Author's response to reviews

Title: Impact of singlehood during pregnancy on dietary intake and birth outcomes- a study in the Norwegian Mother and Child Cohort Study

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Author's response to reviews: see over
Dear editor, BMC Pregnancy and Childbirth.

Thank you for considering our paper "Impact of singlehood during pregnancy on dietary intake and birth outcomes- a study in the Norwegian Mother and Child Cohort Study " (MS: 3554453141414721) for publication in BMC Pregnancy and Childbirth.

We are pleased that the reviewers have a positive opinion of our work, and want to thank them for their constructive comments. We have carefully considered their comments and changed the manuscript accordingly. Changes to the manuscript have been highlighted in red font. In the following we provide the answers to how the comments have been addressed.

Reviewer 1, Jane Willcox

Reviewer's report:

Thank you for the opportunity to review this paper. It is pleasing to see a paper focusing on singlehood in pregnancy. I make the following comments for consideration in the review of this paper.

Major compulsory revisions

1. Research question and aims. I found it difficult to align the two different aims and lines of analyses in the paper.
   a. Comparison of dietary intake
   b. Comparison of birth and foetal outcomes

   These seemed to me to be two separate lines of enquiry only linked the adjustment for energy and protein in one adjusted analyses. I wonder if it would serve the paper better to solely focus on nutrition related adjustments to link the first and the second analyses or drop the second one altogether. They may even fit well as two separate papers, each with its own interesting story. The links between the two lines of inquiry needed to be discussed comprehensively in the method and discussion to tie it together.

   Answer: Previous studies have examined either marital status or maternal diet in relation to pregnancy outcomes, and the aim of this study was therefore to examine the impact of marital status on pregnancy outcome taking maternal diet into account. Your suggestion of focusing mainly on nutrition related adjustment is actually what is shown in Model 1 in Table 4. We decided to include also other adjustment variables because diet is not independent of other lifestyle variables (smokers have poorer diet than non-smokers and both smoking and diet differ with socio-economic status etc). Instead, we have edited the introduction and discussion and we trust that the two lines of enquiry have been tied better together.
Minor essential revisions

1. The document changes in tense throughout. Please check for consistency.
Answer: This has been amended.

2. Line 134. Why did you choose p<0.100?
Answer: We chose p<0.100 for inclusion of potential confounders because p<0.1 is commonly used as a cut-off in this context. Alternatively, also p<0.150 and p<0.250 have been used, but due to the large study sample we decided on p<0.100.

3. Continue to use your abbreviations eg singles living along through the document to make it less wordy
Answer: The author instructions states that abbreviations should be used as sparingly as possible, but we introduced abbreviations for the three marital groups [“singles living with parents” (SP), “singles living alone” (SA) and “Married/cohabiting” (M/C)] in the first version of our manuscript. However, although abbreviations do save some words, we decided during the revision that we would use the abbreviations only in the tables. If however, the editor or reviewers prefer the use of these abbreviations, we will gladly change this.

4. Often the discussion section repeats large amounts of the results. Start the discussion with a brief summary of the results then discuss the salient points.
Answer: The discussion has been rewritten and we have made an effort to avoid repetition. It has been substantially shortened and made more focused.

5. Tables 2, 3. I wonder if it would read better if the p values were in the column next to the data they represented?
Answer: We followed this suggestion for Tables 2 and 3. However, only one column was moved as the data needed to be presented before the p-values. In addition, to make Table 3 less comprehensive we deleted the columns describing mean and SD.

Discretionary revisions

1. Line 73 “fetched” maybe reword to “collected”?
2. Line 142 reword
3. Line 147 reword
4. You know the data well. I would have loved some more suggestions for future research.

Answer:
1. Line 73 “Fetched” has been reworded to “collected”.
2. Line 142 has been reworded as follows: “The single groups were younger, and had lower education and income than the married/cohabiting group”.
3. Line 147 has been reworded as follows: “There were also major differences between the two single groups, with singles living alone being a more heterogeneous group than singles living with parents (Table 1).”
4. It is challenging to give suggestions for future research. Intervention trials are costly and difficult (one example is the LIMIT trial in Australia, described by Dodd et al., BMJ 2014). Hence, our primary suggestions for future research is to shed more light on singlehood in pregnancy would be qualitative studies exploring i) women’s health beliefs and behavior regarding their own and their children’s health, ii) women’s perceived facilitators and barriers to health behavior change and iii) exploring women’s sense of coherence (single living, cohabiting).

6. In the tables could you please highlight the significant values?

Answer: We have considered replacing the p-values in Tables 1-3 with symbols denoting statistical significance. Due to multiple comparisons we decided to include the actual p-values, making it possible for the reader to judge the magnitude of the reported differences.

Reviewer 2, Anne Britt Vika Nilsen

Reviewer's report:

The article is interesting, and has an important message to those who work with lifestyle changes and health improvement. The authors could address more specifically, what was previously known and what does this study add?

Answer: Thank you, we hope the revisions based on your comments and those of the other reviewer have clarified these questions.

- Major Compulsory Revisions

1) The discussion could be shorter and more clear and concise and more focused, the main finding was difference in the overall diet with regard to marital status. A recent study using data from the MoBa may be of interest to the authors: Henriksen, R.E., T. Torsheim, and F. Thuen, Loneliness, social integration and consumption of sugar-containing beverages: testing the social baseline theory. PLoS One, 2014. 9(8): p. e104421.

Answer: Thank you for the interesting reference. This reference has now been cited both in the introduction and the discussion.

Furthermore, all reviewers suggested revising the discussion and in we have now substantially shorted and rewritten the discussion in order to make it more concise and focused. As suggested, the main findings are described in the first part of the discussion.

2) The conclusion in the manuscript might go beyond what is supported by the data, the conclusion in the abstract is more precise. May be the two last sentences could be under clinical implications? (Dietary changes and smoking cessation have low cost………) Thus smoking was not the included in the aim of the study, and the relationship between smoking and some adverse pregnancy outcomes are well known.

Answer: Thank you for pointing this out. We have moved the two last sentences to the last part of the discussion and elaborated this part. We also included a reference pointing out that maternal smoking, poor gestational nutrition and low pre-pregnancy weight are the most important modifiable risk factors for foetal growth restriction in developed countries.

- Minor Essential Revisions
1) The inclusion of participants is clear and concise – figure 1 is fine. There is a large group of participants in the MoBa without marital status (n=1092). Larger than the two groups investigated (Single living alone n=909 and single living with parents n=218), have the authors tested the outcomes in this group?

Answer: Thank you; this has been addressed in the methods and results. The following paragraph has been added to the last part of the results: “In a sensitivity analysis we included women with missing or inconsistent information on marital status in a ‘missing marital information group’. Comparison of age, education, smoking status, parity and the prevalence of SGA, LGA and preterm delivery in this group and the three groups married/cohabiting, singles living alone and singles living with parents, showed that the missing group comprised women from all three groups. Relative to the referent group (married/cohabiting), the missing group was not associated with any pregnancy outcomes (data not shown). Furthermore, including the missing group in the analysis of marital status versus pregnancy outcomes did not change the associations reported in Table 4”

2) Reference number 22 is should be more exact.

Answer: We have amended this reference.

3) The article has many references; may some of them be removed? Which are most important in relation to the context?

Answer: Yes, during the revision a number of references have been removed or replaced. While there were 60 references in the originally submitted manuscript, the number is now 43.

4) Table1. Do the authors have any suggestion how to simplify the massive table? Why decimals in the age categories?

(I suggest: Maternal age (years), 20-24, 25-29, 30-34 and #35.
For example; Income (NOK), and only the numbers below, the numbers are missing three zeros (150 =>150,000), and maybe some of the categories could be collapsed?
And: ‘Nausa (week XX)’, remove no, ‘Has felt depressed’, remove no).
Maybe the authors could mark those data which indicate mean and SD? Or change the order n or mean, % or SD.

Answer: We have changed the categories for maternal age as suggested, corrected the numbers for income and collapsed some categories. We also took out as suggested the redundant lines for nausea and depression.
In Table 1 we have changed the order as suggested: n or mean, % or SD. In Table 3 mean and SD is no longer shown, only median and the 5 and 95 percentiles.

6) Discussion (paragraph 6): A previous study reported that women participating in the MoBa had more age-related reproductive and physical health problems… this is not correct citation (reference number 59). The women, participating in the MoBa, giving birth at an advanced or very advanced age who had more age-related reproductive and physical health problems and that they composed of a heterogenous group, including those who are vulnerable (low
education, unemployed, low income and single status). And these groups were underrepresented in the MoBa, as is stated by the authors (reference number 60).

Answer: Thank you for clarifying this. The sentence has been corrected and now reads: “A previous MoBa study reported that women giving birth to their first baby at an advanced or very advanced age compose a heterogeneous group characterized by either socioeconomic prosperity or vulnerability. Single status was among the socio-demographic factors correlated with giving birth at an advanced age [40].”

Reviewer 3, Sharon Kirkpatrick

Reviewer's report

Major revisions

1. Although the authors note that many pregnant women do not have optimal diets, this is also true for the population at large. It would be useful to make a stronger case for examining diet in pregnancy, particularly among single women. Overall, the discussion is well written but the introduction could use a stronger conceptual framework in order to allow readers to better follow the logic and the modeling. Perhaps consider moving some material from the discussion to the introduction to provide a stronger rationale - what is it about being a single mother that might correlate with poor diets and birth outcomes?

Answer: Thank you for raising this point. The introduction has been extensively revised and we trust that the conceptual framework for our study is now more clearly conveyed. Furthermore, all reviewers suggested revising the discussion and in we have now substantiallyshorted and rewritten the discussion in order to make it more concise and focused.

2. It seems that the diets of those living with parents are also not ideal. Isn't it then true that all pregnant women require particular attention with respect to dietary intakes and other factors associated with birth outcomes?

Answer: Yes, we agree that all pregnant women require particular attention with respect to dietary intakes and other factors associated with birth outcomes. However, we conclude from the current study that “single women” is a particular vulnerable subgroup.

3. Lines 33-39: There is a bit of a jump in logic here. Is diet associated with SGA, LGA, preterm? Need to link diet to birth weight and then show links with later health.

Answer: The introduction has been extensively revised and we hope the link between maternal diet and SGA, LGA and preterm have been made clearer.

4. Lines 42-44: Has the higher risk of these conditions been seen among the single parents themselves or among others in the household? Is this likely to reflect other correlates of living in a single parent home, such as SES?

Answer: The statement referred to single status independent of whether or not there were children in the household. However, this part of the introduction has been substantially revised and this topic is now in the first paragraph of the introduction. The link between marital status and SES has also been addressed in the discussion.
Minor revisions:

5. Lines 33-34: Could be more specific here – which dietary patterns and foods?
Answer: The sentence has been revised, but we considered that describing more details of specific foods and diets shown to be associated with pregnancy outcomes would become too extensive.

6. Lines 40-42: Has the population also grown over this time? Has the proportion of children born to single mothers increased?
Answer: The population has grown but still the proportion of children born to single mothers has increased. However, this sentence has been changed. In 1994 9% of all children were born to single mothers whereas in 2013 the figure was 13%.

7. Lines 60-61: Were all pregnant women approached? If not, how were women to be invited to participate identified?
Answer: Recruitment took place in more than 50 hospitals in all parts of the country, and nearly all pregnant women were invited. Some university hospitals with their own ongoing studies were the only exceptions. As written in the methods, women were recruited to the study through a postal invitation in connection with their first routine ultrasound control at week 17-18 of pregnancy. This ultrasound is offered free of charge to all women.

8. Line 101: Please define SGA, LGA, preterm.
Answer: SGA, LGA and preterm have been defined. The following sentences have been added to the methods section: “The pregnancy outcomes included in the present study were a) small for gestational age (SGA), b) large for gestational age (LGA) and c) preterm delivery. The variables SGA and LGA were calculated from the 10th percentile and 90th percentile of birth weight within gestational week for nulliparous and multiparous pregnancies respectively. Preterm delivery was defined as pregnancies with gestational length shorter than 37 complete weeks.”

9. Lines 102-108: Please indicate whether the operationalization of variables in consistent with prior research.
Answer: Yes, the operationalization of variables is consistent with prior research in MoBa as well as in other cohorts, e.g. The Danish National Birth Cohort cited in our paper, ref 16.

10. Line 109: Please provide a rationale for the other variables considered. Are these variables meant to account for systematic differences among the groups or to account for known confounding factors?
Answer: These variables were identified as potential confounding variables.

11. Abstract – not clear which is the referent group.
Answer: The abstract has been revised and the reference group should now be clear.
Other changes: we have changed the spelling of the group denoting women who were married or living with a partner from “married/cohabitants” to “married/cohabiting”.

Again we want to thank Jane Willcox, Anne Britt Vika Nilsen and Sharon Kirkpatrick for their valuable comments. We believe that your comments and the subsequent revisions have improved the manuscript, and we hope that it will now be acceptable for publication.

Yours sincerely, on behalf of the authors,

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