Author’s response to reviews

Title: Sleep and weight loss in low-income overweight or obese postpartum women

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Author’s response to reviews:

Dear editor and reviewers

We greatly appreciate your comments to improve the quality of the manuscript. In this revision, all the revised texts are in bold.

Reviewer reports:

Ree Meertens (Reviewer 1): General:

COMMENTS

The paper is about a timely subject (relation sleep and (over)weight), is concise, well-written and gives a nice overview of the literature in the introduction. The paper reports about fluctuations in weight (as there is no effect of the intervention), that are related to an improvement in sleep parameters (an improvement that is hard to explain). The authors should try to make clearer in the introduction and discussion that they in fact follow natural occurring fluctuations in sleep and weight in this target group (as there is no effect of the intervention, that not even addresses sleep).
COMMENTS

In the statistical analysis section, the authors promise more analyses than they provide in the Results section.

RESPONSE. We carefully checked the Statistical Analysis and Results sections and confirmed their consistency. The mixed-effects regression analyses were used to derive the P-values in Tables 1 and 2 for the significance of changes across time, adjusting for within-subject clustering from repeated measures. We did not present coefficient or odds ratio estimates from the regression models since the descriptive statistics (Mean±SD and %) in the tables are more straightforward to illustrate the change across time. We have added relevant explanation as footnotes in Tables 1 and 2.

With respect to the relation sleep-weight only the results are presented for women losing more than 5% weight (compared to women who lose less than 5% weight).

RESPONSE. Our aim for this analysis was to examine whether weight change across time was related to sleep. ≥ 5% weight loss was the only weight outcome with significant change across time (T3 vs. T2). We have revised the text to clarify the purpose and presented the rationale in the statistical analysis section.

COMMENTS

Abstract:

-The trial registration seems to be not about 'sleep' at all. How useful is it to refer to a registration that does not refer to the paper's subject?

RESPONSE. The study was registered using the ‘funded study title’, which was required. Therefore, the wording sleep was not appeared on the trial registration.

COMMENTS

Introduction:

-Nice overview of the literature. Maybe only add whether the studies summarized are about participants that are in weight loss interventions or not.

RESPONSE. In this revision, we specified types of prior studies, e.g., prospective longitudinal study and intervention studies.
“A recent meta-analysis that included 11 prospective cohort studies in adults aged 18 to 72 years found that short sleep duration was associated with weight gain [15]. Consistent with results of the meta-analysis, a systematic review that included 3 prospective cohort studies and 1 cross-sectional study reported that short sleep duration was associated with postpartum weight retention [16]. Also, three other prospective cohort studies consistently reported the association between short sleep duration and weight gain in adults [17-19]. Given the growing evidence showing that short sleep duration is a potential risk factor for weight gain in adults, it seems logical to examine the association between sleep duration and weight loss. To date, no prospective cohort studies but two lifestyle behavior weight loss intervention studies for middle-aged overweight or obese adults have examined such association.”

Background. Page: 4, line:93.

…” only three weight loss intervention studies for middle-aged overweight or obese adults have investigated the association between sleep quality and weight loss. These studies have yielded mixed results [20-22].”

COMMENTS

Methods:
- Intervention is not about 'Sleep' at all. Please discuss in the discussion section why sleep in improved over time? Influence of season? The child growing older and sleeping better (and so the mother)?

RESPONSE. Revised. Discussion. Pages 9-10, lines: 209-216

“..the overall improvement in sleep might have been impacted by our intervention that included stress management. A recent study of pregnant women has reported that higher levels of stress were associated with poor sleep [29]. In our intervention study, we found that the intervention group had a significant reduction in stress at T2 and the positive change remained at T3.
However, our results also revealed that the comparison group had a significant reduction in stress at T3 [30]. Finally, it is possible that as children grow older, they might sleep better and require less attention from their caregivers at night.”

COMMENTS

Results:

-In the methods (Statistical analyses) the authors promise several regression analyses, which I cannot find in the results section.

RESPONSE. Please see our response on the same comment above.

Why a focus on more than 5% weight loss? I do not think it is an argument that this changes significantly over time, as this change must be compensated by a group of women who gain more weight over time (as the means of ‘weight’ do not change significantly over time). Analyze relations sleep with weight (loss) also in other ways.

RESPONSE. The purpose was to examine whether weight change across time was related to sleep. ≥ 5% weight loss was the only weight outcome with significant change across time (T3 vs T2). We have revised the text to clarify the purpose and presented the rationale in the statistical analysis section. The rationale for using 5% weight loss was revised.

Background. Page: 3 Lines: 71-74

“Evidence is clear that a loss of ≥ 5% of weight reduces risk of cardiovascular diseases (e.g., by improving glucose, insulin, triglyceride [12, 13], systolic and diastolic blood pressure, HDL cholesterol) [13] and cancer mortality [14]. Therefore, it is critically important to help young adults lose weight.”

COMMENTS

Discussion:

-See before: try to explain why sleep improves over time.

RESPONSE. Please see our response on the same comment above.
Elizabeth Anne Cayanan, Ph.D. (Reviewer 2): Review: Sleep and weight loss in low-income women with overweight or obesity Corresponding Author: Mei-Wei Chang

Thank you for this manuscript examining the association between weight loss and sleep duration, quality and disturbance in a cohort of young, low income post-partum women. This is an important area of research that lacks high quality, longitudinal evidence. The current study presents a secondary analysis of pooled participants in a community based lifestyle behavioural weight management intervention whereby the control and intervention group did not significantly differ in their weight or sleep parameters during the trial.

COMMENTS

The authors state the basis of this analysis was that short sleep duration seems to be associated with increased weight, therefore it should be confirmed whether weight loss impacts upon sleep duration.

RESPONSE. In the original submission, we presented the association between sleep duration and weight loss in both text and Table 3. There was no association between these two variables.

COMMENTS

It is curious as to why this secondary analysis was conducted as the authors of the current paper have stated that the mean weight loss during the trial was not significant and therefore examined the changes in sleep in the entire cohort where there was no substantial weight loss achieved. This does not align with the basis of their analysis. RESPONSE. We have previously published results of intervention effect on mean weight loss (primary outcome). This means that we compared the differences in mean weight loss between the intervention and comparison groups and did not find the difference in mean body weight at T2 and T3 between groups. The no difference between two groups does not necessary translate to no mean weight change between T2 and T3 as a cohort (combining intervention and comparison groups). Also, it does not translate to ≥ 5% versus < 5% weight loss.

They measured significant changes at each timepoint in sleep duration, quality and disturbance but the lack of association with change in weight may have been largely due to the participants not losing clinically or statistically significant weight during the trial.

RESPONSE. Please see our response on the same comment above (reviewer #1).
In lieu of this, it seems the hypothesis being tested was not valid as weight loss was not achieved and therefore it cannot be determined whether it impacted on sleep duration/disturbance/quality.

In addressing this limitation, the authors dichotomised the sample by weight loss responders (>5% and <5%) however this dramatically altered the number of participants in each group and was very likely underpowered. RESPONSE. In our original submission, we acknowledged that this present study could be possibly underpower.

Nonetheless, this analysis produced no significant results and there did not seem to be any association between weight loss and these parameters of sleep. The academic writing of this manuscript requires revision, whereby the interpretation of some statements is challenging due to sentence structure. It would be helpful for the authors to ensure there are explicit statements regarding their findings and approach to this analysis to aid the reader in their interpretation of meaning.

Careful revision of this manuscript may allow clearer meaning to be deciphered. I have provided extensive constructive suggestions to aid the authors in their review if this is of benefit to them.

RESPONSE. This paragraph appears to be a summary statement of review. We have addressed this reviewer’s comments (see below).

COMMENTS

Abstract:

Line 42- "Body weight was measured in person" is awkward. Suggest revision eg was measured directly or participants were weighed and > 5% etc


“All participants were assessed and weighed at baseline…”

COMMENTS

The results are confusing in lines 46-47: "Also, a significantly higher proportion of women lost > 5% of body weight over time" compared to what? Revise wording. Are you trying to say that each time period produced a greater amount of body weight? Eg the longer enrolled, the greater the weight loss in subsequent periods? Consider revision.
‘However, a significantly higher proportion of women lost ≥ 5% of body weight at T3 (23.1%) than T2 (12.5%, p = 0.001). “

COMMENTS

Line 48 perhaps should be stated earlier? The weight loss achieved during the study was not significant despite women losing a greater volume of weight as time progressed?

Also, the numbers quoted are confusing, are you suggesting that loss of 190.9 lbs was the weight measured or the change in weight? These large numbers should be described as mean weight per timepoint rather than weight change so as not to be misleading.

You have stated that >5% body weight loss was considered meaningful and reported on proportions of women who lost weight and yet stated there was no significant body weight change over time. It seems strange that 5% was achieved when only 2.9 pounds seems to be lost between T1 and T3 as per the results.

“There were no significant mean body weight changes at T2 and T3. However, a significantly higher proportion of women lost ≥ 5% of body weight at T3 (23.1%) than T2 (12.5%, p = 0.001).”

COMMENTS

Line 49: remove the word also and state "were not significantly associated with …"

“Sleep duration, quality, and disturbance were not significantly associated with ≥ 5% of weight loss.”
The conclusion states that there was no improvement in sleep outcomes with >5% weight loss but stated that there was no significant change in body weight over time. If weight didn't change, then why would these outcomes change?

RESPONSE. In the original submission, we only stated that losing ≥ 5% of initial body weight (at baseline) was not associated with improvement in sleep duration, sleep quality, or sleep disturbance in low-income postpartum women with overweight or obesity.

Why are you suggesting that prospective studies further investigate a trend that is not existing. Perhaps be more specific, studies that achieve a greater or significant volume of weight loss should investigate the impact upon sleep duration, quality and sleep disturbance.

RESPONSE. Revised. Conclusion. Page: 11  lines: 245-247

“The study findings contribute to scientific knowledge in the association between sleep and weight loss in a group at a high risk for adverse health outcomes.”

Background

Line 61: revise to "who are low income earners"

RESPONSE. We kept the term ‘low-income,” which is commonly used in the literature.

“Approximately 50% of young, low-income women are overweight or obese prior to becoming pregnant”
COMMENTS

Line 64: no need to include the definition in brackets - it is a common descriptor

RESPONSE. The definition of excessive gestational weight gain is needed so readers would understand what we meant by excessive gestational weight gain.

COMMENTS

Lines 71-74: revise sentence structure and grammar

RESPONSE. Background. Page: 3, lines: 71-74

“Evidence is clear that a loss of $\geq 5\%$ of weight reduces risk of cardiovascular diseases (e.g., by improving glucose, insulin, triglyceride [12, 13], systolic and diastolic blood pressure, HDL cholesterol) [13] and cancer mortality [14]. Therefore, it is critically important to help young adults lose weight.

COMMENTS

Line 78: reference these studies or merge with next sentence

RESPONSE. Please see our response on the same/similar comment above (Reviewer #1).

COMMENTS

Line 84 - 86: Why would this contradict. This may be more relevant in discussion or you should provide an explanation for the contradiction

RESPONSE. We removed the study that included midlife women because it was not a longitudinal study.

COMMENTS

Line 86: Tense needs revision

RESPONSE. Revised. Background. Page:4, line: 85
“… duration is a potential risk factor for weight gain in adults”

COMMENTS
Line 92: you are moving around a lot in describing multiple populations. I think it is important to denote the group you are interested in/studying - you have established mothers are at risk directly as a result of the maternity period and yet all the evidence for the associations with weight and sleep are in men or in general obesity. It would be helpful to be more explicit in your interpretation of this.

RESPONSE. Ideally, we would prefer include prior longitudinal studies of postpartum women and/or low-income postpartum women. However, this was not feasible, because there were no studies focused on postpartum women. In this revision, we removed the study of men only.

COMMENTS
100: revise punctuation, this should not be a colon
RESPONSE. Revised.

COMMENTS
Perhaps line 99 on is easier to state that 2 longitudinal studies examining the association between sleep disturbance and weight loss have fund no association.

“In terms of the association between sleep disturbance and weight loss, one weight loss intervention study for middle-aged obese women has reported no association [23]. This non-significant finding was supported by a 30-year prospective cohort study [24].”

COMMENTS
Line 103: who was the sample - 30 years is a long time, how long was their weight loss intervention?
RESPONSE. We clarified that it was a prospective cohort study (see response above)

COMMENTS
Lines 105-111 are much clearer, this is easier to read and understand. It is important to be more explicit in earlier parts of the introduction.
RESPONSE. Thank you

COMMENTS
Methods
Line 113: WIC needs to be defined before the acronym used

“Participants were recruited from the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)”

COMMENTS
Line 119-120: revise, Height and weight were measured during recruitment and used to calculate BMI
RESPONSE. Revised per suggestion. Method. Page: 5, lines: 118-119
“Height and weight were measured during recruitment and were used to calculate BMI.”

COMMENTS
127: name the intervention (eg weight loss intervention)

“…community-based lifestyle behavior intervention study aimed at prevention of weight gain.”
You should state the goal of the program/intervention early in the methods.

RESPONSE. Please see the response above.

Line 140 - 141: this would potentially have impacted upon your findings as the program promoted lifestyle changes that may have not been potent enough for weight loss but still had an impact on other aspects of health and behavior in comparison to the control.

RESPONSE. Please see our response on the same/similar comment above (Reviewer #1).

Line 172 - do you mean significant change at each timepoint or just at the beginning and end of the trial?

RESPONSE. At each time point.

Line 173 - compare (tense incorrect)

RESPONSE. Typo corrected.

Results

Line 189: please be clear in what you mean by "trend" - it is presumed this is not significant. But I think you must also consider that this trend is in a magnitude of weight loss that was not clinically relevant.

RESPONSE. In the original submission, we stated “Although there were no significant changes in mean body weight across the 3 time points, there was a trend of decreasing mean body weight over time.”
“Table 3 presents results of comparison between women with < 5% and those with ≥ 5% weight loss”

Discussion

COMMENTS
Line 200: You should explain why the cut off of >5% weight loss was selected and mention its clinical relevance

RESPONSE. Please see our response on the same/similar comment above

COMMENTS
Line 206 : didn't you just make this statement above?

RESPONSE. Removed.

COMMENTS
Line 207 - reference 21 is not recent, it is 13 years old. Revise wording. This sample tested people who had achieved 10% weight loss and had a very small sample n= 10 so again I would be considering whether it is comparable to your analysis.
RESPONSE. Removed.

COMMENTS
Line 214: This reference (Foster et al) measured sleep disordered breathing with PSG but did not mention sleep duration in their publication. Please revise the use of this reference as it is a different tool and cohort to the sample you are considering.

RESPONSE. Removed.

COMMENTS
Line 221: It is not clear why you are describing a behavioural/psychological drive in eating; you are studying a cohort in which these drives are presumed relevant to all of the sample or if not, should have been measured as a confounder using a tool such as the three factor eating questionnaire. If you are suggesting this as a drive of inconsistent findings, you need to also state why these factors would be relevant to each of the cohorts tested in your comparative studies. Try to be explicit in why this is important if you are going to suggest it as a mediating factor.

RESPONSE. Removed.

COMMENTS
Line 226: revise tense "examined"

RESPONSE. Revised.

COMMENTS
Line 229: Please ensure the methods are clear that you have not used the entire tool as intended. By using a sample of items from the PSQI, the validity of the tool is questionable as it has not been used as it was intended. If validated for this sample of questions, you should state that and reference. Otherwise it is a limitation and you should not claim it is validated for this purpose.

RESPONSE. We carefully reviewed the reference (please see below). This survey has 7 subscales. Each subscale and the PSQI global score have discriminate validity and reliability
(internal consistency and test-retest reliability). We also revised the statement to reflect the reviewer’s comment. Method. Page:7, line: 150-151

“We used subscales from the Pittsburgh Sleep Quality Index, which has established validity and reliability for each subscale.”

COMMENTS
Line 233: these should be stated as strengths

RESPONSE. Remove the wording “limitation.”

COMMENTS
Line 235: revise - not valid if you did not use the tool as designed

RESPONSE. Please see response above regarding the PSQI survey.

COMMENTS
Line 253-255: requires revision- aren't you suggesting the reciprocal? Weight loss does not have an effect on sleep.

RESPONSE. We examined the association and did not suggest the reciprocal.

COMMENTS
It should be mentioned that this longitudinal study only covered a period of seven months which is considered short in weight loss intervention.

RESPONSE. In the original, we stated that the final data collection took place seven months from baseline (in background section). We also stated that our intervention lasted 4 months.
COMMENTS

The lack of clinically or statistically significant weight loss may have blunted the impact upon sleep quality, duration and disturbance. This should be discussed early in the discussion.

RESPONSE. Please see our response on the same/similar comment above (Reviewer #1).