Author's response to reviews

**Title:** Overweight and obesity in expectant parents: socio-demographic patterns and within-couple associations. A population-based, cross-sectional study.

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**Author's response to reviews:** see over
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Dear Editor
BMC Public Health

Thank you for the valuable comments on our manuscript entitled: ‘Overweight and obesity in expectant parents: socio-demographic patterns and within-couple associations. A population-based, cross-sectional study’.

We appreciated the thorough review, and the manuscript has now been revised according to the suggestions made by the two reviewers. In the attached letter we have listed reviewers’ questions and suggestions (bold italics) followed by our responses (normal font). Red font has been used to indicate the revised sections in the manuscript. All authors have approved the changes made in the final manuscript.

Furthermore, before this round of review on request of the editor, English language versions of the questionnaires were uploaded. These additional files have now been shortened to encompass only the specific questions of interest to the sub-study now reported on. This is a requirement of the Salut Programme Management at the Västerbotten County Council in Sweden, responsible for the study as a whole.

We hope that the revised manuscript will be acceptable for publication in BMC Public Health.

Best regards on behalf of all authors

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REVIEWER I: ANNE JAASKELAINEN

Overall, this is a well written, clear and balanced paper. Methods are adequately described, conclusions are in line with the results, limitations are properly acknowledged and references are up to date.

Minor Essential Revisions

Reviewer:
1. I would suggest using the abbreviation ‘95% CI’ in the text wherever confidence intervals are presented.

REPLY:
Changes are made in the manuscript according to the reviewer’s suggestion.

Reviewer:
2. British English used with a couple inconsistencies (meters, generalizable)

REPLY:
Thank you for noticing this. We have now consistently used British English.

Reviewer:
3. In Table 4, likelihood of overweight for women and men: are identical odds ratios, confidence intervals and numbers of subjects correct?

REPLY:
Thank you for this valuable comment. We have reviewed the analysis and the results presented in the table with assistance of an experienced statistician, and the numbers given are correct. However, we decided to make some changes in mode of presentation in Table 5 (page 22) to increase clarity, as the repetition of odds ratios may be confusing for the reader. We have now presented odds ratios for pregnant women with overweight/obesity in relation to partner weight only. Accordingly the table heading has been changed from ‘Likelihood of overweight and obesity in relation to partner weight in expectant couples’ to ‘Likelihood of overweight and obesity in relation to partner weight’.

The following changes have been made in text:
The sentence ‘The likelihood of being overweight or obese increased relative to partner’s overweight or obesity, with women and expectant fathers being more than six times as likely to be obese if their partners were obese, compared to if their partners had normal weight (OR 6.24, CI 4.21-9.25)’ has been changed. The suggested text is ‘The likelihood of being overweight or obese increased relative to partner’s overweight or obesity, and women were more than six times as likely to be obese if their partners were also obese in comparison with women whose partners were of normal weight (OR 6.2, CI 4.2-9.3).’ (Page 2, Abstract).

The sentence ‘For example, pregnant women and expectant fathers who had an obese partner were more than six times as likely to report obesity, compared to if their partners reported normal weight (OR 6.24, CI 4.21-9.25, both genders)’ has been changed. The suggested text is ‘For example, pregnant women who had an obese partner were more than six times as likely to report obesity, in comparison with women whose partners reported normal weight (OR 6.2, CI 4.2-9.3) (Table 5)’ (Page 10, second paragraph).
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The sentence ‘Third, BMI was correlated within couples, and the likelihood of being overweight or obese increased relative to partner’s overweight or obesity, with women and expectant fathers being more than six times as likely to be obese if their partners were obese, compared to when their partners were normal weight’ has been changed. The suggested text is ‘Third, BMI was correlated within couples, and the likelihood of being overweight or obese increased relative to partner’s overweight or obesity, with women being more than six times as likely to report obesity if their partners were obese, in comparison with women whose partners reported normal weight.’ (Page 12, first paragraph).

Discretionary Revisions
Reviewer:
4. In Table 6, I would separate different occupations by slashes instead of commas.

REPLY:
Changes have been made according to the reviewer’s suggestion (Table 7, previously Table 6), and also in Table 1 and 6 for consistency purposes.
REVIEWER II: KATIE A MEYER

Summary
This paper quantifies BMI (from self-reported weight and height) among pregnant women and expectant fathers. The research questions are clearly stated at the end of the Introduction. The statistical methods do not account for the clustered nature of the data. Limitations of the study design, including low response rate and self-reported weight and height, are noted by the authors, but perhaps down-played in their discussion. Data were collected via surveys. Greater clarify is needed in describing how the sample was recruited (prior to enrollment in antenatal care), and how the male/female sample sizes correspond to parents and couples. I believe the paper would benefit significantly with additional analyses (see #5 under Major Revisions). As written, the paper will be of most interest to workers with a specific interest in data from Swedish samples.

Major revisions

Reviewer:
1. Please tell us how pregnant women were identified prior to enrollment in ANC.

REPLY:
Women were identified as potential participants when they contacted ANC to make a booking for the enrolment visit. Notably almost 100% attend ANC, which implies that the study has the potential of covering (almost) all pregnant women during the study period. This has now been clarified in text.

Extract from text with added text segments indicated:
‘Data were collected between January 2008 and December 2011 through mailed questionnaires to pregnant women and their partners at enrolment in ANC. Women were identified as potential participants when they first contacted ANC to make a booking for their enrolment visit, where after the midwife sent the questionnaires to the pregnant woman and her partner (using the woman’s address). The questionnaires were returned on the day of enrolment (mean gestational age was 10 weeks calculated in relation to last menstrual period). The pregnant women’s questionnaires were used by midwives as a basis for health and lifestyle counselling, while partner questionnaires were used for epidemiological surveillance and research purposes only. Questionnaires included variables on socio-demographics, BMI, health and lifestyle (questions used in the study are presented in additional file 1 and 2). No reminders were sent. Since 2006 use of the questionnaires has been rolled out across the county in a stepwise fashion in ANC clinics, reaching countywide implementation in mid-2010. ‘The ANC services reach almost 100% of pregnant women in Sweden, which means that almost all pregnant women and their partners in the county have been invited to answer the questionnaire from mid-2010.’ (Page 5, fourth paragraph and page 6, first paragraph).

2. Please include sample size information in the methods. Specifically, explain how you obtained more mothers and fathers than couples. Did you consider restricting the analysis sample to instances where you have complete couple data?

REPLY:
This is an important comment, and we have now provided some additional information on sample sizes. We considered restricting the analyses in Table 7 (previously Table 6) to only included couples, however after discussions within the research group, we decided to use all
data obtained on pregnant women and expectant fathers to increase the robustness of the analyses. As suggested in comment no. 5, we have now accounted for within-couple clustering in the analyses (Page 24, Table 7).

3. Your response rate is quite low. Do you have data on non-respondents that could inform about potential differential selection, such as comparing your sample to the county as a whole on demographics (e.g., age, SES, region)?

REPLY:
It is true that the response rate is quite low, as in many studies around the world nowadays. This is worrying, and as the reviewer points out this requires us to attempt to better understand the nature of our non-response, and its possible influence on the reported results.

We have therefore added some additional information on selected variables from the general population in Västerbotten and Sweden, respectively, including information from the Swedish Maternal Health Care Register. A new table has been created to facilitate readers comparing our sample to the county as well as to the country as a whole (Page 19, Table 2). We have also added some information in the results section (Page 9, first paragraph), and extended the discussion regarding this issue.

Extract from text with suggested changes indicated:
‘Socio-demographic characteristics of study participants are given in Table 1. Key variables are compared to the general population in Västerbotten and Sweden, respectively, in Table 2.’ (Page 9, second paragraph).

‘The strengths of this study are the large population-based sample, which enhances the external validity of the study findings, the inclusion of expectant fathers, and also that couples could be identified. The pregnant women had a similar mean age and mean BMI at enrolment as the general pregnancy population in Västerbotten and Sweden [10, 75]. The proportion of overweight expectant fathers was also consistent with previous reports for Swedish men aged 30-44 years [43]. However, study participants had on average a higher level of education than the general population aged 25-44 in Västerbotten and Sweden, and were less likely to be born outside Sweden. Previous studies have shown that low socio-economic groups are over-represented among non-respondents in population-based studies [76, 77], and these groups are also more likely to have poorer lifestyle and health than respondents [76]. This phenomenon is also likely in this study given the difference in educational status between the study population and the general population in Västerbotten and Sweden. Information on BMI was missing for 14.3% of pregnant women and 4.7% of expectant fathers, resulting in exclusion from analyses. We found that the excluded women were younger, with a significantly lower
proportion having a university degree, and they were significantly more often in employment compared to the whole sample of women. Correspondingly, the excluded expectant fathers contained a significantly lower proportion with a university degree and were significantly less likely to be in employment.’ (Page 15, first paragraph).

4. In addition to a general underestimation of BMI from self-reported weight and height, studies have documented differential bias in BMI including by SES, race, gender, and weight (in US samples). You reference a Norwegian study and the lack of differential bias (prevalence altered, but not exposure-outcome associations). Please specify what outcome they were studying and comment on why you think their findings apply to your study.

REPLY:
This is a relevant comment. We have realised that the Norwegian study is not suitable as a reference in this context, as the exposure-outcome associations were not fully comparable to those in focus in this study. We have therefore removed this citation from the manuscript, and instead elaborated the discussion about potential differential selection.

Please find an extract from the text with suggested changes indicated under comment number 3.

5. The statistical analysis should account for within-couple clustering.

REPLY:
The analysis of likelihood of overweight and obesity in expectant parents for different combinations of socio-demographic risk factors (Table 7, previously Table 6) has now been revised to account for within-couple clustering as suggested by the reviewer. The odds ratios and their confidence intervals have been adjusted accordingly. The methods section has been extended to include information on how this analysis was done.

Extract from text:
‘Because a correlation of BMI within couples was found, an exchangeable correlation structure was assumed and the parameters were estimated with generalised estimating equations.’ (Page 8, first paragraph).

6. Currently, the analysis focuses on: 1) the prevalence of overweight and obesity among men and women in the study, 2) that weight is correlated within couples, and 3) that weight is associated with SES. There is not as much new in these findings as there could be, given your data. It would be particularly interesting to show us how SES corresponds to those associations. You reference mating studies, which have shown that SES may influence the co-occurrence of overweight/obesity within couples in potentially important ways. For example, higher-SES, higher-BMI men may marry lower BMI women as compared to lower-SES higher-BMI men. Such a pattern could concentrate overweight/obesity in lower-SES populations if the risk to the child is greater from two (as compared to one) overweight parent.

REPLY:
We agree that additional analyses on how SES may influence the co-occurrence of overweight/obesity within couples would have been valuable. However, we hesitate to present further results in the already extensive results section with its seven tables in this paper. The suggested analyses will be considered in forthcoming papers from the Salut Programme.
research group.

7. **Stratification by parity and years of cohabitation may yield important results.** This comment is important. We agree that data on parity and years of cohabitation would have been highly valuable. However, such data are not available and we are therefore unable to present this information.

8. **You refer to 'interaction analysis' in your Methods section, but it is not clear what those analyses included. Please provide more specific information, and explain why you dichotomized variables for your interaction analysis.**

REPLY:
We extended the interaction analyses to also include categorical variables with more than two levels, and the result revealed no interactions. We therefore removed the sentence in brackets ‘(all variables dichotomised)’ (Page 8, first paragraph). The interaction analyses were performed by adding cross-product terms in the regression models.

9. **Given the high prevalence of overweight/obesity, it would be more useful to see prevalence estimates (and ratios). Odds ratios do not provide a good estimate of prevalence ratios with these outcomes.**

REPLY:
The reviewer has a point in that prevalence estimates would be valuable to present in this study. We have therefore added prevalence estimates in Table 6. However, we suggest keeping the odds ratios because our experience is that this is a more common way of presenting findings, and thus, we believe that odds ratios are easier for the reader to comprehend given the nature of the data included in this study. We have discussed the reviewer’s comment with two experienced statisticians who support our decision.

**Discretionary revisions**
1. **I suggest one decimal point for BMI.**

REPLY:
Changes are now made in text where appropriate. We have also removed one decimal point for prevalence, Confidence Intervals, and Odds Ratios, except where the Odds Ratio was close to 1.0.

2. **I would suggest writing '<0.001' rather than .000 for p-values.**

We have now changed .000 to <0.001 for p-values as suggested.