Reviewer's report

Title: Characteristics of positive-interaction parenting style among primiparous teenage, optimal age, and advanced age mothers in Canada

Version: 1 Date: 22 Apr 2017

Reviewer: Thomas Schofield

Reviewer's report:

I've never been faced with writing a review quite like this one. With respect to the Authors, the methods remain inappropriate. Authors were made aware in their original submission that they were making a methodological decision that was 1) very flawed and 2) simple to correct. In response, they wrote "The purpose of this paper was epidemiological and exploratory in nature." If the authors are saying that epidemiological researchers do not care whether or not they are using flawed techniques, we must agree to disagree. Where the Authors and I do agree is that the paper is exploratory, with all the concerns that entails. An important part of the peer review process is to ask the authors to buttress the credibility of their chosen analytic approach by running alternative specifications, and reporting back on the degree to which their pattern of results holds. It is likely the Authors are aware of this, which makes their decision to dig in and refuse the reasonable requests for alternative analyses so troubling. This reviewer is left to conclude that the pattern of results only emerged when the data are sliced and diced, and therefore cannot be trusted enough to merit publication in ANY journal (let alone in a BMC Journal). There is no difference across the three age categories in terms of self-reported parenting. The authors are good enough to acknowledge this. Consequently, the heart of this manuscript is Table 2, the degree to which these socio-demographic characteristics are correlated with positive interaction parenting across the three groups. Unfortunately, the current manuscript (like the previous draft) completely fails to adequately address this point. If there are no *significant* differences across the three age groups in the association between socio-demographic variables and positive-interaction parenting, there is no story to tell. Right now the authors appear to want readers to look at Table 2, eyeball the boldfaced coefficients in the 'optimal age mother' group, and then conclude that those associations between sociodemographic characteristics are only correlated with parenting among that age group. That approach is as fundamentally flawed as looking at a graph of means and deciding that groups must be meaningfully different without consulting a t-test (or statistical equivalent). If the 95% confidence intervals overlap across the three groups like they do in Table 2, the associations between sociodemographic characteristics and positive-interaction parenting are probably invariant across the three age groups. This may be why the authors are disinclined to run the statistical moderation test between sociodemographic characteristic and maternal age group I suggested previously. In essence, the authors are trying to publish null effects in BMP pediatrics and passing them off as something else. If the Authors would have the reviewers view the matter differently, they might have engaged beyond the superficial responses given in their letter.
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.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I am able to assess the statistics

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