Author's response to reviews

Title: Workplace Standing Time and the Incidence of Obesity and Type 2 Diabetes: A Longitudinal Study in Adults

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Version: 2 Date: 16 December 2014

Author's response to reviews: see over
Re: Revision of the manuscript entitled “Workplace standing time and the incidence of obesity and type 2 diabetes: a longitudinal study” (MS: 1924060351438233)

Dear Dr. Munir,

We thank you for the opportunity to revise our article and also thank the reviewers for their interesting suggestions related to the above referenced manuscript. We concur with the majority of comments and recommendations raised and feel that we can satisfactorily respond to all of them in a revised version of the paper. We thus resubmit the article along with a cover letter that describes the modifications we performed to take into account all of the points.

Thank you again for the attention that will be given to this contribution.

Best regards,

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RESPONSE TO REVIEWERS’ COMMENTS

General note: Modifications that have been made to the article as a result of the reviewers’ comments have been highlighted to facilitate their identification.

Reviewers’ Comments to Authors:

Reviewer 1 (John Buckley)

As the main concern is with comment category 3, at this point I will only focus here and specific to Table 2, which is the main underpinning analysis. Although the trend data aims to add precision to the baseline-follow-up measures, it is difficult to see that even after multivariate correction that the trend loses significance? It’s obvious that in the Overweight/Obesity measures that following multivariate corrections that the delta odds ratio between the Rarely Never Standing group to the All the time Standing Group had been cut from a difference of ~30% (0.94 to 0.63) to ~11% (0.97-0.85), yet the confidence intervals in the latter still do not include or cross zero, and the width of these intervals is very similar to the uncorrected values. This suggests that the 11% change in the odds ratio is still significant. This would seem to hold true for the Impaired Blood Glucose tolerance measures too where the delta-OR is 5% yet the confidence intervals represent a significant change. Furthermore, it would be helpful to report the actual p-values, independent to whether they are above or below the alpha of 0.05.

We thank the reviewer for this comment. We have added the NS p values to our Table 2 and reported the delta ORs in the revised version of the manuscript (see p.9). However, all confidence intervals (in both the unadjusted and adjusted models) cross 1.00 (reference), suggesting that ORs are NOT individually significant but the trend is significant only in the age- and sex-adjusted models. We think there was a misunderstanding on the part of the reviewer because he said that confidence intervals should cross zero to be NS, which is not the case (it should cross 1.00 to be NS).

The paper is about the specific links with Obesity and Diabetes, but then the authors expand the conclusions to “Chronic Diseases” as per this statement: Our results suggest that standing time alone is not sufficient to prevent the incidence of chronic diseases. The authors would be better to stick with relevance to the measures analysed and state: …to prevent the incidence of obesity and Type2 diabetes, which has implications for the risk of other chronic diseases.

We agree with the reviewer and have modified our conclusion to be more specific (see p.10 and 13).

Reviewer 2 (Nyssa Hadgraft)

This is an interesting and well-written paper that seeks to investigate the association between standing time in the workplace and the incidence of obesity and type 2 diabetes in a Canadian population. There is increasing interest in interventions aimed at reducing sitting time in the workplace, many of which involve replacing sitting with standing. In this context, there is a need
for greater evidence on the health effects of standing – which this study seeks to address. There are a few areas which the authors should attend to, in order to improve their manuscript.

Major compulsory revisions

1. The main aim of this paper is to investigate the association between standing time at baseline and incidence of obesity/overweight and T2D/IGT over a follow-up period. However, the authors reference a sample size of n=293 throughout, including Table 2 and Figure 1, which appears to include participants who have these chronic diseases at baseline. Could you please explicitly state (i.e. in the statistical analysis and results sections) how many participants were included in each of the logistic regression analyses? For example, if only those with normal weight at baseline were included in the logistic regression analysis investigating the incidence of overweight/obesity, this would reduce the n for this analysis substantially.

   We thank the reviewer for this comment. We have added this information in the revised version of the paper (see p.8-9).

Minor essential revisions

2. In the ‘Covariates’ section of the methods section, it is stated that “These 6 covariates were chosen because of their association with the exposure and outcomes”. Could you please indicate if this is based on previous published research, and if so, reference relevant studies?

   We have added this information in the revised version of the paper (see p.7).

3. In the results section, it is stated that “total annual family income and submaximal working capacity were the main effect modifiers of the relationship (data not shown)”. This statement suggests that these variables are confounders, not effect modifiers, particularly in the context of the previous sentence which notes that associations become non-significant after adjusting for these and other variables.

   We agree with the reviewer and have therefore modified the sentence in the revised version of the article (see p.9).

4. Discussion (page 11): It is noted that the definition of standing time may include walking or other movement. This appears to be a limitation of the measurement tool as the study specifically aimed to investigate standing, rather than other movement. It would be worth referencing this in the limitation section. In this paragraph there is also some discussion of the benefits of light-intensity physical activity other than standing. As standing is also generally considered to be light intensity PA, the language in this paragraph should be altered slightly to reflect this.

   We have added this information in the revised version (see p.11-12).

5. Discussion (page 11). It is asserted that “The results of the present study emphasize the potential health importance of such light intensity activities”. As the findings of this study were
found to be non-significant after adjusting for all covariates, this statement appears to be overstepping the results.

As suggested by the reviewer, we have decided to remove this statement from the article (see p.11).

Discretionary Revisions

6. It would be helpful to include a table of results for the analyses investigating the associations between change in standing time and the outcome variables, in addition to the figure. Particularly as Figure 2A does not appear to show a linear trend across the categories. It would also be beneficial to provide greater detail of how these analyses were conducted in the statistical analysis section – e.g. were logistic regression analyses conducted in addition to the chi-squared test?

We thank the reviewer for this suggestion. However, we think that adding a table of results will reduce clarity in the manuscript while not providing crucial information to the readers. As mentioned in the statistical analysis section, only chi-squared tests were used for this analysis.

7. Consider including the results of the statistical tests assessing associations between standing time/changing in standing time and the outcome variables with Figures 1 & 2. This would aid comprehension as standalone figures.

See above comment. P values of the chi-squared tests have been reported in the figure legends.

8. As participants were included who had OW/OB or IGT/T2D at baseline, have the authors considered conducting a cross-sectional analysis of the association between standing time and these outcomes at baseline?

Yes, we considered conducting a cross-sectional analysis but decided to go with the more robust analytical study design, i.e. the longitudinal analysis.

9. Discussion – the authors provide detailed discussion of a few studies (e.g. Buckley et al.; Thorp et al.) which found beneficial associations between standing and health outcomes. This level of detail may be more suited to the introduction, as would the description of the proposed biological mechanisms. This would strengthen the rationale for the present study and the discussion section could refer to these findings in a more general manner.

We thank the reviewer for this suggestion. There are many different ways to deal with this issue and many journals nowadays prefer to have very short introduction sections (typically 1 to 1½ page max). We decided to use this approach for this paper and believe that our rationale is adequate (this is the first study to look at workplace standing time and the incidence of obesity and type 2 diabetes).