Reviewer’s report

Title: Early maladaptive schemas as predictors of maternal bonding to the unborn child

Version: 0 Date: 19 Nov 2018

Reviewer: Amanda Cooklin

Reviewer’s report:

PSYO_D_18_00135

Thank you for the opportunity to review this paper.

The authors use data from a cohort of parents recruited in pregnancy and followed until 6 months postpartum. This study focuses on data collected in pregnancy, and investigates the relationship between maternal bonding to the fetus and early maladaptive schemas (relationship / attachment styles). The authors also investigate whether this relationship is mediated by maternal depression.

This is an interesting topic of study and the authors have rich data to bring to this. However, the analyses are simple univariate, without any controls for other confounders relevant here.

The main exposure here - EMS - is a scale which comprises of 18 different schemas. This is unwieldy and makes it difficult to make sense of the various correlations presented with the main outcome (two subscales of the MAAS), attachment to developing fetus.

I suggest some more data reduction / handling techniques would make the paper more 'digestible' and support the presentation of key findings more cleanly. Theoretically, is it anticipated that each of the EMS would have an equal negative relationship with maternal-fetal bonding - both subscales? It appears not from results reported here, so can these schemas be condensed, or grouped in some way? A theoretical approach to underpin some further analyses might help draw out the main relationships here.

The rationale is interesting - although lacks a little grounding in why EMS should be in focus (other than there is a gap) - some more around attachment styles and how they might then influence maternal-fetal attachment would be welcome to build a stronger rationale.

The factors associated with MAAS p4 Background - please provide the direction of these relationships so the reader can have a sense of how MAAS is shaped by social circumstances.

More detail about recruitment and sampling is required - who did and did not agree to participate in the study? How were partners recruited? What biases do each of these recruitment decisions bring to the sample - and what are the implications for the results. The sample are quite socio-economically advantaged, presumably this is protective (for EMS, MAAS) so this needs to be addressed.
The multivariate analyses, if I have understood correctly, model all the EMS subscales - together - as 'predictors' of the two MAAS subscales. Some have lower than acceptable Alpha - why were these retained? The authors present the correlations between EMS and main study outcomes (EPDS, MAAS etc) - but not with each other, presumably the EMS indices are highly correlated? Is it then appropriate to use them together in a regression? Further - and importantly - why are analyses not controlled for other potential confounders here - the authors have these data (e.g. demographic, prior history, first parent etc..) so it is puzzling that this has been omitted.

Due to the 'modeling together' of the EMS - it is likely that some other notable relationships are masked, or lost - back to my earlier point about some consideration in handling the exposure more carefully.

The depression / mediation analyses are interesting, and add a dimension to the paper.

**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.

No

**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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