Reviewer's report

Title: Knowledge translation tool to improve pregnant women's knowledge of gestational weight gain and the risks of gaining outside recommendations: a non-randomized intervention study

Version: 1 Date: 30 December 2014

Reviewer: Christine Olson

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Discretionary Revisions
1. "Knowledge translation tool intervention" seems like too many words stuck together in a phrase. Is there any way to shorten (maybe omit intervention) or change the words around to make the reading easier. I especially noticed this in the Abstract.

2. Lines 327-330 – Face-to-face is more successful than what?

Major Compulsory Revisions
The major revisions that must be made to this manuscript relate to the lack of detail in the description of the methods. The lack of detail makes it hard to evaluate the quality of the research and the conclusions of the authors.

1. A clear and accurate description of the intervention itself and how it was implemented needs to be added to this manuscript. The description in the manuscript, as currently written, has the potential to be misleading. For example, in lines 118-120, the intervention is described as "an interactive web site" "designed to facilitate discussions regarding GWG between women and their health care providers." Then on lines 148 – 151, we learn that clinic staff who enrolled women into the study asked women their pre-pregnancy weights and heights and entered them into the web site. The staff then printed off two copies of the "Me and My Baby" weight gain chart and handed one to the woman and put the other in the medical record. "Health care providers were instructed to plot women’s weights on the print-out of the knowledge translation tool at each visit and to discuss their GWG progress."

This minimal description raises many questions: Could women log into the interactive web site on their own? Were there additional components to the intervention beyond the “Me and My Baby” single sheet of paper with the weight gain tracker? What is the justification for creating an interactive web site when this intervention as described could be done quite simply with pre-printed “Me and My Baby” sheets, one for each BMI group?

2. A clearer explication of the timing of the data collection activities including the collection of the primary outcome and also in relation to the exposure to the intervention needs to be included. As this reviewer understands what is currently written in the manuscript, the data for the knowledge outcome measures are
collected in the baseline survey that is completed by intervention participants between the time of entry and 20 weeks gestation. Women enter at an average of 17.2 weeks gestation (line 210). This raises several questions: How many health care visits did intervention women have with health care providers before the outcome was assessed in the baseline survey? Or what proportion of women actually had at least one health care visit in which the tool was discussed before they were assessed for the outcome? In order to attribute the differences the authors see in their outcomes, to the intervention, there has to be some minimal exposure to the intervention in some proportion of the women in the intervention group.

3. In the statistical analysis section (lines 187 – 198), no mention is made of including interaction terms for the interaction between the intervention and characteristics of the women. That would seem to be an important question for determining whether the intervention was more effective in some sub-groups of women than it was in others.

In addition, there are other revisions that should be made that will improve the manuscript.

4. In lines 366 – 370, there is only a very small number of limitations stated and, from my perspective, some of the most important limitations are not discussed. For example, to use the weight gain tracking tool, the initial BMI categorization and the weight gain goal is based on self-reported data. This is a well-known problem in obesity research and the error introduced by lack of measurement varies with pre-pregnancy BMI.

In addition, the authors give very little notice to any potential missing data problem. They state several times that they use complete case analysis and give the impression that this solves any missing data problem. Greater transparency about missing data is needed in the manuscript so the reader can judge whether it is a problem or not. How much data are missing? Are there differences in missing data by characteristics of the women?

5. Line 305 and other places in the manuscript – The authors make the claim that the women in the intervention group “had better knowledge of GWG”, but they don’t actually directly measure knowledge of GWG. They measure whether the women report receiving counseling from their HCP and then “reported GWG recommendation by HCP” (not clear what this is). There are some strange findings for this variable that are not discussed in the manuscript also in that the proportion who reported within the IOM guidelines in intervention and control are very similar (51.6% and 48.4%).

6. Tables 3a – 3e report unadjusted and adjusted intervention effects. The symbols and footnote particularly for the Adjusted Odds Ratio column for the variables that are not in the model are confusing. Also in Tables 3c, for example, for a covariate that is significant in the unadjusted model and therefore included in the adjusted model, all Odds Ratios and 95% CI should be shown, even if they are not significant. Overall, it seems there must be a clearer and more efficient
way to present the results in these tables.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests.