Author’s response to reviews

Title: An evaluation of whether a gestational weight gain of 5 to 9 kg for obese women optimizes maternal and neonatal health risks.

Authors:

Abaigeal Thompson (abaigeal.thompson01@utrgv.edu)

James Thompson (jthompson@cvm.tamu.edu)

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Author’s response to reviews:

Sirlei Morais (Reviewer 1)

I feel very happy to congratulate the authors for the excellent and important text, which is very well written, very well structured and with excellent statistical analysis. Here are some suggestions for improvements, as I see it. There are some methods described in the results, which are unnecessary.

In the 2 paragraph you write "Table 3 presents the baseline ....", and still in the same paragraph "An odds ratio of" - these concepts must already be known to the readers and are unnecessary - besides being methods and not results. Withdraw

Author response: Table 3 is introduced in this way because it is not a simple “intercept” to the model – it is the actual baseline value. With this value and the odds ratios, readers could convert risks to other forms (like additive risk vs. odds ratios). The unnecessary description of odds ratios has been deleted.

In the results you have 3 paragraphs in a row starting with "For women", change this, because the results can be look likes a statistical report.
Author response: We are interpreting this suggestion to mean that the 3 nearly identical paragraph introductions look too technical – We agree and have modified the sentences. If we missed the intent of the comment, please elaborate.

Table 1 is totally unnecessary since it is a worldwide concept, and should only come as a reference in the methods.

Author response: We have been concerned about too many tables and we are happy to delete Table 1 and renumber the other tables.

In tables 4 and 5, I suggest to show, within each obese level, the ones that have the best chance (among the three levels of weight gain) of each results, in order to align with tables 6 and 7.

Author response: We have completed this suggestion.

To conclude, I strongly recommend the publication of the manuscript

Author response: thanks for the suggestions

Edson Martinez (Reviewer 2)

The article aims to evaluate whether a five to nine kg weight gain, for obese women, optimizes a set of maternal and neonatal health outcomes, using the national vital statistics database. However, the weak point of the article is that it put an overemphasis on the Bayesian modelling, and this is not the main contribution of the study. From a theoretical statistical perspective, the presented Bayesian model is not innovative and the authors should give more emphasis to the importance of the results for the health area. Therefore, I think the authors should remove the expression "A Bayesian interaction model for evaluating" from the title of the manuscript, since
this expression makes the reader think that a new statistical model is introduced to describe the relationship between the variables.

Author response: We agree – the author providing the statistics (JAT) is often overzealous. The title has been changed.

Considering that my expertise lies in Bayesian methods in health, I have the following additional considerations:

1.) In the Results section, it is unclear when the posterior predictive p values were used (as informed in the last paragraph of the Methods section).

Author response: this has been clarified in the methods. “The posterior predictive p value was used to evaluate all odds ratios with a p-value < 0.05 referred to as statistically significant (12).”

2.) The Methods section suggests that it was used MCMC simulation methods, but it is unclear in the manuscript (there are some vague comments just in the discussion section).

Author response: This has been clarified in the methods. “The implementation used Markov Chain Monte Carlo (MCMC) and the software OpenBUGS 3.2.3 (11).”

3.) Please provide information about the prior distributions used in the Bayesian analysis.

Author response: This was provided (too cryptically, I suppose, in the appendix). It is now also in the methods. “The logit of the rate parameter was then provided a vague normal prior (Rijk) with a mean = 0 and variance = 100, for each combination of GWG and ppBMI.

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\text{Logit}(\mu_{ijk}) = Rijk
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4.) Why posterior medians instead of posterior means? I suppose that the posterior probability densities are skew, but this should be clear in the manuscript.

Author response: This is a good question. When the author performing the modeling (JAT) first moved to Bayesian (clinical) statistics exclusively, he brought a preference to use the distribution mean. After a few years he changed to using median, exclusively. The reasons are multiple. First, if the distribution is not skewed, the median is the same (at least as good) as the mean for reporting the middle of the posterior distribution. When the distribution is skewed, the median is preferred. Thus, there is no disadvantage to using median. Second, most Bayesians including the author are likely estimating the full posterior distributions as opposed to 1 or more summary statistics describing the posterior distribution and this can be lost if one presents the mean. Third, skewness may occur due to scaling. For example, the priors of this model, are updated in the logit scale – they are not skewed under visual evaluation of the full posterior. Interestingly, the odds ratios, estimated on the log scale are also not skewed with a visual evaluation of a plot of the full posterior distribution. However a changing of scale can introduce skewness but when using the median, exclusively, there is no need to introduce this confusion.

Author response: Having said all this, we do not object to presenting the mean instead of the median but we would sure have a lot of explaining to do for other clinicians for whom we insisted on using the median. We note that the reviewer did not ask us to change to the mean so we stayed with our preference. We will change if we are asked to. We are very grateful to have received a quality review from someone who performs Bayesian analyses in health sciences.