Author's response to reviews

Title: Associations between obesity, socio-demographic factors, life events, lifestyle and psychological well-being in older Europeans

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Author's response to reviews: see over
Reviewer's report-1
Title: Associations between obesity, socio-demographic factors, life events, lifestyle and psychological well-being in older Europeans
Version: 1 Date: 29 December 2011
Reviewer: Awat Feizi
Reviewer's report:
Below you will find comments obtained during the peer review for the submitted manuscript entitled: "Associations between obesity, socio-demographic factors, life events, lifestyle and psychological well-being in older Europeans"

1. Minor Essential Revisions
Title of manuscript should expressed or modified in which the role dependent and independent variables clearly identified.

Change of title: "Associations between obesity (BMI and waist circumference) and socio-demographic factors, physical activity, dietary habits, life events, resilience, mood, perceived stress and hopelessness in older Europeans” inserted

2. The following item are major Compulsory Revisions
Methods section:

• What is the study design? What is the time period of research? Please present it carefully.
  Date and that the study was cross-sectional inserted at start of method section.

• Please correct the number of sample for GB to 1182 in table 1. Also present the corresponding percents to frequencies of male and female. What are the numbers in parentheses? They are SD or SE, please identify it.

  GB = N=1182 has been corrected in Table one and (N=1722) throughout.
  GB n=1822 was an error. Corresponding percents have been included for males and females within each country. It is indicated in a footnote at the bottom of table one that the variables are presented in Mean (SD).

• For each used instrument you should present which type of factor analysis has been used; explanatory or confirmatory and what are the number in parentheses (loadings); for example for FFQ it is not clear which type of factor analysis was used and …

  The factor analysis explanation has been expanded as requested (Line 127-136).

• Regarding to BHS instrument: regular factor models cannot be used for analyzing dichotomous variables. They are only applicable for continuous or at least ordinal variables.

  The BHS was not factor analysed, an equally weighted summed score was entered for each case. This section has been amended (page 8).
Regarding to PSS instrument: if you extracted two factors you should compute factor scores for each one and use them separately as predictor variables. To clarify, the 4 items were summed and entered as 1 variable. This has been amended accordingly (page 9).

Your statistical analysis has major drawbacks. Accordingly the obtained results should be modified according to following recommendation:
First: the authors in methods section mentioned that the one way ANOVA have been used for …. In this regard due to two dependent variables (BMI and WSC) they should use Two- way MANOVA, considering sex and country as independent variables.

Statistical analysis was modified and a two way MANOVA was conducted using country and sex as the fixed factors and the anthropometric measures as the dependent variables. This has been described in the text. Subsequently, this analysis delivered the same results. Height and age have been described in the text as they were also analysed by MANOVA across country and sex (i.e. table 1) (line 236-238).

Second: Regarding to regression models: it is highly recommended the authors use Multivariate multiple regression, i.e. simultaneously consider the BMI and WSC as dependent variables and regress them on independent variables.
Variable “Resilience” two times has been used, although the second one has been adjusted, definitely in this case the colinearity problem will be occurred in fitted regression models!

BMI and WC have been modelled separately, because BMI and WC are differently caused and are likely to have different consequences for health. For this reason the outcome (dependent) measures were not seen as part of the same variable system and hence the separate analyses. As previous studies (reviewed in the introduction) imply, we hypothesise that BMI and WC are triggered by different psychological factors and have different consequences for psychological well-being. This has been clarified at the end of the introduction.

Additionally, resilience (and the other continuous variables) were inserted twice, once as the main effect and then as a part of the interaction term. Resilience and other continuous measures were centred in order to minimise the effects of collinearity.

According to the above recommendations, the results section needs major revisions. Results section should be rewritten more precisely. (e.g. the percents in parentheses are not meaningful. The sample size for GB has been wrongly written 1172?)

This sample size for GB was n= 1182

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Reviewer's report-2

Title: Associations between obesity, socio-demographic factors, life events, lifestyle and psychological well-being in older Europeans
Version: 1 Date: 15 November 2011
Reviewer: Fiorella Marcellini
Reviewer's report:

Minor Essential Revisions:
1) Please explain in the methods the motivation of using new variables with interaction of sex in the regression models and include in the discussion the results obtained for the variables adjusted/not adjusted.

Variables were inserted twice as it is necessary in the regression model to test for main effects (variable alone) and interaction effects (variable* sex) within the same model. Main effect variables were centered, in order to reduce the likelihood of collinearity. Interactions were included in order to look at the differential impact for a number of independent measures on the outcome (dependent) measures in terms of a person’s sex. In other words it was hypothesized that the effect of (say) resilience on BMI was different depending on whether the person was female or male.

2) Please indicate clearly in the tables the new variables created after centring for the mean and multiplying by sex; there are two different notation, such as “Age * sex” and “General mood 1, where 1=controlled for sex”.

This has been amended and is now consistent throughout table 2-5.

3) The authors should try to run the models firstly with all the original variables including sex and then using only the variables controlled for sex, to assess if the model is more powerful.

Response to point one rebuts this.

4) Please, enrich the discussion part on the resilience results, as it is the most interesting and innovative part of the analysis.
Further discussion of the resilience finding included on pages 15 and 17 (last paragraph of the discussion).

Discretionary Revisions:
5) The overall aim of the paper is in line with the current research topics in the field of the positive psychology and well-being effects on protecting and enhancing health. The work of Carol Ryff on psychological well-being dimensions and its protective role should be cited, to support both the discussion and the introductive part.
Carol Ryff’s work has been referred to in the introduction (page 4).

6) The choice of the tools and questionnaires should be better justified, most of
all the reason of using shorten version, instead of the full one. Of course, the psychometric dimensions of the different versions were taken into account, but a more exhaustive battery for construct as resilience, depression (trait) would have been better.

**Owing to ethical constraints on space in the questionnaire, fully validated short versions of the RS, MS and the PSS were used. That these shorter versions have all been validated by the original authors has been clarified in the method section.**

**The Beck hopelessness scale was selected and not the full Beck depression inventory also to limit the length of the questionnaire. That this makes it difficult to compare our results with those of previous studies has been emphasised in the discussion (top of page 17).**

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**
I declare that I have no competing interests.

**Reviewer’s report-3**

**Title:** Associations between obesity, socio-demographic factors, life events, lifestyle and psychological well-being in older Europeans

**Version:** 1  
**Date:** 27 November 2011

**Reviewer:** Chrystalleni Lazarou

**Reviewer’s report:**
The study aims to examine associations between obesity, socio-demographic factors, lifestyle and psychological factors in elderly of two European countries. The design and the tools used for the study are described thoroughly. Please consider the following suggestions.

**Background**

Line 49 This is an old reference and a reference that refers to young populations. Some recent reviews for young populations conclude to a somewhat different finding.

Please see the following:


In any case, I think that references that refer to studies in old populations would be more appropriate for this paper.

**The reference in line 49 referring to results in children has been removed. Work carried out by Lazarou on adults has been referred to instead.**

Lines 70-71
There are at least one EU funded projects that among other things do compare more than one culture in one study: EPIC Elderly

Lines 86-87
...On the other hand, some others have employed large, representative samples, as those mentioned above...

**EPIC studies (Bullo et al., 2011; Kyrozis et al., 2009) have been referred to at the beginning of the introduction and discussion (Paragraph 1 and lines 363, 377).**

Discussion
A general remark for the discussion is that more discussion on relative results from other studies in elderly cohorts is expected.

**EPIC research papers have been referred to as above.**

Lines 366-368
I am not sure if this is the only reason. Several studies have failed to reveal such association, but they have employed other than FFQs dietary collection methods.

**That previous studies have not considered diet has now been discussed (page 381-5).**

Lines 375-377
**What might be the reason for this observation?**
It has now been emphasised that this finding concurs with some other studies. It is also pointed out that although validated as part of Beck Depression inventory, that our research considered hopelessness and not depression per se makes it difficult to make direct comparison with previous studies.

It would be useful to add a small section on this study’s strengths and limitations. Some of them are referred in conclusion, but it’d be more appropriate to move them in the last section of the discussion.

**Section on strengths and limitations moved out of the ‘conclusion’ above into main body of text.**

Conclusion
Conclusion should be revised in accordance with the above suggestions (i.e. moving limitations and strengths in main discussion)

**See response to previous comment.**

**Level of interest:** An article whose findings are important to those with closely related research interests
**Quality of written English:** Acceptable
**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.