Reviewer’s report

Title: The Association between Gestational Weight Gain Z-Score and Stillbirth: A Case-Control Study

Version: 1 Date: 14 Nov 2018

Reviewer: Lisa Bodnar

Reviewer's report:

This manuscript describes a secondary data analysis of the SCRN (Stillbirth Collaborative Research Network) case-control study that seeks to understand the role of gestational weight gain in the etiology of stillbirth at 20 weeks or more. The authors found that low gestational weight gain z-score (a measure of weight gain that is standardized for gestational age) is associated with an increased risk of stillbirth. There is a need for high-quality research exploring this research question, especially to inform future evidence-based gestational weight gain guidelines. I have several major concerns about the manuscript:

1. The authors evaluate total gestational weight gain at delivery with stillbirth risk, but very minimally address the serious problem of reverse causality (that the death of the fetus reduces subsequent gestational weight gain). They perform a sensitivity analysis limiting the exposure measure to weight gain at the last prenatal visit, but this doesn't capture weight gain before the fetal death (or perhaps it does if the measurement occurs while the fetus was still alive, but the authors do not explore this). This is a serious limitation because this bias would inevitably lead to lower total weight gain among pregnancies that end up with a fetal death, which is what the authors report.

2. A major advantage of the SCRN data is the rigorous phenotyping of stillbirth cause. Evaluating gestational weight gain relative to cause-specific stillbirth would be an important contribution to the knowledge base, but the authors do not explore this. Further, they do not separate stillbirths according to early or late stillbirths, which is another way of looking at different etiologies of stillbirth.
3. The literature review is incomplete. At least one methodologically-advanced paper is in the literature on gestational weight gain and stillbirth that is not cited. This paper provides a strong approach that addresses many of the limitation of the current analysis. Although the present paper may not be able to use the same approach, it will guide the authors in a more nuanced approach to their own paper.

4. The authors apply two different gestational weight gain z-score charts to their data. However, the mean z-score using the normal weight woman standard only was well above 0 SD, which suggests a suboptimal fit. The authors should explore other analytic approaches to determine the appropriateness of the charts to their data. I don't agree with their conclusions in lines 282-285 that the findings are robust to the choice of standard. The higher risk associated with excessive weight gain is unique to the FGLS standard.

5. Confounders included in the final model are not listed in the text. This is a major omission.

Minor concerns:

1. The rationale for excluding underweight women, women with preexisting diabetes, and women delivering twins. Gestational weight gain z-scores exist for all of these groups (even twins), and the exclusion of women with diabetes but not hypertension before conception is unjustified.

2. Measures of variance of gestational weight gain (absolute gain and z-scores) should be shown in the abstract, tables, and all results text.

3. Line 100 in the introduction is redundant with the earlier statement about overweight and obesity, and is unclear why it's needed.

4. The 4 studies the authors cite on gestational weight gain and stillbirth should be cited directly in the introduction, not the systematic review.

5. It is more appropriate to state that the IOM committee to reevaluate gestational weight gain guidelines committee asked for research with stillbirth as the major endpoint.

6. Lines 113-114 should be referenced.

7. The introduction should be shortened.

8. The text on the exposure measure should be shortened. Z-scores are intuitive to calculate.
9. P-values should be removed from Table 1. Significance testing is not appropriate. Please see the recent American Statistical Association statement on the use of p-values.

10. The sensitivity analyses require n's for the reader to evaluate the appropriateness and value of the approaches and results.

11. The mean gestational age is needed to be presented along with the mean gestational weight gain for cases and controls. I imagine these are very different and should be presented for the reader to evaluate weight gain appropriately.

12. Table 3 does not present results and should be eliminated. These values should be presented as a footnote to relevant tables or figures.

13. The z-scores are shown as 40-week equivalents, and results are interpreted in this regard. However, the vast majority of stillbirths do not deliver at term. Therefore, this is not helpful.

14. The evaluation of effect modification on the multiplicative scale (rather than the additive scale) is not justified.

15. The major exposure based on weight and height, but lines 137-138 do not make it explicit where these variables are ascertained from. It is just stated "medical records and the maternal interview". If maternal interview, when did the interview occur? Were these self-reported or measured?

16. Z-scores, if appropriate for the population, represent percentiles of a distribution. Thus, it is unclear why the authors don't present their results as z-scores. The use of percentiles makes this much more confusing and harder to compare to existing literature.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

No
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Acceptable

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal