Author’s response to reviews

Title: Individual and Organizational Factors that Affect Implementation of Evidence-based Practices for Children with Autism in Public Schools: A Cross-Sectional Observational Study

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Author’s response to reviews:

Dr. Justin Presseau
Associate Editor, Implementation Science

Re: 18-00452R1 titled "Individual and Organizational Factors that Affect Implementation of Evidence-based Practices for Children with Autism in Public Schools: A Cross-Sectional Observational Study"
Dear Dr. Presseau,

We appreciate the opportunity to provide revisions to our submission. We have revised the manuscript to incorporate the reviewers’ suggestions. Attached to this letter is an itemized list of each item with a direct response in italics. Please let me know if you need any additional information to assist in your review.

Thank you very much.

Associate Editor’s report:

There are a few minor issues that remain to be addressed, including a few that have surfaced as a result of the most recent revisions to the tables:

1. Table 2: It is not clear why the correlations between EBPAS and ILS/ICS are not presented in Table 2. If it is possible to add these to the table, it would help to speak to points about the relationship between individual and organizational factors.
   
   We have combined the correlations between EBPAS and ILS/ICS in Table 2.

2. Table 2: It is not clear why the dependent variables are not included in Table 2. Adding these would allow the reader to appreciate the bivariate associations between IVs and the DVs.

   We have added the dependent variables in Table 2.

3. Table 2: I suggest including only the actual variables used in the main regressions in the correlation matrix in Table 2. In other words, there does not seem to be a need for the total scores AND the individual sub scale for ILS and ICS if the main analysis only uses the total scores. Reducing the correlation matrix to the total scores would make the table more easily interpretable on its own and more directly linked to the regressions subsequently reported.

   We have included only the actual variables used in the main regressions in Table 2.
4. Table 3 is a little confusing. The ‘unadjusted’ analysis seems to amount to the same analysis as would be presented in the bivariate correlation matrix if the correlation matrix also included the dependent variables. I suggest adding the dependent variables to the correlation matrix in Table 2 and removing the top portion of Table 3 referring to the ‘unadjusted models’ from Table 3, and instead more clearly focusing Table 3 only on the adjusted models. I would also separate each regression with a line because at the moment, at first glance table 3 reads as if it were a multiple regression (i.e. all independent variables added into same model) but what is actually presented (I think) are 14 different regression analyses. That needs to be made clearer and mentioned in the limitations (i.e. that these analyses may be capitalizing on chance when running so many analyses needs to be made clearer in the limitations section).

We have added the dependent variables to the correlation matrix in Table 2. We have removed the top portion of Table 3 referring to the “unadjusted models.” We have separated each regression with a line to distinguish the difference analyses. We have added a note in the limitations on page 16 to the revised manuscript that indicate that multiple analyses were conducted and may capitalize on chance.

5. Table 3: consider reporting the Bs for the covariates for each regression run as well so that it is clear what contribution each IV/CV makes to the R2 for each regression.

We have added the Bs for the covariates that were included for each regression model.

6. Table 3: Why no B and R-square in Table 3 for PRT intensity for EBPAS? I would add these (even if no covariates were included; this would be made clear by adding the covariates for all the regressions)

We have added B and R-square in Table 3 for PRT intensity for the EBPAS.

7. Table 3: p<0.1 is not a statistical significance standard. I would remove any mention of that from table 3 (should only be using p<.01 and p<.05 as these are, for better or worse, the consensus standards for significance testing)

We have removed the p<.1 note from Table 3 in the revised manuscript.

8. To some extent, using the term ‘attitudes towards EBP’ does a disservice to the specific individual constructs assessed with each subscale of the EBPAS. “attitudes” is not particularly
indicative of 'intuitive appeal', 'willingness if required', 'openness', and 'divergence'. Consider whether more precise terminology might be preferred over 'attitudes'.

We have specified that attitudes comprised “intuitive appeal”; “willingness if required” “openness”, and “divergence” in the revised manuscript. To be mindful of the brief report and space, we use “attitudes” in the revision; however, we adjusted our terminology to be as specific to the subscales as possible, wherever possible.

9. Discretionary: given that R-squares are presented n Table 3, not clear why f-square is presented elsewhere in the manuscript. Why not use R-square throughout, for consistency?

We have added R-square in the Results section to be consistent with Table 3. The f-square was our estimate of effect size per our previous reviewer’s feedback.

Reviewer #3

Abstract

1. The authors misunderstood my Comment #3; I meant that the first two sentences of the Results section *of the abstract* (not the manuscript text) refer to methods, so should be moved into the Methods section *of the abstract*.

We apologize for our misunderstanding. We have revised the results section of the abstract per the reviewer’s suggestion.

2. Likewise, the authors misunderstood that my Comment #4 was about the Results section *of the abstract* (not the manuscript text), and that I was recommending that they make clear *in the abstract* that there were no significant findings for any EBP other than DTT. Put another way, there were significant findings only for one of the EBPs, which seems useful and important to state upfront in the Abstract.

We appreciate the opportunity to make these revisions. We have stated that there were only significant findings for one EBP, DTT in the revised abstract.
3. And similarly, again, the authors misunderstood that my Comment #5 was about a sentence in the Conclusions section *of the abstract* (not the manuscript text). If the authors addressed Comment #4 as I had intended, such a correction would probably help with my concern in Comment #5.

We added the following statement to the Results section of the abstract: “There were significant findings for only one EBP, discrete trial training.” We hope this revision clarifies our concluding statement that, "Future implementation efforts ought to consider the type of EBP…”

4. Each of these misunderstood comments appeared under an "Abstract" header, which the authors must have missed. I paid particular attention to the Abstract because as we all know, it is often the only section of a paper that is actually read.

We thank the reviewer for their thoughtful review and sincerely apologize for our misunderstanding.

Background

5. The second sentence of this section has become very long and hard to follow. I appreciate that this sentence reflects edits made in response to reviewer comments, but suggest that it be broken into two or even three sentences for readability.

We appreciate this reviewer’s feedback and have revised the second sentence of the introduction.

6. p. 7, third line of text: Delete hyphen after "individual."

Apologies for our oversight. We have made this change.

Measures

7. p. 9, second line under "Independent Variables" header: "Attitudes" is misspelled.

We have corrected the spelling of “Attitudes” and have spell-checked the entire manuscript.