Author's response to reviews

Title: Metabolic syndrome in a cohort of Chinese schoolchildren: prevalence using two definitions and associations with leptin and adiponectin by factor analysis

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Author's response to reviews: see over
Dear Mr. Victorino Silvestre,

Thank you for giving us the opportunity again to revise our manuscript (MS: 197478627736135-Metabolic syndrome in a cohort of Chinese schoolchildren: prevalence and factor analysis). We have carefully read the comments from the reviewers and incorporated our revision into the text. With the assistance of a fluent English speaking colleague - Prof Simon in Prince of Wales Hospital, the Chinese University of Hong Kong, I hope this improved manuscript will meet the high quality of your journal and be accepted early.

Here is a breakdown of the modifications based on the reviewer’s specific comments.

To Prof. Punithavathi Narayanan,

1. The title itself is confusing. do they mean to say "A single underlying factor as cause of metabolic syndrome in a cohort of Chinese school children"?
   Thank you for your suggestion.
   We changed again. I hope this change will make it clearer.

2. Still they have not explained how adding ‘Leptin/adiponectin’ assessment as one of the components will improve the diagnosis.
   This is a very difficult question. At present, we cannot explain well from the results of this cross-sectional study. So we explained in the discussion part: ‘Our factor analysis results suggest that leptin/adiponectin should be considered a component of MetS. However, further follow up studies of leptin, adiponectin and their ratio in children and adolescents are needed to determine if they may be predictors of MetS in adulthood, can increase the stability of MetS diagnosis in children and provide additive value to predicting future cardiovascular morbidity’.

3. discussion requires extensive revision
   Yes, we changed.

4. Some of the discussion points are difficult to understand.
   Yes, this time we make some changes and try to make it clear.

To prof. Constantina Papoutsakis,

1. Responses and manuscript continue to be difficult to understand. The ‘essence’ of a good paper is there but the execution in English does suffer. As written the paper cannot be published. I strongly suggest you use the services of an expert who can edit appropriately the quality of English (phrasing/syntax/grammar/spelling) throughout the paper itself and responses to reviewers as well.
   Thank you for your kind comments. We really get the help of prof. Simon, from Prince of Wales Hospital, the Chinese University of Hong Kong.

2. Revise the title again. I do not understand the title, plus it is not a correct sentence...
   Make your title easy to understand and informative as to your findings.
   We follow your suggestion and change the title again, hope to be more informative.

3. (one minor point) Last sentence of background (in abstract) is long. Re-write.
   Thank you.
4. **Methods:** About the added cut-offs recommended by the working group on obesity in China some correction is needed. You cannot possibly have #95th in the overweight and #95th in the obese category. One of these two equal signs must be removed...

   Thank you, we have made a correction.

5. p.5 You provided in the paper the rationale for including children under 10. However, you did not explicitly list the specific criteria used for children under 10. As written this is still not clear in the paper.

   *We provided the criteria in page 6 as you can see from the highlighted part:* Although IDF definition suggested that MetS as an entity could not be diagnosed in those below 10 years of age, however, to provide a comparison, the individual risk components of MetS were also defined for these younger children as for children aged between 10 to <16 years [4,32].

6. In all tables listing MS criteria, include the specific cut-offs in footnotes, esp. T2 and T3.

   *We have added in T2 and T3, but it seems too long.*

7. Having read the more detailed explanation of the rationale of the exploratory factor analysis, this reviewer wonders why you have not provided prevalence of each MS factor in the entire cohort. You have only provided info in this regard by gender or weight status. This information is missing -prevalence of each MS factor in the entire cohort- and should be listed in abstract (results) and in Tables.

   *We think that our cohort is different from previous community-based or school-based general populations; it comprised of overweight and obese children at risk for MS and their health controls recruited from the cross-sectional population-based Beijing Child and Adolescent Metabolic Syndrome (BCAMS) study. The study included a representative sample of Beijing school-age children (n = 19593, ages 6-18 years, 50% boys). So we think listing the prevalence of each MS factor in the entire cohort may not be very necessary, since we primarily focus on prevalence comparison between two definitions.*

8. (one minor point) What is MAP? Mean Arterial Pressure? This abbreviation is not explained in the body of the paper (saw it was defined in Tables though).

   *It has been explained in page 8: mean arterial pressure (MAP) for blood pressure.*

9. **Discussion:**

   Continues to require extensive revision. As before, there are many areas that are difficult to understand because of the poor quality of English. Please consider whether each paragraph has a main theme and whether the flow from one paragraph to the next is reasonable as put your discussion together.

   *Thank you, we have made some changes.*

10. The newly revised discussion is excessively long. The authors may state the same points with half the length.

   *Yes, we try to make it shorter.*

13. Your findings that Tanner did not alter your results are well supported. However, the discussion still does not include studies that have found the opposite. This needs to be discussed in a way that you include findings that are not in agreement w. yours. Several papers have been published on metabolic syndrome and adipokines in children.

   *Thank you. We try to make it clearer.*

14. At this time, it is still not clear why adiponectin/leptin ratio or either one on its own
would be of added value. Your explanation on healthy vs nohealthy metabolically obese would make some sense if your analysis focused on the obese only. As written, the paper does provided any added value to measuring adipokines, and such an unsupported claim is best removed throughout the paper.

Yes, we have removed the unnecessary part in discussion. Actually we still cannot explain well from the results of this cross-sectional study. So we simply made the discussion like this ‘Our factor analysis results suggest that leptin/adiponectin should be considered a component of MetS. However, further follow up studies of leptin, adiponectin and their ratio in children and adolescents are needed to determine if they may be predictors of MetS in adulthood, can increase the stability of MetS diagnosis in children and provide additive value to predicting future cardiovascular morbidity’.

15. Statistical analysis: State the listed confounding factors in your statistical methods.
We stated the confounding factors (sex and pubertal stages) in the result part for understanding.

16. Analyze the data by adding adjustment for BMI. Chances are all adipokine involvement is mediated by BMI which again is more easily measured than adipokines.
Thank you for your suggestion. Since we used factor analysis to define the clustering features of MS components, unlike other statistical methods such as multivariate regression analysis to look for the independent relationship, factor analysis of the association of adipokines with other MS risk variables, we think, is not necessary to adjust for BMI.