

## Peer Review File

Article information: <https://dx.doi.org/10.21037/jtd-22-1538>

### Reviewer A

The contents of this paper are interesting but the paper was not well written because some important issues were not addressed.

Comment 1: As the authors mentioned that, PSG is the golden standard for the diagnosis of OSAS. The detection ability of the devices is not perfect as introduced in lines 137-143, How did the authors get the ground truth for their cases?

Reply 1: We thank the reviewer for the comment. Dear reviewer, we all know that PSG is the gold standard for OSA diagnosis, but PSG is only suitable for in-hospital monitoring because of its complicated operation, and is not suitable for OSA screening in large sample population. Our Type IV monitoring equipment is portable and compact, which is suitable for OSA screening in large sample population. And we have done a comparative experiment with PSG, and the results show that the device has good sensitivity and specificity for the diagnosis of OSA. Although not as perfect as PSG, good reliability and accuracy can be achieved for screening OSA in large populations. Changes in the text: None.

Comment 2: The authors should also provide more detailed information on how to validate the accuracies of the equipment for the detection of OSAS in lines 137-143. For example, the numbers of cases, the numbers of OSAS patients, and etc.

Reply 2: We very much appreciate the thorough and rigorous review. We acknowledge that the description of the comparative test of the device here is not detailed enough. Changes in the text: We have modified our text as advised (see Page 7, lines 139-142). Detailed data on the number of included cases and the number of cases with different OSA severity were added.

3. The authors should disclose their COI with the producers of the devices used in the paper.

Reply 3: We appreciate the reviewers' comments on conflicts of interest in our research. We have no conflict of interest with the producers of the WISM devices, as indicated in the COI.

Changes in the text: None.

4. The authors should prove the distribution of their data is normal distribution if Student's t-test were used. categorical variables will never be normal distribution as explained in line 169.

Reply 4: We agree with the reviewer's comments. I am sorry that the unclear expression in line 169 may have caused misunderstanding among reviewers. What is expressed here is that one-way ANOVA can be used for categorical variables, and Student's t-test can be used for continuous variables of normal distribution. We also carry out normality test for continuous variables.

Changes in the text: We have modified our text as advised (see Page 8, lines 169-174):“Normality test was conducted on continuous variables, and the level of  $\alpha=0.10$  was.....”

5. The paper looks like a report or note instead of an academic paper. I cannot find apparently academic meanings of the study.

Reply 5 :Many thanks to the reviewers for giving us valuable comments on this research and the paper. Your valuable comments will make our research more perfect. First of all, we are a study of OSA screening practice in a large sample population, so the article will have a description of the feasibility of the screening practice, the success rate of the equipment, the recovery rate, etc., perhaps the description of these practice processes will give you a feeling that our article is not like an academic paper. Our study confirms the feasibility of using objective IV sleep testing equipment to screen a large sample of people in the community for OSA in a short period of time. Our study also reports on the prevalence and risk factors for OSA in the community population during screening practices. This also provides strategies and references for OSA screening in a large sample population in the future, so I think our study is of certain significance. Thank you again for your valuable comments!

Changes in the text: None.

## **Reviewer B**

I congratulated authors for accomplishing such a highly logistics, time consuming, and cumbersome study. The effort that authors put toward this study is highly appreciated. I have no major comments other than pointing out that the phenotypes of OSA currently proposed may interfere with diagnosis of OSA based solely on ODI. ODI may diagnose subtype of OSA patients but still there are some patients will never be diagnosed with ODI alone. The authors may make one paragraph about this in the discussion.

Reply: We thank the reviewer's suggestions, which made our research discussion more perfect. We think these are really worth discussing. We really neglected to discuss the subtypes of OSA in our discussion. For this reason, we have added one paragraph about this in the discussion.

Changes in the text: We have added a paragraph to the discussion (page 20, lines 420-425) to complement the diagnostic value of ODI and its inadequacy in the diagnosis of OSA subtypes.