

## Peer Review Report

# Review Report on The Mediating Role of Depression in Association Between Total Sleep Time and IADL of Middle-aged and Elderly People and the Moderating Role of Gender

Original Article, Int J Public Health

Reviewer: Stefanie Sperlich

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### EVALUATION

#### **Q 1** Please summarize the main findings of the study.

TST is a risk factor for IADL and depression moderates this association

#### **Q 2** Please highlight the limitations and strengths.

Limitations: Cross-sectional use of the panel data

#### **Q 3** Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.

The objective of this study was to analyse the mediating role of depression in the relationship between total sleep time (TST) and disabilities in terms of IADL. Moreover, the moderating effect of gender on this relationship was investigated. The research question is clearly formulated, the introduction provides a good overview of the state of research, and the statistical approach seem appropriate.

However, my main concerns relate to the cross-sectional use of the data that does not allow any conclusions to be drawn about the relationship between TST, depression and IADL. In addition, the manuscript would benefit from a more detailed explanation of the KHB method and the values obtained with it. Finally, it seems that some conclusions in the Discussion section have been drawn too far.

In more detail, I would like to point out the following aspects to be reconsidered:

1. The data used in the study were longitudinal panel data extracted from four waves between 2011 and 2018. These data were pooled and used cross-sectionally to analyse the relationship between TST and IADL, and the mediating role of depression in this relationship. Although the term "relationship" is used in the manuscript, the underlying assumption is that TST has a causal effect on IADL. In my opinion, the number of cases is sufficient to avoid having to pool the waves. Hence, I would recommend using the longitudinal data structure to determine the causal impact of TST on IADL, for example, by examining the impact of TST for the years 2011 and 2013 on depression and IADL for the years 2015 and 2018. In the way the data are currently used, there is also the problem of autocorrelation, which has not been taken into account.
2. The independent variable TST includes both night-time sleep and sleep after lunch (nap). I was wondering whether both types of sleep have the same effects on IADL. Therefore, I would suggest performing sensitive analyses for both types of sleep.
3. The KHB-Method is described very briefly and it remained unclear how the total, direct and indirect effects were obtained. Please explain in more detail how these effects were calculated. Moreover, it is not clear how to interpret the coefficients obtained by the decomposition analyses. For example, what does it mean that the direct effect is 0.04 and the indirect effect is 0.25? (page 8, line 208) Are these are the log odds? Maybe using alternative coefficients such as odds ratio or APE (average partial effects) provided by the STATA KHB command may be useful to better understand the effect sizes. Moreover, in my view, it is not quite correct to

say "After controlling for depression, the direct and indirect effects of TST were 0.04 ... and 0.25. Instead, I would say: We see that TST under/below 6 hours increases the log odds (?) of IADL by 0.29. Controlling for depression, the effect of TST under/below 6 hours reduces to 0.04, leaving an indirect effect of 0.25 that is due to depression. The interpretation given in the STATA-Journal you listed as reference 42 might be helpful to interpret the results (p. 429ff).

4. Results Table 3: Please also briefly comment on the findings on TST over 8 hours, even if they are not significant. I also miss this information in the abstract.
5. Results Figure 3: Please describe the moderating effect of gender in more detail. Is the effect stronger for women or men?
6. Discussion: Page 8, line 225f: The statement that TST increased the risk of IADL can only be made by using, for example, TST in 2011 to predict IADL in 2018. This would significantly increase the significance of the results.
7. Discussion: Page 9, line 226f: it is a bit confusing to use different words for less sleep (inadequate and short). In addition, in my view, your conclusion goes too far when you say "These results implied that long sleep duration directly affects IADL disabilities....." (line 227f). The only conclusion that can be drawn from the findings is that depression has no indirect effect. Other unstudied mediators may be relevant that could make the direct effect disappear.
8. Discussion, page 10, line 258f: "Short sleep duration may lead to depression". This statement comes to me twice as it has been said before (page 9, line 245f)
9. The complex relationship between sleep duration, IADL disability and depression explained in the Discussion section is difficult to follow. Subheadings would be helpful here.
10. Discussion page 10, lines 271 and 274: Please avoid starting a sentence with 'And'.
11. Discussion page 10, lines 278: I don't quite understand the reasoning. If I understand correctly, this is not about gender differences in depression, but about the different effects of depression on IADL in women and men.
12. Conclusion: In its present form (that means based on cross-sectional data), the results do not allow conclusions to be made on the causal relationship between TST and depression and between TST and IADL. It is also possible that depression acts on TST or that IADL acts on TST. For this reason, TST should not be referred to as 'risk factors' for IADL. If the study design remained unchanged, this limitation in validity should be discussed.

#### Minor aspects

13. Please check the manuscript for careless mistakes (e.g. genders or gender, WHO with or without 'the',  $p < 0.05$  instead of  $P < 0.05$ ...)
14. Throughout the manuscript, the categories of TST are not non-overlapping, e.g. in Table 2 for example between the categories 6-7 h and 7-8.
15. Please add in the title of Table 2: Logistic regression results of path a, b, c und c' stratified by gender
16. In Figure 2, the c is written twice (below it should be a c')

#### PLEASE COMMENT

##### **Q 4** Is the title appropriate, concise, attractive?

I think the title is a bit too long

##### **Q 5** Are the keywords appropriate?

yes

##### **Q 6** Is the English language of sufficient quality?

Yes with some restrictions

**Q 7** Is the quality of the figures and tables satisfactory?

Yes.

**Q 8** Does the reference list cover the relevant literature adequately and in an unbiased manner?)

yes

#### QUALITY ASSESSMENT

**Q 9** Originality

**Q 10** Rigor

**Q 11** Significance to the field

**Q 12** Interest to a general audience

**Q 13** Quality of the writing

**Q 14** Overall scientific quality of the study

#### REVISION LEVEL

**Q 15** Please make a recommendation based on your comments:

Major revisions.