Author’s response to reviews

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

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Author’s response to reviews: see over
Dear Editor Silvestre and Dr. Laaksonen:

Re: "Prevalence of the metabolic syndrome among children from six cities of China (MS: 3256618575860065)."

Thank you so much for your email dated Oct 8, 2011. I would like to express our appreciations for the valuable comments from you and the reviewers.

We have revised our manuscript according to all the comments. The responses are below each comment and it is also highlighted using red font in the manuscript. We would like to resubmit this manuscript for consideration of publication.

If you have any questions, please feel free to contact me.

Thank you very much.

Best wishes,

Sincerely yours,

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Reviewer: Editor
The conclusion should also be rewritten.
Response:
We have revised the conclusion as the suggestions given by the Editor as“ The early onset of MetS among children and relatively high proportions of children with at least one or two metabolic abnormalities in cities of China can increase the risk of developing MetS. It implies the necessity to take effective actions to control and prevent the rapid development of obesity among children in developing countries, especial those undergoing nutritional transition.” for the conclusion of Abstract (P2 line49-53)

For the conclusion of the main manuscript text, it’s as “Based on the large population study, we observed the early onset of MetS in Chinese children. The prevalence of MetS and its five individual abnormalities were all significantly higher among overweight and obese children than that among normal weight children. The proportions of children with at least one or two metabolic abnormalities were relatively high. It is urgent for China to take effective strategies and actions to control and prevent childhood obesity and obesity-related chronic disease in children.”(P10 line293- P11 line299)

Reviewer: Satoru Iwashima
There were some minor problem in the present study, especially, in discussion section. I wish to author mention to relationship between the prevalence of childhood and adult MetS in Chinese.
Response:
We did not find study about the association of the metabolic syndrome in childhood with adult metabolic syndrome for Chinese people. But we found one study in USA.
“Morrison JA et al. found that for children with MetS, 68.5% would develop
adult MetS and 15.6% would develop type 2 diabetes mellitus in 25 to 30 years later, while the proportion is 24% and 5%, respectively, for children without MetS developing adult MetS and type 2 diabetes mellitus [31].” (P9 line244-247)

Reviewer: Benjamin Guinhouya

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.

Response:
We added the information about data collection in Subjects’ selection and Quality control of the Method part.

“It took two months for us to finish the physical examination and questionnaire. The detail of the protocol can be found somewhere else [19].” (P4 line95-97)

“About 20 technicians were involved in the survey in each city. The dedicated technicians were responsible for implementing each part of the survey. All the study group members from the collaboration centers had attended the standard training session and the study was completed following the same standardized procedure in each center. Then the data was entered into computer using the EPIDATA software.” (P5 line135-139)

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.

Response:

We are agreed with the reviewer that using IDF criteria to define MetS in young children is not fully suitable. However, considering the importance of early prevention of chronic diseases and the lack of uniform definition of MetS for young children, we still kept the whole study population. In order to avoid the misleading of the results, firstly, we split the description of results into two groups, children less than 10 yrs and children equal to or above 10 years, respectively. Secondly, we also applied other 3 definitions of Mets, which was produced by Cook et al., de Ferranti et al. and Ohzeki et al., respectively (Supplemental table 1). The whole manuscript was revised according.

We added the information in the Result part as” In the sensitivity analysis, we presented the MetS defined by different criteria. The prevalence of MetS was 2.5%, 2.6% and 11.6% among children aged 10-11 years old according to criteria recommended by Cook et al., de Ferranti et al. and Ohzeki et al., respectively. (Supplement Table 1)” (P8 line204-207)

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?
Response:

We use mixed model in present analysis because our participates were selected by stratified cluster nested sampling method. The mixed linear model was used to control for the within class variation.

We have added the information in Subjects’ selection of method “a stratified cluster nested sampling method was used for subject selection.” (P3 line80-81)

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

Response:

We have replaced “rate” with “prevalence” throughout the manuscript. (P7 line185/191/193; P8 line211)

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

Response:

We have made the revision accordingly. (P2 line51; P8 line225/228; P9 line259/264/266)

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.

Response:

We have changed the description “The prevalence of MetS among our overweight and obese participants was 3.5% and 2.1% among children aged 10-11 years and less than 10 years, respectively, which were quite closer to the level of overweight and obese children reported in the French study [26] and Mexican study [28].” (P9 line234 - 238)

Reviewer: Mehmet Agirbasli
1-Even though the article is interesting, the study does not add new information to the literature

Response:
Although the association between childhood obesity and MetS had been studied in previous studies, it did the first report of the MetS among Chinese children applying both the newly childhood MetS criteria for international comparison and the newly recommended waist circumference cut-off point for Chinese children. So we think our results would be important to those with related research interests.

2-They authors studied metabolic syndrome in Chinese children according to the IDF definition. As authors indicated IDF definition comes with major flaws particularly when applied to different ethnic groups. Adhering to the adult values for hypertension, glucose intolerance, dyslipidemia is misleading for 7-11 year old Chinese children.

Response:
We realized the flaw of IDF definition for children less than 10 years. Adhering to the adult values for hypertension, glucose intolerance, dyslipidemia may result in the underestimate of the prevalence of MetS for 7-11 years old Chinese children. However, considering the lack of uniform definition of MetS for young children, we still keep them in our main analysis but split our results in two children less than 10 yrs and children equal to or above 10 years, respectively. Meanwhile, we also applied other 3 definitions of Mets, which was produced by Cook et al., de Ferranti et al. and Ohzeki et al., respectively, to make our results to be comparable with others.

We added the information in the Result part as” In the sensitivity analysis, we presented the MetS defined by different criteria. The prevalence of MetS was 2.5%, 2.6% and 11.6% among children aged 10-11 years old according to criteria recommended by Cook et al., de Ferranti et al. and Ohzeki et al., respectively. (Supplement Table 1)” (P8 line204-207)
We also added the information in the Discussion part as “Applying the IDF criteria for the children equal to or above 10 years may result in underestimate of the real situation; however, the results at least indicated the early onset of MetS in Chinese children. When applying other definitions proposed by Cook et al., de Ferranti et al. and Ohzeki et al., the estimated prevalence of MetS was much higher.” (P9 line238-242)

3- Puberty is an important factor in determining lipid levels among children and adolescents. Even though 11 years old girls were included in the study, no assessment for puberty were reported.

Response:
The puberty information has been collected during the physical examination; we added the puberty as the co-variable in the revision. (P7 line190; P18 line 435-437; P19 line440; P20 line441-442)

4- IDF definition exists for children 10-16 years of age, however in the study metabolic syndrome was assessed from 7-11 years old children. Therefore, assessing cardiovascular risk factors based on adult levels for < 10 yr old children should be cautiously interpreted as indicated by the authors.

Response:
we have re-analyzed the results according to the different age group, 10-11 years and 7-9 years. We added relative discussion as:” Our results indicated the early clustering of the metabolic abnormalities even the MetS cannot be diagnosed for young children, which was important for the prevention of chronic diseases: the early the better.”(P10 line283-285)

5- The authors checked blood pressure at 3 different intervals separated by 2 minutes. This way, they can not make the diagnosis of hypertension which requires 3 separate occasions.

Response:
We totally agree with the reviewer that the diagnosis of hypertension should be based on the measurement of BP on 3 different occasions. However, our article was a cross-sectional study with a big sample, so it’s very difficult to
conduct the measurement on 3 different occasions. Considering some other studies with the measurement on one occasion only, we think it could reflex the basic condition of elevated BP. But we have also added the discussion about the overestimated prevalence of hypertension because of the limitation of diagnosis of hypertension. We added relative discussion as:" Secondly, the prevalence of hypertension may be overestimated, because the diagnosis of hypertension was based on the measurements of blood pressure on one occasion, rather than on three different occasions." (P10 line286-288)

6-The authors should also comment on methodological differences for instance, they used homogenized methods for HDL-C, on the other hand precipitation method is used in the US which can affect the results.

Response:
Different methodology may affect the results. So we added the information in discussion “The different prevalence of MetS we found may be due to the different prevalence of obesity, of course, we couldn’t rule out the reason of different methodology, for example, enzymatic method was used for HDL-C in our study, but the phosphotungstic acid precipitation method was used for Canadian children.” (P9 line247-251)

7-They collected data from 6 different provinces. Certainly they should comment on the socioeconomical differences between the provinces.

Response:
We analyzed the family’s economic level by city and found that the income level was different for six cities. We also re-analyzed the prevalence of MetS by city. There was a little difference, but it was not significant.

We have added the information in results “The difference of socioeconomic level was significant for different cities (p<0.01).” (P6 line168-169) and “The prevalence of MetS among children older than 10 years ranged from 0.3% to 1.5% in different cities (p>0.05).” (P7 line174-176)