Author's response to reviews

Title: Interrelationships among sedentary time, short sleep, and the metabolic syndrome in adults

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Author's response to reviews:

REFEREE #1

Major Compulsory Revisions

Comment: Clarification is needed to distinguish between the current study and that of previous work (i.e. Buman MP, Winkler EA, Kurka JM, et al. (2014). Reallocating time to sleep, sedentary behaviors, or active behaviors: associations with cardiovascular disease risk biomarkers, NHANES 2005-2006. American journal of epidemiology). What are the distinctions in the methodology and findings of the current analysis that are complimentary to such recent work?

Response: Thank you for bringing the study by Buman et al. to our attention. That study was published right around the same time we were finishing writing our paper and it therefore was not captured in our literature review.

The paper by Buman looked at whether replacing sedentary time with sleep would have a positive impact on cardiometabolic risk factors such as insulin resistance. Our study is distinct from Buman’s study in three main ways. First, Buman used the self-reported data from the questionnaire to assess sleep duration whereas we used a more objective proxy measure based on the accelerometer data. Research has suggested that people have difficulties assessing their own sleep and report biased estimates (see references 17 and 18 in the revised manuscript), and we therefore feel that our study has improved upon Buman’s methods. Second, our paper also examined the association between sedentary time and sleep duration, which on its own makes an important contribution to the literature given the lack of adult-based literature examining this association. Third, our study examined whether there was an interaction effect of sedentary time and sleep duration on the metabolic syndrome, which to our knowledge has not been addressed before in the published literature. In summary, while there is some overlap, we feel that our paper is distinct and different from the work of Buman et al. and other publications in this topic area and that it makes a unique contribution to the literature.

To better highlight how our paper differs from Buman’s work and the unique
contributions our paper makes to the literature, we have changed the title of the manuscript, we have completely re-written the Background section of the manuscript and the abstract, and we have made several changes throughout the discussion section of the manuscript.

Comment: Line 120: While only those who had #4 valid days were included in the analysis, it is not clear on which days the assessment period fell (i.e. weekend vs weekdays). Could this could be an important factor to consider given the distribution in sedentary, physical activity and sleep time at work and in leisure-time are likely to vary? To allow for this, perhaps only participants with #10 hours of wear time per day for a minimum of four days, including at least one weekend day, should be included in the analysis?

Response: As indicated in the Methods (lines 115-116), we used previously established criteria for cleaning the accelerometer data, including the 4 valid days criteria. This approach is entirely consistent with several previous analyses of the NHANES accelerometer data (eg, Troiano PR et al., Med Sci Sport Exerc 2008; Buman MP et al., Am J Epidemiol 2013; Janssen I and Ross R, Int J Epidem 2012; Matthews CE et al., Am J Epidemiol 2008; etc.). In our experience with the NHANES and other accelerometer datasets, in adult populations it makes little difference as to whether a criteria of ‘any 4 valid days’ or ‘4 valid days including at least one weekend day’ is used.

Comment: Covariates: Given their potential effects on the cardiometabolic outcomes, was medication data available and considered? Such as lipid-lowering, anti-hypertensive, and aspirin medications (if available).

Response: As indicated in the Methods (lines 169-174), medication use was considered when defining the metabolic syndrome outcome. For instance, if someone was taking hypertensive medications they were considered to have the high blood pressure component of the metabolic syndrome irrespective of what their blood pressure measures were. This approach is entirely consistent with the criteria that are used to measure and define the metabolic syndrome (refer to reference 25 in the revised manuscript).

Comment: Line 305: While accelerometers have many advantages over other measures of physical activity and are highlighted as a strength in this study – their limitations should be acknowledged (e.g. accelerometers do not capture all activities, such as swimming, cycling or load-bearing activities, sleep). Additionally, accelerometers require several decisions regarding data reduction procedures (e.g., non-wear time threshold, cut-points) and there is currently a lack of consensus in the literature on the most optimal procedures to use. Furthermore, cut-points, although useful for summarizing the data, underutilise the wealth of information that is captured by accelerometers. Particularly in this case, the impact of sleep is more complex than simply duration differences. It is also likely that sleep will be overestimated in the current context.

Response: To address these comments, we have modified and added to the limitations paragraph. This paragraph now includes the following “The sleep
duration measure, while objective, was still a proxy measure and it is likely that sleep duration was overestimated with this proxy measure. Finally, the accelerometers used in NHANES did not accurately capture all activities (e.g., swimming, cycling, load-bearing activities) and there is a lack of consensus in the literature around the optimal accelerometer data reduction and cleaning procedures."

Discretionary Revisions

Comment: While not a focus of the paper, were any gender interactions identified? – given the gender differences that have been previously reported in sedentary behaviour research with cardiometabolic outcome measurements.

Response: We did not identify any gender interactions in our early analyses, and therefore for the sake of having a more efficient presentation of the results we decided to combine the men and women.

Comment: Line 215: add space to ‘regressionwas’

Response: This typo has been corrected.

Comment: Line 233: add space to ‘TVand’

Response: This typo has been corrected.

Comment: It seems important to differentiate sedentary behaviour from sedentary ‘time’ with reference to the current study’s objective and screen based activities. Objective assessment of activity level, while more precise, is not able to differentiate the behavioural context.

Response: This is a good point. We have made this distinction throughout the revised manuscript.

Comment: Non adjustment for obstructive sleep apnoea is a limitation. However, I note that this is acknowledged by the authors. Assumedly there is no information available to account for this? Perhaps that should be stated if so.

Response: This has now been acknowledged in the limitations paragraph (line 309-310).

REFEREE #2

Comment: I suggest that the authors revise their manuscript carefully and resubmit before it can be further considered. There is considerable overlap, in both research question and data utilised, with an analysis recently published by Buman et al (AJE, 2014). At the very least, the authors need to rewrite their paper and make explicitly clear how their work is differentiated from, and builds upon, the findings from Buman et al. If they can make a case for their carefully, they should resubmit.

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That study was published right around the same time we were finishing writing our paper and it therefore was not captured in our literature review.

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