Author's response to reviews

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

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Author's response to reviews: see over
Dear Dr. Nakamura

MS: 7811122801098365

Relationship between Self-Reported Sleep Quality and Metabolic Syndrome in General Population

Thank you for returning the above paper, and we are very pleased that, in a revised form, it should be suitable for publication in BMC Public Health. We found the comments very constructive, and we truly appreciate your time in letting us know where you have felt the article has been lacking.

Please find with this letter the revised article, which takes your comments into consideration. Please also find the specific your comments (in italics) and our replies thereto. Furthermore, we have pointed the revised text as underline.

We sincerely hope that you should now find the paper acceptable for publication. If, however, you feel that the paper needs further revision, we would be grateful for the chance to do this.

With Best wishes,

Noriyuki Okubo
Reviewer comments:

My concerns and suggestions are listed as follows. The authors described interesting relationship between metabolic syndrome and sleep quality. This manuscript is fit for the journal and acceptable, on the condition that the authors revise a few points as mentioned below.

1. What is the purpose of separating males and females for analyses?

   In the previous studies about sleep, outcomes in males have been difference from those in females. So, we analyzed the data separating males and females to exclude sex bias, and we added the purpose of it to our manuscript.

2. There have been many previous researches that reported the association between sleep duration and metabolic syndrome. However, this study only showed a weak association between them. Please explain.

   We evaluated sleep assessments by PSQI. Sleep duration assessed by PSQI was categorized into <5 hours, 5-<6 hours, 6-≤7 hours and >7 hours because it focused on short sleep duration induced by sleep disturbance. However, average of sleep duration of the participants in this study was 460 minutes (7.7 hours) in males and 430 minutes (7.2 hours) in females. The weak association between sleep duration and metabolic syndrome seen in this study could be explained by a small number of short sleepers and we added the cause of the weak association
to our manuscript of the part of limitation.

3. According to the definition of PSQI, any people with scores above 6 are considered to have sleep disorders. However, most of the subjects in this study scored below 6 - would it be appropriate to use PSQI scores to determine sleep disorders in this study?

We think that the use of PSQI in our study has been appropriate. PSQI was designed for screening test. Many studies of which participants extracted from general population have been conducted with PSQI, hence weak power of analysis caused by low prevalence of sleep disorder could not limit results of our study.

4. Other factors such as eating habit is considered to have effects on metabolic syndrome. Was the corrections made in this study sufficient?

As the reviewer mentioned, we recognize that eating habit is also associated with metabolic syndrome, however, we have not investigated about it because a brief and suitable questionnaire about eating habit have not been contained in our study. So, we have added to limitation about this.

5. Authors should seek assistance in the translation and revision process to ensure that all sentence structure and grammar are correct.

We have proofread our manuscript.
Dear Dr. Sabanayagam

MS: 7811122801098365

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Thank you for returning the above paper, and we are very pleased that, in a revised form, it should be suitable for publication in BMC Public Health. We found the comments very constructive, and we truly appreciate your time in letting us know where you have felt the article has been lacking.

Please find with this letter the revised article, which takes your comments into consideration. Please also find the specific your comments (in italics) and our replies thereto. Furthermore, we have pointed the revised text as underline.

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With Best wishes,

Noriyuki Okubo
**Reviewer comments:**

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.

   We have revised our manuscript according to your suggestion.

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

   We have added the sentence which contains study design, study setting, exposure assessment and outcome definition in brief according to your suggestion.

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.

We have added the supportive statistics to results according to your suggestion. Brief explanations about sleep disturbance and sleep latency have also been added.

4. Conclusions: ‘…… global PSQI 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.

We have added the sentence about PSQI score components, which have contained sleep duration, to ‘Methods’. We have deleted the sentence of “…implementing preventive aspects of metabolic syndrome”, because it was inadequacy as you mentioned.

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.

We have added previous studies concerned with the relationship
between sleep quality and metabolic syndrome to ‘Background’ according to your suggestion.

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’.

We have clarified the aims of this study by deleting the sentence “We surveyed the prevalence ....” according to your suggestion.

**Methods:**

7. How the participants were recruited into this study? Is this a screening project?

The participants were recruited from Iwaki health promotion project, which is screening project. More detailed profiles about study participants have been added to our manuscript.

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?

In Japan, almost all adults among general population are literate.
The participants of our study can understand meanings of the questionnaires and answer them. Therefore, we recognize that self-reported questionnaire doesn’t provide sample bias. In spite of it, we could not deny the possibility that the answers from participants did not reflect their real symptoms and lifestyle. We have added the sentence about a limitation of the self-reported questionnaire to “discussion”.

We assessed whether or not the participants exercised regularly, because complex questionnaire would make the participants confused. This is one of the limitation of the study, so we have added it to the limitations. As the assessment of working hours per week, we recognized that working hour of housewives is hours of doing housework, and that all working hours contained working hours of add job working because we were interested in working hours physical and mental stress.

We have added more detailed information about exercise habits and working hours to “Methods” for good understanding of readers.

Statistical analysis

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

In the previous studies about sleep, outcome in males is difference from those of females. Hence, we analyzed the data separating males and females to exclude gender bias.

10. Line 4: The term ‘incidence of drinking status’ is confusing. What is the relevance and how was it assessed?
We assessed whether or not the participants drank alcohol beverages regularly according to previous studies. We have revised the term of “incidence of drinking status” to “drinking habits”.

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

To clarify the issues of this study, we have deleted the sentences about the correlation between global PSQI score and its components at the part of methods and results.

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

We have added the sleep assessment which is reference category to Methods. There are not cut points in PSQI components and we put them into logistic regression analysis without any process.

**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.**

We have deleted the exact results from “Results” according to your suggestion.

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?

We have added the above term to “Methods” according your suggestion.

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.

We have revised the sentence of ‘…..it is considered irrational to use…’ to ‘measuring sleep duration only is insufficient for understanding global sleep status.’

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.

We have revised the mention of previous studies under ‘Discussion’ according to your suggestion.
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.

We have revised the sentence of “...new findings to prevent and improve metabolic syndrome’ to ‘...sleep disturbance were associated with metabolic syndrome’, because it has been inappropriate expression.

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

We have added the limitation of screening population to ‘Discussion’ according to your suggestion.

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

We have deleted the term of ‘risk’ and ‘prevent metabolic’ etc. in manuscript according to your suggestion.

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

Thank you very much for your advices. We avoided the over interpret and revised the manuscript including ‘Conclusions’.

**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