

## Peer-review Comments and Author Responses

### Reviewer 1

1. *Good job on the manuscript. The writing aspect is great. I'd suggest submitting the tables in another format. Submitting them in EXCEL makes it harder to understand. I also have added a few comments in red inside the Manuscript document (copied below). I'm really anxious to hear your response. Overall, the manuscript was very detailed. Question that may have arisen were more related to the reasoning behind some choices rather than asking for more details.*

Thank you for your positive feedback about the manuscript. We attached a Word document with the included tables beside the Excel spreadsheet.

2. *TITLE:*

*The title is really good. It is simple, concise and it summarizes the goal of the article. The abstract is also good. The number of words did not extrapolate the word limit. It gives the reader a good perspective of the subsections of the manuscript.*

Thank you for your comments on the title. We tried to be concise while stating clearly the main objective of the manuscript.

3. *“There have been efforts to elucidate the magnitude of the sleep-depression relationship. Joo et al. (2022) found that men and women with poor sleep quality from the Korean Community Health Survey in 2018 were seven times more likely to suffer from depression. In addition, Hu et al. (2020) found that older adults from Chinese nursing homes were three times more likely to be depressed. Understanding the consequences of poor sleep quality in the elderly may help promote public health interventions to improve their quality of life. Policies to increase physical activity in the geriatric population may improve sleep quality and avoid negative consequences on mood.”*

*The last sentence of the last paragraph may need some rewording. Honestly, I'd advise not using it, as it gives the reader the expectation that the authors will describe which physical activities can help this population.*

We agree with the reviewer that describing the effect of physical activity on the geriatric population is beyond the scope of the manuscript. Therefore, we have removed the last sentence of this paragraph in the revised manuscript. Nevertheless, we mention briefly how physical activity can modulate the effect of poor sleep quality and depression in this population in the discussion.

4. *“It is still being determined if the findings from Joo et al. (2022) apply to different cultures and age groups. Also, the COVID-19 pandemic has exacerbated sleep disturbances, especially for the geriatric population in nursing homes. (Islam et al., 2021; Abeysekera & De Zoysa, 2021). Therefore, the influence of the pandemic on this association has yet to be studied. As such, we aim to replicate the association model between depression and sleep quality, as reported by Joo et al. 2022 in the Brazilian elderly population from the Brazilian Longitudinal Study of Aging (ELSI-Brazil). This study will allow for the identification and deeper understanding of potential cross-culture differences in sleep quality, increasing the external validity of the results.”*

*I'd suggest not mentioning about the COVID impact as this was not covered in the Statistical plan nor it was mentioned in the discussion. Describing it in the introduction gives the reader an expectation that the authors will discuss the impact of COVID in this population.*

We also agree with the reviewer that mentioning the impact of the COVID pandemic is unnecessary, as we did not consider this variable in our model. As such, we have removed these sentences from the introduction.

5. *Overall, the Introduction is great. It only has three paragraphs. It shows the authors were able to introduce, explain the reasoning behind the study and goal of the study in a concise form.*

Thank you for your positive comments. Our goal with the introduction was to state clearly the relevance of our study.

#### 6. *Methods*

*The Methods section is very well explained. After reading it there is not many questions to ask for the authors. However, some questions arose that need to be addressed:*

1. *Congratulations on planning a sensitivity analysis. This can only give more power to the results.*
2. *Why mentioning that ELSI collected data regarding COVID aspects if the variables were not used in the model, for instance, in a sensitivity analysis?*
3. *Ethnic discrimination was used as a proxy for stress. The studies used to sustain the idea were done with migrants. Using Ethnic discrimination as a proxy for stress in this population may underestimate the level of stress in the population. Have you considered other variables as proxies?*
4. *The Statistical Analysis was well described. Very good job!*

We greatly appreciate the reviewer's acknowledgment of the statistical and sensitivity analysis used in our manuscript. We also agree that mentioning the variables gathered during the COVID pandemic is unnecessary and may cause confusion to the reader. Thus, we have removed it from the methods section.

Furthermore, we agree that using "ethnic" discrimination as a surrogate for stress is not straightforward as there could be other daily stressors (eg, social, physical, etc.). We would like to correct the meaning of this covariate in the original ELSI-Brazil dataset. The question regarding discrimination was "Have you felt victim to any sort of discrimination?". And then the interviewers would ask more specific scenarios of discrimination, such as when seeking medical help, in social gatherings, within the family, in the workplace, and due to place of living. Therefore discrimination includes a more broader domain than the ethnic domain.

Notwithstanding, we believe discrimination is an acceptable surrogate variable for stress in our model. Sutin et al. (2015) found that perceived discrimination in older adults is negatively associated with life satisfaction and loneliness. Eventhough their study's population differs from ours ethnically, the similarity of the age and the longitudinal design of the study make this

conclusion valid. In addition, Hosler et al. (2019) found an association between discrimination and depression in a more ethnically diverse population.

Considering the limitations of the predetermined dataset, we are certain that discrimination is the best surrogate variable for replacing the stress covariate in the original model. We have rectified this misunderstanding in the final manuscript and have cited the previous references to justify our decision for using this variable in our model.

## 7. Results

*The Results section was well written. I really enjoyed the use of subsections as it helps readers to understand what will be displayed in each paragraph. All the results mentioned in the section were in tune with what was described in the Methods.*

Thank you for your positive feedback. We greatly appreciate your positive feedback about the writing of the results section.

## 8. Discussion

*First, congratulations on being careful not using the word "cause". In the introduction it is mentioned that there's no clear temporal association between Sleep and Depression. However, the goal of the study was clear not to find any causal relationship, but an association. The discussion was well written. I'd only suggesting adding the Conclusion section so the last 2 paragraphs can have a greater importance. Adding the limitations on the Discussion makes readers think the limitations will be further explained.*

Thank you for this kind suggestion. We have decided to include the last paragraph in a separate 'Conclusion' section. However, we agreed that the last paragraph of the discussion should mention the limitations of the study.

Overall, it is a well written manuscript. Congratulations on the study.

Thank you greatly for your kind words. We are truly grateful for the positive feedback on our manuscript and have made profound changes as you suggested.

## Reviewer 2

*The manuscript offers valuable insights and observations regarding the link between sleep disturbances and depression in Brazil. While the connection between poor sleep quality and depression is established in other populations (Joo et al., 2022), it is crucial to replicate these studies in specific countries, like Brazil, due to differing populations and the potential challenges in replicating strategies from other countries.*

## Comments:

- 9. The manuscript should provide a rationale for including participants in their 50s, especially since the study focuses on older adults, and Brazilian law defines older adults as those aged 60 and above (<https://academic.oup.com/gerontologist/article/58/4/611/4948404>). This clarification is necessary to align with the stated study objectives.*

Thank you for your comment. We chose to keep the same age as defined by the ELSI cohort, which is 50 years and older and in fact keeping the cutoff with 50 years old would increase comparability with the study being replicated.

*10. The CES-D scale is designed to measure depressive symptoms in the general population (<https://journals.sagepub.com/doi/10.1177/014662167700100306>). The manuscript should be cautious in claiming that participants are depressed based only on this scale, which does not provide a clinical diagnosis. If a clinical diagnosis is present, this should be explicitly stated.*

We used this scale based on the study by Briggs et al. (2018). The authors validated the scale for epidemiological studies in a population with similar characteristics than the present study. Therefore, we believe that its use in this study is justified.

Briggs, R., Carey, D., O'Halloran, A. M., Kenny, R. A., & Kennelly, S. P. (2018). Validation of the 8-item Centre for Epidemiological Studies Depression Scale in a cohort of community-dwelling older people: Data from The Irish Longitudinal Study on Ageing (TILDA). *European Geriatric Medicine*, 9(1), 121–126. <https://doi.org/10.1007/s41999-017-0016-0>.

*11. The manuscript should clarify whether patients with diagnosed depression or those on antidepressant medication were included or excluded from the analysis. The rationale behind this decision and its potential impact on the results should be thoroughly discussed.*

We included patients with previous diagnosis of depression and current use of antidepressants; however, our main hypothesis was evaluated using the CES-D scale. We agree with you about the impact of these variables. This is why we included a secondary analysis with a table in the appendix.

*12. The manuscript seems to miss crucial analyses, such as the interplay between sleep quality-sex, depression-sleep quality, and sex-depression. Given the study's conclusion about the link between sleep quality, depression, and gender, it's essential to address whether one of these covariates could act as a confounder.*

We greatly appreciate this observation. We have adjusted gender as a variable and obtained the same results for both women and men in the logistic regression analysis. Other studies such as Joo et al. have also considered that there is no statistically significant difference between gender, depression and sleep quality in subgroup analysis.

*13. The title should reflect the study's findings more accurately, especially if gender plays a significant role in the association between sleep quality and depression.*

Thank you for this suggestion. We kindly have suggested another title.

*14. The cross-sectional nature of the study and its inability to establish causality should be prominently discussed. Additionally, the limitations of using a dichotomous question for assessing sleep quality should be acknowledged, especially when compared to more comprehensive tools like the PSQI used in other studies (Joo et al., 2022). While it's understood that this is a secondary analysis, and you didn't show the scale, the potential biases introduced by this method should be mentioned in the discussion.*

We agree that this is a cross-sectional study and, therefore, we have added this limitation.

15. *While the manuscript suggests strategies to decrease depression, it should also propose methods to enhance sleep quality. Given the uncertainty about cause-and-effect relationships, addressing both aspects are vital.*

Thank you for your comment. We have added other methods to enhance sleep quality in the discussion, such as low and low-moderate intensity exercises, which have shown benefits in improving sleep quality.

By addressing these points, the manuscript can significantly improve.

We thank again the peer reviewers for their wonderful and valuable comments. We look forward to read their opinions about our response and the opportunity for our research to be considered in your prestigious journal.

Sincerely,

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