Reviewer's report

Title: Prevalence of the metabolic syndrome among children from six cities of China

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Reviewer: Benjamin Guinhouya

Reviewer's report:

Major compulsory revisions.

Review of the paper entitled: “Prevalence of the metabolic syndrome among children from six cities of China” submitted to BMC Public Health by Xu et al.

This paper is about the very interesting and timely topic of the metabolic syndrome (MetS) in children, and conducted in China, a country in transition. The manuscript is well-organized and relatively easy to read. However, the reading of the manuscript raises a number of weaknesses that need to be improved.

General comments

1- In its current form the methods section does not allow getting a clear idea about the recruitment of children as well as the data collection procedure. Because of a particularly large sample size, one can ask to know what was the number of technicians involved in the data collection; what was the logistic of data collection; what was the duration and the period of the data collection phase; how standardize was the protocol, etc. All these important information strongly lack yet.

2- The IDF definition was solely used in this study to define the MetS of children based uniquely on the argument of easiness in international comparisons. Even if this is a good point of view, since the IDF definition is among the most recent and widely recommended definitions of MetS, it should be recognized that no consensus is reached yet, and that other definitions still be used among children. Thus, I would suggest that the authors add/test multiple definitions (i.e., two to three definitions more), including the Japanese criteria released by Ozeki et al. (2006) to take into account some potential Asian specificity. Furthermore, there is a concern regarding the way the IDF criteria have been used by the authors. The fact that the IDF definition recommended not applying the diagnostic criteria of the MetS to children aged less than 10 yrs is a prerequisite for using these criteria. This should not be viewed, as done by the authors, as a limitation to their study but as a prerequisite. The sample is high enough to apply rigorously the definition even if a part of the sample might be excluded.

Specific comments

1- Can you clarify why it was important to use a mixed linear model in this study,
as such a modelling is appropriate to only repeated and nested data?

2- Please, “prevalence” should not be accompanied by “rate”. By definition “prevalence” is nota rate. Please correct throughout the manuscript

3- Please be consistent in writing “MetS” throughout the manuscript

4- In the discussion section (P8, paragraph 1), perhaps a direct comparison of the prevalence of MetS among overweight and obese children involved in the current study to the French and Mexican studies may be more relevant. If a study to study comparison is performed, the justification of the lower prevalence of MetS in overweight/obese children by a lower prevalence of obesity in the Chinese general pediatric population is poor, and should be enhanced.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.