Reviewer's report

Title: Handgrip Strength, Depression, and All-Cause Mortality in Korean Older Adults

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Reviewer: Hsi-Chung Chen

Reviewer's report:

Park et al. used a representative sample in Korea to examine the relationship between depression and hand grip strength (HGS) with all-cause mortality. A total of 13901 older adults (aged 60 years and more) were longitudinally followed for three years. The results showed that in males, regardless of the depression status, low HGS predicted an elevated risk for all-cause mortality. In contrast, in females, the relationship between low HGS and elevated mortality is only observed in those who had depression at baseline evaluation. Accordingly, the authors concluded that low HGS and depression contributed to elevated risk for all-cause mortality. In addition, the authors suggested that the relationship between HGS and depression with the mortality risk was modulated by sex. In general, large and representative sample makes this study stand out. However, some drawbacks seriously compromised the scientific merits of this manuscript.

[Major concern]

The content of this manuscript seriously deviated from its main findings. According to the result section and tables, whether depression or low HGS predicts elevated risk for all-cause mortality has never examined by Cox regression. However, the authors spent a lot of space to introduce and to discuss the independent effect of depression and low HGS on mortality risk. Just reflecting the authors' review, the relationship between depression and low HGS with mortality risk has been well-known in the literature. In this study, the most interesting finding, which has opportunity to add to the literature, should be that 'sex moderates the synergic effect of depression and low HGS on mortality risk' (Table 3). Unfortunately, the authors failed to provide background for this observation and did not discuss this main finding at all. For example, the authors discussed a lot regarding why sex moderated the relationship between depression and mortality risk- a point which was not illustrated in the analyses.

[Minor concern]

1. In the second paragraph of the Introduction section (lines 39), gender is a moderator, not a confounder. Besides, in this paragraph, the evidence of the cited article did not adequately
support the hypothesis—sex may play a role in determining the relationship of low HGS and depression with the mortality risk.

2. In the Methods section, which hand was used to test HGS?

3. In the Methods section (determinants of confounders), the authors introduced frailty as a confounder (line 35). However, in the following analyses, including illustrations in tables, I failed to identify this variable. In contrast, physical activity, which was estimated by International Physical Activity Questionnaire, was included in the analysis. Is frailty erroneously depicted?

4. In Statistics, the proportional hazard assumption should be examined.

5. Obviously, the present study aimed to illustrate a three-way interaction effect, i.e. moderated moderation. According to Table 4, the authors seemed attempt to illustrate how sex moderates the moderation effect of depression on the relationship between low HGS and elevated mortality risk. However, the authors did not examine this 3-way interaction terms (sex x depression x HGS). At least, the authors should examine the statistical significance of interaction term (depression x HGS) among both sex.

6. The authors claimed that both low HGS and depression were significantly associated with increased all-cause mortality risk (lines 9-12, the first paragraph of Discussion section). However, this statement was not supported by the description in the Result section. Except for univariate analysis in table 2 and 3, no Cox regressions examining the independent effect of low HGS and depression on mortality risk were illustrated.

7. The current form of Discussion section was more likely an introduction to the background. The authors should discuss more about why and how sex moderated the moderation effect of depression on the relationship between low HGS and elevated mortality risk.

8. In the 5th paragraph, there is a typo (line48, "Om the other hand).

9. Through table 1 to table 3, the subtotal of participants who consumed alcohol is not equal to the numbers shown above the saddle lines. However, the percentages for these two levels (no drinking and two times a week) were summed up to 100%.

10. In table 3, the p-value for alcohol consumption in women is lost.

11. I suggest reorganize table 4 to support the content of this manuscript. First, because the statistical significance remained in the full models, model I and model II is unnecessary.
Second, the independent effect of depression and HGS in the entire sample should be illustrated. Thirdly, stratified by sex, demonstrating the independent effects of depression and HGS is helpful. Finally, illustrating the relationship between HGS and mortality risk, stratified by depression status among each sex group (the current form of table 4).

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Unable to assess

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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