Dear Dr. Adolph,

Thank you for the thoughtful questions about our manuscript, “Real-time lexical comprehension in young children learning American Sign Language (ASL).” We have addressed your questions in our revision, and we believe that the manuscript is substantially improved. Please also find below a point-by-point response to your questions.

We apologize for the delay in our resubmission, and we thank you for your consideration of this revision. Do not hesitate to contact us if you have any questions or concerns.

Sincerely,

Kyle MacDonald

1. The sample size seems very small for a study of individual differences such as this (only 16 deaf children). Can you address this issue with further justification?

We appreciate and share your concern about the sample size of our study since we do not want to contribute an unreliable finding to the literature. That said, we think it is worthy of publication despite the small sample size for the following reasons. First, children learning American Sign Language from birth (native ASL-learners) are an extremely difficult population to recruit. The incidence of deafness at birth in the US is only .002-.003%, and less than 10% of the 2-3 per 1000 children born with hearing loss have a deaf parent who is likely to be fluent in ASL (Mitchell & Karchmer, 2004). Moreover, a rapidly increasing number of deaf infants receive cochlear implants and are raised in exclusively oral language environments, further reducing the population of potential native ASL-learners. Since the primary goal of the current study was to measure developmental changes in processing efficiency in native ASL-learners, we felt that it was critical to set strict inclusion criteria. In order to be in our sample, children had to be exposed to ASL from birth, their caregiver(s) had to be fluent in ASL, and ASL had to be their primary mode of communication. We think these criteria were important for allowing us to measure ASL processing skills under ideal language learning circumstances.

Second, we think that both the deaf and hearing children in our sample should be considered monolingual ASL-learners (see our response to question #2 below). Both groups of children were living in homes in which ASL was the primary language, and therefore, all of the children had similar language experiences, receiving almost exclusively ASL input from caregivers. In the manuscript, we also present evidence that these groups did not perform differently on either language processing measure. Thus, when both hearing and deaf children are included in the analysis, the total sample size is 29 monolingual ASL-learners: still a small sample, but larger than those used in previous research on ASL development.

Finally, we think that the adult data (n=16) and the substantial amount of prior work on individual differences in real-time comprehension of spoken language helped to constrain our analyses and to inform our interpretation of our findings, making us more confident in the results reported in the manuscript.

We do, however, realize that it is still a small sample, so in our discussion we emphasize that replication of these findings is an important next step. To better address this concern in the manuscript, we have expanded the justification of our sample in the Participants section to make it clear that our sample size was limited by the rarity of this population.

2. All the analyses pool the deaf and hearing children - that seems risky at best and possibly misleading depending upon the data structure.

This is an important concern, and we thank you for pointing it out. It is true that the deaf and hearing children differ in their capacity to access auditory information. However, we took great care to include only children who were exposed to ASL from birth through interaction with at least one fluent ASL caregiver. In fact, the majority of our hearing children (10 out of 13) had two deaf caregivers, and all participants used ASL as their primary mode of communication. So although the hearing children couldexperience spoken language, their language input was almost exclusively ASL since ASL was the primary language of the home. This situation parallels the situation of many children learning Spanish as a first language in the United States. These children speak and hear Spanish exclusively in the home, but then go on to become bilingual when they enter school and have more opportunities to experience English outside the home.

To address this important concern in the manuscript, we have expanded our description of the sample to include a justification for treating deaf and hearing children as monolingual ASL learners. We have also moved the “effects of hearing status” analysis to the beginning of the results section. This analysis directly compares deaf and hearing children’s performance on the Visual Language Processing task, and provides evidence that the two groups perform similarly on both processing measures. .

3. The main result (that processing efficiency correlates with vocabulary size seems almost a tautology), can you clarify why this is an important advance?

AF/VM comments here.

4. Can you provide more information about the possible effects of the "priors" selected on the results obtained, for your Bayesian analysis.

We agree that it is important to show that our analysis is robust to the specification of the prior distribution. In the supplementary information, we provide much more detail about the Bayesian models used to analyze our data. We also present a sensitivity analysis of our model, which shows that our estimates of the associations between age/vocabulary and accuracy/reaction time (RT) are robust to different parameterizations of the prior distribution and to different cutoffs for the analysis window.

To better address this concern in the manuscript, in the results section we explicitly point the reader to the supplementary materials if they have questions about the role of the specification of the priors or the analysis window.