Author’s response to reviews

Title: Impact of parent-adolescent bonding on school bullying and mental health in Vietnamese cultural setting: evidence from the Global School-based Health Survey.

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RESPONSE SHEET

Editor-in-Chief
Journal of BMC Psychology

Dear Editor,

We are pleased to submit our revised manuscript entitle “Impact of parent–adolescent bonding on school bullying and mental health in Vietnamese cultural setting: evidence from the Global School-based Health Survey”.

We found reviewers’ comments useful and made revisions accordingly.

Thank you for your consideration. We look forward to hearing your favorable reply soon.

Sincerely,
Authors’ Response to the Reviewers’ Comments

[PSYO-D-18-00092R1]: Impact of parent–adolescent bonding on school bullying and mental health in Vietnamese cultural setting: evidence from the Global School-based Health Survey.

Authors would like to thank all reviewers for constructive suggestions and helpful comments on our manuscript. We have revised the manuscript to meet each of the reviewers’ suggestions and comments as below. The changes we made in the manuscript are marked in red.

Reply to Reviewer 1:

Richard Rowe, PhD (Reviewer 1): I am grateful to the authors for being so responsive to my comments. I believe the revision process has substantially improved the ms. I still think a few further revisions:

1. The authors have made helpful revisions regarding defining their approach to parental bonding in response to the comments of both reviewers. However, the introduction still needs to clarify what the concept of overprotection means.

Response:

- According to the reviewers’ comments, we revised a sentence to clarify the concept of overprotection (Introduction section, page 4, line 2-4).

2. p4 line 8. It is stated that parent-adolescent connection has changed in Vietnam; this claim needs evidential support.

Response:
- According to the reviewer’s suggestion, we revised the sentence and added a reference (ref. 21) (page 4, line 12-14).

3. In my original review I highlighted the problem that the language of the paper makes causal assumptions. While the authors have made attempts to reduce this language, it still remains in several places, including the abstract. For example, p4 line 12 talks about the protective and negative elements of bonding. However, it is possible that the characteristics of bonding are the result of the child's mental health state rather than the cause. I think this is particularly relevant to the parenting variables; increased monitoring may well result from the child being very difficult to monitor, for example. I believe the paper would be improved if the authors discussed the alternative causal pathways that may exist between variables in the main section of the discussion rather than leaving this until the limitations section. They may also find reference to the following paper helpful. This paper reviews evidence on the causality of a number of the relationships discussed in this ms:


Response:

- According to the reviewer’s comments, we amended sentences to avoid using languages suggesting causal assumptions (Page 2, line18-19; page 4, line 17; page 12, line 6-9). We also discussed alternative causal pathways related to parenting variables (Discussion section, page 13, line 21-24; page 14, line 1-2) by citing a reference suggested by the reviewer (ref. 39).

4. The aOR term needs to be defined in the abstract.

Response:

- Definition of aOR was added in the Abstract (Page 2, line 15).

Nora McIntyre (Reviewer 3):

General comments

- I am not convinced that parental control can be described as one dimension in parent-child bonding. I might expect parent control to be a predictor of decreased bonding rather than them being part of the same construct. Do authors have a basis for putting acceptance and control
under the same umbrella, then naming that bonding? I am open to persuasion but please provide a strong basis for this decision from the literature. Otherwise, I would suggest using a more neutral term to group acceptance and control, such as parent-child ‘relationship' or 'dynamics'. To be clear, I do not recommend using 'connection' instead of bonding because it shares the same problem.

Response:

- In response to this comment, we used the term “parent-adolescent bonding” referring two dimensions, care and overprotection, based on a concept provided by Parker et al. [Ref. 16: Parker G, Tupling H, Brown LB: A Parental Bonding Instrument. British Journal of Medical Psychology 1979, 52(1):1-10.] In our evaluation,

Two sentences were amended to clearly refer adoption of this concept and components of “parent-adolescent bonding” (Page 4, line 2-4; page 6, line 4-5).

Introduction

- Please provide a convincing argument for choosing to focus on Vietnam out of all the Asian countries. Is it representative of wider Asian cultures in some way? Is there a particular aspect of the Vietnamese mindset that particular disposes its members towards loneliness and suicide ideation? The authors seem to present the cultural specificity to be a major contribution of this research. It is therefore important to demonstrate why culture should be such a focus in parenting and child mental health research. By the way, I would describe this area of the world as East Asian rather than Asian, as there are vast differences in mindset and values among Asian nations.

Response:

- According to reviewer’s comments, we referred “Southeast Asian culture” instead of wider “Asian culture” throughout the manuscript (Page 2, line 20). Among 11 countries in Southeast Asia, Vietnam is one of the top 3 countries that show high GDP growths from 2007 until now (World Bank national accounts data, and OECD National Accounts data files). As a result of economic growth and increased internal migration followed by economic growth, Vietnamese family structure and the roles of parents have changed in modernized families. We revised the paragraph by adding two new references (Page 4, line 11-14).

- No hypotheses have been presented at the end of the Introduction which actually makes the Results and Discussion sections difficult to follow. Please add these and organise the Results and Discussion sections accordingly.

Response:
According to the reviewer’s comments, we amended a paragraph to present hypotheses of this study (Page 4, line 17-20) and reorganized the Discussion section (Page 12-14).

Method

- The parent acceptance measure contains an item that is more monitoring-related than acceptance (or understanding or nurturing) related. I think Reviewer 1 disputed the use of this item as part of the control dimension, but I don't think moving it under acceptance is appropriate either. Authors should consider dropping data based on this item altogether, framing monitoring as 'positive control' in contrast to 'negative control' that the current parent control item reflects, or integrating a third parenting dimension (monitoring) into the manuscript's framework. I believe Reviewer 2 suggested something similar previously.

Response:

- According to the reviewer’s comments, we integrated a third parenting dimension (monitoring) in the manuscript’s framework instead of combining two items in ‘parent acceptance’ dimension. We revised all related paragraphs (Page 2, line13, 18; page 6, line 4-5; page 7, line11-13; page 9, line 6-8, 13-14; page 10; line 4; page 12, line 7-9, 19).

- Reviewer 1 picked up on the use of single items to as dimensions in whole constructs being examined in this paper. I am not satisfied with authors' responses to this comment. I am not convinced that having a large sample made it that much more difficult to utilise an existing, established and more reliable scale (PBI) for investigating the key constructs in this study. The sensitivity argument is flawed, because the topic of parent-child relationship has already been raised via the students' participation in this study: to request multiple responses to closed questions such as in the PBI is not a significantly greater demand. I'm afraid this is a major limitation in this research and is possibly insurmountable, unless authors provide a more convincing rationale or collect new data using a multi-scale instrument.

Response:

- Multi-scale instrument (PBI in this case) is considered as a good measurement for parent-adolescent bonding. We analyzed data from Global School-based Student Health Survey using GHSH questionnaires (80 questions) which provides variables individually represented by a single item. We added a sentence to discuss this matter (Discussion section, page 14, line 20-24).

- As Reviewer 2 had already pointed out, the variables that you describe as binary actually appear to be continuous. Please amend your phrasing.
Response:

- In response to the reviewer’s comment, we would like to explain two reasons that we describe the variables as binary. First, although the question measured bullying was “how many days...?”, the response were categories as: “0 days”; “1 or 2 days”; “3 to 5 days”; “6 to 9 days”; “10 to 19 days”; “20 to 29 days” and “all 30 days” instead of the continuous response. Similarly, mental health outcomes (loneliness) also had category answers (never/rarely/sometimes/most of the time/always) (Method section, page 5, line10-19). Second, distribution of these variable is statistically skewed to the left, for example: the skewness of “be bullied” variable was 3.5; and parental control was 0.68. Then, we recoded these category variables as binary variables for further analysis in this manuscript.

Discussion

- The third paragraph in the Discussion seems insightful but it does not yet clearly relate to the findings in this manuscript.

Response:

- According to reviewer’s comment, we revised the third paragraph in the Discussion section (Discussion section, page 14, line 3-11).