

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association between poor sleep quality and depression symptoms among the elderly in nursing homes in Hunan province, China: a cross-sectional study
AUTHORS	Hu, Zhao; Zhu, Xidi; Kaminga, Atipatsa; Zhu, Tingting; Nie, Yu; Huilan, Xu

VERSION 1 – REVIEW

REVIEWER	José Alberto Laredo Universdiad de Castilla-La Mancha, Spain
REVIEW RETURNED	06-Jan-2020

GENERAL COMMENTS	<p>First of all I would like to congratulate the authors for the great work done and for addressing the issue in question as it is an issue of great importance for the quality of life of the elderly and that they remain healthy as long as possible, because the Rest in the elderly plays a very important role at full functional level.</p> <p>As improvements proposed to the authors:</p> <p>In the methodology when you say: "Each component's score ranges from 0 to 3 points so that the global PSQI score ranges from 0 to 21 points", it would be necessary to clarify how you do in Social Support Rating Scale, if 0 is the most negative score and 3 the most positive score or if it would be the opposite to avoid confusion.</p> <p>In the methodology section when you talk about Depression symptoms assesment you affirm: "The GDS-30 consists of 30 true / false questions, and the total score ranges from 0 to 30 points. Participants who scored 11 or greater are considered to be depressive in this study ", this text is repeated almost literally in the first lines of this section, so it is considered redundant.</p> <p>In the line prior to statistical analysis there is a spelling fata that would lack a letter.</p> <p>In the discussion section, when you state: "However, the use of sleep medication is not widespread in Chinese nursing homes, and only 10.3% of residents had taken sleep medication once or more a week," it would be necessary to broaden the discussion about Medication use and sleep quality in relation to the results of this study. According to what you say, only 84 people use sleep medication, but of those 84 people, 83 have poor sleep quality. Also, of people who do not take medication to sleep almost twice have poor sleep quality, so it is considered necessary to broaden the discussion based on these results.</p>
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	When you state: "These findings are in accordance with those of several previous similar studies in both nursing homes and communities," it would be necessary to include the corresponding citations here.
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REVIEWER	Kim Madden McMaster University, Canada
REVIEW RETURNED	02-Mar-2020

GENERAL COMMENTS	<p>Thank you for this interesting article on the association between sleep disturbances and depression in elderly people in China. There are a few minor grammar errors but otherwise it is a well-written paper. I have a few comments for consideration.</p> <ol style="list-style-type: none"> 1. The authors state that patients/consumers were not involved in the design of this study which seems like a missed opportunity. 2. How was sleep duration measured? If this is self-report it may not be accurate. If self-report, is it possible to get any other source of data to corroborate the self-report? 3. I'm not sure how much this contributes to the current literature. We already have evidence from elderly populations that sleep and depressive symptoms are associated. Is there any reason to believe that elderly in nursing homes would be substantially different to warrant another study? 4. Depression symptoms are one thing, but a DSM diagnosis of depression is another thing. The authors accurately describe depressive symptoms in this paper, but would it not be more interesting to look at DSM diagnoses of depression? It is possible that it is the poor sleep making people appear to have depressive symptoms. For example, one of the questions on the GDS is "Do you feel full of energy?" People who have poor sleep may not feel full of energy, not due to depression but due to the sleep issues. 5. Per STROBE reporting guidelines, please add dates of recruitment. 6. Patient consent was obtained. Can that authors elaborate? Was this written informed consent? Who obtained informed consent? 7. There are a lot of analyses. For example, the authors did t-tests/chi-square and crude and adjusted GLM analyses and binary logistic analyses. This seems to be redundant. What do the t-tests/chi-square tests add and why did the authors choose to analyze sleep as both a continuous and binary variable? How did the authors account for multiplicity? 8. What is the justification for the sample size? 9. How were missing data addressed? 10. I am curious how the authors managed to do 817 in-person interviews that each lasted an hour and a half. This is a huge amount of personnel time, yet this study received no funding. How was this achieved? 11. Please report model fit statistics for the adjusted models. 12. The discussion says that this is a large random sample of elderly persons, but it's not really. The nursing homes were random but the sample within the nursing homes does not appear to be random. 13. Did the authors take into account cluster effects within a nursing home?
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REVIEWER	Yi-An Ko Emory University, USA
REVIEW RETURNED	03-Mar-2020

GENERAL COMMENTS	<p>Abstract: “Controlling for other factors?” better to state the covariates. If there’s not enough space, the authors may consider using general terms, like demographics, disease history for example.</p> <p>Several studies have investigated the association between sleep and depression in elderly in different countries a long time ago. So, this association is not something new. It would be interesting to explore mediators actually. In the introduction, in addition to saying that the evidence is scarce in nursing homes, I'd love to learn more about the justification of doing such study. Is this the original study or a secondary analysis of some other parent project?</p> <p>Have data regarding the use of antidepressants been collected and what role does it play in the current analysis results?</p> <p>Categorical data were summarized using frequency count and percentage, not “absolute values”.</p> <p>“Multivariate” should be multivariable.</p> <p>Please remove “general” from “general linear model”, as it is essentially a linear regression model. Did the authors conduct any model diagnostics to check for assumptions and any potential influential points due to outliers?</p> <p>In the results, the authors used “majority” and “minority” to describe Table 1. It might be more informative to the readers to provide actual, rough percentages.</p> <p>In Table 1 for binary variables, can just report one category e.g., use of sleep medications. Similarly for Table 4, the authors can report one line to indicate the effect estimate for poor vs. good and remove the other line without numbers.</p> <p>There’s one additional limitation: lack of a proper survey study design. The findings could be more representative if a proper survey design was used, which consider strata (e.g., city, county) and probability sampling unit. Then, sampling weights can be incorporated into the analysis. However, I understand that an increased amount of effort would be required to achieve and implement the study.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name

José Alberto Laredo

Institution and Country

Universidad de Castilla-La Mancha, Spain

Please state any competing interests or state ‘None declared’:

None declared

Please leave your comments for the authors below

First of all I would like to congratulate the authors for the great work done and for addressing the issue in question as it is an issue of great importance for the quality of life of the elderly and that they remain healthy as long as possible, because the Rest in the elderly plays a very important role at full functional level.

As improvements proposed to the authors:

In the methodology when you say: "Each component's score ranges from 0 to 3 points so that the global PSQI score ranges from 0 to 21 points", it would be necessary to clarify how you do in Social Support Rating Scale, if 0 is the most negative score and 3 the most positive score or if it would be the opposite to avoid confusion.

Answer: Thank you so much for your valuable comment. We have clarified our scoring rules for PSQI and SSRS in our revised manuscript as follows:

"Sleep quality among the participants was assessed using the Chinese version of the Pittsburgh Sleep Quality Index (PSQI). The PSQI is a self-rated questionnaire that assesses sleep quality in the past month. It contains nineteen items grouped according to seven components: subjective sleep quality, sleep latency, sleep duration, sleep efficiency, sleep disturbances, use of sleeping medication, and daytime dysfunction²³. These components were defined as follows in this study: subjective sleep quality (good or poor); sleep efficiency (reduced sleep efficiency was sleep efficiency <85%); sleep latency (increased sleep latency was sleep latency >1); sleep duration (<7 h, 7-8 h and >8 h); sleep disturbances (any kind of sleep disturbance ≥1 time/week); use of sleep medication (use of sleep medication ≥1 time/week); and daytime dysfunction was daytime dysfunction of at least once a week. Each component's score ranges from 0 to 3 points such that 0 is the most positive score and 3 the most negative score. Therefore, the global PSQI score ranges from 0 to 21 points," (Page 8).

AND

"Social support among the elderly was assessed using the Social Support Rating Scale (SSRS), which was developed by Xiao³¹. The scale consists of 10 items and is divided into three parts: objective support, subjective support and availability of social support. For items 1 to 4, and 8 to 10, each item's score ranges from 1 to 4 points. For item 5, the first, second, third, and fourth options represent 1, 2, 3, and 4 points for each support source, respectively. In addition, for items 6 and 7, each option is 1 point when selected and zero when not selected. The total social support score ranges from 12 to 66 points, and higher scores indicate higher social support." (Page 9)

In the methodology section when you talk about Depression symptoms assesment you affirm: "The GDS-30 consists of 30 true / false questions, and the total score ranges from 0 to 30 points. Participants who scored 11 or greater are considered to be depressive in this study ", this text is repeated almost literally in the first lines of this section, so it is considered redundant.

Answer: Thank you so much for your valuable comment. We have deleted the repeated part in our manuscript .

In the line prior to statistical analysis there is a spelling fata that would lack a letter.

Answer: Thank you so much for your valuable comment. We have corrected this spelling error in our manuscript.

In the discussion section, when you state: "However, the use of sleep medication is not widespread in Chinese nursing homes, and only 10.3% of residents had taken sleep medication once or more a week," it would be necessary to broaden the discussion about Medication use and sleep quality in relation to the results of this study. According to what you say, only 84 people use sleep medication, but of those 84 people, 83 have poor sleep quality. Also, of people who do not take medication to sleep almost twice have poor sleep quality, so it is considered necessary to broaden the discussion based on these results.

Answer: Thank you so much for your valuable comment. We have broadened the discussion in our manuscript in relation to this outcome.

When you state: "These findings are in accordance with those of several previous similar studies in both nursing homes and communities," it would be necessary to include the corresponding citations here.

Answer: Thank you so much for your valuable comment. We have added corresponding citations in our manuscript.

Reviewer: 2

Reviewer Name

Kim Madden

Institution and Country

McMaster University, Canada

Please state any competing interests or state 'None declared':

None declared

Please leave your comments for the authors below

Thank you for this interesting article on the association between sleep disturbances and depression in elderly people in China. There are a few minor grammar errors but otherwise it is a well-written paper. I have a few comments for consideration.

1. The authors state that patients/consumers were not involved in the design of this study which seems like a missed opportunity.

Answer: Thank you so much for your valuable comment. We have acknowledged this missed opportunity by indicating it as a limitation in our study.

2. How was sleep duration measured? If this is self-report it may not be accurate. If self-report, is it possible to get any other source of data to corroborate the self-report?

Answer: Thank you so much for your valuable comment. Sleep duration was measured by self-report as indicated in the PSQI, and we have no any other source of data to corroborate. Thus, this measurement may not be accurate and we have viewed it as a limitation.

3. I'm not sure how much this contributes to the current literature. We already have evidence from elderly populations that sleep and depressive symptoms are associated. Is there any reason to believe that elderly in nursing homes would be substantially different to warrant another study?

Answer: Thank you so much for your valuable comment. The evidence about the relationship between sleep quality and depressive symptoms is abundant among studies conducted in communities. However, the social and living environment in nursing homes is totally different from that in the communities. Thus these factors warrant separating the elderly into those in nursing homes and those in communities when performing these investigations. Nevertheless, there is dearth of literature about the elderly in nursing homes as regards the relationship between sleep quality and depression symptoms. Therefore, this necessitated conducting this study.

4. Depression symptoms are one thing, but a DSM diagnosis of depression is another thing. The authors accurately describe depressive symptoms in this paper, but would it not be more interesting to look at DSM diagnoses of depression? It is possible that it is the poor sleep making people appear to have depressive symptoms. For example, one of the questions on the GDS is "Do you feel full of energy?" People who have poor sleep may not feel full of energy, not due to depression but due to the sleep issues.

Answer: Thank you so much for your valuable comment. We could not perform diagnosis of depression without a Psychiatrist. Therefore, in this cross-sectional study, we just focused on the impact of poor sleep quality on depression symptoms rather than depression.

5. Per STROBE reporting guidelines, please add dates of recruitment.

Answer: Thank you so much for your valuable comment. We have added dates of recruitment in our survey in the methods section. The recruitment process was conducted from October 2018 up until December 2018.

6. Patient consent was obtained. Can that authors elaborate? Was this written informed consent? Who obtained informed consent?

Answer: Thank you so much for your valuable comment. Written informed consent was obtained from all participants. We have added this information in the 'patient consent' part in our manuscript.

7. There are a lot of analyses. For example, the authors did t-tests/chi-square and crude and adjusted GLM analyses and binary logistic analyses. This seems to be redundant. What do the t-tests/chi-square tests add and why did the authors choose to analyze sleep as both a continuous and binary variable? How did the authors account for multiplicity?

Answer: Thank you so much for your valuable comment. It seems to be redundant; hence we have deleted the t-test results for sleep quality as a continuous variable.

8. What is the justification for the sample size?

Answer: Thank you so much for your valuable comment. The sample size was calculated using the formula for cross-sectional studies, as follows:

$$n = \frac{Z_{1-\alpha/2}^2 p(1-p)}{d^2}$$

Where $\alpha=0.05$, p is the prevalence of poor sleep quality (54.5% according to a survey) and d is an admissible error (which was 4%). According to the formula, the theoretical sample size was 656, which included an extra 10% to make up for non-response during the study.

9. How were missing data addressed?

Answer: Thank you so much for your valuable comment. Cases of missing data were excluded from this study.

10. I am curious how the authors managed to do 817 in-person interviews that each lasted an hour and a half. This is a huge amount of personnel time, yet this study received no funding. How was this achieved?

Answer: Thank you so much for your valuable comment. Firstly, we received enough support from the administrators of each nursing home. Secondly, our large survey team completed these interviews lasting two month.

11. Please report model fit statistics for the adjusted models.

Answer: Thank you so much for your valuable comment. We have reported model fit statistics for the adjusted models in our manuscript.

12. The discussion says that this is a large random sample of elderly persons, but it's not really. The nursing homes were random but the sample within the nursing homes does not appear to be random.

Answer: Thank you so much for your valuable comment. Indeed, the nursing homes were random but the sample within the nursing homes does not appear to be random. Thus, we have deleted this description.

13. Did the authors take into account cluster effects within a nursing home?

Answer: Thank you so much for your valuable comment. We have not taken into account cluster effects within a nursing home. This may be a limitation in our study.

Reviewer: 3

Reviewer Name

Yi-An Ko

Institution and Country

Emory University, USA

Please state any competing interests or state 'None declared':

None declared.

Please leave your comments for the authors below

Abstract: "Controlling for other factors?" better to state the covariates. If there's not enough space, the authors may consider using general terms, like demographics, disease history for example.

Answer: Thank you so much for your valuable comment. We have revised this sentence and considered using general terms to state the covariates.

Several studies have investigated the association between sleep and depression in elderly in different countries a long time ago. So, this association is not something new. It would be interesting to explore mediators actually. In the introduction, in addition to saying that the evidence is scarce in nursing homes, I'd love to learn more about the justification of doing such study. Is this the original study or a secondary analysis of some other parent project?

Answer: Thank you so much for your valuable comment. The evidence about the relationship between sleep quality and depressive symptoms is abundant among studies conducted in communities. However, the social and living environment in nursing homes is totally different from that in the communities. Thus these factors warrant separating the elderly into those in nursing homes and those in communities when performing these investigations. Nevertheless, there is dearth of literature about the elderly in nursing homes as regards the relationship between sleep quality and depression symptoms. Therefore, this necessitated conducting this study.

Have data regarding the use of antidepressant been collected and what role does it play in the current analysis results?

Answer: Thank you so much for your valuable comment. We have not collected this information about use of antidepressant. Therefore, we did not investigate its role in the current analysis results.

Categorical data were summarized using frequency count and percentage, not “absolute values”.

Answer: Thank you so much for your valuable comment. We have revised this information in our manuscript.

“Multivariate” should be multivariable.

Answer: Thank you so much for your valuable comment. We have revised this information in our manuscript.

Please remove “general” from “general linear model”, as it is essentially a linear regression model. Did the authors conduct any model diagnostics to check for assumptions and any potential influential points due to outliers?

Answer: Thank you so much for your valuable comment. We have removed “general” from “general linear model” in our manuscript. Moreover, we have added model fit statistics for these models.

In the results, the authors used “majority” and “minority” to describe Table 1. It might be more informative to the readers to provide actual, rough percentages.

Answer: Thank you so much for your valuable comment. We have revised this information and provided rough percentages in this part.

In Table 1 for binary variables, can just report one category e.g., use of sleep medications. Similarly for Table 4, the authors can report one line to indicate the effect estimate for poor vs. good and remove the other line without numbers.

Answer: Thank you so much for your valuable comment. We have made corresponding revisions in Table 1 in our manuscript.

There’s one additional limitation: lack of a proper survey study design. The findings could be more representative if a proper survey design was used, which consider strata (e.g., city, county) and probability sampling unit. Then, sampling weights can be incorporated into the analysis. However, I understand that an increased amount of effort would be required to achieve and implement the study.

Answer: Thank you so much for your valuable comment. Indeed, this is an important limitation in our study because we did not consider strata and sampling units as well as cluster effect. Therefore, we have acknowledged this limitation in our study (Page #).

VERSION 2 – REVIEW

REVIEWER	JA Laredo-Aguilera Universidad de Castilla-La Mancha Spain
REVIEW RETURNED	09-Apr-2020

GENERAL COMMENTS	Congratulate the authors, they have made the proposed modifications and the study has improved
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REVIEWER	Kim Madden McMaster University, Canada
REVIEW RETURNED	20-Apr-2020

GENERAL COMMENTS	<p>I thank the authors for their thoughtful edits; however, there were several of my comments that were not sufficiently answered.</p> <ol style="list-style-type: none"> 1. The authors said in their response letter that "we have deleted the t-test results for sleep quality as a continuous variable", It does not appear to me that this was done, and the t-test methods remain in the methods section. 2. The authors specify their sample size calculation in the response letter but the sample size justification should be in the manuscript as well, with references/justifications for each number used in the formula. Additionally, it appears that the authors have used the sample size calculation for prevalence studies, not the primary objective of this study which is "to examine the association between poor sleep quality and depression symptoms among the elderly of nursing homes in China." I recommend asking a professional statistician for assistance. 3. The authors state in their letter that cases with missing data are excluded, but this also needs to be reported in the manuscript, per STROBE recommendations. This item was marked "N/A" on the STROBE checklist which I don't believe should be "N/A". 4. I would prefer if they authors had made an attempt to take clustering into consideration rather than just list it as a limitation. A professional statistician could assist with this.
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REVIEWER	Yi-An Ko Emory University, USA
REVIEW RETURNED	15-Apr-2020

GENERAL COMMENTS	Thanks for the response. I have no further comments.
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VERSION 2 – AUTHOR RESPONSE

Thank you very much for your valuable comments and critiques, which have improved the clarity of our manuscript to a greater extent. Accordingly, we have thoroughly revised our manuscript based on the editors' and reviewers' comments and suggestions. The changes are highlighted in red for added words, or strikethrough for deleted words. Additionally, minor language, grammatical and stylistic errors have been corrected. Our specific point-by-point responses to the comments and queries have been addressed and itemized as follows:

I thank the authors for their thoughtful edits; however, there were several of my comments that were not sufficiently answered.

1. The authors said in their response letter that "we have deleted the t-test results for sleep quality as a continuous variable", It does not appear to me that this was done, and the t-test methods remain in the methods section.

Answer: Thank you so much for your valuable comment. Sorry we overlooked this in our previous revision. Now we have deleted the t-test methods description in the methods section as suggested (Page 10).

2. The authors specify their sample size calculation in the response letter but the sample size justification should be in the manuscript as well, with references/justifications for each number used in the formula. Additionally, it appears that the authors have used the sample size calculation for prevalence studies, not the primary objective of this study which is "to examine the association between poor sleep quality and depression symptoms among the elderly of nursing homes in China." I recommend asking a professional statistician for assistance.

Answer: Thank you so much for your valuable comment. We have described the sample size calculation in the methods section as follows:

"To examine the impact of sleep quality on depression symptoms among the elderly of nursing homes, a sample size was calculated using the formula as follows:

$$n = 4\{(\mu_{\alpha} + \mu_{\beta})/\ln[(1 + \rho)/(1 - \rho)]\}^2 + 3$$

Where, $\mu_{\alpha}=1.96$ when $\alpha=0.05$; $\mu_{\beta}=0.84$ when $\beta=0.80$; and ρ is the correlation coefficient between sleep quality and depression symptoms in the elderly in nursing homes. Thus, ρ was taken as 0.231, according to a previous study²³, when calculating the sample size. Therefore, using the formula, the theoretical sample size was 145 and, after including an extra 10% of 145, to make up for subjects who may be withdrawn during the study, the total sample size was 160" (Page 6).

3. The authors state in their letter that cases with missing data are excluded, but this also needs to be reported in the manuscript, per STROBE recommendations. This item was marked "N/A" on the STROBE checklist which I don't believe should be "N/A".

Answer: Thank you so much for your valuable comment. We have since reported this information in the methods section as follows: *"Of the remaining 829 older adults, 12 were excluded for incomplete data.* Additionally, we checked every item carefully in the STROBE checklist according to our manuscript and updated our presentation of the STROBE checklist.

4. I would prefer if they authors had made an attempt to take clustering into consideration rather than just list it as a limitation. A professional statistician could assist with this.

Answer: Thank you so much for your valuable comment. We have made an attempt to analyze data again using Multilevel linear and logistic regression models, for which we considered individual participants (level 1) nested in nursing homes (level 2). However, the empty model results showed that the variance of intercept was only 0.76 ($G = 0.76, p > 0.05$) and no statistical significance compare to linear regression model (Wald $\chi^2 = 2.32, p > 0.05$). Further, the intraclass correlation coefficient (ICC) was calculated as follows:

$$ICC = \frac{\sigma_{\mu 0}^2}{\sigma_{\mu 0}^2 + \sigma^2}$$

Where $\sigma_{\mu 0}^2 = 0.76$ was the variance of intercept, and $\sigma^2 = 56.08$ was the variance of residual so that the ICC was 0.02 in this study. It indicated that the variation in depression symptoms score in the elderly, attributed to nursing homes, only accounted for 2% of the variance partition coefficient (VPC); and the clustering effect in the depression symptoms among different subjects was not significant. Under these circumstances, we also calculated the effect of sleep quality on depression symptoms in nursing homes. We found that, if we take nursing homes as cluster into consideration, then there was almost no impact on the results as well as conclusions between sleep quality and depression symptoms. In this regard, the multilevel linear regression coefficient was 5.12 between sleep quality and depression symptoms, when considering the clustering effect, but this was 5.19 in the previous manuscript. Therefore, the preceding results precluded us from using the multilevel analysis methods in this study.

VERSION 3 – REVIEW

REVIEWER	Kim Madden McMaster University, Canada
REVIEW RETURNED	01-Jun-2020
GENERAL COMMENTS	Thank you, the authors have sufficiently addressed my comments.