Author's response to reviews

Title: Associations between psychological stress, eating, physical activity, sedentary behaviours and body weight among women: a longitudinal study

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Version: 3 Date: 25 June 2013

Author's response to reviews: see over
Reviewer comments and author responses for BMC Public Health

MS: 1752151208345668

Associations between psychological stress, eating, physical activity, sedentary behaviours and body weight among women: a longitudinal study

Reviewer #1 (Comments to the author):

Minor essential revisions

1. Overall comment: This paper is well written and was a pleasure to read. The background section concisely outlines the relevant literature and provides a clear rationale for the study. The method is generally well detailed. My comments relate to making the methods and results sections clearer.

   Please see responses to specific queries below.

2. Abstract: Results: Line 1: “was the higher BMI” - ? At baseline? F’up? or Both? please clarify.

   We have amended the abstract to read “Higher perceived stress in women was associated with a higher BMI at baseline and follow-up, and to increased odds of being obese in cross-sectional and longitudinal analyses.”

3. Background: page 4: Line 5: “sedentariness” – is this a word?!

   We have amended this to ‘sedentary behaviours’.

4. Methods: Sample: page 5: Line 1: you mostly use the term “neighbourhoods” but then switch to (line 6) “suburb” – I suggest you stick to the one term.

   The term “neighbourhood(s)” has now been used throughout.

5. Methods: Sample: page 5: Line 6: fix wording- suggest reword to "all those who were eligible were invited to participate'.

   This correction has been made as requested.


   This term has been deleted. Please refer to comment 6b for further details.

6b. how did you get their mobile phone contact details – you just had electoral roll.

   We apologise for this oversight. Yes, at the baseline (T1) mailout we only had electoral roll details and hence have now deleted reference to final call/sms/email reminders.

7. Methods: Sample: page 5: Line 19: word missing "denoted “as” females..."

   This correction has been made as requested.
8. Methods: Sample: page 6: Line 2: did they consent to follow up at the time of completing their baseline survey? If so, please specify.

Page 6: Line 2 has been amended to clarify that the participants consented to follow-up (T2) at the time of completing their baseline (T1) survey. The sentence now reads, “Three years following the T1 survey, all participants who consented to further follow-up in their T1 survey and remained in a READI neighbourhood (n=2850) were sent a T2 survey, which repeated most of the questions in the T1 survey.”


The term “remained eligible” has been changed to “remained in a READI neighbourhood” for further clarification. We have also reported how many women this affected by adding the sentence “Fifty-one women were excluded as they moved out of a READI neighbourhood.”

10. Methods: Weight and BMI: Page 6: Line 3: “as not overweight/obese (15.79 –24.99kg m-2)”– This is not WHO terminology as per the reference, nor is it one recognised category.

BMI categories have been amended to follow WHO terminology as per the reference.

11. Methods: Weight and BMI: Page 6: Line 5 “the underweight category” – not specified. But should be <18.5kg m-2. this section on BMI needs reworking.

We have added the underweight category (BMI<18.5kg m²) and have reworked this section as requested.

12. Measures: Dietary Intake: Page 7: (overall) You haven’t measured dietary intake, you have actually measured food habits. Edit terminology throughout.

The term “dietary intake” has been replaced with the term “food habits” throughout.


Measures: Food Habits: Page 7: Line 2 now reads “and they are likely important”.

14. Measures: Dietary Intake: Page 7: (overall) So they completed the entire FFQ but you only analysed 6 food items? The FFQ tool has not been validated to be dissected in this way and this should be noted in the limitations.

We analysed 6 food items that were selected based on their high energy/low nutrient content and they are likely important contributors to high-energy intake and obesity risk. We have added several references to justify our approach. We have also added a comment on this issue in the limitations section.

15. Results: Page 10: bottom line: ‘stress was not predictive of being “in the overweight “(insert) BMI category, but” higher stress levels (remove ‘however’ and join sentences.)

This correction has been made as requested.
17. Results: Page 11: Line 10: p=0.011) add to the end of the sentence, “but there were no other dietary associations.”

This correction has been made as requested.

18. Discussion: Page 11: Line 6 of discussion section: “overweight or obese” – Be careful not to imply a longitudinal relationship with overweight.

We have deleted the term “overweight”. Discussion: Page 11: Line 4 now reads “The present study found that higher stress in women was associated with increased odds of having a higher BMI, and of being obese.

19. Discussion: Page 12: Line 26: “the present study....measures of dietary intake.” This is likely to be due to the limitations in your tool, and you measured six food habits, not dietary intake.

The term “dietary intake” has replaced with the term “food habits” throughout. Please see our response to comment 14 for further details.

20. Discussion: Page 13: Line 3: when comparing to other studies, state what methods these studies used to measure dietary intake.

The methods these studies used to measure dietary intake have been added to Discussion: Page 13: Line 5.


The term “frazzled” has been deleted from Discussion: Page 13: Line 8.

22. Discussion: Page 13: Line 19: Indeed. The longitudinal analysis was based on just over 10% of your original sample. You should discuss what this bias may mean for your results.

We have added the following text to Discussion: Page 14: Line 3 – “For example, longitudinal analyses in this study were based on a sample of which approximately 10% were originally sampled. Since we have no information on weight status or stress from non-respondents to the initial mailout, we cannot conclude how this bias may affect results. However, such response and attrition are not atypical for this population”

23. Discussion: Page 13: Line 26: I suggest adding a comment to the end along the lines of "Particularly, the role of PA in reducing stress (and weight) could be emphasised in specifically targeted programs'.

The comment has been added to Discussion: Page 14: Line 19.

24. Discussion: In the introduction, you raised the issue of direction of causality in the stress obesity relationship. But as far as I can tell, you only examined stress as a predictor of an obesity outcome, not the reverse. Perhaps justify that decision to test in that direction more strongly in your aim.

We have modified our Introduction to better justify the focus on the temporal associations tested (i.e. stress predicting obesity-related outcomes), and also included a statement in the Discussion
about the possibility that these associations operate in the reverse direction, which while outside of the scope of this study, is an important hypothesis to be tested in future research.

25. Ref no. 18 appears to have an error in the 4th author

Reference 18 only has three authors. We believe the reviewer was referring to reference 28. VV©zina has now been amended to Vézina.
Reviewer #2 (Comments to the author):

Thank you for asking me to review this manuscript, which I read with interest. The paper describes the relationship of psychological stress with body weight and weight-related behaviours (diet, physical activity and sedentary behaviour) in 1382 women recruited from disadvantaged communities in Victoria, Australia who were participants in the READI study. The study advances our understanding of the relationship between psychological stress and overweight and obesity in disadvantaged women. Most previous studies exploring the relationship between stress and body weight have not assessed the role of health-related behaviours. In this study, the authors found that higher stress was significantly associated with overweight and obesity, and with lower levels of physical activity, greater sedentary behaviour and more frequent fast food consumption. These findings are important in public health terms particularly with the study's focus on women from disadvantaged background who we know have higher prevalence of obesity than more advantaged groups, and given the clustering of unhealthy behaviours in disadvantaged groups as demonstrated in recent review by the King's Fund. The study has a number of strengths: it is based on a large sample of 1382 women randomly sampled from disadvantaged communities and analyses are based on longitudinal data over a three year period and are adjusted for important confounding factors. The methods are appropriate and well described. Statistical methods are appropriate. The study is not without its weaknesses, however, and the sample of 1382 women represent 28% of the READI baseline study population, thus limiting the external validity of findings. The other main weakness is that the primary outcome, body mass index, is based on self-reported height and weight, and so the prevalence of overweight and obesity are likely to have been underestimated. 1 The manuscript is well written and structured. I would suggest some discretionary revisions and one or two minor essential revisions:

Please see responses to specific queries below.

Discretionary revisions

1. The results section of the abstract does not describe the association between stress and television viewing time clearly. The direction of association is not stated.

We have amended this to read “increased television viewing time”.

2. While analyses are adjusted for important confounding factors (age, education, marital status, smoking, number of children and country of birth) it would be interesting to know what the effect of employment status on the associations of interest given.

All models had adjusted for employment status. We have added “employment status” to Statistical Methods: Page 8: Line 16. “All analyses controlled for age, education level, marital status, employment status, smoking status, the number of dependent children and country of birth, all assessed at baseline.” We also added “employment status” to the footnotes for Table 1 and 2.

3. Interpretation of findings focuses on the possible influence of stress in predicting diet, physical activity and sedentary behaviour. This is entirely justified but I wonder whether the authors have considered reverse causation. Given what is known about the benefit of diet and physical activity for wellbeing, I wondered whether lack of physical activity and poor diet might predict stress.

See also response to Reviewer 1, Comment 24. Here the aim was to determine whether perceived stress was associated with BMI, food habits and sedentary behaviours. We do acknowledge that
relationships could operate in the reverse direction, but full analyses of all associations in the opposite direction would substantially increase the word and table count beyond that appropriate for a single paper. We have hence modified our Introduction to more strongly justify the direction of effects we have tested, and also added a sentence to the Discussion: Page 14: Line 7 as follows:

“It should also be acknowledged that associations between stress and weight could operate in the reverse direction to that tested in the present study. That is, weight gain and obesity may lead to increased stress, for instance due to weight-related stigmatization or poor physical or mental health associated with obesity. Consistent with this hypothesis, several studies have shown an association between obesity and future symptoms of depression [59-61]. This remains a question for future research.”

4. Height and weight are self-reported and this may have led to an underestimate of prevalence of overweight and obesity. What effect might this have had on study findings?

We have added a comment to the Discussion: Page 13: Line 23 stating: “Height and weight were self-reported which may have led to an underestimate of prevalence of overweight and obesity. This may have resulted in misestimation of the strength of associations between stress and overweight and obesity. However, recent evidence suggests substantial agreement between self-report and measured height and weight among Australian women.”

**Minor essential revisions**

1. I was not entirely clear of the justification for the indicators of dietary intake and wonder whether there are references to support the approach taken. References 40-42 appear to relate to the FFQ used, but is not clear whether these references also relate to the choice of variables.

We have added several references to justify our approach. Please refer to Reviewer 1, Comment 14 for further information.

2. Page 12, second paragraph, first sentence should read ‘Evidence of the associations....’

This correction has been made as requested.

3. Table 1, row headings – replace ‘M’ with Mean.

This correction has been made as requested.

4. Authors’ contributions – should read ‘GA and KB assisted JM in performing statistical analysis...’

This correction has been made as requested.
Reviewer #3 (Comments to the author):

The study contributes to the growing literature examining longitudinal associations between perceived stress and later risk of obesity. Unique strengths of the study are examining a high risk population (i.e. women living in socioeconomically disadvantaged areas) and assessing behaviors related to energy-balance in addition to weight outcomes. Limitations of the work include reliance on self-reported data, a modest response rate and a considerable loss of participants during the follow-up, but these are clearly mentioned in the discussion section. The study is well-conducted and well-written and therefore I have only minor suggestions for revision. Additionally, I have included some discretionary revisions that the authors can choose or not to take into consideration.

Please see responses to specific queries below.

Minor Essential Revisions

1. Abstract: please replace the last word of the methods section “weight-related outcomes” with “weight-related behaviors” in order to be more specific. Moreover, it could be noted more clearly in the abstract that perceived stress was related to a higher BMI and to increased odds of being obese in cross-sectional as well as in longitudinal analyses.

“Weight-related outcomes” has been replaced with “weight-related behaviours” as requested. We have amended the abstract to read “Higher perceived stress in women was associated with a higher BMI at baseline and follow-up, and to increased odds of being obese in cross-sectional and longitudinal analyses.”

2. Methods: on page 5, the authors mention “Where there were fewer than 150 women living in the suburb, all eligible women were invited to participate in the study.” Please describe how many suburbs had fewer than 150 women residents.

We have reported how many suburbs had fewer than 150 women residents. “Where there were fewer than 150 women living in the neighbourhood (n=3 neighbourhoods), all those who were eligible were invited to participate.”

3. Results: Baseline characteristics of the sample and baseline distributions of the outcomes (BMI and weight-related behaviors) are shown in Table 1 and 2. However, some information on the distributions of the outcome variables at the follow-up phase would also be of interest.

We have added two columns to Table 2 to demonstrate the distributions of the outcome variables at follow-up (T2).

4. Results: Percentages for BMI categories; number of children; potato crisps or salty snack foods; chocolates or lollies; and pies, pastries or sausage rolls do not add up to 100% (Table 1 and 2). Please check the numbers. Furthermore, a piece of information related to the proportion of participants in each BMI category is presented twice (in Table 1 and 2).

We have checked the percentages in Table 1 and 2 and they add up to either 99.9 or 100.1, which is simply due to rounding. We have deleted the information related to the proportion of participants in each BMI category in Table 1.
5. Results: when describing the cross-sectional results (page 10), please avoid using the term “predict”.

The term “predict” has been deleted where we have described cross-sectional results.

6. Results: linear regression analysis is appropriately used to examine the associations between perceived stress and continuous BMI variable. However, it remained unclear to me whether the regression coefficients shown in Table 3 and 4 were unstandardized or standardized coefficients. It would be worthwhile to report both of them, since standardized coefficient can aid interpreting the magnitude of the association.

We reported both standardised and unstandardised coefficient in Table 3 and 4. We have also added footnotes to Table 3 and 4 that read “b Standardised regression coefficient, c Unstandardised regression coefficient with 95% confidence interval”.

7. Discussion: on page 13, the authors mention that “However, that study also found that mothers and fathers with high work-life stress had lower fruit and vegetable intakes, which we did not see in our study.” Please clarify whether this refers to the findings that are not shown in the manuscript or whether intake of vegetables/fruit was not assessed in the READI study.

We agree with the reviewer. This was confusing and not relevant to our findings, hence we have removed this sentence.

8. Discussion: the authors appropriately mention the main limitations of the study, but the possible effects of these limitations (e.g., using self-reported weight and height to calculate BMI) on the findings deserve more consideration.

Please refer to Reviewer 2, Comment 4. We have added a sentence regarding the possible effects of self-reported height and weight to the discussion.

Discretionary Revisions

1. The manuscript presents results on seven variables related to food/beverage intake, including 1) potato crisps or salty snack food; 2) chocolate or lollies; 3) cake, doughnuts or sweet biscuits; 4) pies, pasties or sausage rolls; 5) fast foods; 6) pizza; and 7) non-diet soft drink. Perceived stress was found to be related to the consumption of fast foods (both cross-sectionally and longitudinally), but not to other food variables. The authors could consider reducing the number of food variables by combining similar food groups into same variable, for example 1, 4 and 6 can be considered to represent non-sweet energy-dense foods, whereas 2 and 3 represent sweet energy-dense foods.

We did consider this discretionary revision. However, since it would lead to the loss of specific detail about food groups we decided to retain the seven food variables.

2. Studies investigating the associations between symptoms of depression and obesity have generally provided similar results than the literature reviewed in the manuscript. Even though perceived stress and depressive symptoms are separate phenomena, they are closely related and therefore the large depression-obesity literature might be briefly mentioned in the introduction or in the discussion section. For recent systematic reviews and meta-analyses on the longitudinal associations between depression and obesity, please see e.g., Atlantis & Baker, International Journal of Obesity, 2008; Blaine, Journal of Health Psychology, 2008; Rooke & Thorsteinsson,
We have added a sentence to the Discussion: Page 14: Line 10 that reads “Consistent with this hypothesis, several studies have shown an association between obesity and future symptoms of depression [59-61].” Please see Reviewer 2, Comment 3 for further information.