Author's response to reviews

Title: Association of weight misperception with weight loss in a diabetes prevention program

Authors:

Andrea L Hernan (andrea.hernan@greaterhealth.org)
Vincent L Versace (vincent.versace@flinders.edu.au)
Tiina Laatikainen (Tiina.Laatikainen@pkssk.fi)
Erkki Vartiainen (erkki.vartiainen@thl.fi)
Edward D Janus (edwarddj@unimelb.edu.au)
James A Dunbar (director@greaterhealth.org)

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Author's response to reviews: see over
Dear Rita Aguirre,

Thank you for forwarding on the reviewers comments for the manuscript entitled “Association of weight misperception with weight loss in a diabetes prevention program” (Manuscript number: 2098848284953081).

We thank the reviewers for their valuable comments and suggestions for this manuscript.

Please find below our response to the reviewers comments. We have provided a point by point response to each of the reviewers concerns and have made changes where necessary to the manuscript using track changes.

### Reviewer 1

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<td>Minor essential revisions: 1. It would be beneficial if the authors start the 'background' with a few 'general' sentences to acknowledge the importance of weight in the risk/aetiology of diabetes, and the role of weight loss in preventing the disease.</td>
<td>We have modified the first paragraph of the Background to incorporate your suggestion.</td>
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<td>2. It would be better if the authors clarify a bit more how the 'objectively measured' weight was actually measured at baseline, 3 months and 12 months, e.g. using same scales? Were the scales standardised? etc.</td>
<td>Details of the scales used have now been included in the Methods. Standardised procedures for measuring weight were undertaken according to the European Health Risk Monitoring protocol as already mentioned in the third line of the same paragraph.</td>
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<td>3. The authors are encouraged to investigate the possibility of presenting the findings stratified by gender, or at least comment on this aspect somewhere in the 'results' or 'discussion'.</td>
<td>The analysis has been stratified by gender. For each of the three models using the female data the B coefficients were similar to the pooled results and were also significant. For each of the three models using males the B coefficients were less than the pooled results but positive and the results were non-significant. The non-significant male results may have been due to a lack of</td>
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power (n=62), compared to the female sample (n=189). We choose to pool the results to increase our overall sample size. This is covered in new text inserted into the results and in the discussion under the Limitations subheading and is as follows:

There was a significant relationship between weight misperception and weight loss both at three and 12 months (B=0.205 and B=0.220 respectively) for both genders combined (Table 2). The same positive relationship was found for males and females, but was only significant in the latter. This was possibly because of the relatively low number of males in the sample (data not shown).

4. The discussion section would be more balanced if the limitations have been explicitly acknowledged.

A strengths and limitations heading has been added to the Discussion to clearly express the limitations in this study.

Discretionary Revisions:

5. I appreciate the paucity of related literature, but it would be interesting if the discussion compare/contrast the findings against any similar studies elsewhere.

A paragraph discussing the available literature in comparison to this study’s findings has been added to the Discussion section.

6. The authors might also comment on how they believe the findings are generalisable to other populations.

We have now addressed this in the discussion as requested.

In larger implementation studies which we are now undertaking in the Australian community our intervention has taken on board these findings which may also be relevant in other populations such as those with hypertension and arthritis.

We have also addressed some of the potential statistical issues related to generalisability in the limitations section. It reads as follows:

The generalizability of these results might be questioned due to the non-random selection of participants who were approached in the waiting rooms of GP clinics. Whilst it could be argued that this sampling regime may exclude those facing barriers to attending GP clinics (e.g. due to social and/or physical access issues), the screening tool used ensures the sample ultimately comprised participants suited to the
intervention (i.e. those at moderate to high risk of developing T2D). We believe the advantages of using a well-established and validated screening tool outweigh the drawback of non-random approaches and provides for robust findings that are generalizable. With specific reference to the population of rural patients in an Australian setting, a likely benefit of this recruitment process in the original GGT DPP is that GPs are typically where the majority of people seek care in rural settings and the people attending are those more likely to be high risk. Conversely, it could be argued the results aren’t generalizable to those who do not routinely seek care with GPs or potentially face issues of access.

Reviewer 2

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| 1. There are a number of points that the reviewer wants clarified about the reference population and potential sources of bias and the hypothesis. | The GGT DPP study had previously been published in detail with all the methodology including the selection criteria and demographics of participants, as well as the intervention. We have not reported all this in detail again but focused on the specific issue of weight misperception.  

| 2. The major issues that seriously threaten the internal validity are related to many potential biases and confounders that you do not systematically address. | As the analysis presented was a retrospective study using data from the GGT DPP it is difficult to systematically address all biases and confounders, however we have done this as far as possible (see response 5 below).  

With reference to the sample used, it has previously been analysed and reported in the publication by Hernan et al (2012 – published in BMC Public Health) and as part of Laatikainen et al. (2007).  

We have addressed the issue of internal validity in the **Strengths and Limitations** section: |
The lower proportion of males in the sample could be viewed as a limitation with potential implications for internal validity through selection bias. By carrying out an analysis stratified by gender it was found that the direction of the regression coefficient was the same for both genders despite being non-significant for males.

The potential bias of a larger proportion of women is also covered in the results section:

There was a significant relationship between weight misperception and weight loss both at three and 12 months ($B=0.205$ and $B=0.220$ respectively) for both genders combined (Table 2). The same positive relationship was found for males but not for females, but was only significant in the latter. This was possibly because of the relatively low number of males in the sample (data not shown).

Potential confounders are covered in the results section:

The variables baseline BMI and baseline weight were not found to be statistically significant, indicating that the relationship between weight misperception and weight loss at the two time periods were independent of baseline weight. That is neither baseline weight nor BMI were significant predictors of weight loss. Age and years of formal education were also non-significant explanatory variables of weight loss (data not shown). Approximately 7% of the variation of the weight loss at three months and 3% at 12 months were explained by weight misperception.

3. In addition, a better description of the methods used is fundamental. For example, the study design seems to be a cohort. Nevertheless, as the Diabetes Prevention Project was probably designed for other purposes, the design could have been a retrospective one, perhaps after a post-hoc hypothesis arose. I am not implying this. You simply have to give enough space for the reader to understand what this Project was about, what were the interventions, etc.; and we have slightly expanded the methods section as requested and provided a reference which gives detailed information about the GGT DPP intervention – diet and lifestyle changes including weight loss in a population at moderate to high risk of developing diabetes. In the text we have made it clear that this study is a retrospective post-hoc analysis was undertaken with data collected during the implementation of the GGT DPP.
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<td>if these interventions have something to do with the results observed.</td>
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<td><strong>4.</strong> Please state if the reference population was urban or rural and, if possible, ethnicity, education level, income level, etc. A detailed description of the selection process is fundamental. Major selection biases could have arisen (due to “opportunistic screening” and “individuals (...) presenting at local general practices”).</td>
<td>Demographic characteristics of the participants collected included age, sex, marital status, education and employment level. Age, sex, marital status and employment level were reported in the first paragraph of the Results section. Education in years has been added to this paragraph. All participants resided in rural locations in south-east Australia so this has been clearly stated in the first paragraph of the Methods section. Ethnicity and income level were not collected during the GGT DPP. A sentence has been added to the Methods section which outlines how study nurses opportunistically screened potential participants using the FINDRISC questionnaire. Further, a reference has been added to the methods section (reference number 13) which provides more detail on the selection process of participants.</td>
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<td><strong>5.</strong> So, you also need to at least try to assess whether these biases (there are many more, also information ones) could have influenced results. Special care is to be taken regarding their direction and magnitude. Although their complex interaction might be impossible to predict, at least a sincere acknowledgement will give the readers the opportunity to make their own judgments.</td>
<td>Please refer to the third response from reviewer 1 where we detail our response to the potential bias of over representation of females. This includes discussion on the direction of the regression coefficient and makes mention of potential issues of power when stratifying by gender. This will allow the reader to make their own judgements about this bias. The other potential bias that may have played a role was marital status. The sample we analysed had &gt;70% classified as ‘married’. The results of the regression analysis using the ‘married’ class were almost identical to those reported in the manuscript. When using the ‘unmarried’ class the regression coefficients were all in the same direction (i.e. positive) and had a mixture of significant and non-significant results – we suspect the non-significant results may have resulted from a reduction in power due to a smaller sample size (i.e. a similar pattern to the analysis stratified by gender). We have not included this in the manuscript due to the large proportion of ‘married’ participants.</td>
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6. Similar problems are related to many possible confounders. You report three regression models that do not contribute to the understanding of your work (actually they add confusion to it). Please try to describe possible confounders and explain what you did to try to address them (for example, adjustment). If you do include regression models, please take time to describe how you built them (criteria for including variables, etc.), why you chose one model over others, and especially what variables were included and what other possible confounders could not be measured and included. Of course, a detailed discussion of possible implications is mandatory.

The aim of this study was to determine if weight misperception was significantly linked to weight loss during the GGT DPP. We included baseline weight and baseline BMI to test whether these potentially relevant explanatory variables were statistically significant in predicting weight loss. The consistent explanatory variable in the three regression models presented was weight misperception. Baseline weight and baseline BMI were not significant therefore we concluded weight misperception is a variable that may partially contribute to weight loss in a diabetes prevention program over time. We don’t believe this logic confuses the understanding of the work.

In response to other possible confounders we have included age and years of formal education. These are referred to in the text in the methods section:

Linear regression (‘Enter’ method) was used to investigate the relationships between the dependent variables of percentage weight change at three and 12 months, and the explanatory variables of weight misperception, baseline weight and BMI. Weight misperception was included in all models with baseline weight and BMI included separately. These analyses were carried out on the pooled sample of males and females, and also stratified by gender. Linear regression was also used to assess the influence of age and years of formal education on weight misperception.

They are also referred to in the results section:

The variables baseline BMI and baseline weight were not found to be statistically significant, indicating that the relationship between weight misperception and weight loss at the two time periods were independent of baseline weight. That is neither baseline weight nor BMI were associated with weight loss. Age and years of formal education were also non-significant explanatory variables of weight loss (data not shown). Approximately 7% of the variation of the weight loss at three months and 3% at 12 months were explained by weight misperception.
We trust that our responses adequately address the issues raised by both reviewers and that this manuscript is now acceptable for publication in BMC Public Health.

With Kind Regards,

Andrea Hernan
(Corresponding author)

Greater Green Triangle
University Department of Rural Health
Deakin University and Flinders University
PO Box 423
Warrnambool
VIC Australia 3280
Phone: +61 3 5563 3505
Fax: +61 3 5563 3144
Email: andrea.hernan@greaterhealth.org