Reviewer's report

Title: Do parental heights influence pregnancy length?: A Population-based prospective study, HUNT 2

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Reviewer: Alkistis Skalkidou

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Comments

The authors use data from a population-based study in Norway (HUNT2) linked with Register data in order to examine the association of parental height with pregnancy length and risk for pre- and post-term pregnancy.

Major Compulsory Revisions

Abstract

1. The sentence in the Results
   “Shorter women …. had higher risk of pre-term delivery” is not entirely correct, as the p-value for the association when EDD is based on LMP is 0.32.

2. I believe that the adjustment for cardiovascular risk factors in the analyses must be stressed in the abstract, for it is this which is unique about the study design.

Introduction

3. The authors state that fetal growth has been shown to influence pregnancy length, but do not later adjust for it in the analyses, nor do they comment on the possible ways to do that.

4. The second paragraph introduces the hypothesis that unfavorable cardiovascular risk factors might underlie a possible association between maternal height and pregnancy length. In the analyses, later, the authors adjust for such factors in the third model used. Has the possibility of an interaction between height and cardiovascular profile been considered? Have you considered analysing using a multi-level approach or path analysis?

5. The authors might want to check the exact positioning of the references, for example reference nr 6.

Methods

6. It is understandable that the HUNT2 study participation rates and results are presented elsewhere. What the reader does need to know, on the other side, is whether a non-response analysis was made (participation rate at 70%) and in
that case, if there is something that would bias the results in this sub-study.

7. On page 5, the authors state that “… singleton births were recorded” and then “Among these births, 1022 were excluded either due to multiple pregnancies (n=252)….” Do the authors mean that there were 252 multiple pregnancies that resulted in singleton births?

8. The authors present multivariate analyses results both for all births as well as only for spontaneous deliveries only what pregnancy length is concerned. It would be very useful for the readers to also see results for spontaneous deliveries for pre- and post-term pregnancy risk. Bearing in mind that clinical routines influence inductions, and thereafter rates of post-term deliveries, as well as the lack of statistical significance in the association between maternal height and risk of pre-term delivery when LPM is used, the reader is lacking this separate analysis. Please include it.

9. As far as the possible confounders that were adjusted for in the models, could the authors provide an explanation about the lack of including spontaneous onset of delivery in Model 2? The authors might have thought about stratifying, like they did in Table 4. But this should be clearly stated. Short women were shown to have a higher rate of elective caesarean delivery. Could the authors comment on this and why this could be a possible confounder or effect modifier?

Results

10. No information is provided about the extra tables, additional tables 1 and 2. Why are these analyses performed and what do they contribute to the results? Please expand.

11. Table 2. The authors did not comment on how they chose 163 cm as a cut-off point.

12. Table 2. The readers could benefit from a measure of statistical significance of the presented associations (no information on p-value and statistical tests used when the authors discuss the results)

13. The description of some Table 2 results are misleading. Short mothers do not have a higher rate of medically induced deliveries, but do not higher rates of elective and acute caesarean deliveries. Please rephrase. This difference in rates of elective caesarean deliveries has not at all been discussed and considered in the following analyses, despite the fact that it affects rates of post-term deliveries and pregnancy length (please describe guidelines for planning of elective caesarean deliveries in Norway during the study period. At week 38 or 39?) Please discuss in the discuss the possible reasons for this association. Was it more due to humanitary reasons (anxiety for vaginal delivery, depression?) Mothers with depression are reported to have shorter duration of gestation and are at risk for pre-term labor. Do the author have such information in the HUNT 2 data that they can include as a confounder?

14. The definition of SGA involves the use of duration of gestation as this is
calculated from the EDD derived from US examination. The authors very well describe possible problems arising from the introduction of systematic misclassification of gestational length from US examination, but do not do so in the case of SGA. Please expand even here. How could this bias influence rates of SGA in short vs long mothers?

15. Table 6 analyses should be repeated for deliveries after spontaneous onset of labour, both because of the higher rate of elective cesareans among short women, and because of the systematic misclassification due to US guided definition of gestational duration. The fact that clinical induction protocols are based on the US derived EDD leads to biased definition of post-term pregnancies. The inclusion of just spontaneous deliveries could partly help overcoming this.

16. Another possible strategy which might help eliminating the systematic misclassification caused by US determination of EDD is analysing just those pregnancies without mismatch between the US derived EDD and the LPM derived EDD (ie, +2 days)

Discussion

17. The readers might benefit more if the plausible explanation between the observed results between LPM and US derived EDD were presented directly after the first discussion in the paragraph (now included as paragraph nr 4 AND Implications of these findings). Much of the information included under “implication of these findings” should be changed to the beginning of the discussion, along with information on the misclassification issue in paragraph nr4. A Swedish study by Skalkidou et al, Epidemiology 2010 is discussed (“A large population based study in Sweden reported that to replace LMP…. Pg 13”) but not included in the references.

18. The difference in gestational length based on LMP derived data is no more than 2 days. This is being discussed in paragraph nr 5. According to LMP, there was no difference in rates of pre-term births. Could the authors discuss other possible bias in their findings, ie. could it be that shorter women have shorter/longer cycles or irregular cycles which is not being considered when determining EDD by LMP and Naegel’s rule and could explain this 2-day difference?

19. The sentence on pg 10 “Both methods have limitations, but the fact that robust positive associations were obtained by both methods” is not true when considering pre-term deliveries by LMP in Table 6. The same might be true if the analyses are repeated on for spontaneous onset. Please rephrase.

20. Bearing in mind the above mentioned bias introduced by the US examination, which the authors explain in the discussion, the reader might expect the other studies discussed to be presented in the light of clinical routines around EDD in the country-time period performed. How was the EDD determined in each of those studies?
21. When discussing the parental contribution in pregnancy length, the authors might want to consider including references on correlates of intrauterine growth, which affects gestational length and is affected much more by maternal IGF levels, while growth after delivery is affected also by paternal genes.

Minor Essential Revisions

The use of the English language could be improved. For example, on pg. 13 “the rate of post-term pregnancies was reduced more among short women….” or on pg. 10 “LMP method is limited by uncertain maternal memory…..”, pg. 9 “associations with pregnancy length for unfavourable pre-pregnancy levels of common cardiovascular risk factors…..”

Discretionary Revisions

The authors use this study design combining data from the HUNT2 study and the MBR in order to control for metabolic and other known risk factors on an individual basis. Otherwise, a more plausible design would be to use population-based data from MBR. I would suggest that the authors stress this in their work.

One usually sets the group considered as “normal” or “not as risk” as the Reference category. In this study, short women, they ones considered at risk, are presented as the Reference category. The authors might want to consider this.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:

I Declare that I have no competing interests