Reviewer’s report

Title: Effects of a lifestyle intervention during pregnancy to prevent excessive gestational weight gain in routine care - the cluster-randomised GeliS trial

Version: 1 Date: 20 Sep 2018

Reviewer: Christine Marie Olson

Reviewer's report:

Overall, the authors have done a very good job in responding to the reviewers' recommendations and comments, especially related to the process evaluation. This version of the manuscript demonstrates a high-level of responsiveness to reviewers. One consequence of this responsiveness is that the DISCUSSION section has gotten quite long and frankly, seems a bit rambling. I would recommend a very thorough editing and tightening up of this section.

One of the points that may get lost in the long DISCUSSION is that the authors' major explanation for the null finding is the quality of the intervention as delivered. The authors expand on this to say the intervention was not based on behavioral science principles and it was not delivered by a dietary expert. They make a reasonable case for both of these. But from my experience and understanding of the literature on public health interventions, it is quite common for "scaled-up" projects to be unsuccessful in achieving the same results as the pilot project. I wonder if there are some key findings in this area that could be included here. The challenge of "going to scale" is a common theme in nutrition interventions.

My other comments are more specific and minor:

1) In the ABSTRACT, the authors indicate 10 regions in Bavaria were included and in the body of the manuscript they say 5 were included. Please be consistent.

2) Line 85 on page 6 - I don't think "pairwise" is needed in this sentence since it is already stated that "paired cluster randomization" was done.

3) Line 141 on page 8 - "Protocoling" is not the right word to use here.

4) Lines 187-191 on page 10 and line 210 on this same page - The primary results that are presented exclude the 132 preterm births which means the sample size for women is 1885, as stated clearly in the cited lines. On line 210, it says that data are included for 2017 women. To me, this seems misleading. There are 2,261 women included in Table 1 and 1,885 in Table 2. Nowhere in this paper are data presented for 2017 women.

5) Line 230 on page 11—A result is presented on the intra-class correlation coefficient in the text. If it is going to be presented, something more needs to be said so the meaning is clearer to the reader.
6) Lines 382-386 on page 17 - I don't see how recruiting more nulliparous women into the trial explains an imbalance between the treatment groups. I'd suggest deleting this statement. You had an imbalance and you adjusted for it in the analysis. That seems to me to be a sufficient explanation.

7) Lines 406-409 on page 18 - Reference 26 seems to me to be about an intervention that is integrated into routine prenatal care. It is in the reference list, but not cited or discussed here. That seems to be an omission.

Authors’ responses to comments from Reviewers 1 and 4: Overall, I think the authors have responded appropriately and effectively addressed each comment. Reviewer 4 has some philosophical differences with the approach to the study that the authors have taken so the authors say more in their response to reviewers than they do in the manuscript. But nonetheless, I think the authors have responded appropriately.

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

Yes

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

Yes

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Yes

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