Author's response to reviews

Title: Parenting Stress: A Cross-Sectional Analysis of Associations with Childhood Obesity, Physical Activity and TV Viewing

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Author's response to reviews: see over
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Lorenzo Iughetti
Section Editor- Nutrition, diet physical health, and endocrinology
BMC Pediatrics

Re: Rebsubmission of manuscript #6073698221372452, “Parenting Stress: A Cross-Sectional Analysis of Associations with Childhood Obesity, Physical Activity and TV Viewing”

Dear Dr. Iughetti,

Thank you very much for the opportunity to address the reviewers’ comments and resubmit our manuscript (MS #6073698221372452), “Parenting Stress: A Cross-Sectional Analysis of Associations with Childhood Obesity, Physical Activity and TV Viewing” to BMC Pediatrics. Below we provide our responses to the reviewers’ comments. We have also made a number of revisions based on these comments (highlighted in yellow in the manuscript), which we hope will make this paper suitable for publication. We appreciate your continued interest in this manuscript and we look forward to hearing from you.

Sincerely,

Kathryn Walton, MSc, RD
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RESPONSE TO REVIEWER COMMENTS

Parenting Stress: A Cross-Sectional Analysis of Associations with Childhood Obesity, Physical Activity and TV Viewing
Manuscript # 6073698221372452

Thank you to the reviewers for their detailed and helpful comments. Below please find our responses.

Editors Comments:

1. Title page: Please include the email addresses of all authors on the title page. Please ensure that it is the same with the one entered on the submission system.

Response: We have formatted our title page in accordance to the sample on the journal’s webpage, including the addition of all authors’ email addresses.

2. Acknowledgements Section

Response: We have added an acknowledgements section, highlighting the source of funding for this study and manuscript production.

Reviewer #2:

3. Reliability of the PSI-SF is needed

Response: A Cronbach’s alpha test was conducted to test the reliability of the 12-items of the PSI-SF used to measure parenting stress level in this study. The Cronbach’s α was found to be 0.91 indicating good internal consistency. We have added this information to our methods section: “We found the 12-items of the parent distress subscale to have good internal consistency (Cronbach’s α=0.91).”

4. The data appear to be sound, however, the following suggestions are made to improve the convincibility of the findings. Given that the authors reported some previous studies on the topics, it would be logical to include a post-hoc power analysis incorporating the lowest meaningful effect (i.e., lower limit of the OR confidence interval) for the identifying if there was sufficient power in the analyses. In addition, supplemental reliability information on the scales would be helpful especially since the work is a secondary data analysis and it is self-report.

Response: Given the secondary nature of our study we were unable to perform an a priori power analysis. However, while there is a consensus about the importance of doing prospective power analysis there is concern in the statistical literature regarding the
computation of post hoc power. For example, Hoenig and Heisy (2001) argue that post-hoc power analyses should not be used to aid in the interpretation of statistically non-significant results. As a result, we have included confidence intervals for odds ratios which provide sufficient information for interpreting our results.

Abidin (1990) reported a Cronbach’s alpha reliability coefficient of 0.87 for the Parent Distress Subscale of the PSI-SF. The question regarding parental limits on child TV time was found to have Cronbach’s alpha reliability coefficient of 0.87 (Arredondo et al, 2006). Reliability coefficients were not available for our other two secondary outcome measures, child active play and time spent watching TV.

References:


Reviewer #3:

5. Please consider clarifying the fifth sentence in the Statistical Analysis sub-section of the Methods section by stating that "Parental marital status and EDUCATIONAL ATTAINMENT were included in all models" (correction in BOLD). This then removes the need for the last sentence of the paragraph.

Response: This wording has been modified as suggested: “Parental marital status and education attainment were included in all models due to their known association with both increased stress and obesity risk.”

6. There appears to be a word or two missing in the first sentence of the Measures - Stress sub-section of the Methods section.

Response: The words omitted have been added: “Parenting stress was measured using the 12-item parent distress sub-scale of the Parenting Stress Index Short Form 3rd Edition (PSI-3-SF).”

7. Please clarify the acronyms, NASPE and AAP.

Response: These acronyms have been clarified in our manuscript. NASPE= National Association for Sport and Physical Education, AAP= American Association of Pediatrics.
8. The Statistical Analysis sub-section describes that "married/living with a partner" was contrasted with "single" parents in the analysis; but there are other categories listed in Table 1 that should be listed here as well for clarity.

**Response:** For clarity we have included all marital status categories listed in Table 1 in the body of the manuscript. Thus, marital status “married/living with a partner” was contrasted with “single/divorced/separated” in our statistical analysis section: “Both were categorized as binary variables: married/living with a partner vs. single/divorced/separated and graduated high school or less vs. some college/technical school or degree.”

9. The age range included in the study was small; only four years. However, this is a relatively dynamic developmental period for children. I suspect that TV watching and physical activity patterns change quite drastically across this period for some children. Why didn't the authors adjust their findings for age? Did they try to stratify their analysis into two age groups (e.g., 2-3, 4-5)? Even with very small sample sizes, it might reveal very different point estimates worth discussing.

**Response:** We re-ran our analyses stratified by age to find out if age played a role in our findings. We stratified analyses into two age groups as suggested by Reviewer 3; 2 and 3 year olds and 4 and 5 year olds. When stratifying by age we did not find any substantive differences with respect to the effect of parenting stress on child weight status, weekday or weekend active play, TV viewing times on weekdays or weekends or parent TV limiting. Larger confidence intervals were found, especially in the 4 and 5 year olds due to much smaller sample sizes in these stratified analyses. We have added the following text to our statistical analysis section: “As the preschool age group is a dynamic time of development, we also stratified our analyses by age using two groups, 2 and 3 year olds and 4 and 5 year olds (data not shown); we did not find any substantive differences in our results and thus only present results for the total sample.”

10. Perhaps the authors should use their Discussion section to raise the need for longitudinal analyses to follow-up on the study. They may also want to temper their stated conclusion (including in the abstract) that parental stress is not likely associated with increased risk of obesity in children.

**Response:** Based on the reviewer’s suggestion, we have highlighted in our section focusing on future research, the need for longitudinal studies. Such studies will allow us to understand how parenting stress may affect obesity risk as children age. Furthermore, specific to our finding that stressed parents are less likely to limit the amount of TV their children watch, longitudinal analyses will help us to understand the impact setting limits has on child obesity risk over time. We have added the following text to our discussion: “There is a need for longitudinal analysis to understand how parenting stress may affect
obesity risk over time as well as the directionality of the association. Furthermore, longitudinal analyses would allow us to understand how reduced parental TV limits affect the time preschoolers spend watching TV as they age.”

11. The conceptual framework of the study should be clarified. For example, it's not clear whether the first two sentences of the second paragraph of the Background apply to parents or children in this study; specifically, effects of stress on metabolism, appetite and activity. More on the third sentence in the second paragraph in the Background would be helpful for the reader, too. How and why do the authors believe that parenting stress is associated with TV restriction and TV watching; and with physical activity? And what would the associated impacts on over-weight/obesity be? (This may be a good place to introduce the idea of a life-course perspective... habits developed in early life around these behaviours could impact weight gain/loss across the life course.)

Response: We have clarified the framework of our study, highlighting how parenting stress may impact children’s obesity risk through causing physiological stress among the children themselves or through diminished parenting which may impact a parent’s ability to model and promote healthy behaviours known to help decrease obesity risk (increased physical activity and decreased TV watching). We have added the following text to our Background section: “Emerging evidence shows that enhanced levels of stress, associated with chronically high levels of cortisol, can lead to neuro-endocrine responses that alter metabolism, appetite and activity levels and, consequently, obesity risk in both adults and children.” As well as, “Research suggests that high parenting stress may lead to increased obesity risk among children in two ways, 1) triggering the child’s own physiological response to stress and 2) parent stress may lead to compromised parenting which promotes unhealthful behaviours.”

12. Please clarify how NASPE and AAP categories/cutoff’s utilized in this study are relevant to the outcome of childhood obesity.

Response: We used the NASPE and AAP recommendations as both regulatory bodies considered obesity prevention when developing their cut-offs. We added the following text to our Outcome Measure section: “We used the NASPE and AAP recommendations as both considered obesity prevention when developing their recommendations.”

13. Please be clearer about the age ranges covered in the Koch et al and Parks et al studies being compared in the Discussion. Is it appropriate to directly findings of these studies?

Response: Based on the reviewer’s recommendation, the age ranges of the children studied in the Koch et al and Parks et al studies have been clarified in the manuscript. The children in the Koch et al study were between the ages of 2 and 5 years and the children in the Parks et al study were between the ages of 3-17. Given the small body of literature focusing on parenting stress and child overweight/obesity and the similarity of ages studied we believe it is appropriate to compare our study to these existing studies.
14. While the authors raise some plausible limitations in their Discussion section, they don't follow-up through to discuss the specific impact of these limitations on their findings (2nd and 3rd ones, in particular). Please briefly extend discussion on the relevance of limitations.

Response: We have clarified and expanded on the relevance of the limitations of our study. Specifically for the second and third limitations we have discussed the possible impact on our results: “This possible over-reporting of healthful behaviours through the use of parental report measures and the self-selection bias of our recruitment methods may have biased our results toward the null.”