Author's response to reviews

Title: Measurement properties of the Minimal Insomnia Symptom Scale (MISS) in an elderly population in Sweden.

Authors:

Amanda Hellström (amanda.hellstrom@bth.se)
Peter Hagell (peter.hagell@med.lu.se)
Cecilia Fagerström (cecilia.fagerstrom@bth.se)
Ania Willman (ania.willman@bth.se)

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Author's response to reviews: see over
Cover letter

We would like to thank the reviewers for taking interest in reading our paper and for the suggestions of improvements that has been made. This is our reply to reviewer’s comments.

Reviewer 1

Major concerns:

1. A ROC analysis has been made but a gold standard is missing. The criteria used as gold standard has not higher validity than the MISS itself.

   We agree that this is a weakness of our study. However, the method used here is comparable to that in the original MISS study by Broman et al. (2008). We now address this weakness in more detail in the discussion and also emphasize the need for evaluation of the MISS against clinical diagnostic criteria (pp 9 and 10).

2. The authors do not distinguish between insomnia and sleep difficulties. The concepts should be specified or how the authors operationalize them. Also the connection between the created criterion variable and the ICSD-2 should be clarified and in what sense the authors use the concept incidence.

   When reviewing the manuscript, we have tried to make the concepts clearer for the reader. Also the connection between the criterion variable and ICSD-2 has now been clarified (p 6).

Minor concerns:

3. Sleep disturbances do not increase with increasing age, but insomnia do (page 2).

   This has been corrected and “sleep difficulties” has been changed to “insomnia” in the text.

4. There is no good evidence that insomnia or sleep disturbances in women generally are caused by fluctuations of sex hormones.

   The text about insomnia in women has been revised.

5. On page 3 some of the text is missing concerning how the MISS was developed.

   We have now corrected this.

6. Psychometric details of the MISS in other samples than that in this study seem irrelevant in the introduction.

   The introduction has been changed, and shortened. All details about previous psychometric testing have been left out.

7. There are many repetitions in the introduction about the need of a short instrument. This only has to be mentioned once.

   We agree with the reviewer on this comment and the text has been revised and altered in order to be less repetitive and more direct.
8. Comments on the sleep questionnaire PSQI seems out of place in this study, since that questionnaire has not been used.

We agree, and the comments on the PSQI have been omitted.

9. The authors should consider including a fourth item to the MISS, in order to increase validity and also cover the aspect of daytime impairments.

This is something that we have discussed among the authors of the study. The reason for not including a fourth item is that we strived to test the same scale as Broman et al. (2008), because then we can compare the two studies with each other. Another concern is the design of our study with only one questionnaire that has been used. There is no item in the USI-25 that properly covers daytime impairments. Therefore no suitable fourth item is available in the studied sample. When constructing the criterion variable three different items were combined. We found them to describe daytime impairments in the suggested combination. It would have been beneficial with a broader item covering daytime impairments and also duration and frequency of the sleep difficulties. We have added this discussion under the “Limitations” section.

Overall comments:

10. When it comes to implications of insomnia, the authors should mention sick leave and disability pension, hence economical implications.

This is of course true and we have now included this aspect of insomnia in the introduction.

11. There are several screening instrument that are shorter than the PSQI, which the authors compare the MISS to. PSQI is furthermore not a screening instrument especially design for identifying insomnia.

We have now mentioned these other instruments that are available for insomnia measurements.

12. In terms of analysis a structural equation modeling (CFA) should be considered by the authors.

Since data is on an ordinal scale we find this suggestion problematic. In this study item-total value has been calculated as well as the inter-item correlation. We assert that this should be sufficient in order to interpret data and judge the affinity between items. Furthermore, since our aim was to reproduce a previous study testing the MISS, we did not focus on exploring underlying structures of the scale. We now also emphasize the need for more robust psychometric analysis, such as Rasch analysis (p.12).
Major concerns:

1. **It is unclear how the sampling was carried out and if the sample is supposed to be representative of the elderly population?**

   We have amended the text in the methods section, hopefully making the sampling process and the characteristics of the sample more clearly to the reader.

2. **Was the same gold standard used in the previous study in the general population and since the cut-off in the present study proved to be higher – should the cut-off vary between the age clusters or by gender?**

   This is an interesting point. Looking at Table 5 the prevalence of insomnia varies between genders and is higher among women and persons 90 years or older. However, when dividing the sample, the subgroups tend to be very small, and the statistics uncertain. In a greater sample this should be tested. Please also see our response to Reviewer one’s first comment.

3. **The paper should be re-written so that the information is presented in a more logical manner.**

   We have now re-arranged the paper in which the English language has been revised once again and hopefully you will find that the flow and logic of the text has improved.

Minor concerns:

4. **The title of table 5 refers to Incidence of insomnia, which is not measured in this study.**

   The title of Table 5 has been corrected.