Reviewer's report

Title: Optimal cutoff of Waist-to-Hip Ratio for Predicting Cardiovascular Risk Factors among Han Adults in Xinjiang

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Reviewer: Hsin-Jen Chen

Reviewer's report:

This study tried to find “optimal” cut off points for WHR on metabolic disorders in Han people living in Xinjiang. This is not a new topic, and the authors did not do a good job to convince why they select Han people in Xinjiang. Although focusing on a population in a remote area, it did puzzle me why the authors only examined data for Han people and neglected the other two ethnicities, and why they forgot waist circumference (some study suggest waist circumference alone could serve as a better indicator of visceral adiposity than WHR does.)

Major revisions

1. The author did not describe their sample. There should be a table to describe the characteristics of this sample, at least the distribution of age categories. Since the authors used a lot of age-standardization procedure, they supposedly categorized the sample by age.

2. The authors did not mention clearly sample sizes in their WHR categorization. Sample size is very important information in epidemiologic study. As the authors in this paper did many categorizations, the sample size for every category should be put clearly, including in Table 1, 2, 3, 4, 5, and 6.

3. The authors did age-standardized statistics for Table 1-4. Did they use this age-standardization procedure for Table 5-6, too? I’d recommend them to do it, because WHR is usually changing with age. If age-standardization or age-adjustment was not made in these statistics, the changing statistics with WHR might have been contaminated with the influence of age. If the authors did age-standardization for sensitivity and specificity estimation, please mentioned the procedure in the Methods section.

4. Please describe what SPSS procedure was used for age-standardization, and what reference population age structure was used to standardize to.

5. The authors would add more value to this paper if they’ll examine waist and BMI in addition to WHR, and compare the three adiposity indicators. Meanwhile, comparing three ethnicities living in Xinjiang would make this paper more scientific value.

6. Please rearrange the background and discussion. Much background information (such as the existing literature about WHR cut points, comparisons between BMI and visceral fats, background information about the place and
people in Xinjiang) should appear in background section, rather than in discussion section.

7. The authors only named the cross-sectional design as their limitation. Some limitations may hide in the details not described in the manuscript yet. First, what is the non-respondent rate, and were there systematic factors for refusal to participate? Second, blood was collected into EDTA tube. Were blood cells anduffy coat removed immediately after blood collection and centrifugation? This is related to some blood biochemistry measurement error. Third, choice of reference population age structure would also affect the final statistics; the authors should talked about what age structure was used for reference, and how this choice could impact the final estimates.

8. In discussion, there are too many speculative statements explaining their findings, but with flimsy arguments. Please try to reduce unnecessary conjectures in the discussion. In the paragraph beginning at the line 222, the authors used the term “incidence” to describe their results, but in fact, cross-sectional study does not provide incidence estimate. At final, the writing style and train of thinking is not smooth in the current version. Readibility should be improved. Please put more effort into editing.

Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests