Author's response to reviews

Title: Socioeconomic disparities in prepregnancy BMI and impact on maternal and neonatal Outcomes and postpartum weight retention: the EFHL longitudinal birth cohort study

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Author's response to reviews: see over
“Socioeconomic disparities in prepregnancy BMI and impact on maternal and neonatal outcomes and postpartum weight retention: the EFHL longitudinal birth cohort study” by Ng, Cameron, Hills, McClure, and Scuffham

We wish to thank the reviewers for their very helpful comments. We have addressed all the comments and our responses are listed below.

Response to Reviewer 1:

Major compulsory revisions:

1. In the background, from line 19, the obesity problem is ‘easily’ explained as a result of adverse behavioral factors such as lack of knowledge, poor diet and inactivity. This is probably partly true, but I would expect more references to this statements and one of the references is also quite old? And one could also change the ‘is a result’ to have been shown to…..

   In this revised manuscript, we have replaced the old reference and added new latest references to the statements. We have also revised the sentence “… is a result…” as suggested by the reviewer (lines 19-21).

2. The recruitment found place at 24 weeks of gestation. This made the comparisons of the baseline characteristics shown in table 1 troublesome. This is explained by in text from line 135: “Moreover, the percentage of low birthweight infants (<2500g) was approximately half that of babies born in the general population as prospective mothers were recruited towards the end of the third trimester. For the same reason, our sample did not include any infants born before 28 weeks gestation, and had a smaller proportion of infants born between 28 and 36 weeks gestation. Moreover, the percentage of stillbirths was smaller than in the general population”. And it is also mentioned as a limitation of the study. But when you read the table you anyway get the impression of big differences and my question is if it could be an idea to compare the two samples by including only children after gestational week 24 also in the sample of the deliveries in the region? Alternatively, make this limitation more visible, also in the table.

   We now compare the two samples in Table 1 by including only children with gestation of 28 weeks or more in the sample of the deliveries in the study region. The comparison between our cohort and the deliveries in the study region is now more visible. The results showed that our cohort include fewer mothers under 20 years of age, and fewer low birth-weight and low gestational age babies than in the general population. This implies that mothers who are at risk for childbirth complications or adverse birth outcomes could be underrepresented in our sample. We have revised in the manuscript (lines 143-149 and 270-276) according to these new results.
3. **Representativity:** The sample is recruited from an area representing 30% of Queensland’s population. Is the population representative for the rest of Queensland, or Australia according social representativity? The authors could also be more detailed in the description of the consequences of the relatively low participation rate – 43.8 % (=2254 women)- with the response rate being lower when including those who answered the BMI variable (=2009) and at 1 year follow up (1316 women).

In this revised manuscript, we have discussed the representativity of our sample and added a new reference from Queensland Health for the health profile in Queensland (lines 51-53). Moreover, we have described the potential consequences of the relatively low participation rate in the Discussion section (lines 290-293).

4. **Results:** From line 143: …’These women and their partners were also more likely to be unemployed (p=0.038 and p=0.017)….' When reading the table 2 it looks as if it is the opposite – among the underweight women 16% are unemployed and among the overweight 9.9 % is unemployed and 10.3 among the obese women.

We have corrected this mistake in the revised manuscript. In particular, the findings observed from Table 2 have been updated in the Results section (lines 152-158) and the Discussion section (lines 233-235). The differences in baseline characteristics between the prepregnancy BMI groups are now more visible.

5. **The conclusion of the study focuses on the need for interventions…,** but this is not really the conclusion of this study. The conclusion in the abstract seems more precise to show the essence of this particular study.

We have added a sentence in the Conclusions section (lines 306-307) to highlight the contributions of our findings in this study to the evidence-base concerning the predisposition of obesity prepregnancy on adverse maternal and perinatal outcomes.

**Minor essential revisions:**

6. **Line 115.** (and also line 126) A clarification of what variables were adjusted for in the text would make this part easier to read.

In this revised manuscript, we have clarified in lines 125-128 which variables were adjusted for in the logistic regression models considered in Tables 3 and 5.
Response to Reviewer 2:

Minor essential revisions:

1. Introduction (line7): The authors should qualify the association between maternal obesity and preterm delivery as only induced/iatrogenic preterm delivery is associated with maternal obesity.

   We have clarified this point by indicating that obesity leads to induced preterm delivery (lines 6-7) and by replacing Reference [5] by Bhattacharya et al.’s paper published in BMC Public Health.

2. The introduction should end with a justification of why this study was needed.

   We have revised the last paragraph of the Introduction session (lines 30-35 and 38-42) to highlight the strength of the present study and to justify why this study was needed.

3. Methods: The authors should make clear that pre-pregnancy BMI was self reported while the postnatal weight was measured. I am a little concerned that at least some of the postnatal weight retention may be artificial due to a reporting bias. Is it possible to validate at least part of the data using case notes?

   Both pre-pregnancy BMI and the postnatal weight were self-reported. We have clarified this in the Methods section (lines 77 and 84-90). Potential bias in self-reported prepregnancy BMI and postpartum weight has been discussed in the text (lines 283-288). Because of this potential bias, we considered using the highest weight retention quintile, instead of the postpartum weight retention raw data, in the identification of risk factors that are associated with high postpartum weight retention (Table 5).

   We have information on maternal weight at birth from hospital perinatal records for both Gold Coast and Logan hospitals. It is therefore possible to validate part of the self-reported prepregnancy weight data and ensure that they are not mistakenly reported as the maternal weight at birth.

4. A flow diagram of loss to follow up of the cohort would be useful to assess attrition bias, although there was a good proportion of responses at the 1 year follow up.

   We have added a flow diagram of recruitment and loss to follow-up of the cohort in Figure 1; see also lines 70-71. The discussion concerning attrition bias has been provided in the text (lines 288-293).
5. The discussion addresses most limitations. It might be useful to discuss the effects of adjusting for variables that could essentially be path variables and the impact of residual confounding.

We have added discussion on the potential bias arisen from the adjustment of path variables in a model (lines 264-267 and a new reference [43]) and the impact of residual confounding (lines 280-282).