Author's response to reviews

**Title:** Prevalence of the Metabolic Syndrome Using Three Proposed Definitions among Chinese Adults in Shanghai

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**Author's response to reviews:** see over
Re: MS-1063432382349306
Title: Prevalence of the Metabolic Syndrome Using Three Proposed Definitions among Chinese Adults in Shanghai

Dear Dr. Natalie Pafitis:

Thank you for your letter of March 23, 2010 informing us to revise the above-mentioned paper to your journal for publication. We have revised the manuscript as suggested by the reviewers and as elaborated point by point below (revisions are bolded and marked in blue or red in the manuscript file):

Editor’s Comments:

1. Document, within your manuscript, the specific names of the ethics committees which approved your study.

   Response: As suggested, we have documented the specific name of the ethics committee within the manuscript (Line 6-7, Paragraph 1, page 7).

2. We recommend that you ask a native English speaking colleague to help you copyedit the paper.

   Response: As recommended, we have consulted with Dr. Charles E Matthews, an investigator in the US, on English writing of this manuscript and marked all language corrections
in red in the revised manuscript.

REVIEWER 1:

1. Authors should consult a native English to improve their English expression, such as in Abstract section most “is” should be expressed as “was” and definite article “the” should be deleted. The last sentence in Background paragraph 1 was not easy to understand. Background section paragraph 3 “However, a universally accepted definition of the MS does not exist”. May it can be well expressed as “Nowadays there has been no universally definition of the MS accepted by worldwide”.

   Response: As suggested by the editor, we have consulted with Dr. Charles E Matthews, a native English speaking investigator, to improve our English expression. We have also modified the last sentence in Background paragraph 1 as “It is estimated that the risk from the MS for major cardiovascular events is approximately twice as high as for those with the syndrome compared to those without it, and the risk for type 2 diabetes is around five-fold greater for those with MS”, and replaced the sentence of “However, a universally accepted definition of the MS does not exist” in Background section paragraph 3 with “So far, however, there has been no definition of the MS accepted universally.”

2. Title: This cross-sectional study population was from Pudong New Area of Shanghai, because it wasn’t a stratified random sampling study in Shanghai city, so the results can’t represent the whole Shanghai population. I suggest authors add Pudong before Shanghai in the title. This can better explain the subjects of the paper.

   Response: We agree with the review and have replaced the word “Shanghai” with “Pudong New Area of Shanghai” in the title and in Abstract.

3. Material and methods “Data collection” section: Waist circumference (WC) was measured at a level of 2.5 cm above the umbilicus. This was not an internationally recognized program.
Response: We agree with the reviewer that the WC should be measured at the midline between the lower border of the ribs and the iliac crest in the horizontal plane after a normal expiration, and factually, we measured the WC in our population in the same way. The level of 2.5 cm above the umbilicus is exactly at the midline between the lower border of the ribs and the iliac crest. We have used the same protocol in several previous studies (Shu XO, et al. Int J Cancer. 2001;94:449-5. Xu WH, et al. Am J Epidemiol 2005;161:939-47. Zhang X, et al. Stroke 2009;40:1098-104.), and found it was easily understood by the interviewers and applicable in the field work. To avoid misunderstanding, we have changed the related sentence as shown in Line 11-12, Paragraph 2, Page 7.

4. Results section: suggest authors list some sub-title and then describe, compare and analysis them, not only describe the results.

Response: As suggested, we have listed sub-title in the Results section.

5. Discussion
“Interestingly, the age-specific prevalence was higher in men than in women in early adulthood but appeared lower during elderly period. This age-specific prevalence pattern was much different from most previous studies [5, 10, 11]” How can your interpret this phenomenon? Do you have any explanation? Would you mind to comment this result?

Response: As suggested, we have provided possible explanation for the result (Line 7-10, Paragraph 1, page 13.).

REVIEWER 2:

1. Page 7, Data collection paragraph: The WC was measured at a level of 2.5 cm above the umbilicus, did it correct? The stand measurement is measured at the midline between the lower border of the ribs and the iliac crest in the horizontal plane after a normal expiration. It is very important since the WC is a major criteria for the MetS

Response: As suggested by Reviewer 1 in Question 3, we have we have made changes in the
related sentence (Line 11-12, Paragraph 2, Page 7).

2. Page 25, Table 4: Dyslipidemia was most strongly associated with the MetS, but the authors only adjusted the age in the logistic regression model. Since the BMI and alcohol consumption are important confounders for triglyceride, I will suggest adjust these confounders in the logistic regression model.

Response: We agree with the reviewer that we should adjust BMI and alcohol consumption, as well as cigarette smoking and exercise in the association between dyslipidemia and the MS. As suggested, we have re-run the analysis and presented the updated results in Table 4.

3. Figure 1: In my opinion, Figure 1 should be modified and make it more readable.

Response: As suggested, we have modified the figure 1.

We thank the reviewers for their helpful comments and suggestions and hope the revised manuscript has addressed all their concerns. I look forward to hearing from you concerning the final decision of your journal regarding the acceptance of our paper.

Sincerely yours,

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