Reviewer's report

**Title:** Relationship between Self-Reported Sleep Quality and Metabolic Syndrome in the General Population

**Version:** 2  **Date:** 22 February 2014

**Reviewer:** Charumathi Sabanayagam

**Reviewer's report:**

This study assesses the association between self-reported sleep quality defined by global PSQI score and metabolic syndrome in a general population in Japan. Authors concluded that the global PSQI score and its components were associated with metabolic syndrome. The study is not novel as the authors claim, and the methodology is not sufficiently detailed. My comments are as follows:

**Abstract:**

1. **Background:** Authors mix up exposure (sleep quality) and outcome (metabolic syndrome) throughout the manuscript. Authors should be consistent in their usage of these two measures.

2. **Methods:** Need to mention study design, study setting, exposure assessment and outcome definition in brief.

3. **Results:** Authors should provide supportive statistics (p-value/OR and 95% CI) to back up the statement related to sleep latency, sleep disturbance and metabolic syndrome. Again, authors should clarify what they mean by sleep disturbance or sleep latency score.

4. **Conclusions:** ‘…… global PSI score and its components were associated…..’, what were the components assessed? This should be mentioned under ‘Methods’. What is the relevance of ‘sleep duration’ in conclusion, has this been assessed? Given the cross-sectional nature of the study, the final statement, ‘…implementing preventive aspects of metabolic syndrome’, is over-interpretation of the data.

**Introduction:**

5. It seems that there is no previous study that has examined the association between sleep quality and metabolic syndrome. However, under ‘Discussion’, they have compared their findings with previous similar studies. Authors should cite and mention studies that have assessed the relationship between sleep quality (by PSQI or other measures) and metabolic syndrome before.

6. ‘We surveyed the prevalence …’, is this an aim of the study? If so, analysis and results related to this aim need to be provided (e.g. age standardized prevalence etc.). As the purpose is to examine the association, suggest removing ‘Prevalence’ from the aim, but to provide it under the ‘Results’.

**Methods:**

7. How the participants were recruited into this study? Is this a screening project?
8. Measures and definitions: As the information is collected by self-reported questionnaire data, have the authors included only those who are literate? How exercise habits were assessed, need to provide the exact questionnaire. ‘Working hours per week’, how was this assessed for housewives or odd job workers?

Statistical analysis

9. What is the rationale for stratified analysis by gender?

10. Line 4: The term ‘incidence of drinking status’ is confusing. What is the relevance and how was it assessed?

11. What is the rationale for examining the correlations between global PSQI score and its components?

12. Logistic regression analyses: Cut points and reference category of the various sleep parameters assessed need to be provided.

Results:

13. Authors should avoid repeating the exact results (p, SD etc.) provided in Tables under ‘Results’. Rather, a brief and clearer interpretation would suffice.

14. Authors mention ‘sleep latency score’, ‘sleep medication score’ under ‘Results’. These terms should be defined under ‘Methods’ for readers to understand their relevance. Similarly, the terms, ‘habitual sleep efficiency’, ‘daytime dysfunction’ and ‘use of sleep medication’ mentioned in Tables, should be defined under ‘Methods’. What do the categories of these scores represent, e.g. sleep medication score, 0,1,2,3?

Discussion

15. Page 16, line 3: ‘…..it is considered irrational to use’. Authors use self-reported questionnaire to assess sleep parameters and self-reporting has similar such biases.

16. Authors claimed that no study has ever reported the association between PSQI and metabolic syndrome. Several studies on this topic have been published before for e.g. Jennings et al, Cho et al, Kazman et al, Hung et al. etc. Authors should pay more attention to literature review and discuss the limitations of previous similar studies and how this study addresses those limitations.

17. Page 16, second last sentence: ‘…new findings to prevent and improve metabolic syndrome’, the cross-sectional data is not suited to make this statement.

18. Bias related to use of screening population need a mention.

19. Authors should avoid use of the terms, ‘risk’, ‘prevent metabolic syndrome’ etc. in a cross-sectional study.

20. For the same reason, conclusions need to be revised, not to over interpret.

**Level of interest:** An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests