Dear Dr. Pickering,

Thank you very much for the opportunity to revise and resubmit the article “Inhibitory limits on young children's comprehension of negation” to *Journal of Memory and Language*. We appreciate your and the reviewers’ thoughtful feedback and we have taken it into account in our revision. Below is a detailed list of revisions that we have made in response to the individual points raised.

We look forward to hearing from you regarding the manuscript; please do not hesitate to contact us if you have any further questions or concerns.

Sincerely,

Ann E. Nordmeyer and Michael C. Frank

**Editorial Comments**

*JML specializes in language processing (and of course memory) and I think it is rather unclear whether your paper is likely provide a clear theoretical advance in understanding children's processing.  As I see it, Reviewers 2 and 3 are unhappy with your processing account, in particular worrying about whether you explain the effects of inhibition and pragmatics appropriately.  Thus Reviewer 3 does not accept that children first calculate the affirmative and then negate it, whereas Reviewer 3 is also concerned about pragmatic felicity.  Reviewer 3 (and also Reviewer 1) particularly worry about the interpretation of your results in terms of inhibition.*

We thank the editor for this clear synthesis of the primary concerns that the reviewers expressed. The comments that we received on this manuscript were very helpful as we reflected on the interpretation of our data. Regarding Reviewer #3’s concern, it was not our intention to strongly endorse a “calculate the affirmative, then negate” interpretation, though it is very understandable in retrospect why Reviewer #3 read the manuscript this way. Our goal in the submitted manuscript was to present this account as a possible interpretation, given the patterns we saw in the data. Upon further reflection, however, we agree that it is not a convincing explanation for our results.

All three reviewers mentioned the role of pragmatics in processing negative sentences. We also are very interested in the role that pragmatics plays in the processing of negation, and as we were revising this manuscript we received data from an experiment of adults (Nordmeyer & Frank, *Proc. Cog. Sci.*,2014) which provides further evidence that context plays a critical role in the processing of negative sentences. The combination of the reviewers’ comments and this new adult data has convinced us that a pragmatic interpretation of the data we present here is more appropriate. For this reason, we have re-written this manuscript to focus on the role that context plays in children’s acquisition of negation.

We are still interested in the possible role of inhibitory control, however, and we discuss this as a possible factor in our General Discussion. But rather than endorsing inhibitory control as the primary explanation, we are interested in how the contexts set up by our stimuli influence the *inhibitory demands* placed on children as they process these sentences. We thus present inhibitory control as one of several possible explanations for our findings that are worthy of further exploration.

*It seems to me that you might find it easier to present your findings in relation to language acquisition (and therefore to submit your work to a child language or perhaps general developmental journal).  I think you have a harder job with JML, because you need to focus on the processing account and convince a rather differently oriented readership.  But I do think your work is interesting and potentially quite informative and am willing to consider a revision if you feel you can address the reviewers' concerns.*

We hope that our revised manuscript, with a pragmatic account of our data, present a more convincing account of children’s processing of negation. We believe that this interpretation is more appropriate for JML, dealing as it does with the contextual and cognitive factors that influence children’s processing.

**Reviewer #1**

*This is a beautifully executed paper in its technical details. And, in an important way, it is also rare: It is surprising how little is done on how children comprehend negation. The results are consistent with several existing theories, but by using eye movements, the work reveals the dynamics of comprehension at the onset of the referent, allowing an exciting window onto language processing by children while they interpret a negated referent. By doing so, the authors offer a provocative and relatively novel way of understanding the processing of negation and how it develops. I strongly recommend publication, but make a number of suggestions below, some expository, and some more theoretical.*

Thank you very much for the kind words.

*Perhaps I'm misunderstanding, but could it simply be integration rather than inhibitory control, additionally (the authors discuss this briefly on p. 25)? One possibility in explaining the results is that children haven't adequate ability to integrate the negating element with the kernel referent to be negated. This means it is not inhibitory control per se (though I think this is an entirely viable and interesting thesis), but rather the development of working memory. This would, of course, fit well with some traditional sentence processing theories generally (from Just and Carpenter to Rick Lewis and others). By failing to integrate the negating element, it seems that the general pattern would be predicted: E.g., switching to the referent though it has been negated, and so on (i.e., "radically local processing").*

Thank you for this comment. We agree that children may have difficulty with integration, and that hence that it is another possible explanation for our data. We have included this account in our General Discussion, where we have expanded our discussion of possible interpretations of the pattern of data that we see.

*It is quite interesting that Experiment 2, if anything, showed a slight improvement in processing negation by children. This does suggest interesting pragmatic constraints may be at work already in the language comprehension of children. I wonder if the authors might discuss what would happen in their task if children were asked to \*press or click or some action\* towards the 'boy with no apples'? In general, the task constraints for relatively inattentive children are already very lax, in the sense that children may not be actively integrating the instructions as their overt behavior is not so tightly entwined in the task requirements. If carrying out some action on the figures were involved, I wonder if this would help? The implications may be interesting; it may suggest that actions in the context of language may facilitate the inhibitory processes (e.g., sentence processing could be bootstrapped by the inhibitory processes required in real-world use of language)? Could this also mean that the inhibition required is \*intensified\* as now a more overt behavioral activity will also have to be avoided?*

We agree that this account is a very interesting possibility, and it is one that we have considered. In fact, the role of pragmatics in children’s performance on this task is now the primary question that we address in this work. In response to this suggestion (and related other suggestions by the other two reviewers), we have substantially revised the manuscript, as described above.

Regarding the suggestion that children’s performance might be improved if they had to directly engage with the characters, we have actually collected pilot data on a behavioral task where an experimenter read a picture book with children in which the goal was to “find Elmo’s friends”. The experimenter read sentences to children such as “Elmo has a friend named Jane. Jane has no apples. Can you find Jane?”, with stimuli identical to the pictures used in Experiment 1. We found that the majority of children under 3 selected the incorrect referent on every single negative trial (they were at ceiling on positive trials). We did not see any children who performed at chance on this task – children either selected the correct referent close to 100% of the time, or selected the incorrect referent with similar consistency. This finding is one of the reasons that we thought inhibitory control might be playing a role in children’s processing, and was the impetus for switching to an eye-tracking task (which we thought might reduce performance demands more generally).

*As a final theoretical note, I wonder if the authors can further discuss whether they consider the children to have fully processed the kernel sentence. I assume not, and that inhibitory processes are not entirely "linguistic" in the sense that the authors envision a logical operation over some abstract structures that the children have extracted from the sentences. Or perhaps they do. Further discussion may be interesting. I could imagine some readers being curious about the manner in which inhibitory control processes are involved in language processing generally, if that's the suggestion, as it is, in some respects, a quite provocative thesis. In fact the authors may also want to consider some interesting work by Cargill, Farmer, et al. who used mouse tracking to assess the language comprehension in children and also found subtle hints of inhibitory processes -- at least this may help explain it -- in dealing with syntactic ambiguity. (This reference is just a minor suggestion, as the authors may not find it closely related enough.)*

Thank you for this comment. We agree that our initial framing of the paper, with our focus on inhibitory control, did not clearly define the role that inhibitory control might play in the processing of these sentences: whether these inhibitory processes were operating over linguistic processing specifically. Because we have moved away from inhibitory control the primary interpretation of the data, however, we have chosen not to expand this discussion in our revised manuscript.

*Though once I understood the onset-contingent analysis, it was crystal clear and elegant, I wondered if the authors could somehow rephrase or restructure the introduction of this analysis. I found myself ogling the lines trying to recall which ones were to the target vs. distractor and what each meant. Then it occurred to me that one simple change might help: changing the y-axis on Fig. 6 to "Proportion Looks Switched," or something like that? This could be a quick way to do it.*

We appreciate this suggestion to make interpreting the onset-contingent plots clearer, and have changed the y-axis label to “Proportion looks switched” on both of these plots.

*I wondered if Table 3 could be in an appendix. They appear to contain several statistically significant 4-way interaction terms and in general are filled with curious detail that may best be left outside the body of the paper. Indeed, these interactions may be fodder for a much longer results section, and the authors do discuss these interactions in the results. This is a relatively minor suggestion. Some readers unfamiliar with the modeling approach may find the vast table incommensurate with the justifiably punchy and clear results section.*

We have placed Table 3 in an appendix. Thank you for the suggestion.

*Perhaps I missed this in the explanation of the methods, but it appears, in the stimuli, that the potential referents change identity -- different boys, so to speak. I seem to have missed explanation for this in the paper, and perhaps it would be worth bringing up, as I could imagine stimuli in which identities of referents are preserved across context and trial.*

The goal of these contexts was to give participants a glimpse of what the world of each trial looked like – a sense of the “base rate” of different characteristics, so to speak. To this end, the context is meant to represent a sample of possible characters in the world, which might lead participants to form expectations of what the next character will look like. We have explored the role of these contexts in a set of experiments with adults (Nordmeyer & Frank, *Proc. Cog. Sci.* 2014) and find that they facilitate the processing of negation. We have included a brief explanation for our inclusion of these contexts in the introduction to Experiment 1.

**Reviewer #2**

*The authors set up the study as an investigation into children's comprehension of non-existence which they discuss as an early acquired function of negation, and they refer to a paradox - early acquisition of negation yet problematic even in adult comprehension (p.3). This conceptualisation is too simplistic. Greater consideration of the precise details of the acquisition process is needed, for example the contexts in naturalistic interaction in which non-existence is typically used (alluded to in the Discussion), to better set up the focus of the study and the questions it will address.*

Thank you very much for this comment. We have re-framed this paper to focus on the role that context and pragmatics play in children’s acquisition of negation. We definitely agree that understanding the naturalistic contexts of negation is important to consider, and we have expanded our discussion of this issue in the introduction as well, adding a section that specifically discusses the contexts in which negation occurs and how these contexts might influence the pattern of acquisition that has been seen in previous work.

*When children refer to non-existence, it is almost certain that they have in mind an expected state or event, or are referring to something that has been previously mentioned in conversation (the authors touch on the role of pragmatic felicity, but it is not core to their arguments, p.7).  In the studies presented in this paper, it is not clear what the child's expectation should be, especially if the target objects change on each trial. At the very least, they would have to infer an expectation over a number of trials that one person always has nothing vs. something (Study 1), or that there is always a contrast in object (Study 2). So it is not particularly 'surprising' (p.8) that children, especially 2-year-olds, might find these contexts difficult, and it is too simplistic to state that the difficulty is 'unlikely .due to a lack of conceptual understanding' (p.17) and infer instead that inhibitory control is critical.*

*…*

*Similarly, the authors seem to assume that 'simply matching no to non-existence' (e.g. p.25) should be easier than mapping 'no' to the non-existence of something in particular (p.17), but this isn't how children use negation, there is generally a pre-supposed referent, and a contrast between two referents (Study 2) may make negation more rather than less pragmatically felicitous.*

Again, our thanks for this comment. We have considered the role of pragmatics on the processing of negation extensively in our work with adults, but had not previously applied this lens to our developmental work. This comment, as well as similar comments from other reviewers, convinced us that the role of context and pragmatics is of critical importance in interpreting this work. We have revised the manuscript to focus on the different contexts of Experiment 1 and Experiment 2, and how these different contexts might influence children’s processing of the same negative sentences. In particular, we discuss the fact that Experiment 1 contains a pragmatically unusual use of nonexistence negation in our revised discussion.

*If children are learning form-function mappings from the language they hear (e.g. see Cameron-Faulkner et al., 2007), and children's language comprehension & production skills are closely linked to prototypical contexts/forms of use (e.g. Brandt, Kidd et al., 2007; 2009, Rowland & Noble, 2011), then the change from a prototypical to more abstract context is likely to cause problems. It is, of course, an interesting question when children are able to comprehend forms and functions in more abstract contexts, and in an adult-like way. Thus, in acquisition, rather than context 'mitigating cognitive challenges' (p.8), this can be conceptualised as non-prototypical contexts increasing cognitive challenge. A fuller consideration of these issues should be presented in the Introduction, including the role of form-function frequencies and mappings alongside other 'factors determining the order of acquisition (of negation)' (p.4).*

We thank the reviewer for these references, and we have expanded our introduction to include a discussion of the form-function frequencies in children’s input, and the role that this might play in children’s acquisition of negation.

*Figure 1 (p.10) - It would be helpful to also show how the 'feedback' worked. Was there any evidence that performance improved over trials? What was the purpose of giving feedback?*

We included the feedback after an initial pilot of the experiment that did not include feedback after test trials. In this initial version, children were fussier and overall appeared more likely to dislike the experience of participating in the experiment. The feedback was included to motivate children to look to the correct picture, and create a more “story-like” narrative to the experiment (e.g. helping Elmo find his friends). We have clarified this in the introduction to Experiment 1.

*Were different characters and target objects shown on every trial, or were they re-used? This is important in terms of determining how likely it is that participants were able to work out or have a prior expectation about the state of events in the trial scenes.*

*…*

*Why were there 3 characters in the context scenes (2 with the target item?) but only 2 in the trials? This may have led to the expectation that both characters in the trial could have had 'apples' particularly in Study 2 where everyone had something (perhaps accounting for why adults in Study 2 took longer to resolve negative sentences, and looked to the distracter if they were initially looking at the target. When all characters were holding something, it may have been necessary to check that the alternative WAS holding the target item to be fully sure that the target was NOT holding that item).*

Different characters and target objects were used on every trial. The methods section was altered slightly to make this clearer. Our goal was, as described in the above comment, to give participants some expectation of what the characters in each trial might look like. We have explored the role of these contexts in a set of experiments with adults (Nordmeyer & Frank, *Proc. Cog. Sci.,* 2014) and find that they do lead adults to form these expectations and hence to process negative sentences more quickly.

The suggestion that participants in Experiment 2 may have been attempting to “check” the distractor character to confirm that they were holding target items before orienting back to the correct picture is a very reasonable interpretation of the data and one we are interested in considering. We have included this possibility in an expanded discussion of the results of Experiment 2 where we mention a number of possible explanations for this pattern. We thank the reviewer for drawing our attention to this possibility.

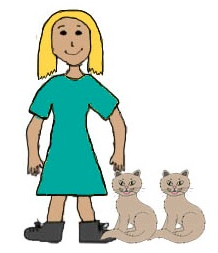
*Was the positioning of the target character and the location of the character with/without the object counterbalanced for left/right?*

Both the position of the character, as well as the location of the character with the objects, were counterbalanced in both the contexts and the trials. We have added these details to the methods section.

*p.11 - for clarity, it would be helpful to include a Figure showing the materials used in the practice trials.*

As stated in the methods section, the practice items were arranged similarly to the trial items. The items used in the practice trials were different, but because different items were used in each individual trial as well, we feel it is unnecessary to include a figure of the practice trials. We’ve included the practice trials below; if either the reviewers or the editor would like us to revisit this decision, we are happy to include them in a revision.

Practice trial 1: Look at the girl who has cats



Practice trial 2: Look at the boy who has umbrellas

Experiment 1:

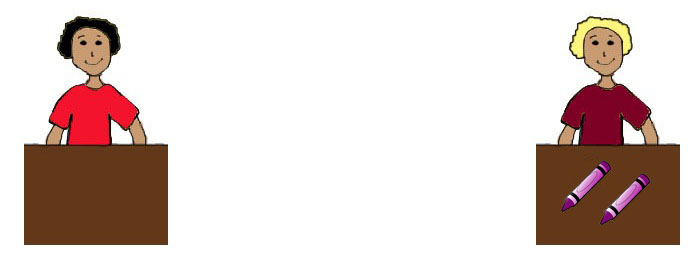


Experiment 2:



Practice trial 3: Look at the boy who has no crayons.

Experiment 1:



Experiment 2:



*p.12 - the authors argue that 'the word no is not sufficient for disambiguation without knowing what is being negated', but in Study 1 this may not be true, at least for the adults. All trials included a character with nothing, so the word 'no' was sufficient to decide which picture was appropriate (but not in Study 2). This seems to play out in the adult data (Fig.3) - adults who initially looked at the distracter, appear to be slightly quicker to shift to the target for negative than positive sentences, presumably indicating that they have already begun to initiate a shift on hearing the negator prior to the onset of the target noun. This pattern is not evident in Study 2 where the target noun is necessary to disambiguate. If this is the case, taking a measure of proportion of looks from the onset of the target noun is potentially problematic, as positive and negative sentences have been differentially processed by this point in time, so for the adults, an apparently equal performance on the two sentence types may reflect an initial time advantage for the negatives (the finding that children have particular problems with these sentences still stands).*

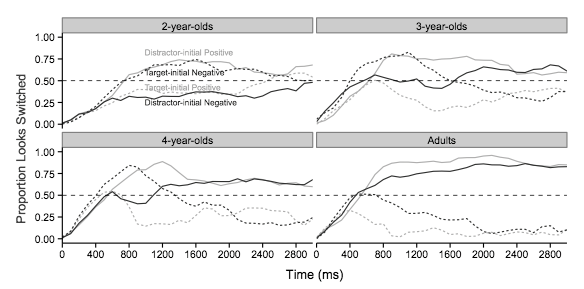
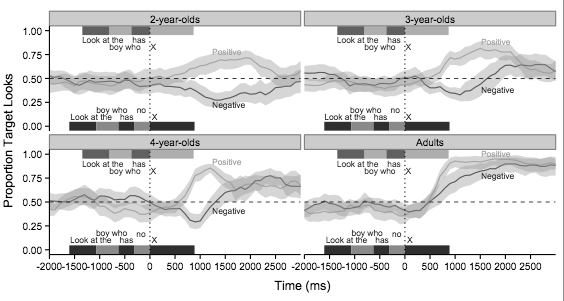
We thank the reviewer for this observation. We agree that it is possible that adults in Experiment 1 could make a prediction about the target of negation before they hear the disambiguating noun, and we’ve acknowledged that possibility in the “Eye-tracking analysis” section of Experiment 1. We have also enhanced our discussion of the adult data for both Experiment 1 and Experiment 2, in which we address the possibility that adults may be identifying the referent of negation before the disambiguating noun. However, because this would not change the interpretation of the child data, we have continued to use the target noun as the point of disambiguation in our analyses.

*Figs 3 & 6 - I found these Figures very confusing to interpret. If the graph shows proportion of looks to Distracter scenes (as appears to be indicated in the key for Fig3), then I don't understand why the adults are looking at the distracter after hearing the sentence. Perhaps the solid lines show looks to the target instead?*

Thank you very much for drawing our attention to this. There was a misleading typo in the key for Figure 3 that made interpreting these graphs very difficult. We’ve corrected this and we hope that the key for this figure is clearer now.

*Colours in figures don't translate very well into B&W, consider changing for wider accessibility.*

In designing our color scheme, we have tried to select colors which differ sufficiently in luminance so that lines for positive and negative sentences can be distinguished easily both in color and in grayscale. A B&W soft proof of the figures is attached below.



*p.12-13: the 2 & 3yr-olds do not appear to respond in an adult-like way, even to positive sentences, particularly towards the latter end of the time window. Why might this be the case?*

This pattern of data—asymptotic looking below adult levels—is ubiquitous in looking-time experiments with children (see e.g. Fernald et al., 1998; 2008). The difference in children’s gaze patterns towards the end of each trial is likely due to children’s more limited attentional focus compared with adults; likely children are losing interest in the stimuli by this point in the trial and are looking away from the target or the screen altogether.

*p.16 - why was the time window 600-2600ms selected? p.21, why these specific early and late time windows?*

These windows were selected post-hoc, after seeing the data. We intended this window choice as a test of whether the drop in performance seen early in the trial (for 3- and 4-year-olds in Experiment 2) and the improvement in performance seen later in the trial were statistically significant.

After considering this comment, however, we made the decision to change the time window used for analysis throughout the paper to be more consistent with previous developmental looking time studies. A review of previous looking time literature (Fernald et al., 2008; Haith, 1993) suggests that it is reasonable to assume that children need 200 – 400 ms to program an eye movement depending on the complexity of the stimuli and the task, as well as some time to process the auditory stimuli. We selected a 300 ms onset time for the analysis window, in line with the recommendations in Fernald et al. (2008), and end the analysis window at 2300 ms. In Experiment 2, we bisect this window at 1300ms to look at the effects of early and late looking behavior. This adjustment did not change our key findings. We include one model of three-year-olds’ looking behavior in the original 600-2600ms window, but we clarify that this is a post-hoc decision made to test the pattern that we saw in the time-course plots of the data.

*p.16-17 - Greater consideration of the model results would be helpful, for instance, is there any significant effect of sentence type in the 4yrs & adult data? Also p.21-22, given the potential complexity of 3 and 4-way interactions.*

Thank you for this suggestion. We have included several additional analyses in the results sections of both Experiment 1 and Experiment 2 that we hope will clarify the results of the longer models, including separate models to test for the effects of sentence type in each age group.

*p.19-20 - the authors express surprise that Study 2 appeared easier for children, but adults responded more slowly for negative sentences than Study 1. This could reflect (a) greater pragmatic felicity of the contrast context for children, (b) the process of checking both characters in Study 2 to be sure that one of the two did have the object (if I've understood correctly, two characters had the same item in the context scene, and all characters had something, making this to my mind a less obvious inference than simply noting that one character doesn't have anything while the others do), and (c) the fact that to disambiguate required the noun in Study 2 but only the negator in Study 1, thus giving a head start in processing for negative sentences in Study 1.*

*…*

*This [disussion] is brief, and generally well written, but should be modified in line with changes to the introduction and a greater consideration of methodological and interpretative issues, especially with respect to the extent to which these data reflect inhibitory control processes vs. pragmatic & methodological factors*

Thank you very much for these possible explanations for why Study 2 was more challenging for adults, but easier for some children. We agree that all of these are important explanations to consider here. We have expanded our results and discussion sections to include some of these possibilities (such as the suggestion that adults might be identifying the referent of negative sentences upon hearing the word “no” in Experiment 1), and we have devoted considerably more space to weighing these possibilities in the General Discussion.

**Reviewer #3**

*This is an interesting paper on children's processing of negated sentences. The authors provide intriguing eye-tracking data suggesting that children under four have real difficulty understanding the semantic contribution of the negative element of a sentence, and propose that this is due to a failure of inhibitory control. I find the data interesting, but the theory rather unconvincing.  
…  
I've put the two reasons for my doubts below. I think that this is an interesting line of work and the data is robust, which is why the review is short. But it is maybe not ready for JML, in that it raises many questions while not really providing any conclusive answers.*

We thank the reviewer for these comments. The questions raised by this review have lead us to reconsider the theoretical interpretation that we presented in our previous submission. In this revision we have focused on the role that context plays in each of our experiments, and the ways that context and pragmatics influence children’s processing of negation. This interpretation is congruent with additional data that we have collected on the processing of negation in adults (Nordmeyer and Frank, *Proc. Cog. Sci.,* 2014), which also supports the role that context plays in the processing of negative sentences.

*1) I don't buy the calculate-the-affirmative and then negate it story. The pragmatics of negation are fairly complex, but intuition says that they are violated in almost every published experimental study of negation. It is no wonder that adults take longer to understand negation if the element is unexpected and pragmatically infelicitous. I think Nieuwland and Kuperberg make a point that is underrated by the present paper.  
  
In fact, it looks to me that the authors' data from adults is consistent with Nieuwland's claims: When you make negation felicitous ("the boy who has no apples" is a pretty good description  of the unlucky kid with nothing, when paired with an apple-toting friend), adults are fast to respond. When you make negation infelicitous ("the boy who has no apples" is a pretty strange description of a boy lucky enough to have presents (!) when paired with an apple-toting friend), then adults are slower to respond.  
  
Of course, my feelings about this theory would be neither here nor there if the data strongly supported it. But I'm not sure it does. It is pretty notable that the purportedly inhibition-easing method used here improves children's performance, but impairs adults' performance. This suggests that either the inhibition theory is wrong (at least for adults), or that the authors aren't using a valid experimental manipulation of inhibition.*

*2) What sort of inhibition are children performing? Is it inhibition of the affirmative meaning, or some sort of inhibition of a salient visual signal? Are the authors talking about inhibition having an effect on children's understanding of negated sentences? Or about inhibition having an effect on children's overt responses to negated sentences (e.g., their eye movements in this visual world task)?*

*These things are quite different, and it understandably leads to the discussion in the paper getting confusing. It also makes it hard to interpret the exact rationale for the change in design for Experiment 2. Under the inhibitory-control-of-eye-movements story (presented in the rationale for the study),  then maybe it is easier for participants to inhibit the strong bottom-up attention-drawing signal from the apple-boy, in order to look at the similarly-strong bottom-up attention-drawing signal from the present-boy. But it isn't obvious that this manipulation makes it easier to inhibit the affirmative meaning associated with "the boy who has no apples", which is the claim in the discussion.*

We thank the reviewer for these comments, and we will respond to both of them here. It was not initially our intention to strongly endorse the calculate-the-affirmative then negate account; rather, we intended to raise it as a possible explanation for the pattern of data seen. We do see how the reviewer could have interpreted our discussion as a making a stronger claim, which was an error on our part. Because our revised manuscript focuses primarily on the role of context in children’s processing of these sentences, we no longer explore such an account as a possible explanation for our data.

We do believe that there may be some inhibition of a salient visual signal, and that this then poses a challenge for children’s overt responses to negative sentences. We raise the possibility that in certain contexts (such as the contexts set up by Experiment 1) these inhibitory demands might make negative sentences might be particularly difficulty for children to respond to. This suggestion is now in the discussion, as opposed to being the primary organizing account of the manuscript. Instead, congruent with the reviewer’s comments, we believe that the pragmatics of negation are an important factor to consider, and this is the primary factor we focus on in our revised paper.

*To reiterate, in case of revisions, I do not think the authors make a good case for the relationship between their evidence and the argument that inhibition difficulties constrain children's understanding of negation. First, because I think their data provides evidence against an inhibition-of-the-affirmative account of adult processing. Second, because they are not very precise in explaining what sort of role inhibition might be playing in their theory.*

*I really do think the data are interesting, but I'm a child language person so I would say that. Unfortunately, because I don't think the espoused theory really holds up, I'm not sure that this paper is right for JML. If the data doesn't provide a solid buttressing for a theory of why children find negation hard, then we're just left with an exploration of the conditions under which children start to understand "no", which is perhaps more suitable for a child language journal.*

Once again, we thank the reviewer for these comments. We ultimately agree both that the role of inhibitory control (especially in terms of a inhibit-the-affirmative account) is not the correct interpretation of this data, and that we were not clear initially in explaining the precise role that inhibition might be playing. We hope that our revised manuscript, with our new framing of the paper in terms of the role of pragmatics, will convince the reviewer that this paper is appropriate for JML.