Author's response to reviews

Title: Prevalence of dyslipidemia and its association with insomnia in a community based population in China

Authors:

Yiqiang Zhan (zhanyiqiang@gmail.com)
Fen Zhang (w524zf@163.com)
Jinsong Wang (driinsongw@gmail.com)
Yihong Sun (dryihongs@gmail.com)
Rongjing Ding (drrongjiingd@hotmail.com)
Dayi Hu (drdayihu@hotmail.com)
Jinming Yu (jmyu64@gmail.com)

Version: 3
Date: 24 September 2014

Author's response to reviews: see over
Responses to the reviewer

Reviewer: Erik Berglund

Major comments:
1. Conclusion in abstract must be restricted to women with frequent insomnia.
   Reply: Thanks for the comments. We revised the conclusion in the abstract as suggested.

2. Line 67: Change: ”evidence” to “research”.
   Reply: Thanks for the comments. “evidence” was changed to “research”.

3. Line 111-113: reference on defined diabetes and hypertension is needed.
   Reply: Thanks for the comments. Two references were added regarding the definition of diabetes and hypertension.

4. In the Methods section the gender and age distribution of the sample should be included. Preferably under a “Sample” heading.
   Reply: Thanks for the comments. We added a paragraph under the heading “Sample” about the gender and age distribution.

5. Number of participants and response rate must be including in the Methods section.
   Reply: Thanks for the comments. We added a paragraph under the heading “Sample” about the number of participants and response rate.

6. Cochran-Armitage method is not a well-known method, explain it more and describe more closely were it have been used on the data.
   Reply: Thanks for the comments. We added a sentence and reference regarding Cochran-Armitage method in the statistical analysis part.

7. In results, please provide numbers of those with no, occasional and frequent insomnia.
   Reply: Thanks for the comments. We added the numbers of people with different insomnia status in the results.

8. In table 1, please provide absolute numbers in top of columns of the subgroups.
   Reply: Thanks for the comments. The numbers were added in the top of columns.

9. Are the analyses in table 3 logistic regressions? Why, if your outcome variable is continuous and normally distributed? If a true association exist wouldn’t it be easier to find it in a linear regression? Please elaborate on this and if possible include the results of such test in a sensitivity analysis in relation to your other findings.
Reply: Thanks for the comments. Yes, table 3 was done using logistic regression models. We do concur with the reviewer’s comments that since the outcome variable is continuous, it might be more appropriate to observe an effect if linear regression was used. We did linear regression models and found the association between insomnia and lipid was only significant for total cholesterol. We added this result in the results part. The reasons we chose normal/abnormal serum lipid is mainly because it is more important to know whether an individual is diseased or not and this diagnosis is recommended in the Chinese Adult Dyslipidemia Prevention Guideline.

10. The authors adequately conclude in limitations that no conclusions about causality can be drawn from the study. Yet their phrasing (in the abstract conclusion and in line 179) is that insomnia is associated with a higher risk of dyslipidemia, which easily could be misunderstood as a causal statement. Please rephrase in abstract as well as in discussion/conclusion that insomnia was associated with higher occurrence/prevalence of dyslipidemia.

Reply: Thanks for the comments. We revised the manuscript regarding the wording “risk” and changed it the “prevalence” throughout the manuscript including discussion and conclusion.

11. There is a need to demonstrate that results of this study have meaningful implications. The authors have not presented convincing evidence that insomnia is predictive of treatment outcomes and that modifying this factor improves health outcomes on dyslipidemia in women.

Reply: Thanks for the comments. We added a paragraph to discuss this issue in the discussion part.

Reviewer: Suttajit Sirijit

Reviewer's report:
This is a community based cross-sectional study investigated the prevalence of dyslipidemia and its association with insomnia. The article was well-written and all limitations were clearly stated. The quality of written English is acceptable and the manuscript does not need to be seen by a statistician. It should be accepted after major and minor compulsory revisions.

Minor essential revisions
1. Sample size calculation should be added.

   Reply: Thanks for the comments. We added the sample size in the “Sample” part.

2. On page 3, line 67-71, please clarify what was the primary objective of the study. Was the objective indicated a priori?

   Reply: Thanks for the comments. We revised this as suggested and the objective was indicated as a priori.

Major compulsory revisions
1. Please provide more explanation why the association was significant only in women but not in men.
Reply: Thanks for the comments. We added more sentences to discuss the gender differences in the discussion part.

2. Depression and other psychiatric disorders might act as a mediator in the association between dyslipidemia and insomnia. Please add discussion on this matter.

   Reply: Thanks for the comments. We added more sentences in the discussion part to discuss depression and psychiatric disorders as a mediator.