Author’s response to reviews

Title: Interaction of sleep quality and psychosocial stress on obesity in African Americans: the Cardiovascular Health Epidemiology Study (CHES)

Authors:

Aurelian Bidulescu (abidulescu@msm.edu)
Rebecca Din-Dzietham (rdin@msm.edu)
Dorothy L Coverson (dcoverson@msm.edu)
Zhimin Chen (robchenz@gmail.com)
Yuan X Meng (ymeng@msm.edu)
Sarah G Buxbaum (sarah.g.buxbaum@jsums.edu)
Gary H Gibbons (ggibbons@msm.edu)
Verna L Welch (verna_lamar@hotmail.com)

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Author’s response to reviews: see over
Dear Dr. Le Good,

We are pleased that you have reviewed our manuscript, Interaction of sleep quality and psychosocial stress on obesity in African Americans: the Cardiovascular Health Epidemiology Study (CHES), for publication in BMC Public Health. We thank the four reviewers, the senior scientific editor, and the editorial office for their constructive and helpful comments. We have incorporated the changes suggested and briefly present our responses below.

Please ensure that you include further discussion of the objectives and limitations of your work.

We followed on your suggestion, and enriched further on the limitations of our study design. We also modified the study objectives to better reflect the aims of our work.

We would also request that you address a few editorial concerns, namely:
- Please include a statement on ethical approval and informed consent.

We added the paragraph that was inadvertently omitted previously. We apologize for it.

Referee 1
Dr Helen Skouteris
Reviewer's report

Major Compulsory Revisions

The aim of this study was to examine the association between sleep quality and BMI and determine whether this association is modified by psychosocial stress. The introduction does not provide a clear
rationale for your study nor are the aims clear. There is a whole literature on stress and sleep deprivation, yet this is not outlined or used to create the rationale for this study.

Thank you for pointing out this important aspect of our report. Accordingly, we enriched our introduction and discussion to provide a clearer rationale for our study and for our study aims. We added a couple of seminal articles to underscore the putative bidirectional relationship between stress and sleep deprivation.

The first aim stated is “to test whether sleep quality is inversely correlated with BMI and obesity”. An additional aim, is to “test whether the above putative association is positively modified by psychosocial stress, i.e., people with higher stress will exhibit more obesity than people of lower stress”. You have not tested that. Instead, you have reported that general perceived stress interacts with sleep quality continuous; there appears to be an increased relative risk in the medium stress group compared to the other two groups yet this is not mentioned. Your data suggest that stress may moderate the association between obesity and sleep quality; hence, your initial aim needs to reflect this investigation of these three variables and the inter-relationship between them.

Indeed, our data suggest that there is an increased relative risk in the medium stressed participants compared to the other two groups, and therefore stress may modulate the association between obesity and sleep quality. We followed on your related suggestion above, and modified accordingly our second initial aim as well as the discussion related to our findings to reflect the complex interrelationship between these three variables, sleep, obesity and stress.

You also suggest at the end of your Results section that this interaction may increase the likelihood of obesity; given the cross-sectional nature of this research, causation needs to be treated with caution.

We followed on your advice and modified across the whole text in order to stress the notion of association instead of risk.

How was CVD comorbidity assessed?

CVD comorbidity was self-reported through the administered questionnaire; we additionally specified this aspect within our methods section.

Was no dietary intake information a limitation?

Definitely, no dietary intake information constitutes a limitation of our study; we specifically mentioned it within our discussion section. Thank you for pointing it.

Can your findings be generalized to AA men given the high proportion of women in the study?

It might be argued that we had almost one third of participants that were males, similarly with several observational investigations that are known to incorporate significantly more women than men. Nevertheless, we partially agree and in order to address this issue, we listed it as a potential limitation.
It is surprising that no family income information was collected. The perceived stress may be due to financial stress or food insecurity. Socio-economic disadvantage is a strong predictor of obesity. Obesity combined with food insecurity presents a greater risk for major weight retention in women post the birth of the babies. Some discussion of this link and literature is needed and recommendations for future research in terms of really understanding what type of stress may be contributing to the association between obesity and sleep quality. The other point to consider that other psychological variables, such as depression, are known to be related to both obesity and sleep quality; is it stress or some other psychological factor that is impacting here, given that no other psych variables were measured?

Thank you for your comment and suggestion. Actually, we collected not only family income information but also financial stress. Accordingly, we added a series of analyses that incorporated these important variables. We discussed the results of these analyses and made recommendations for further research.

On the contrary, the psychological variable depression was not available within our investigation. We added it as a limitation of our study.

Level of interest An article of importance in its field
Quality of written English Acceptable
Statistical review Yes, and I have assessed the statistics in my report.
Declaration of competing interests 'I declare that I have no competing interests'

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Referee 2
Prof Bernardo Carpiniello
Reviewer's report

Major Compulsory Revisions

1.Background-Methods
1.1 The Authors fundamentally consider the hypothesis that poor quality sleep and psychosocial stress may be synergic in increasing the risk of obesity; thus, in their study, they consider sleep quality and perceived stress as independent variables and BMI as the dependent one. The hypothesis that obesity may influence both sleep quantity and/or quality and perceived stress was ignored and not explored by authors.

Within the limitations of the cross-sectional design of our investigation, we agree with the suggestion. Accordingly, we added a series of analyses in order to assess the bivariate association of obesity – sleep, obesity – stress and sleep – stress.

1.2 Moreover, the authors do not consider and consequently do not examine in their study the possible role of emotional disorder (in particular depression) as a mediating variable linking sleep disorder, perceived stress and obesity, taking into account the well established relationships of depression with obesity, sleep disorders and stress. This is a very pertinent observation. Unfortunately, as also specified above, we did not collect this psychological variable. We added this fact among our study limitations.
2. Discussion

2.1 The Authors state that “our investigation, similarly with studies such as CARDIA and Whitehall II that consider sleep duration, found no association between sleep quality as a continuous score and obesity”. In our opinion, this statement is somewhat confusing, given that sleep duration is a very different parameter from sleep quality.

We agree, and modified the statement to reflect these distinct measures.

2.2 The Authors consider that “when sleep quality global score is considered continuous, we found an interaction between sleep quality and stress, especially in the middle stress tertile. This association was not present when using the defined PSQI categories”. Unfortunately they don’t comment further these data, attempting to explain in some way these puzzling evidences.

We could point to possible power issues when categorizing a continuous variable, but as we decided to follow one of the reviewers’ suggestions and use the tertiles instead of the predefined PSQI categories this result is no longer relevant.

2.3 In commenting the opposite data regarding the obesity risk according to level of stress (poor sleep versus sleep disorder), The A. suggest that “the fact that sleep disorder versus healthy sleep had an opposite slope of the association with obesity in terms of interaction with stress when compared with poor sleep versus healthy sleep suggest that the sleep disorder category might be a pathologic condition that is produced by a different sleep disturbance mechanisms”. What this means? Which is the different mechanism supposed by the authors?

We initially considered sleep-related respiratory pathological conditions such as obstructive sleep apnea (versus more benign respiratory conditions such as obesity-related poor sleep) but, by presenting those results only centered on the significant interaction, we decided to drop that statement altogether.

2.4 The study found that only two Pittsburg Questionnaire components, namely sleep latency and daytime dysfunction, were interacting with perceived stress on obesity, whereas the global quality score was not. As a consequence they conclude that “an issue of sensitivity of the questionnaire and/or an issue of calibration of its subdomains” is raised by these results. Why the A. exclude the possibility that results may be true and not an expression of limitations of the instrument?

We agree, and modified accordingly our presentation and interpretation of the results.

2.5 The authors acknowledge some limitations of the study, such as the limited physical activity assessment and the marked prevalence of females in their sample, but fail to recognize other limitations, such as the fact that BMI and sleep quality are based only on self-report measures.

We added these limitations to the list; thank you for pointing it out.
2.6 In their final consideration the A. state that “poor sleep quality was associated with BMI and the association of sleep quality with obesity was positively modulated by perceived stress”, which is a somewhat surprising conclusion, taking into account that they state just few lines above that “is premature to suggest that sleep is a cause or solution to the obesity epidemic in absence of more sensitive sleep quality assessment tool”, a sentence that sounds as a criticism to studies based upon instruments such as the Pittsburg Questionnaire, included their own study.

We revised the manuscript to include the results using sleep categorized only using the sleep quality tertiles (and not the Pittsburgh proposed cutpoints) and thus that statement was dropped altogether.

-Minor Essential Revisions

None

- Discretionary Revisions

1. Discussion

The A. say that “noteworthy was the association of both physical activity and smoking with sleep quality..”, but don’t deserve any comment to the significant association they found between female gender, history of MI, and sleep quality, which in our opinion are worth to be considered.

We totally agree, and additionally commented on the significant associations found with female gender and history of MI.

Level of interest An article whose findings are important to those with closely related research interests
Quality of written English Acceptable
Statistical review No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests 'I declare that I have no competing interests'

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Referee 3
Dr José Pedro L. Nunes
Reviewer's report

Major Compulsory Revisions

Concerning the paper entitled “Interaction of sleep quality and psychosocial stress on obesity in African Americans: the Cardiovascular Health Epidemiology Study (CHES)”, by A. Bidulescu et al., this reviewer is of the opinion that it is an interesting report, and I would recommend acceptance after revision.

The major point to be addressed is the following. Obesity is associated to sleep disturbances, especially sleep apnea. Although sleep apnea may be associated to insomnia, a more frequent finding is increased sleepiness. The use of benzodiazepines has been shown in previous reports to be associated to lower values for the apnea/hypopnea index. The authors stated that “Global sleep quality (GSQ) score was computed...
as the sum of response values for the seven components (sleep quality, sleep latency, sleep duration, habitual sleep efficiency, sleep disturbance, use of sleeping medication and daytime dysfunction) of the Pittsburgh Sleep Quality Index (PSQI) scale”, and the GSQ score was used for further analysis.

This reviewer would recommend a separate analysis for individual components of the GSQ score, in relation to obesity, aiming at evaluating a possible relation between obesity and typical patterns for sleep apnea. Sleep latency, sleep duration and use of sleeping medication are parameters of particular interest to be compared in obese versus non-obese patients.

Thank you for this suggestion. We totally agree. Accordingly, we run a whole series of additional analyses in order to separately assess the relationship between obesity and individual components of the GSQ score. They are presented in Tables 2 and 3.

Level of interest An article of importance in its field
Quality of written English Acceptable
Statistical review No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests I have no competing interests.

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Referee 4
Kristen L. Knutson
Reviewer's report
This paper examines the cross-sectional association between sleep quality, stress and BMI in a sample of African-Americans. The question of whether stress mediates the association between sleep and BMI in African Americans is an important one, however, I have some comments that need to be addressed.

Major Compulsory Revisions
- The major limitation to this analysis is the use of self-reported height and weight and a single sleep questionnaire (also self-report). The authors must include this limitation in the discussion.

Thank you for pointing this out; we added it to our limitations.

- The authors alternate between PSQI divided into 3 categories versus tertiles. For example, Table 1 & Table 3 use the categories, but Table 2 uses the tertiles. In the text, they alternate as well, which is confusing. For example, on page 6 the second paragraph under the results that discusses Table 1 states, “sleep quality categorized into tertiles.”, but Table 1 does not use the tertiles. The authors should only use one of these methods (tertiles or categories).

We agree, and consequently decided to use the sleep quality tertiles instead of the predefined Pittsburgh categories (which are still debated in terms of specificity of the cutpoint of 5 for different types of “poor sleep”).

Also, the authors should explain why they are using 3 levels rather than the standard PSQI dichotomy of <=5 versus >5 (see Buysse et al, Psych Res 1989).
We initially decided to use the three-level categories based on the generally validated cutpoint of 5 for “good sleep” and the upper cutpoint of 10 for “chronic sleep disorders” as described in several references, the most recent being the following one: van Mark A, Weiler SW, Schroder M, Otto A et al., The impact of shift work induced chronic circadian disruption on IL-6 and TNF-α immune responses; Journal of Occupational Medicine and Toxicology 2010, 5:18. We finally decided that tertiles using ranking will better reflect the overall distribution of our participants.

Once the authors select which 3-level PSQI variable to use, then Table 1 & 2 can be combined. Finally, if tertiles are used, the authors should state what the PSQI score ranges are within each tertile.

We followed on your suggestion, and combined the two tables into the present Table 1. Within this table, we indicated the PSQI score ranges within each tertile.

- Please add self-reported sleep duration to Table 1 and describe it in the Methods section.

We added sleep duration to our Table 1, and described it within the Methods section.

- In the Introduction & Discussion the authors erroneously state that no association was observed between habitual sleep duration and quality and BMI in the CARDIA study. See Lauderdale et al, AJE, 2009.

We apologize for the statement; the present sentence was modified to reflect the actual findings of that study.

- The authors transition between sleep duration and sleep quality as if they are the same measure (e.g. in the Introduction the first paragraph discusses sleep duration and the second discusses sleep quality). The authors need to be clear about the distinction between these measures in both the introduction & the discussion.

Throughout the text, we clearly made the distinction between these measures. Thank you for your point.

- Table 4 is unnecessary and the information can just be included in the text.

We agree and consequently included the information within the body text.

Minor Essential Revisions
- In the Methods section, state the possible responses for the physical activity question (yes/no).

We clearly mentioned that aspect of our questionnaire.

- In Table 1 there is a mistake under Physical Activity; No for low stress “107 (80.8)”. We made that correction; thank you for pointing it out.

- The results in Table 3 are confusing, specifically what the p value represents. For example, the OR for low stress & sleep quality groups
are not significant based on the confidence intervals, but he p value is .04. The description “p-value of significance of regression coefficients” is unclear. Please edit this table for clarity.

We agree, and decided to present only the significant interaction. As the Table 3 became redundant, we decided to drop it altogether.

- The following sentences in the discussion are unclear: “Somewhat surprising was the finding that among obese participants more had a health sleep, but the number of those with poor sleep was very close” (Page 8, first paragraph) and “Against these caveats are a thorough assessment of psychosocial stress and a complete Pittsburgh sleep questionnaire” (page 10).

We agree and decided to change those two sentences as highlighted in the text.

Discretionary Revisions
- The authors should consider stratifying by two age groups, e.g. 30-49y & 50-65y since other studies have reported stronger associations between sleep & BMI in younger groups (<50y).

We followed on your advice, and stratified by the two suggested age groups.

Level of interest An article whose findings are important to those with closely related research interests
Quality of written English Acceptable
Statistical review Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests I declare that I have no competing interests.

We hope that with these changes now incorporated our manuscript is ready for publication. We are proud to publish in BMC Public Health, a journal actively constructing an international space of high scientific level for scholarly communications.

Sincerely,

Aurelian Bidulescu, MD, PhD, MPH
Assistant Professor
Cardiovascular Health Research Program
Cardiovascular Research Institute
Department of Community Health and Preventive Medicine
Morehouse School of Medicine
720 Westview Drive SW
Atlanta, GA 30310-1495
Tel / Fax: 404-752-1082 / 1042