October 23, 2023

Dear Editor Backus and Reviewers,

Thank you for taking the time to review this manuscript and to submit comments. I have addressed each of these comments below in red. For consistency, all changes to the manuscript are also in red. I appreciate your efforts to improve this manuscript and am confident that, in its revised state, it will contribute meaningfully to the field in the ways that the reviewer has recognized. The first section incorporates comments from reviewer #1, the second responds to comments from reviewer #2, and the last incorporates information about additional changes I have made to the manuscript to improve general flow, incorporate very recent research articles, and adhere to word counts.

**Response to Reviewer Comments**

Please consider citing López Otero, Cuza & Jiao (2023) as it is another example of a study that looked into cumulative exposure as a proxy for overall activation within Putnam and Sánchez’s (2013) framework.

This citation has been added where mentioning cumulative exposure as a proxy to activation.

Change “Bhatt et al., 2015” in your in-text citations for “Montrul et al., 2015”

This has been changed; thank you for your correction.

I understand that Torrego (1998) proposes that DOM in Spanish derives from the dative marker. However, it can be confusing that you refer to it as a dative marker as it does not mark indirect objects. It is an accusative marker or simply DOM. The manuscript also refers to DOM as a preposition, which is not accurate either given that, for instance, prepositions are not sensitive to the lexico-semantic features of the objects that they mark. See page 10/27 lines 14-15.

This is a valid critique of the terminology in this study and I have opted to use the term *differential object marker* for greater clarity. In this way, I do not need to commit to defining which part of speech this marker is or what type of case it marks.

Ticio (2015) is included in your in-text citations but does not appear in your list of references.

I have cited this data and have made an additional reference to Requena (2022) in the manuscript that finds that the data reported by Ticio could have alternative analyses.

A limitation in your methodology that should be acknowledged is that the study does not look into the marking of inanimate objects. Therefore, it cannot be determined whether these speakers are acquiring the animacy-driven nature of Spanish DOM or whether they could just be overextending this marker to inanimate contexts.

This is an important consideration that is admittedly overlooked in this study. I have added this to the limitations section, along with citations that corroborate the finding of DOM overextension.

Please consider rephrasing this sentence: “Research on DOM with adult HS has been consistent with children”.

I have rephrased this sentence to highlight that the findings in both are relatively similar.

Please consider citing Hur (2022), in which she explores lexical frequency effects in the acquisition of DOM among heritage speakers in several contexts: both animate and inanimate objects as well as with different types of verbs according to how frequently they select animate versus inanimate objects.

I have added this citation and have talked about the importance of verb type for future research.

Please add “HS5” to this piece of text: “HS in fifth grade (n = 41; ages 10-12)”.

I have made this correction.

Rephrasing your hypotheses would help the reader follow your results and discussion. State your hypothesis clearly and frame it within previous proposals and studies. Then, reveal how that hypothesis should be confirmed by translating it into a prediction that can be easily verified with your methodology.

I have added 1-2 sentences to each prediction to better contextualize them based upon existing results. I trust that the predictions are clearer at this stage.

Please include appendices with the methods you used: questionnaire, BESA, DELE (which version did you use?), SCT and MST (include test items). Also, please include a screenshot of the SCT and MST to show what the participants saw when completing the tasks.

For copyright reasons, I would not feel comfortable submitting the DELE or BESA materials. Although we may be able to add words to address the reviewers’ comments, I fear that the materials would consume too much space and cause me to deviate substantially from the word count. I have added the screenshots to the manuscript and renumbered the figures accordingly and have made a reference to my dissertation where these materials can be found.

Please include the information for number of Spanish-speaking parents regardless of them being monolingual or not.

This information has been added to the participants section.

For clarity, please reveal that the tasks also looked at the acquisition of mood before explaining that you controlled for the verbs included in the test items “to avoid confounding variables in HS’ mood systems”. It can be confusing for the reader.

I have removed this comment and have left the information presenting the fact that this experiment looks at mood morphology where it was.

Change “/a/” for “-a” in page 14 line 39 or 40 as you refer to it as a morphological inflection and not in phonetic-phonological terms.

This is now changed.

If I am not mistaken, tables in APA format do not have vertical lines. Please change those accordingly.

I have removed vertical lines from the tables, with the understanding that further changes may take place during typesetting.

Please explain why you do not include z values in Table 3.

I have added *z* scores to the table.

Change the dot to a comma in page 19/27 line 22: “Before turning to a discussion of results. Figure 5 presents…”

This is now changed.

Please include an appendix with a table including the results presented in Figure 5.

Due to space limitations, I have chosen not to include a table as an appendix. However, readers can review the code and data on the GitHub repository that I have shared in the manuscript.

A suggestion for further analysis of your child data would be looking at the effects of the children’s ages in months instead of categorically (HS5 versus HS7/8).

This is an excellent suggestion, but I regret to inform that I do not have participants’ ages in months in the data that was collected. For this reason, only the present categorical groups can be represented.

**Responses to Reviewer #2**

What is the author truly analyzing? The article appears to indicate that it is examining production and comprehension. However, I have reservations regarding whether the tasks utilized in this study effectively assess these two aspects. For instance, how can one evaluate comprehension without a proper comprehension component? Similarly, how can you assess production without accounting for production time?

Thank you for this comment. I am in full agreement that my data are not appropriately equipped to address “true” comprehension, and this is an issue in existing studies that have reported asymmetrical performance between production and underlying knowledge. As a result, I have made a concerted effort to rephrase throughout the manuscript, clarifying that my tasks distinguished between productive and receptive knowledge, but were not equipped to evaluate comprehension of DOM (although such a task may have been revealing).

The author does not mention whether the participants had received formal Spanish language instruction or had taken Spanish classes. This information could be highly relevant for understanding the results, particularly when interpreting the results obtained by the older participants.

Some children participated in bilingual education; the effect of bilingual schooling is addressed in another manuscript blinded here for anonymity purposes. I have mentioned this in the manuscript.

More information is required about the tasks employed in the study. For example, it would be helpful to know what software was used to record the oral responses. Were the tasks timed? If so, what was the rationale behind timing or not timing them? This timing factor can significantly impact the results and conclusions, distinguishing between implicit and explicit findings.

I have included a sentence in the task description describing that both were offline tasks. Children completed these tasks using computers in their schools, which sped up the data collection process and made it more feasible at the tail end of the COVID 19 pandemic, when this study took place. It is a valid observation that the present study may target more explicit knowledge of DOM, but this is consistent with many previous studies on this topic, including one evaluating bilingual children that was published in IJB (Cuza et al., 2019).

Many studies have previously reported DOM variation among monolingually raised native speakers from Mexico. The author should acknowledge this in the study, as well as the fact that DOM variation has been documented in the Spanish spoken in the United States. Could this variation potentially influence the results?

This is an astute observation and I have added multiple references concerning DOM variability. However, there is no evidence of DOM variability across these references with animate and specific direct objects that are proper nouns (and are therefore [+definite]). Furthermore, the comparison group used DOM categorically, as did a similar group of adults in Cuza et al. (2019), an article with younger children that was published in IJB. I have clarified the lack of dialectal variability in DOM with the specific context tested in the manuscript. Thank you for pointing this important consideration out.

Further explanations are needed for some of the tables and graphs presented in the paper.

I have clarified the figures in the prose of the manuscript. Please also note that I removed what was previously Figure 3, the visual summary of the GLMM model, because it was redundant with Table 3. All of the outputs of this model are in that table, so further visualizations are not necessary, particularly in a journal such as IJB that prioritizes concise research reports.

The author should provide a more thorough explanation of how the results either support or fail to support the hypothesis (Page 20).

Thank you for this comment. I have removed the very brief summary of results relative to the research questions at the end of the results section, and have fully restructured the discussion to provide an analysis of the data relative to the research questions and hypotheses. I believe that this greatly benefits my discussion section and makes the theoretical contributions of my manuscript easier to parse.

**Additional Changes**

* I have made the syntactic analysis of DOM more concise and coherent.
* I have made the review of past research on DOM briefer in an attempt to adhere to IJB’s effort to make research reports concise. Rather than providing overviews of specific studies, I have concentrated on commonalities across them, particularly with adults.
* I added Arechabaleta Regulez and Montrul’s (2023) study on DOM to this paper. It was published very close to when I first submitted this manuscript, and I was not yet aware of it.
* I restructured much of the discussion to answer the research questions more concisely and make more succinct connections to acquisition theory, in line with the reviewers’ comments.
* I condensed the introduction and section #2 into a single section. Much of the information throughout these sections were repetitive and this provides a more concise presentation of the theories as they relate to the present project.
* I cited the R packages used to conduct the statistical analysis.
* I reformatted Table 2 to reflect raw scores rather than percentages. This is a better way to report the standard deviation and also is less redundant with Figure 3.
* I have adjusted the statistics to better reflect the data from the heritage speakers. There are now two statistical models: one comparing all groups, and the other evaluating task, proficiency, and frequency of use with HS only. The second model also explores the interactions between task and proficiency as well as task and frequency of use. These interactions are revealing because they provide useful insight into Putnam and Sánchez’s (2013) predictions.
* I have adjusted the graphs to include HS data only for proficiency and frequency of use, in alignment with the modified statistics.
* I have added commentary on the number of HS who have converged on SDB DOM levels.
* I have pluralized “HS” in all relevant contexts throughout the manuscript (HSs reads better, and is consistent with the use of the term “HLs”).