Author’s response to reviews

Title: Influence of weight gain, according to Institute of Medicine 2009 recommendation, on spontaneous preterm delivery in twin pregnancies

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To
Editor-in-Chief
BMC Pregnancy and Childbirth

RE: Ms. No. PRCH-D-16-00410R3 - Influence of weight gain, according to Institute of Medicine 2009 recommendation, on spontaneous preterm labor in twin pregnancies

We thank the Editor and the Reviewer for their comments that gave us the possibility to improve our manuscript.

Listed below is a point-by-point response to the comments, and attached are the files of the revised version of the manuscript, which we hope you find suitable for publication in BMC Pregnancy and Childbirth.
Best regards,

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Technical Comments
Comment #1: Author contributions section: Please use initials to identify individual authors rather than full names.

Response to comment #1: Initials rather than full names have been used to identify individual authors within the author contributions section.

Editorial comments
Comment #1: Many thanks for your revision of your manuscript and for your patience during the peer review process. As you will see, the reviewer has indicated that a few outstanding points remain which can be addressed by making appropriate changes to the manuscript text and including some additional details. We urge you to address these points to the best of your ability during the next revision, since unfortunately we will not be able to allow a further round of revision. If you need more time to perform your revision, please do let us know and we can arrange that.

BMC Pregnancy and Childbirth operates a policy of open peer review, which means that you will be able to see the names of the reviewers who provided the reports via the online peer review system. We encourage you to also view the reports there, via the action links on the left-hand side of the page, to see the names of the reviewers.
Response to comment #1: We thank the Editor for his understanding about our concerns due to delay in the peer review process, and for providing a quick answer to our last request for information.

Attached is our point-by-point response to the comments, provided to the best of our ability, as suggested by the Editor.

We are happy the Editor has set the present one as the last round of revision. This is because in every round of revision the Reviewer #2 has raised new comments that were not present in the previous round, and most of them were not related to our responses. Moreover, we have thoroughly addressed them due to our strong interest to publish our work in a high rank journal as we consider BMC Pregnancy and Childbirth.

In addition, there is a point on which we would like to recall the Editor's attention and that we will underline in the response to the Reviewer's comments as well. Reviewer #2 argues that other authors have published similar data compared to those contained in our manuscript. The Reviewer is admitting, at the same time, that our study "was planned and conducted before these other studies were published". We have advised, in all our communications to the editorial board, during the last two years, that such a long peer review process would affect our chance to publish our work, since several research groups have been very active in the field. At the moment, we acknowledge that other works have been published on the subject but we decided neither to cite them, nor to discuss them, since this would mean to change all the body of our manuscript. And we do not feel, for the sake of truth, that this would be fair towards the researchers who participated to our research project and contributed to make it original.

For this reason, as stated also in the response to the Reviewer's comments, within the revised version of the manuscript we will modify the sentences regarding the originality of our study but will not change the body and the structure of our work. This because we understand that, even though no other work is a duplicate of our project, it is inevitable that a two-years peer review affects the originality of a research.

We are sure that the Editorial Board will positively consider our availability and patience in waiting the extremely long peer review process and addressing all comments and will allow us, as we requested within our last mail to the editorial board, to have a final answer within a short amount of time.

Reviewers’ Comments

Comment #1: While this topic remains important, and there are strengths to the approach, my support for this manuscript remains limited. I appreciate the authors' efforts to respond to my suggestions. Concerns remain in both scientific and writing aspects of this study.
Response to comment #1: We thank the Reviewer for his comment and for acknowledging 1) the importance of the topic; 2) the efforts put by the authors in addressing all suggestions; and 3) the strengths of the approach.

The Reviewer is reporting that scientific concerns remain, moreover he just complimented with the authors for improving the manuscript according to the suggestions provided during the two previous round of review. Indeed, as we see from the present comments, none of the Reviewer's previous scientific concerns have been carried in the present round of revision or referred as inadequately addressed.

We feel that, excluding those concerns related to writing that will be discussed within comment #7, the remaining issues raised by the Reviewer regarding our manuscript, are minor revision comments that might improve the readability of our manuscript, and that we will be happy to address, rather than scientific concerns.

Comment #2: One of the premises of this paper is that outcomes for twin pregnancies, specifically preterm delivery. However, preterm delivery was examined in:


It is understandable that this study was planned and conducted before these other studies were published, but they need to be acknowledged.

Response to comment #2: The Reviewer is absolutely right when stating that our study "was planned and conducted before these other studies were published". Indeed, during the last two years, we were very concerned about the risk carried by such a long peer review process in terms of affecting the originality of our work.

At the moment, we do acknowledge that other works have been published on the subject but we decided neither to cite them, nor to discuss them, since this would mean to change all the body of our manuscript. And we do not feel, for the sake of truth, that this would be fair towards the researchers who participated to the research project and contributed to make it original, since the length of the peer review process is not depending on the authors, especially if they provided response to the Reviewers' comments in a very short time, as our group did, and as we are doing in this occasion as well. But again, since we cannot ignore that other contributions have been
published on the subject in the last two years, we have revised the paragraph regarding the originality of our study, as reported within the response to comment #3.

However, and of great interest, even if other studies where published, none of them is to be considered a duplicate of our study. We have thoroughly evaluated the references reported by the Reviewer with the following results:

a) Compared to the study published by Ozcan et al in 2017 (Assessing weight gain by the 2009 Institute of Medicine Guidelines and perinatal outcomes in twin pregnancy), we analyzed our population according to gestational weight gain, without a classification on pregestational BMI. As already explained in the previous revision rounds and within the Discussion section, considering that IOM recommendations were already pBMI correlated, we simplified our analysis and divided our population considering if patients met, exceeded, or presented lower gain weight according to pBMI IOM cut-offs, without performing further stratification of the study groups. The rationale for it was to make the influence of weight gain on preterm delivery clearer and useful in clinical practice;

b) Compared to the study published by Pettit et al in 2016 (Maternal and neonatal outcomes in women with twin pregnancies with excessive gestational weight gain), our work presented a different analysis. Specifically Pettit et al. compared adequate vs. excessive weight gain, although in their study an 18% of women with inadequate gestational weight gain is included. In contrast we did not exclude this group of women, contributing to study obstetric outcomes also in patients with a low weight gain in pregnancy.

Comment #3: Therefore, the conclusion that, "The present study represents, to the best of our knowledge, the first report evaluating if and how weight gain according to pre-gestational BMI, proposed by IOM revised guidelines, has an influence on the development of spontaneous PD in twin pregnancies," is problematic in two ways. First, a quick search identified several studies that examined weight gain in twin pregnancies and PD. Second, this study only examines "if," not "how" (i.e., there is not an examination of mechanisms).

Response to comment #3: We thank the Reviewer for his comment.

As the Reviewer for sure knows since he gave comments during all revision rounds, our manuscript was submitted on May 2016. In the last months we have requested several times to allow us a more expedite peer review, in order to save the originality of our work, being conscious that many research groups were very active in the fields and that, the more the time was passing, the higher was the risk to affect the originality of our research and its chance to be published, that was a raising concerns of the authors and myself.
In this sense, the Reviewer is right when suggesting that our conclusion has become problematic. For this reason on page #13 of the revised version of the manuscript, the paragraph Strengths and limitations of the study has been rephrased and now reads, "The novelty of our work was to evaluate if there is an effect of IOM guidelines on the development of spontaneous PD in a cohort of twins at both term and preterm".

Comment #4: It would be helpful to include the rates of induction and c-section in the manuscript itself, not just in the response to reviewers, so that all readers may understand this potential for bias and how this may impact generalizability to their own setting. I am still concerned about bias, if these procedures are associated with pBMI and/or gestational weight gain.

Response to comment #4: We thank the Reviewer for his comment and included the rates of induction of labor and cesarean section on page #6 of the revised version of the manuscript that now reads "Exclusion criteria of our study were induction of labor (15% of all twin pregnancies at our Institution), elective cesarean section (4% of all twin pregnancies at our Institution), monoamniotic twins, intrauterine demise, fetal malformations, twin-to-twin transfusion syndrome, and gestational age at delivery < 28 weeks".

The Reviewer raised his comment regarding the potential bias introduced by induced labor and elective cesarean section within the previous round of revision. We are attaching the answer previously provided to comment #3 of the second round of revision "We decided to set induced labor and elective cesarean section as exclusion criteria in order to avoid a potential source of bias". This concept is included on page #6, within the second paragraph of the Materials section of the revised version of the manuscript.

Comment #5: End of second paragraph of materials: it would be helpful to be more specific and state that there are not IOM recommendations for underweight patients pregnant with twins, because there are for singletons, which makes this sentence confusing as written.

Response to comment #5: We agree with the Reviewer and better clarified this point according to his suggestion, on page #6 of the revised version of the manuscript that now reads "Patients with pBMI< 18.5 were also excluded since there are no IOM recommendations for underweight patients in case of multiple pregnancies".

Comment #6: Table 3: why not include actual p-values (as in other tables) instead of "ns"?
Response to comment #6: We accept the Reviewer's comment and included actual p-values instead of "ns" within Table 3.

Comment #7: Writing weaknesses. Continued awkward writing: "overweighted" and "cuts-off"; Short (1 to 2 sentence) paragraphs remain; Abbreviations should be spelled out on first use (INES, ACOG); Significant digits (hundredths of grams in weight implies greater precision than seems likely in these measurements); References have multiple errors in formatting

Response to comment #7: We thank the Reviewer for his comments regarding the writing of our manuscript that will improve its readability. Changes and corrections were incorporated, if needed, and highlighted within the revised version of the manuscript.