Author's response to reviews

Title: Sleep Quality in Middle-aged and Elderly Chinese: distribution, associated factors and associations with cardio-metabolic risk factors

Authors:

    Nazanin Haseli-Mashhadi (hasseli57@yahoo.com)
    Tony Dadd (tony.dadd@unilever.com)
    An Pan (apan@sibs.ac.cn)
    Zhijie Yu (zjyu@sibs.ac.cn)
    Xu Lin (xlin@sibs.ac.cn)
    Oscar H Franco (o.H.Franco@warwick.ac.uk)

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Editor BMC Public Health

Subject: re-submission of manuscript

Dear Editor,


We considered carefully all the reviewers and editorial comments, which were very useful to further improve our paper for BMC Public Health. In the attached letter we give an overview of the changes we have made in response to the editorial and reviewers comments (in bold type the changes in the manuscript).

We hope this revision makes our manuscript now suitable for publication in BMC Public Health.

Yours truly,
(On behalf of the authors)
Reviewer 1:

1. The authors present a large set of data somehow related to subjective ratings of sleep quality in Chinese residents of Beijing and Shanghai. I have two major problems with the manuscript: 1) sleep quality appears to me superficially assessed. This lack of detail stands in sharp contrast to the multitude of variables reported. If the prevalence of sleep disorders (which would be the desirable information here) is the key variable to be explained, then its assessment should be a valid one. There are many standardized questionnaires available and to only base sleep quality on a single self-rating, is not sufficient.

We agree with the reviewer’s comment. We are aware of other standardized questionnaires (e.g. the Pittsburg Sleep Quality Inventory), which we admit are more adequate to measure sleep disorders. Nevertheless, this is a large cross-sectional study conducted in more than 3000 participants from North and South of China with the aim to evaluate the effect of nutrition and ageing in this population’s health. Considering the size and the type of study, it resulted logistically impossible to incorporate longer questionnaires in the general questionnaire of the study, since this would limit the level of participation and the possibility to evaluate other relevant areas related to nutrition and ageing –besides sleep- in the evaluated group. It is possible that by using a simple level number of questions to address the duration and quality of sleep in our population we have underestimated the true prevalence of sleep disturbances, nevertheless, this underestimation would result in a generalized effect as it has been non-differentially administered to all participants in the study. We acknowledge this issue in the discussion section of the revised manuscript as presented below:

“Furthermore, sleep patterns were measured using a questionnaire; a criticism of this type of measurement is that answers are provided by the participants and can be affected by recall bias. Furthermore, due to the size of the population and the extent of the general questionnaire, for logistic reasons the questions that we used are not as thorough as alternative validated questionnaires designed to
evaluate sleep disturbances (e.g. The Pittsburg Sleep Quality Inventory). Nevertheless, these factors could only generate a non-differential misclassification of the exposure (sleep patterns) that would attenuate the size of the effects hereby reported and the level of sleep disturbances in the population studied, but not necessarily our results and conclusions. Prospective evaluations and studies that provide more sensitive measures of sleep patterns in Chinese populations are required to clarify the determinants that affect sleep quality. This will also allow us to assess whether sleep disturbances are associated with early deviations from adequate cardio-metabolic health.”

2. The manuscript lacks a clear statement. Of course, this is related to point 1). What do I learn from the manuscript besides the fact that subjectively rated poor sleep is related to sleep duration and a number of life style and disease-related factors? Clear hypotheses would have benefitted the manuscript.

We appreciate the reviewer’s comment and in the revised version of the manuscript we have modified the introduction section to more clearly present the a-priori hypotheses that motivated us to conduct these analyses. It is important to highlight the relevant epidemiological gap that exists on data about the sleep patterns and its associated factors among Asian –and elderly- populations. Furthermore, data on the potential associations between sleep patterns and biomarkers of cardio-metabolic disease are limited and generally evaluated in studies of smaller sample size. The introduction section of the revised manuscript has been modified accordingly:

“Furthermore, although ample data exist for different populations, there is limited information on the levels of sleep disturbances among Chinese populations, the possible factors associated and the potential association of sleep quantity and quality with biomarkers of early cardio-metabolic disorders.

Therefore, in order to evaluate the levels of sleep patterns in Chinese and contribute to complement its related gap of epidemiological evidence and knowledge, in the current study we aimed to evaluate: (i) the distribution of sleep quality; (ii) factors that determine sleep quality; and (iii) whether self-reported sleep quality is associated with biomarkers of cardio-metabolic disease in middle-aged and elderly Chinese.”

3. The reasons for the choice of statistical analyses should be elaborated on. Did the authors conduct corrections for multiple testing, for example, and if so, which?

We appreciate the comments of the reviewer. The statistical methods were selected based on previous literature taking into account the study design, the research questions and the nature of the outcomes studied. They are all standard statistical techniques used in epidemiological research. We have presented these reasons and explained how the methods were selected in the methods section. No further clarification has been included in the revised version of the manuscript considering the limitation of length.

With respect to multiple testing, we considered a Bonferroni correction but, as is common with exploratory epidemiological research, did not have a pre-specified
number of independent hypotheses to work with. This means that any Bonferroni correction may be misleading and thus we feel it is more honest to present the original p-values as statistics for comparison of evidence between our various null hypotheses in their own right, rather than actual levels of probability. This is an open indication of the hypothesis-generating nature of epidemiological research which we feel does not detract from the main conclusions of the paper.

Reviewer 2:

1. Specify some primary hypotheses a priori to create focus. The focus should related to a gap in the sleep epidemiology literature. Other findings relating to confounders will be reported but will not be the focus of the paper.

We appreciate the reviewer’s comment. In the revised version of the manuscript we have modified the introduction section to clarify the hypotheses and aims of the current analyses. We apologize for the previous lack of clarity and hope that the revised sections of the manuscripts appear clearer and easier to understand. We have also in the subsequent results section decreased the amount of results presented in relation with confounders that did not constitute the central variables in the manuscript. On the other hand, we have not modified the results and discussion sections related with the analyses of evaluating the associations between sleep levels and biomarkers of cardio-metabolic disorders since we considered this as one of the central aims of this manuscript as well as one of its more relevant strengths and considering the lack of data existing on the evaluation of the association between sleep levels and biomarkers of cardio-metabolic disorders in Asian populations and in general in large populations studies and studies evaluating elderly populations we have decided to maintain this area of the manuscript as previously submitted. Nevertheless, if the editor considers the contrary we will be glad to further summarize the information presented in relation with the association of sleep levels and levels of cardio-metabolic biomarkers and factors.

The introduction section has been modified as follows:

“Furthermore, although ample data exist for different populations, there is limited information on the levels of sleep disturbances among Chinese populations, the possible factors associated and the potential association of sleep quantity and quality with biomarkers of early cardio-metabolic disorders.

Therefore, in order to evaluate the levels of sleep patterns in Chinese and contribute to complement its related gap of epidemiological evidence and knowledge, in the current study we aimed to evaluate: (i) the distribution of sleep quality; (ii) factors that determine sleep quality; and (iii) whether self-reported sleep quality is associated with biomarkers of cardio-metabolic disease in middle-aged and elderly Chinese.”

2. This is not a study of sleep disorders but of subjectively assessed sleep quality, so self-reported measures are appropriate. However, the authors should discuss how the measure of sleep quality used is similar or different from that used in other epidemiological studies. Clinical measurement of sleep disorders is
not necessary in population epi studies.

We agree with the comments of the reviewer. We are aware that due to the mere size of the study and the length of the general questionnaire in this case administered to approx. 3000 participants we had to decide to include only a couple of questions inquiring the levels, quality and duration of sleep in the evaluated population. We have modified the revised manuscript accordingly and added the following discussion in the discussion section:

“Furthermore, sleep patterns were measured using a questionnaire; a criticism of this type of measurement is that answers are provided by the participants and can be affected by recall bias. Furthermore, due to the size of the population and the extent of the general questionnaire, for logistic reasons the questions that we used are not as thorough as alternative validated questionnaires designed to evaluate sleep disturbances (e.g. The Pittsburg Sleep Quality Inventory). Nevertheless, these factors could only generate a non-differential misclassification of the exposure (sleep patterns) that would attenuate the size of the effects hereby reported and the level of sleep disturbances in the population studied, but not necessarily our results and conclusions. Prospective evaluations and studies that provide more sensitive measures of sleep patterns in Chinese populations are required to clarify the determinants that affect sleep quality. This will also allow us to assess whether sleep disturbances are associated with early deviations from adequate cardio-metabolic health.”

3. Mention as limitations of the study that obesity and an alcohol use were coded with limited ranges. Had morbid obesity or higher levels of alcohol use been included as categories, these risk factors might have become significant.

We agree with the reviewer’s comment and have modified the discussion section and added a sentence regarding the potential lack of power in our study to identify a significant association between obesity and alcohol consumption in our population. We considered that this is mainly due to the lack of individuals in the high extremes of the distribution of these two variables. Additionally its worth mentioning that a different classification of BMI is used among Chinese being BMI >=28 considered obesity.

“Furthermore, this is affected by the limited number of participants in the extreme categories of alcohol consumption and BMI, which limits the statistical power to identify a potential association between these two variables and sleep patterns in our population.”

4. Reanalyze the data with bmi>35 as a category and drinking frequency categories.

We appreciate the reviewer’s comment and have taken a second look into the data considering a different category of BMI and drinking frequencies, we have found no different in the results and this is due mainly to the lack of participants in the extremes of the distribution of these two variables. As mentioned in the point above it is worth considering that Chinese have allocated a different classification of BMI in which levels of BMI equal or above 28 are considering
obese. Therefore, we have decided not to present these additional analyses in the revised version of the manuscript.