW context, RW-inconsistent statements show a penalty in reading times relative to RW-consistent statements. Likewise CW-inconsistent statements in a CW-context show a reading time penalty, even if they are RW-true. Most interesting of all, though, is the finding of an early penalty for RW-inconsistent statements, on the target item itself, in a CW context. This finding suggests that, in addition to evaluating statements in the context provided, readers are also evaluating information from the input against the real world even in a CW context. The RW-inconsistent penalty shows up on the critical word itself, before the CW congruence effect, which suggests a very early RW evaluation process.

Given the results of Ferguson and Sanford just described, it is perhaps less surprising that the computations underlying the NAI effects observed in our questionnaire studies also showed up on-line, in our eye movement study, during the interpretation of a sentence rather than only as part of a post-sentence deliberative process of making acceptability judgments. Both the line of inquiry begun by Ferguson and Sanford and a fuller understanding of NAIs is likely to be revealing and to broaden and deepen the study of language comprehension. The ability to cope with descriptions corresponding to reality and to other possible states of affairs may be basic to human cognition, and it may underlie abilities crucial to theory of mind. For theories of sentence processing to make contact with such issues, even along a narrow channel such as the present project defines is, we submit, a step forward in understanding language comprehension and human cognition.

Finally, we think that exploring the discourse structuring effects of the question under discussion is important. Within linguistic theory, the notion of alternative is important especially in formal accounts of focus. But what the present results suggest is that the everyday language notion of alternative is also important -- where there are alternatives there is a question about which one is correct. In the present paper, we've suggested that even implicatures can introduce alternatives that have a powerful effect on the language comprehension system. We have focused on effects that are observed in sentences that present some processing difficulty, namely elliptical sentences that are arguably ungrammatical or that are ambiguous. However (in common with Kertz, 2008, 2011) we suspect that they may also be observable in facilitating the integration of unexceptional, nonelliptical sentences, and are examining this possibility in ongoing research.

Research Highlights

- We describe a novel implicature, the NonActuality Implicature (NAI)
- The presence of an NAI increases acceptability of non-matching elliptical sentences
- The presence of an NAI increases the likelihood of choosing that constituent as an antecedent of an ellipsis
- The presence of an NAI facilitates reading of sentences with ellipses
- We present an interpretation of these effects in which the NAI constrains the likely Question under Discussion (QUD)

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Appendix 1: Materials from Experiments

Materials from Experiment 1a

1.
- **a.** This information was released but Gorbachev didn' t. (passive-active)
- **b.** This information should be released but Gorbachev didn' t. (NAI, passive-active)
- **c.** Someone released this information but Gorbachev didn' t. (active-active)
- **d.** Someone should release this information but Gorbachev didn' t. (NAI, active-active)

2. A trip was planned for August but Anne didn' t

A trip should be planned for August but Anne didn' t

Someone planned a trip for August but Anne didn’t

Someone should plan a trip for August but Anne didn’t.

3. A joint meeting was scheduled but the director didn't

A joint meeting should be scheduled but the director didn't

Someone scheduled a joint meeting but the director didn’t.

Someone should schedule a joint meeting but the director didn’t.

4. A private firm was hired but the director didn't

A private firm should be hired but the director didn't

Someone hired a private firm but the director didn’t

Someone should hire a private firm but the director didn’t

5. An urban planner was consulted but the supervisor didn't

An urban planner should be consulted but the supervisor didn't

Someone consulted an urban planner but the supervisor didn’t

Someone should consult an urban planner but the supervisor didn’t.

6. A hybrid car was ordered but the energy expert didn't

A hybrid car is supposed to be ordered but the energy expert didn't

Someone ordered a hybrid car but the energy expert didn’t.

Someone is supposed to order a hybrid car but the energy expert didn’t.

7. A radical expansion was planned for the branch office but the home office didn't

A radical expansion is supposed to be planned for the branch office but the home office didn't

Someone planned a radical expansion for the branch office but the home office didn’t.

Someone is supposed to plan a radical expansion for the branch office but the home office didn’t.

8. A taxi driver was called but Sally didn't

A taxi driver is supposed to be called but Sally didn't

Someone called a taxi driver but Sally didn’t.

Someone is supposed to call a taxi driver but Sally didn’t.

9. A planning committee was put together but the CEO didn't

A planning committee is supposed to be put together but the CEO didn't

Someone put together a planning committee but the CEO didn’t

Someone is supposed to put together a planning committee but the CEO didn’t

10. This research proposal was revised but the head investigator didn't

This research proposal is supposed to be revised but the head investigator didn't

Someone revised this research proposal but the head investigator didn’t

Someone is supposed to revise this research proposal but the head investigator didn’t

11. The cookies were made but the babysitter didn't

The cookies should be made but the babysitter didn't

Someone made the cookies but the babysitter didn’t

Someone should make the cookies but the babysitter didn’t

12. The house was sold but the new agent didn't

The house should be sold but the new agent didn't

Someone sold the house but the new agent didn’t

Someone should sell the house but the new agent didn’t

13. A painting was donated but the usual donor didn't

A painting should be donated but the usual donor didn't

Someone donated a painting but the usual donor didn’t

Someone should donate a painting but the usual donor didn’t

14. A financial consultant was contacted but the director didn't

A financial consultant should be contacted but the director didn't

Someone contacted a financial consultant but the director didn’t

Someone should contact a financial consultant but the director didn’t

15. This oriental rug was repaired but the original seller didn't

This oriental rug should be repaired but the original seller didn't

Someone repaired this oriental rug but the original seller didn’t

Someone should repair this oriental rug but the original seller didn’t

16. The new employee was trained but the manager didn't

The new employee is supposed to be trained but the manager didn't

Someone senior trained the new employee but the manager didn’t

Someone senior is supposed to train the new employee but the manager didn’t

17. A deck was added to the cottage but the tenant didn't

A deck is supposed to be added to the cottage but the tenant didn't

Someone added a deck to the cottage but the tenant didn’t

Someone is supposed to add a deck to the cottage but the tenant didn’t

18. This tractor was inspected but the mechanic didn't

This tractor is supposed to be inspected but the mechanic didn't

Someone inspected this tractor but the mechanic didn’t

Someone is supposed to inspect this tractor but the mechanic didn’t

19. A new strategy was devised but the director didn't

A new strategy is supposed to be devised but the director didn't

Someone clever devised a new strategy but the director didn’t

Someone clever is supposed to devise a new strategy but the director didn’t

20. A diplomatic exchange was arranged but the ambassador didn't

A diplomatic exchange is supposed to be arranged but the ambassador didn't

Someone official arranged a diplomatic exchange but the ambassador didn’t

Someone official is supposed to arrange a diplomatic exchange but the ambassador didn’t

Materials from Experiment 1b

Type I

1. This information was released but Gorbachov didn't.

This information needed to be released but Gorbachov didn't.

2. A trip is planned for August, but Anne didn't.

A trip must be planned for August, but Anne didn't.

3. A joint meeting was scheduled, but the director didn't.

A joint meeting should be scheduled, but the director didn't.

4. A private firm was hired, but the Chancellor didn't.

A private firm should be hired, but the Chancellor didn't.

5. An urban planner was called in, but the supervisor didn't.

An urban planner needed to be called in, but the supervisor didn't.

6. A hybrid car was ordered, but the energy expert didn't.

A hybrid car needed to be ordered, but the energy expert didn't.

7. A radical expansion was planned for the branch office, but the home office didn't.

A radical expansion needed to be planned for the branch office, but the home office didn't.

8. A taxi driver was called, but Sally didn't.

A taxi driver needed to be called, but Sally didn't.

9. A planning committee was put together, but the CEO didn't.

A planning committee must be put together, but the CEO didn't.

10. This research proposal was revised, but the head investigator didn't.

This research proposal must be revised, but the head investigator didn't.

11. The garden is planned, but Patricia didn't.

The garden must be planned, but Patricia didn't.

12. The chief engineer was alerted, but the building inspector didn't.

The chief engineer must be alerted, but the building inspector didn't.

Type II

1. The cookies may have been made, but the babysitter didn't.

The cookies had to be made, but the babysitter didn't.

2. The house may have been sold, but the new agent didn't.

The house had to be sold, but the new agent didn't.

3. The fruit may have been stirred, but the chef didn't.

The fruit had to be stirred, but the chef didn't.

4. A diet may have been selected, but the nutritionist didn't.

A diet must be selected, but the nutritionist didn't.

5. This project may have been approved, but the planning board didn't.

This project must be approved, but the planning board didn't.

6. A painting may have been donated, but the usual donor didn't.

A painting must be donated, but the usual donor didn't.

7. A financial consultant may have been contacted, but the treasurer didn't.

A financial consultant must be contacted, but the treasurer didn't.

8. A designer may have been consulted, but the assistant didn't.

A designer must be consulted, but the assistant didn't.

9. This office may have been tested, but the official inspector didn't.

This office must be tested, but the official inspector didn't.

10. This oriental rug may have been repaired, but the original seller didn't.

This oriental rug needs to be repaired, but the original seller didn't.

11. A new park may have been designed, but the town didn't.

A new park had to be designed, but the town didn't.

12. This house plan may have been changed, but the architect didn't.

This house plan must be changed, but the architect didn't.

13. A new strategy may have been devised, but the director didn't.

A new strategy must be devised, but the director didn't.

14. This new employee may have been trained, but Personnel didn't.

This new employee needs to be trained, but Personnel didn't.

15. A greenhouse may have been added on to the cottage, but the tenant didn't.

A greenhouse must be added on to the cottage, but the tenant didn't.

16. This tractor may have been gone over, but the mechanic didn't.

This tractor must be gone over, but the mechanic didn't.

Materials used in Experiment 2. Alternatives separated by /

1. John met a girl who went / would have gone Katmandu. Fred did last month

___ Fred met a girl last month.

___ Fred went to Katmandu last month.

2. I called a reporter who interviewed / wanted to interview Tony Blair. Carson did last year.

___ Carson called a reporter last year.

___ Carson interviewed Tony Blair last year.

3. Fritz quoted a politician who met / is eager to meet al-Maliki. Pablo did last month.

___ Pablo quoted a politician last month.

___ Pablo met al_Maliki last month.

4. The supervisor chastised a worker who bought / would have brought production to a halt. Fred did recently.

___ Fred recently chastised a worker.

___ Fred recently brought production to a halt.

5. The CEO hired a guy who designed / wanted to design customized software. The Vice-President did in the past.

___ The Vice-President hired a guy in the past.

___ The Vice-President designed customized software in the past.

6. Rick interviewed a man who greased / wanted to grease pigs at the fair. Sam did last year.

___ Sam interviewed a man last year.

___ Sam greased pigs at the fair last year.

7. Lucy found out about a librarian who wrote / wanted to write mystery novels. The office manager did in the past.

___ The office manager found out about a librarian in the past.

___ The office manager wrote mystery novels in the past.

8. A manager complained about a gardener who behaved / almost behaved rudely to visitors at the Orchid Show. The receptionist did earlier in the week.

___ The receptionist complained about a manger earlier in the week.

___ The receptionist behaved rudely last week.

9. Paul described at length a worker who blocked / would have blocked the deliveries. The owner did yesterday morning.

___ The owner described a worker yesterday morning.

___ The owner blocked the deliveries yesterday morning.

10. A ranger watched a hiker who climbed / almost climbed to the top of an impossibly craggy peak. Greg did last year.

___ Greg watched a hiker last year.

___ Greg climbed to the top of the peak last year.

11. Pablo worried about an official who investigated / would have investigated him. Mr. Davis did earlier this year.

___ Mr. Davis worried about an official earlier this year.

___ Mr. Davis investigated Pablo earlier this year.

12. Emma called a friend who went / wanted to go to Costco. Anne did on Monday.

___ Anne called a friend on Monday

___ Anne went to Costco on Monday

13. Paula described a teacher who sang / would have sung in class. Apparently Maxine did in the past.

___ Maxine described a teacher in the past.

___ Maxine sang in class in the past

14. Carla wrote letters to the man who built / was eager to build simple pet robots. The graduate student did when she lived in Cambridge.

___ The graduate student wrote letters to the man when she lived in Cambridge.

___ The graduate student built simple pet robots when she lived in Cambridge.

15. Francesca mentioned a lady who wore / wanted to wear flashy wigs. Sandra did last week.

___ Sandra mentioned a lady last week.

___ Sandra wore flashy wigs last week.

16. Chris knew an amazing Iraqi guy who studied law / would have studied law in Baghdad. Anna did a while back.

___ Anna knew an amazing Iraqui guy a while back.

___ Anna studied law in Baghdad a while back.

17. Before the tsunami, Bill heard about an Indonesian lady who immigrated / could have immigrated to Amherst. Anna-Teresa did recently.

___ Anna-Teresa heard about an Indonesian lady recently.

___ Anna-Teresa immigrated to Amherst recently.

18. Sheila praised a man who ran / would have run for the Senate. Her brother did earlier.

___ Sheila's brother praised a man earlier.

___ Sheila's brother ran for the Senate earlier.

19. Caitlin was incredibly angry at the guy who was / would have been her lab partner. Paul was last year.

___ Paul was angry at the guy last year.

___ Paul was Caitlin's lab partner last year.

20. Ben helped a boy who lived / would have lived on the streets. George did last year.

___ George helped a boy last year.

___ George lived on the streets last year.

21. Lisa called an acquaintance who worked / could have worked in Rome. Her neighbor did last year.

___ Lisa's neighbor called an acquaintance last year.

___ Lisa's neighbor worked in Rome last year.

22. Jessica visited a relative who married / should have married a French man. Gayle did last year.

___ Gayle visited a relative last year.

___ Gayle married a French man last year.

Materials from Experiment 3

1. This information was released but Gorbachov didn't, even though lives were clearly at stake.

This information needed to be released but Gorbachov didn't, even though lives were clearly at stake.

2. A trip was planned for August but Anne didn't, despite the fact that she wanted to go.

A trip had to be planned for August but Anne didn't, despite the fact that she wanted to go.

3. A joint meeting was scheduled but the director didn't, at least not before his secretary spoke to him.

A joint meeting needed to be scheduled but the director didn't, at least not before his secretary spoke to him.

4. A private firm was hired but the director didn't, since he thought his assistant could take on the responsibility.

A private firm needed to be hired but the director didn't, since he thought his assistant could take on the responsibility.

5. An urban planner was consulted but the supervisor didn't, even though he called many other experts.

An urban planner had to be consulted but the supervisor didn't, even though he called many other experts.

6. A hybrid car was ordered but the energy expert didn't, even though it seemed like a great idea.

A hybrid car needed to be ordered but the energy expert didn't, even though it seemed like a great idea.

7. A radical expansion was planned for the branch office but the home office didn't, since there was a major miscommunication.

A radical expansion needed to be planned for the branch office but the home office didn't, since there was a major miscommunication.

8. A taxi driver was called but Sally didn't, although the number was programmed into her cell phone.

A taxi driver had to be called but Sally didn't, although the number was programmed into her cell phone.

9. A planning committee was put together but the CEO didn't, since he had no extra time for meetings.

A planning committee had to be put together but the CEO didn't, since he had no extra time for meetings.

10. This research proposal was revised but the head investigator didn't, although it wouldn't have taken very long.

This research proposal had to be revised but the head investigator didn't, although it wouldn't have taken very long.

11. The cookies may have been made but the babysitter didn't, despite the fact that she loved to bake.

The cookies had to be made but the babysitter didn't, despite the fact that she loved to bake.

12. The house may have been sold but the new agent didn't, although the property was in a great location.

The house had to be sold but the new agent didn't, although the property was in a great location.

13. A painting may have been donated but the usual donor didn't, since he had given a sculpture instead.

A painting needed to be donated but the usual donor didn't, since he had given a sculpture instead.

14. A financial consultant may have been contacted but the director didn't, despite the fact that he often asks for advice.

A financial consultant had to be contacted but the director didn't, despite the fact that he often asks for advice.

15. This oriental rug may have been repaired but the original seller didn't, although it would have increased the price.

This oriental rug had to be repaired but the original seller didn't, although it would have increased the price.

16. The new employee may have been trained but the manager didn't, even though they needed a new cashier.

The new employee needed to be trained but the manager didn't, even though they needed a new cashier.

17. A deck may have been added to the cottage but the tenant didn't, since it was much too expensive.

A deck needed to be added to the cottage but the tenant didn't, since it was much too expensive.

18. This tractor may have been inspected but the mechanic didn't, despite the fact that it was required by law.

This tractor needed to be inspected but the mechanic didn't, despite the fact that it was required by law.

19. A new strategy may have been devised but the director didn't, even though he was usually very creative.

A new strategy needed to be devised but the director didn't, even though he was usually very creative.

20. A diplomatic exchange may have been arranged but the ambassador didn't, since it was against his policy.

A diplomatic exchange needed to be arranged but the ambassador didn't, since it was against his policy.

Appendix 2: Linear Mixed Models fitted to data

Experiment 1a: Parameter estimates, standard errors and p-values for fixed effects. Random intercepts and random slopes for both factors were included for subjects and items in the model.

Effect	Estimate	Standard Error	*t*-value
Intercept	3.283	0.127	25.91
NAI: present	−0.338	0.091	−3.72
Voice: mismatch	−1.746	0.129	−13.55
Interaction	0.738	0.116	6.34

Experiment 1b: Parameter estimates and standard errors for fixed effects: primary model. Random intercepts and random slopes for both factors were included for subjects and items in the model.

Effect	Estimate	Standard Error	*t*-value
Intercept	1.625	0.098	16.60
NAI: Present	0.545	0.094	5.79
Non-NAI: Type II	0.240	0.095	2.53
Interaction	−0.238	0.109	−2.18

Experiment 1b: Parameter estimates, standard errors and p-values for fixed effect: Model fitted to just Type II items. Random intercepts and random slopes for both factors were included for subjects and items in the model.

Effect	Estimate	Standard Error	*t*-value
Intercept	1.865	0.110	16.951
NAI: present	0.307	0.0865	3.554

Experiment 2: Parameter estimates, standard errors and p-values for fixed effects. Random intercepts for subject and item were included in the model.

	Estimate	Standard Error	*z*-value	*p*-value
Intercept	0.574	0.203	2.823	0.005
Condition: modal	−0.424	0.166	−2.553	0.011

Experiment 3: Parameter estimates, standard errors and p-values for fixed effects, Region 3.

Measure	Effect	Estimate	Standard Error	*t*-value
First Fixation	Intercept	270.66	14.75	18.35
	NAI present	−25.39	10.57	−2.40
Gaze Duration	Intercept	366.95	24.01	15.28
	NAI present	−37.19	14.48	−2.57

Experiment 3: Parameter estimates, standard errors and p-values for fixed effects, Region 4.

Measure	Effect	Estimate	Standard Error	*t*-value	*p*-value
Go-Past Time	Intercept	1416.36	87.55	16.177	NA
	NAI: Present	−137.39	55.43	−2.478	NA

Measure	Effect	Estimate	Standard Error	*t*-value	*p*-value
Regressions out	Intercept	−0.881	0.206	−4.282*	1.853-05*
	NAI: Present	−0.545	0.203	−2.689	0.0072*

* Logistic regression: z and computed p value reported

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Table 1

Mean Acceptability Ratings (with standard deviations) (1 = Unacceptable, 5 = Highly Acceptable), Experiment 1a

NAI Implicature	Match Active-Active	Mismatch Passive-Active	Mean
Absent (8a, 8c)	3.28(1.28)	1.54(1.28)	2.41
Present (8b, 8d)	2.95(1.01)	1.94(0.76)	2.45
Mean	3.12	1.74	

Table 2

Mean acceptability ratings (with standard deviations) (5=highly acceptable, 1 = unacceptable), Experiment 1b

Condition	Type I(14)	Type II (15)
No NAI (9a, 10a)	1.63(0.89)	1.86(1.04)
NAI (9b, 10b)	2.17(1.15)	2.17(1.22)
Difference	0.55	0.31

Table 3

Eyetracking measures (in ms except for % regressions) for Experiment 3.

Region	Presence of NAI	Presence of "may"	First Fixation	Gaze	Go-Past	% Regressions Out
2	No-NAI	Past only	246	289	371	15
		May	250	282	377	15
	NAI	Past only	252	291	380	15
		May	242	284	340	11
3	No-NAI	Past only	272	377	440	9
		May	270	363	399	5
	NAI	Past only	235	339	413	6
		May	258	328	369	6
4	No-NAI	Past only	190	1047	1473	29
		May	178	907	1361	34
	NAI	Past only	176	1038	1318	21
		May	187	956	1243	24