RESPONSE LETTER (MANUSCRIPT NO.67320)

Dear Editor:

We would like to thank the Editor and the Reviewers for carefully going through the manuscript and providing valuable comments. Following the Reviewers' suggestions, we have made extensive modification on the original manuscript. The details are as follows, and highlighted in revised manuscript. We hope that our revisions and responses can effective address the reviewer's concerns.

Sincerely yours,

X. H. Sun

Response to the comments of Reviewer #1

The study entitled "Effects of mindful breathing combined with sleep-inducing exercises in insomnia patients" used a quasi-experimental design to examine whether home-practice effects of mindful breathing combined with a sleep-inducing exercise are beneficial for patients with insomnia. Two groups of patients (40 in the control group and 40 in the observation group claimed by the authors) were assessed for their performance in sleep quality (using PSQI), insomnia severity (using ISI), and general anxiety (using GAD-7). The results indicated that the observation group had better sleep quality, insomnia severity, and general anxiety than the case group in post-assessment and follow-up assessment. The study topic is interesting and the use of mindful breathing combined with sleep-inducing exercises could be potential treatment for patients with insomnia. However, I have several concerns when reading the paper.

1. I think that the authors should clearly indicate that they use a quasi-experimental design for readers to better understand the study design. Moreover, I think that "observation group" is not a good term to define the group who received additional treatment (i.e., mindful breathing combined with sleep-inducing exercises". It would be better to indicate that this group is a "treatment group".

Reply: Following with reviewer's suggestion, we add the direct explanation that a quasi-experimental design was used in this work. The modulation was made in the abstract and main text: "*A quasi-experimental design was used in the present work, in which the patients with insomnia were included and grouped based on hospital admission:*"; "*In this study, we employed a use a quasi-experimental design to investigate the effects of mindful breathing combined with a sleep-inducing exercise as adjunctive therapies for insomnia patients.*"

We agree with the reviewer's comment and "observation group" has been corrected to "treatment group" in the revised manuscript.

2. The authors have tested GAD-7; however, the performance of GAD-7 was not mentioned in the Abstract.

Reply: We thank the reviewer for pointing our mistake. The performance of GAD-7 was added in the abstract along with that of PSQI and ISI: *"The PSQI, GAD-7, and ISI scores before the intervention and at 1 week after post-intervention were not significantly different between the groups.*" ; "*The GAD-7 scores were 2.75 ± 1.50 vs. 7.15 ± 2.28, and the ISI scores were 8.68 ± 2.26 vs. 3.38 ± 1.76 for the treatment vs. the control group, respectively*".

3. Based on the principle of person-first language, I would suggest using "people with insomnia" or "patients with insomnia" instead of "insomnia patients".

Reply: Thanks for the reviewer's comment. The "insomnia patients" has been corrected to "patients with insomnia" in the revised manuscript.

4. The authors claimed to test the home-practice effects of mindful breathing combined with a sleep-inducing exercise. However, the treatment has been given since the patients were in the hospital. Therefore, the authors should explain how they distinguish the effects from the treatment provided in the hospital settings and those from the home-base.

Reply: Thanks for the reviewer's comment. In our work, the hospital treatment lasts for 5 days and the rest of time is home practice. The control group received routine therapies and care, while the treatment group was intervened with mindful breathing and a sleep-inducing exercise in addition to the routine therapies and care. The intervention starts since the patients were in the hospital. Table 3 showed that 1 week of intervention with the routine pharmacological and physical intervention therapies administered to the patients with insomnia during the hospitalization period did not significantly affect the treatment group. Thus, the effectiveness of the two practices was not demonstrated within that short time-frame. The reason may arise from the same routine therapies and care and no obvious effect of mindful breathing on sleep quality in a short period. However, the discharged patients presented significant improvements in sleep quality at 1 and 3 months of the intervention, as shown in Table 4 and 5. Therefore, the enhancement of sleep quality can be attributed to the mindful breathing and a sleep-inducing exercise during the home practice. The effects of the treatment provided in the hospital are mainly to let the patients learn mindful breathing practice as well as receiving routine therapies and care. Herein, we added relevant explanations in the revised manuscript: "*Our results showed that 1 week of intervention with the routine pharmacological and physical intervention therapies administered to the patients with insomnia during the hospitalization period did not significantly affect the treatment group. Thus, the effectiveness of the two practices was not demonstrated within that short time-frame. However, compared with the control group, patients in the treatment group exhibited significant improvements in sleep quality, sleep latency, sleep efficiency, sleep duration, daytime functioning, anxiety level, and insomnia severity at 1 and 3 months of the intervention. The enhancement of sleep quality can be attributed to the mindful breathing and a sleep-inducing exercise during the home practice.*"

5. The description "The numeric data were expressed as $\chi 2 \pm SD$, and t-tests were performed" is wrong. The authors should consult a statistician to describe the statistics accurately.

Reply: We thank the reviewer for pointing our mistake. The statistics were corrected in the revised manuscript: "The data analysis was performed using SPSS 17.0 statistical software. The numeric data were expressed as $\chi^2 \pm S$, and t-tests were performed".

6. Apparently, the authors did not consider the inflation of type 1 error in their study. Please consult a statistician to tackle this issue.

Reply: Thanks for the reviewer's comment. Following with the suggestion of statistician, differences with P < 0.01 were evaluated as statistically significant.

7. The presentation of p=0.000 is wrong. P-value can never be 0. Therefore, when the p-value is very small, please use p<0.001 to present.

Reply: We thank the reviewer for pointing our mistake. The presentation of p value when it is very small was corrected to "p<0.001" in Table 3 and 4 in the revised manuscript:

8. The authors mentioned that "The groups were comparable in baseline characteristics, with no significant differences". However, the authors did not provide such comparison statistics and the baseline characteristic information (except for the age and gender).

Reply: Thanks for the reviewer's comment. Following with the reviewer's comment, we provide a table containing the comparison statistics and the baseline characteristic information. As shown in Table R1, the married and university graduate numbers were added here except for the age and gender. 40 patients were assigned to the control group that included 38 married people and 34 university graduator. 40 patients were assigned to the treatment group that included 37 married people and 32 university graduator

Table R1. Comparable of the Baseline characteristics of the patients with insomnia between the treatment group and control group.

Characteristic	Treatment (N=40)	Control (N=40)

Age, years—mean±standard deviation	52.4 ± 11.5	51.3 ± 10.2
Male sex—no./total no. (%)	13/40 (32.5)	15/40 (37.5)
Married—no./total no. (%)	37/40 (92.5)	38/40 (95)
University graduate—no./total no. (%)	32/40 (80)	34/40 (85)

9. I may overlook. However, it seems that the authors did not mention whether there is loss to follow-up. If there is any loss to follow-up, this should be clearly mentioned with the information how the authors take care of the issue of loss to follow-up.

Reply: Thanks for the reviewer's comment. In our work, one nurse was responsible for following-up with patients after they were discharged and a "WeChat" group was set up to follow-up with the patients after their discharge. There isn't any loss to follow-up, both groups have the complete follow-up information. Herein, we added the relevant information in the revised manuscript: "*A "WeChat" group was set up to follow-up with the patients after their discharge. They were reminded daily to practice the 2 interventions at the specified time as instructed and reported from their homes daily via "WeChat." The follow-up period ended when the patients had practiced for 3 months. Both groups have the complete follow-up information*".

10. I think that providing a flowchart can enhance the readability of the manuscript. That is, the readers can have a better idea regarding the study design and study process.

Reply: We thank the reviewer for the valuable suggestions, and a flowchart of the study design and process was added in the revised manuscript.

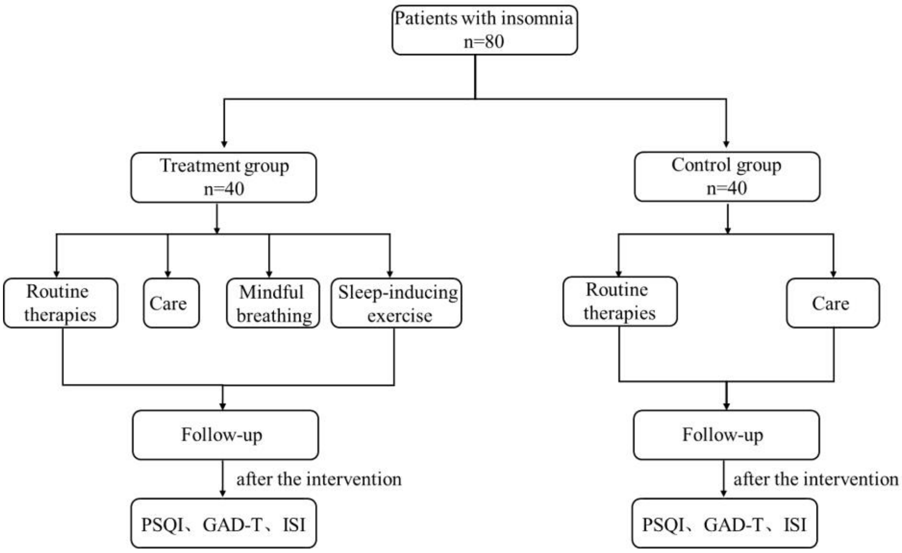

Figure R1. Flowchart of study design. PSQI: Pittsburgh Sleep Quality Index, GAD-7: Generalized Anxiety Disorder 7-item scale, and ISI: Insomnia Severity Index.

11. The authors assessed GAD-7; however, they did not mention anything regarding the effects of mindfulness on anxiety in the Introduction.

Reply: Thanks for the reviewer's comment. We added the relevant introduction about the effects of mindfulness on anxiety in the revised manuscript: "Mindfulness is a well-researched psychological practice and can be an effective nonpharmacological intervention. For example, the efficacy of mindfulness-based interventions enables the reduction of anxiety and depression.[9, 10] More importantly, its stability and effectiveness have been also demonstrated for in many studies about the insomnia[11]".

12. Pakpour and his colleagues have done some studies in the cognitive therapies on sleep. I believe that their studies are relevant to the present submission. I would strongly encourage the authors to elevate their study quality with the use of previous studies.

Reply: We thank the reviewer for the valuable suggestions, and we added the relevant introduction about Pakpour's studies in the cognitive therapies on sleep in the revised manuscript: "Pakpour's group proposed a cognitive behavioral technique (CBT-I) app-based intervention for insomnia and demonstrated that the patients with insomnia showed improved sleep hygiene behaviors, enhanced sleep quality, and less insomnia severity after receiving the CBT-I.[8, 9]".