Author’s response to reviews

Title: The effect of gender and age on the association between weight status and health-related quality of life in Australian adolescents

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Author’s response to reviews: see over
9th July 2014

Dear Victorino Silvestre: Journal Editorial Office,

Please see below our response (point by point) to the reviewers’ comments regarding the recently reviewed manuscript entitled “The effect of gender and age on the association between weight status and health-related quality of life in Australian adolescents”. The authors of this manuscript are as follows: Kristy Bolton, Peter Kremer, Naomi Rossthorn, Marj Moodie, Lisa Gibbs, Elizabeth Waters, Boyd Swinburn and Andrea de Silva.

We trust that you will find this revised manuscript suitable for publication in *BMC Public Health*.

Thank you for reviewing this paper and we look forward to hearing from you.

Yours sincerely,

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RESPONSE TO REVIEWERS’ COMMENTS

Reviewer 1

Reviewer’s report
Title: The effect of gender and age on the association between weight status and health-related quality of life in Australian adolescents
Version: 1
Date: 15 May 2014
Reviewer: Juan Miguel Fernandez-Alvira

Reviewer’s report:
The manuscript entitled "The effect of gender and age on the association between weight status and health-related quality of life in Australian adolescents" presents an interesting topic which can help to further elucidate how obesity affects the quality of life in adolescents depending on gender and age. However, there are some points that need to be addressed before a decision can be made.

Major compulsory revisions:
1- The manuscript includes demographic data from 1583 participants and anthropometric data from 944 participants. With the available information in the tables and text it is not easy to elucidate what is the actual sample size for each of the associations tested (e.g. Table 2 includes a total sample of 944, but it is also written that sample size varies depending on the specific variable). Anyway, it is not clear how much it varies. If the associations between gender/age and HRQoL would include the 1583 participants (or if the n might differ substantially), then the results might not be comparable and it would represent a basic limitation of the study. It is necessary to clearly state throughout the text and tables what is the actual sample size for each of the associations tested (please, provide in the tables the exact n for each association), so the discussion and conclusions are clearly supported by the results.

Response: We have amended the text and tables as suggested above. We have omitted the statement that sample size varies depending on the specific variable and instead, in each table, the relevant n for each association tested is presented. In addition, Table 2 contains a footnote stating that demographic and survey data were collected from all participating students, however anthropometry was collected from only 944 of the participating students. This is consistent with a statement which has been added to the methodology section under the subheading “Data treatment and analysis”. The total n for this study was 1583, however for the associations presented in Table 2, AQoL data was missing for n=179 (11% of the total sample) and anthropometry data was missing for n=639 (40% of the total sample).

Minor essential revisions:
1- line 23: part of the HRQoL wording is missing. Please, include the missing part.

Response: The text has been amended so the term is complete.

2- The first two paragraphs of the background should be rewritten in order to present the ideas in a more tidy way, as right now it is difficult to follow the ideas easy. For example: the information about obesity could be ordered as follows: prevalence, comorbidities, psychological and social related aspects, and then start focusing on quality of life in the second paragraph.

Response: We have restructured the order of text as recommended. Specifically we have moved the second sentence of the first paragraph to the second paragraph to improve the flow regarding a link between psychological and social related aspects and health-related quality of life.

3- line 178: Clearly state what is the real n for each of the statistical analyses.

Response: The three tables have been updated with all relevant sample numbers for all variables and associations presented. The text in the methodology section under the subheading “Data treatment and analysis” has been updated to clearly state to the reader that demographics and surveys were collected from 1583 students however anthropometric measurements were taken from 944 students as indicated by the sample numbers displayed in the upcoming tables presented. Furthermore, we have included an explanatory note underneath Table 2 which indicates why there is a difference in sample size for the ANOVA analysis presented.
4- line 212: I suggest to clarify a bit in what direction gender moderates the association between weight status and HRQoL, so the readers can have a clear idea of what this moderation implies.

Response: We have amended this sentence so that it clarifies how gender moderates the association between weight status and HRQoL. Specifically, we have altered the sentence so that it now reads:

“In contrast, there was no relationship between weight status and HRQoL. However when gender was added to the model examining weight status and HRQoL, a significant interaction was discerned whereby gender moderated the association between weight status and HRQoL. Specifically, the interaction indicated that relative to males, females who were overweight had significantly poorer HRQoL compared to healthy weight females.”

5- line 215: I suggest to downsize the information about non-significant associations. If they are not significant, I would avoid saying that HRQoL score was lower for one group compared to the other. Please, rephrase the information to avoid confusion.

Response: We have amended the text by deleting the sentence referring to the non-significant association between weight status and HRQoL. Additionally, the mention of healthy weight adolescents reporting higher HRQoL compared to overweight peers (which was not significant p=0.07) has been omitted from both the abstract and the results section.

6- line 219: in the list of potential explanations to the lack of significance in the association between weight status and HRQoL one of the reasons explained is the potential difference between measured vs parental reported or self-reported weight and height. However, the present study includes measured weight and height, which represents an advantage versus self-reported weight and height. Therefore, the present study includes the best scenario compared to other studies, and thus this is not a reason for not finding an association.

Response: We appreciate this comment and have amended the text as suggested. When the paragraph in question was re-examined in light of the reviewer’s comment, the two separate ideas which were combined in this paragraph became apparent. Therefore, we have separated out these two ideas: 1) why we found no association between weight status and HRQoL, and 2) how the finding from the current study compares to findings of previous studies.

We have restructured two of the paragraphs regarding weight status and HRQoL in the discussion section and focused on one idea per paragraph (paragraph 2 of Discussion, line 226, page 11; paragraph 3 of Discussion, line 237, page 11). The first paragraph now focuses on hypothesising why no significant association was demonstrated between weight status and HRQoL, possibly due to the non-condition specific HRQoL assessment tool (AQoL-6D) utilised in this study being less sensitive to weight-related variations in HRQoL. The second paragraph now focuses on the contrasting findings between the current study and previous studies, and potential reasons for the differences in these findings including a variety of methodological issues (e.g. sample sizes/characteristics studied, HRQoL assessment tools, collection of anthropometric measurements).

7- line 239: one of the questions I have after reading the discussion part is what would be the picture if instead of grouping overweight and obese all together, the analyses would be done for overweight and obese separately. The psychological and social implications of obesity and overweight in adolescents might be quite different.

Response: We acknowledge that this is an interesting question. We did initially investigate this possibility however the additional level of partitioning required for this yielded relatively low subgroup numbers (e.g. overweight total 173 – male 85, female 88; obese total 70 – male 40, female 30) meaning analyses would be underpowered. Specifically, for the analysis that compares the healthy weight subgroup (n=710) with the overweight/obese subgroup (n=234), with alpha = 0.05, power is calculated at 0.61, however with the additional partitioning into the three weight status groups power is significantly reduced. For example, for the comparison female overweight (n=88) with female obese (n=30), with alpha = 0.05, power is 0.14 rendering any potential analysis severely underpowered.

We also note that many previous studies (e.g. Boyle, Jones & Walters 2010; Ottova et al 2012, Willee et al 2010; Jansen et al 2013) have also combined overweight and obese weight status categories into a single (overweight/obese) category invariably for the same reason that we have, and studies have demonstrated associations between HRQoL and excess weight overall (regardless of being in a separate overweight or obese weight status category).
We have added a line to the “Limitations and future directions” section (paragraph 2 of this section, line 330, page 15) to acknowledge this idea in future studies:

“Future research could include exploring potential associations between HRQoL and subgroups of excess weight (i.e. analysing overweight and obese categories separately) and in-depth examination of family and socio-economic patterns to determine any influences on HRQoL and weight status such as household finances, familial eating patterns, lifestyle behaviours and relationships.”

8- Table 1: please include the n next to the percentages, so the readers can have a clear mind of what a percentage really means in terms of sample size.

Response: Table 1 has been amended. Specifically, the cell n’s are reported and the corresponding percentages also reported in parentheses.

9- All tables: please include the n for each of the associations tested.

Response: All tables have been amended so that they indicate the n for each association that is tested.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Reviewer 2

Reviewer’s report
Title: The effect of gender and age on the association between weight status and health-related quality of life in Australian adolescents
Version: 1
Date: 8 June 2014
Reviewer: Silvia Bel-Serrat

Reviewer's report:
This study investigates the association between weight status, age and gender and the health-related quality of life in Australian adolescents by multiple linear regression. Health-related quality of life was associated with age and gender but not with weight status in this sample of adolescents. The paper is well written and findings are interesting. There are some issues, however, that the authors should address prior to publication.

Major Compulsory revisions
1) The authors should explain how the schools were selected and if the selection was based on any specific criteria i.e. by SES, among others.

Response: The schools in this study were from a larger health promoting initiative conducted across the state Victoria, Australia (de Silva-Sanigorski 2010). Communities were invited to tender to become intervention sites, and subsequently schools within this community were invited to participate. To match the participating intervention schools, comparison schools were consequently randomly selected based upon demographics such as school type (government/non-government), school size, level of disadvantage and location (urban, regional, rural). The schools were surveyed at baseline, and post-intervention. This methodology has been outlined in the BMC Public Health protocol paper by de Silva-Sanigorski et al. 2010. The methodology section in the current manuscript has now been revised so that it summarises and explains how schools were selected for the study:

“Schools in the current study were part of a larger health promoting study and selected for involvement as previously described[28]. Briefly, schools within intervention communities were invited to participate in the study, and subsequently comparison schools selected using stratified random sampling to match intervention school demographics such as school type, school size, level of disadvantage and location. This study utilises baseline data only, consequently intervention or comparison status of schools is irrelevant.”
2) Please, add how BMI was calculated to the “Anthropometry” section.

Response: We have amended the text and now include detail regarding how BMI was calculated:

“BMI was calculated using weight in kilograms divided by height in metres\(^2\) (kg/m\(^2\)).”

3) As stated by the authors, not all measurements, i.e. anthropometric measurements, were taken in all the individuals and sample size varies across the included variables. Did the authors check whether there was any difference in terms of baseline characteristics among the different sample sizes? That should be mentioned in text.

Response: Two rounds of data collection occurred at each school (round 1 and round 2). Round 1 involved collecting demographic information, survey (AQoL6-D) and anthropometric data from participating students. Due to school-related limits on student access for data collection in round 2, it was only possible to collect demographic information and survey data from these participating students. As data was collected from the same schools at both time points (i.e. round 1 and round 2), the characteristics of the samples are similar overall. The text in the methodology has been updated to make this clearer to the reader:

“Note that demographics and surveys were collected from 1583 students however anthropometric measurements were taken from 944 students only as indicated by the sample numbers displayed in tables presented. Two rounds of data collection occurred at each school. Round 1 involved collecting demographic information, survey (AQoL6-D) and anthropometric data from participating students. Due to school-related limits on student access for data collection in round 2, it was only possible to collect demographic information and survey data from these participating students. As data was collected from the same schools at both time points, the characteristics of the sample at round 1 and round 2 are similar.”

4) As acknowledged in the limitations section, this is a cross-sectional study and causal associations cannot be drawn. For that reason, please, avoid any reference to cause-effect relationships as the term “effects” appears several times along the text.

Response: The text within the aims section, and the discussion has been updated to replace the word “effect” with “association” or “findings” in order to ensure that no cause-effect relationships are inferred from the findings of this cross-sectional study.

5) The authors claim that environmental, economic and cultural factors affect well-being and could explain the lack of results with weight status. However, they also support their findings with studies conducted in other countries different from Australia. It could be interesting if the authors make any reference to the fact that, in spite of cultural differences, among others, results remain similar. That would even add more power to the obtained findings. Related to this, the authors also make reference to the country in which the study from Bonsergent et al. was conducted.

Response: To make it clearer to the reader that the lack of an association between weight status and HRQoL in a study of Fijian adolescents supports the findings of the current study within Australia (whereby communities in both countries experienced socio-economic disadvantage, contained some large rural and regional areas and were culturally and linguistically diverse), an additional statement has been embedded within this paragraph (paragraph 4 of the Discussion, line 252, page 12). This statement summarises the prior content of the paragraph to succinctly convey the idea to the reader that despite cultural differences between Fiji and Australia, the results regarding weight status and HRQoL were similar, and subsequently gives more strength to current study findings:

“However, despite the cultural differences between Fiji and Australia, it is intriguing that the findings regarding a lack of association between weight status and HRQoL in Fiji are supportive and strengthen the current study findings in this adolescent population in Australia.”

6) The authors stated that the employed questionnaire was validated, but a reference is needed in this regard. Was it validated in adults or in adolescents? Which might be the implications of adapting a questionnaire designed for adults for use in adolescents? Please, give more insights about this and state any potential limitations that might be related to that.
**Response:** The methodology section with the subheading “Health-related quality of life” contains detail about the HRQoL assessment tool used in this study (i.e. AQoL-6D) and contains a reference to the methodology involved in adapting the adult AQoL survey to the adolescent population. We have also added the references to validation studies in both adolescent and adult populations.

A strength of the current study is the use of a psychometrically tested and validated assessment tool (as mentioned in the Discussion). The tool has been validated in both adolescent (Keating et al, 2011) and adult populations (Allen et al. 2013). Implications of adapting a questionnaire designed for adults for use in adolescents are discussed in the paper by Moodie et al (2010) who conducted the recalibration of AQoL-6D for Australian adolescents using country-specific time trade-off scenarios and suggested that whilst views of adolescents about the prospect of living in perfect or imperfect health may differ from adults (due to having more life experience), the authors were confident that the concept of trading years of life was understood.

7) **Discussion section, seventh paragraph:** it is stated that “the findings indicate that while overweight adolescents had equivalent levels of HRQoL…”. It is not possible to draw this conclusion from multiple linear regression analyses. The following sentence claims that “this situation was altered when…”. The findings may change but the situation cannot be altered. Please, interpret and rephrase the results properly.

**Response:** The text in this paragraph has been altered by removing the incorrectly interpreted conclusion regarding equivalent levels of HRQoL reported by overweight adolescents as suggested by the reviewer and the results have been stated made clearly:

“Whilst there was no significant overall association for weight status on HRQoL (model 2); when the weight status-HRQoL association was modelled in conjunction with gender (model 3), a more complex association was found which suggests that gender is an important moderating factor and strengthens the relationship between weight status and HRQoL. The association for gender found in model 2 was not significant when the combination term was included (model 3). Together this complex set of findings suggests that the association of weight status on HRQoL is particularly salient for adolescent females but of little impact to males.”

**Minor Essential revisions**

1) **Abstract, methods section:** please, replace “M” by “mean”

**Response:** The text has been updated in the abstract to replace the abbreviations as suggested to “(mean age 14.5 years)".

2) **Introduction, first paragraph:** use “type II diabetes” instead of “type two diabetes”.

**Response:** We appreciate the reviewer’s suggestion and we have adjusted the original text (ie. ‘type two diabetes’) to now read ‘type 2 diabetes’. We have used this term rather than the suggested ‘type II diabetes’ since both the World Health Organisation and International Diabetes Federation both use this term. Furthermore, a quick survey of papers recently published in BMC Public Health also indicates the use of “type 2 diabetes” (see examples listed below).


3) **Introduction, third paragraph:** “…with poorer HRQoL experienced by children…” it seems that it lacks a “were” after HRQoL.

**Response:** The sentence has been amended to make it clearer for the readers:

“Research has expanded to child and adolescent populations and supports the associations found in adult populations whereby poorer HRQoL was experienced by children and adolescents with excess weight.”
4) Methods section, first paragraph: spell SES like socio-economic status or socioeconomic status.

Response: All references to socio-economic status have been spelt consistently throughout the paper as suggested.

5) Methods section, second and fourth paragraphs: refer to the websites as a references by adding them to the reference list.

Response: The websites have been added to the reference list.

6) Methods section, fourth paragraph: the sentence “BMI was calculated and standardized…” should be placed under the third paragraph “Anthropometry”.

Response: The text regarding BMI calculation, standardisation and categorisation into weight categories has been moved to the “Anthropometry” section as suggested.

7) Results section, third paragraph: replace “this effect persisted” by “even”, and “of the covariates” by “among the covariates”.

Response: The text has modified as suggested.

8) Discussion section, fifth and sixth paragraphs: it lacks a reference to the study conducted by Bonsergent et al. (2012).

Response: We have amended the text and inserted reference to the Bonsergent et al. (2012) study. Specifically we make reference to this study in paragraph 5, paragraph 6 and paragraph 7 in the Discussion section.

9) Discussion section, sixth paragraph: it lacks “that” after “autonomy” in “… a process of individuation and autonomy is very important”.

Response: The sentence has been amended and now reads “Adolescents undergo a process of individuation and autonomy that is very important to them”.

10) Refer to SES instead of level of disadvantage.

Response: Table 1 has been modified so that it now refers to SES.

11) Table 1: delete or put in brackets the percentage of males and females. As the sample size varies according to the variables, it would be very informative to have the sample size for each variable under study. It is not clear among which groups there are significant differences, please, clarify using other symbols/superscript letters or adding further symbols.

Response: As suggested, in Table 1 the percentages of males and females have been put into brackets beside the sample size in the top row. For every variable, sample numbers and proportion in brackets have been added to inform the reader more clearly about any difference in sample sizes throughout the rest of the table. Additionally, the legend text underneath the table now displays unique symbols and individual p values resulting from the chi2 analysis. The legend text has also been updated to include information for the reader that the chi2 was performed on the total sample overall for each demographic variable tested.

12) Table 2: please, add in the legend the sample sizes for those variables with different number of subjects.

Response: Table 2 has been updated with sample sizes for all analysed variables. Additionally text has been included in the legend to explain the difference in sample size for the ANOVA analysis presented in this table:

“Note: sample size varies due to demographic and survey data being collected from all participating students, and anthropometric data from n=944 students only.”

13) Table 3: state if beta coefficients are standardized or unstandardized. Replace “coef” by the beta symbol. It would be also informative to also add p values to complement CI.
Response: Table 3 has been updated as suggested and the column with the coefficient has been updated to include the “B” symbol and the legend underneath the table has been updated with this information. Additionally, a line in the methodology section has been added to make it clear to the readers that the coefficients were unstandardized:

“Multiple linear regression (MLR) analysis was also used to test for associations between weight status and AQoL score and effects are reported as unstandardized coefficients (B).”

Individual p values have been indicated by different symbols within the table, and the legend below the table outlines the exact p value.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable