Author's response to reviews

Title: Interaction between Physical Activity and Sleep Duration in relation to Insulin Resistance among Non-diabetic Chinese Adults

Authors:

Hui Zuo (huizuo97@gmail.com)
Zumin Shi (zumin.shi@gmail.com)
Baojun Yuan (jiscdcybi@yahoo.com.cn)
Yue Dai (delia0616@163.com)
Gang Hu (Gang.Hu@pbrc.edu)
Gaolin Wu (wgljiscdc@yahoo.cn)
Akhtar Hussain (akhtar.hussain@medisin.uio.no)

Version: 2 Date: 10 February 2012

Author's response to reviews: see over
Covering letter

MS: 1369393017615319
Title: Interaction between Physical Activity and Sleep Duration in relation to Insulin Resistance among Non-diabetic Chinese Adults

Dear Mr. Victorino Silvestre,

Thank you very much for your email of January 11, 2012, with regard to our manuscript (MS: 1369393017615319) together with the comments from the reviewer. We are very pleased to know that our manuscript presented some potentially interesting information which might be acceptable for publication. We found the comments and suggestions of the reviewer to be very helpful.

According to the comments, we have carefully revised relevant parts in the original manuscript. We also responded point by point to the reviewer’s comments as listed below, along with a clear indication of the location of the revision. We believe your help and the reviewer’s comments have significantly improved the quality of manuscript and made it acceptable for publication.

Thank you very much for your continued attention. We look forward to your favorable decision.

Best regards,

Hui Zuo
Response to the reviewer:

MAJOR COMPULSORY REVISIONS

1. Sleep duration was discussed under the paragraph heading “Additional Variables”. Since it was one of the main variables that was being evaluated as a predictor of insulin resistance, it should be discussed in a separate paragraph under its own heading. The authors should explicitly state how sleep duration was determined. Presumably it was by questionnaire based on subject recall. The rationale behind why these specific three categories of sleep duration were chosen should also be explained. Why not use ≤ 5 hours, 5 to 7 hours, and ≥7 hours. Is there previous data suggesting that these categories are important? If not, then perhaps just using standard deviations of sleep duration might be useful.

RE: Many thanks for the helpful comment on sleep duration! We have put “sleep duration” information in a separate paragraph under its own heading in the revised manuscript as follows:

“Sleep duration

The number of hours of sleep were assessed with the staff-administered questionnaire[29] by the question ‘How much time each day do you usually spend in bed either sleeping or lying there, including nighttime (hours)?’ Since no established classification criteria for sleep duration can be found from published data, we then categorized the variable into three groups: ≤7 hours, 7-9 hours, and ≥9 hours mainly based on the data distribution and sleep reality among Chinese population[32].”

Epidemiologically we cannot find established classification criteria for sleep duration from published data. So the classification of sleep duration (the number of hours of sleep) in our study is mainly based on the data distribution itself and sleep reality among Chinese population. Another study from the same province, China, using the same sleep classification, showed that those who slept for less than 7 h a day had significantly higher (P = 0.005) percentage of energy from fat intake than those who slept for 7–9 h per day. (Shi Z, McEvoy M, Luu J, Attia J: Dietary fat and sleep duration in Chinese men and women. Int J Obes (Lond) 2008, 32(12):1835-1840).

In our sample, the mean and standard deviation of sleep duration (hours) were 8.23 and 1.17. Our classification (≤7 hours, 7-9 hours, and ≥9 hours) was relatively balanced and can ensure enough samples in each category for the analysis. Whereas, the number of individuals whose responded ≤5 hours, 5-7 hours, and ≥7 hours were 8, 67 and 1049, respectively. Only few subjects would be collapsed in the first two groups. As we know, a small sample size can decrease the statistical power of a test. Alternatively, we have also attempted to categorize the variable as ≤6 hours, 7-8 hours, and ≥9 hours. Similar results were observed in our study. It was described in detail in
DISCRETIONARY REVISIONS, question 3.

2. The authors suggest that persons diagnosed with chronic disease could purposely do more leisure time physical activity. The implication is that these persons would also do less occupational physical activity. The authors should point out that in the higher quartiles of HOMA, where the leisure time physical activity is high, the occupational physical activity is lower. To test this further, they could report on the occupational activity amongst those with higher leisure time physical activity compared with lower leisure time physical activities. This might underscore the point that it is not the high leisure time physical activity that is associated with the high HOMA but rather the high leisure time activity might be reflective of lower occupational activity which itself is associated with a high HOMA. I think this is an important point worth making.

RE: Thanks for the reviewer’s comments on this point. Change has been done accordingly. Please see Discussion, paragraph 5, line 3-11 in the revised manuscript.

“It can possibly be explained by that persons performing more leisure physical activity would also do less occupational physical activity. As we can see, in the higher quartiles of HOMA, where the leisure time physical activity was high, the occupational physical activity was lower. The mean occupational activity among those with leisure physical activities was significantly lower than those without leisure physical activities (50.1 vs. 132.1 MET-hrs/week, p<0.001). Therefore, it is not the high leisure physical activity that is associated with the high HOMA but rather the high leisure time activity might be reflective of lower occupational activity which itself was associated with a high HOMA.”

DISCRETIONARY REVISIONS

1. The authors note a positive association between leisure physical activity and HOMA both in crude model and after adjustment for age and gender. They postulate that this might be due to the extremely small proportion (2.5%) of persons performing leisure time physical activity in the study population. Although the proportion of person performing leisure time physical activity was small it did contain approximately 30 persons. One could consider using linear regression as opposed to regression across means to evaluate this association, as there would be more data points against which to assess the association.

RE: In our study, 1008 individuals reported no leisure physical activity, while only 116 individuals performed leisure physical activity. It was therefore not normally distributed. And also, it cannot easily be normalized by transformation. So it was not allowed for linear regression here. We think it would be better to evaluate the association between leisure physical activity and HOMA by comparing HOMA and
analyze the trend across different levels of leisure physical activity.

2. It is interesting that additional adjustment for WHR, education, urban income, smoking, alcohol drinking dietary intakes, other types of physical activity in Model 2, reduced the magnitude of the association with HOMA across all tertiles of physical activity in both Tables 2 and 3. This suggests that one or more of these variables might be capturing the mechanisms underlying the association between physical activity and HOMA. This could be WHR, especially if physical activity and WHR were associated. There have been many studies relating anthropometric measures to underlying insulin resistance. It is interesting to note that these factors have less of an impact on the models evaluating the association between HOMA and sleep duration suggesting that the mechanisms underlying this association may be different from that of HOMA and physical activity. The combination of low physical activity and short sleep duration was noted to have the highest odds of insulin resistance and this further emphasizes this point.

RE: Thanks for the explanation and analysis.

3. The p value for trend of sleep duration categories across quartiles of HOMA may have of more clinical and statistical significance if different categories of sleep duration were used to evaluate these trends. Is there a clinically important difference between 7-9 hours of sleep and >9 hours of sleep? Are these not both adequate amounts of sleep? The authors could consider changing the categories to include categories of lower sleep duration such as <5 hours, 5-7 hours, >7 hours.

RE: As stated above, there were only 8 individuals whose responded ≤5 hours in our study, which may boost the random error and may be not effective to detect possible statistical significance.

Alternatively, we attempted to categorize the sleep variable as ≤6 hours (short sleep duration), 7-8 hours (appropriate sleep duration), and ≥9 hours (long sleep duration). Similar results were observed in our study. There was no statistical significance between sleep duration and HOMA after adjustment for confounders although the increasing trend was observed (p for trend >0.05 in generalized linear models and p for trend=0.403 in the full model by logistic regression). The combination of low physical activity and short sleep duration was associated with the highest odds of insulin resistance in the study (adjusted OR=5.12, 95% CI: 1.89-13.87), compared to those with high physical activity and appropriate sleep duration.

MINOR ESSENTIAL REVISIONS

Background
Paragraph 1:
Line 3: The word noncommunicable should be hyphenated.

RE: Change has been made in the revised manuscript.

Line 4
Should read “type 2 diabetes and cardiovascular diseases (CVD)”

RE: Change has been done.

Paragraph 2:
Line 5: “.. interrelated with each other” is redundant.

RE: “with each other” has been deleted.

Paragraph 4:
Line 1: Again the word “Noncommunicable” should be hyphenated.

RE: Change has also been done.

Methods
Study Population Section
Paragraph 1:
Line 3: The word “the” should be inserted between Chinese and population.

RE: Change has been done. But it has been revised as “to examine the association between economic transformation and health/nutrition status of the Chinese population”.

Laboratory Measurements
The last sentence in this paragraph should be re-phrased as the wording is unclear.
Do you mean the “the presence of diabetes was based on a fasting plasma glucose level of ≥7.0 mmol/L according to the latest American Diabetes Association guidelines, as well as an existing physician’s diagnosis of diabetes, with the exception of gestational diabetes”?

RE: As suggested by the reviewer, change has been done as follows:

“The presence of diabetes was based on a fasting plasma glucose level of ≥7.0 mmol/L according to the latest American Diabetes Association guidelines [35], as well as an existing physician’s diagnosis of diabetes, with the exception of gestational diabetes.”
Additional Variables
Sleep duration should be discussed in a separate paragraph.

RE: Sleep duration has been moved to a separate paragraph under its own heading.

Statistical Analysis
Line 9 states that potential confounders were controlled for. Please state which confounders were adjusted for and why these were chosen.

RE: Change has been made following the comments in the revised manuscript (Statistical Analysis, paragraph 2, line 2-6). “Potential confounders such as age, sex, WHR, education, residence, income, smoking, alcohol drinking, dietary intakes, physical activities and hs-CRP were controlled for in the analysis. These variables were chosen according to previous publications and theoretical considerations.”

Line 13:
Modern statistical practice discourages the use of p values and prefers the reporting of confidence intervals, as the latter are far more informative to the reader. I would recommend reporting the confidence intervals over the p values.

RE: Yes, we agree with the reviewer. In Table 3, we reported odds ratios and 95% confidence intervals for insulin resistance. Also, the adjusted OR and 95% CI for insulin resistance were also reported concerning the combination of low physical activity and short sleep duration. Nevertheless, p values are also necessary to indicate statistical significance in some occasions (for example, p value for trend). So we used both of them.

Table 1:
In the titling of the quartiles of HOMA in the table I would exclude the (low) after Q1 and (high) after Q2 as quartiles are neither high nor low. If you mean that the mean HOMA of the first quartile was representative of a low insulin resistance then I would state it in the results.

RE: The (low) after Q1 and (high) after Q4 have been excluded. Similar revisions have been made in the Table 2 and 3.

Discussion
Paragraph 5
The authors state that “Domestic physical activity and transportation activity was also shown beneficial effect for health[8-10, 40]. Current data are also consistent with what they found”. This point should be expanded upon. Why are these data consistent with the studies referenced?
RE: Change has been made following the comments in the revised manuscript (Discussion, paragraph 4, line 2-12).

“Hu et al.[8] found that moderate and high physical activities including commuting physical activity independently and significantly reduced risk of Type 2 diabetes among the middle-aged general population. Again, he found that daily commuting to and from work reduced the risk for total and CVD mortality among patients with type 2 diabetes[9]. Esteghamati[10] reported a significant negative relationship between commuting activity and insulin resistance. Domestic physical activity was also observed having gender-specific effects on health indicators in Europe[42]. Negative associations between transportation/domestic physical activities and insulin resistance, despite of relative weak magnitude, were also observed in current data, which was consistent with what they found to a large degree.”

Paragraph 6
Line 3
The acronym TPA is used but not previously defined. Presumably it means total physical activity.

RE: The acronym “TPA” has been removed due to the previous revision.

Line 4 to 5
Replace “unfeasible” by “not be feasible” as the latter reads better.

RE: “Unfeasible” has been removed due to the previous revision.

Line 7 to 9
Please rephrase this sentence below. Also People should not be capitalized.
“Furthermore, people who were diagnosed with chronic diseases could have purposefully increased leisure time activity or exercise which could not be detected in our study due to cross-sectional nature.”

RE: As suggested by the reviewer, change has been done.

Conclusions
I would rephrase the second and third sentences so that the paragraph flows better, such as:
“In summary, our findings indicate a significant inverse association between physical activity and insulin resistance in this non-diabetic Chinese population. Occupational physical activity, the main component of total physical activity, underlies this association. The combination of low physical activity and short sleep duration was associated with the highest odds of insulin resistance in the study. Advocating various types of physical activity and appropriate sleep duration may help reduce insulin resistance and its adverse consequences”.
RE: Change has been done following the above comment. Thanks!