Reviewer’s report

Title: A randomized controlled trial to examine the effect of two teaching methods on preschool children’s language and communication, executive functions, socioemotional comprehension, and early math skills

Version: 0 Date: 23 May 2019

Reviewer: Jose Antonio Hinojosa Poveda

Reviewer's report:

In this cluster-randomized, controlled study the authors compared the effects of two interventions with a different accent in learning socioaffective (SEMLA) and academic (DIL) skills in preschool children that were assessed with ECERS-3. The results of mixed models regressions showed that none of the interventions had effects on the skills they were supposed to enhance, although the authors observed some relationships between some variables. This is a nice study on and interesting question. The design is sound and the manuscript is well-written. I have the following comments on this study:

- Even though this is a big study that has been competently developed and conducted, a main criticism has to do with the lack of results. This brings into question the main assumptions of the study. However, this reviewer believes that studies with negative results should be published, provided that data comes from appropriate designs and are competently analyze. Using mixed model regressions seem an appropriate strategy for analyzing data here. However, the authors seem to obviate issues related to the statistical power of their sample that should be considered (and analyzed) in a revised version of the manuscript. Also, a Bayesian approach might shed some light on the null findings.

- Whereas some of the predictions are based in the results of prior studies (e.g., SES effects on language, EF and socioemotional comprehension), other seem rather based in authors' intuitions (e.g., correlations between a high score on language tasks pre-intervention and socioemotional comprehension; higher SES for Swedish children compared to children whose Swedish is less dominant,…). These hypotheses should be also grounded on prior findings.

- Regarding the EEG study, the authors state that active electrodes were re-referenced to the mastoids. They should be more precise while reporting this information: average of the mastoids or linked mastoids?. How many channels (on average) were interpolated?. A 50% rate of epoch rejection is a very high one, did the authors correct for ocular artefacts?. Indeed, a lot of information is missed from this study: the number, duration and
characteristics of the stimuli; the background for the expected ERP effects (why did the authors analyze only activity between 100 and 200 ms?, which component?, Why did they include all the electrodes in the analyses?, which are the basis for creating a selective attention variable after subtracting activity for attended and unattended responses?, Why did the authors suddenly analyze activity between 300 and 400 ms?. Finally, the discussion of these data on p. 56 and 57 is too vague (what kind of general demands in terms of processes?, what kind of differences between pre- and post-intervention sessions that were not intended?).

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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I am able to assess the statistics

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