Reviewer’s report

Title: Dietary variables associated with substantial postpartum weight retention at 1-year among women with GDM pregnancy

Version: 0 Date: 01 Dec 2016

Reviewer: Deirdre Kay Tobias

Reviewer's report:

This manuscript evaluated dietary factors at 6 wks postpartum in relation to 1-yr postpartum weight retention. Women in the highest category of soda intake had a significantly greater odds of postpartum weight retention, compared with women with the lowest intake. While the study population and data collection is quite strong, there are some concerns with the analysis and presentation of the results. The title implies that "soda consumption" was the objective of the study, but this is not the case - dietary factors overall were intended to be evaluated, not just soda. I think that with substantial reworking of the results and organization this manuscript would be quite strong and have a public health message to support efforts to minimize sugary beverage intake in postpartum women.

Major comments:

- The title should not declare the results, but rather state the aim of the analysis. Similarly, the abstract and results focus entirely on soda, presumably since this was the one significant finding.

- The hypothesis of the analysis is not at all clear - it states that the aim was to evaluate which dietary factors were associated with weight retention. The introduction should provide a better background as to why specific dietary factors were hypothesized to be related, from an a priori perspective.

- It is unconventional to see Table 1 stratified by the outcome itself for a prospective cohort study. For epidemiologic studies, Table 1 typically depicts the cohort stratified by exposure status, to get a better understanding of the potential confounding structure.

- If these are characteristics at baseline, how could a mother have had 10 months of breastfeeding? Please clarify the time frame for these covariates.
- Table 2 is confusing and simple crude statistics of the dietary exposure by outcome status are not normally how these results should be presented. If you are intending to show the results for the associations of the various dietary factors with the outcome status (weight retention), then I suggest presenting all age- and multivariable-adjusted models for each dietary factor with the outcome. These crude statistics can be highly confounded and thus misleading.

- I am not sure why only soda was analyzed in Table 3? Did you cherry pick the factors that were significant in Table 2? It seems as though your hypothesis was to investigate all dietary factors, but only soda was highlighted because it was significant. Models 1-3 for all factors, replacing Table 2, would be the most appropriate approach to present results, in my opinion. Further, confounding for some exposures can make a null crude relationship become significant, so choosing factors based on their crude association may miss important components.

- Stratified analyses for soda by GWG could be an additional supplemental table or in-text result. P for interaction should be included in the stratified analyses.

- Is it true that selection of which dietary factors to evaluate was based on their correlations in a validity study of the FFQ? Is this appropriate? It seems that selection of which dietary factors to evaluate should be driven by having a specific scientific hypothesis, rather than picking factors that might perform better statistically.

- Is there a specific hypothesis why carrots, broccoli, etc would have unique relationships with body weight, or could all vegetables be combined into a single food group?

- Looking at the crude numbers in Table 2, it seems very odd that soda would be statistically significant but whole grains have a p=0.6. The contrast in groups for whole grains seems much more striking. Please verify these p-values.

- The multivariable models for Soda did not include any other dietary factors. Several dietary factors related to weight can be correlated with soda and may need to be accounted for to avoid residual confounding.

- The discussion focuses entirely on soda, but does not explain why null results were seen (in the crude analyses presented only) for several dietary factors that have been previously shown to benefit body weight. This requires an explanation in the discussion.
Minor comments:

- Please do not begin a sentence with a numerical value.


- The introduction states that there is only one previous RCT among GDM women. I suggest reading the following systematic review for additional RCTs: Matern Child Health J. 2016 Jul 19. "Appreciating Recent Motherhood and Culture: A Systematic Review of Multimodal Postpartum Lifestyle Interventions to Reduce Diabetes Risk in Women with Prior Gestational Diabetes." Jones EJ1, Fraley HE2, Mazzawi J2.

- In the methods, page 6, "which was compared to women who had <5kg of weight retention"…this phrasing does not make sense, given this is the outcome, not the exposure, and you don't typically compare cases vs. controls, but rather exposed vs. unexposed in prospective studies like yours.

- Case counts can be moved to the results section rather than the methods section of the manuscript.

- A sensitivity analysis excluding women with type 2 diabetes diagnosed during the 1 year follow-up may be interesting, particularly since the introduction hypothesizes weight gain as an intermediate of T2D progression. Reverse causation may be a concern among women with T2D diagnosed prior to the 1 year visit.

- Avoid the term "effects" in observational studies.

- Was non-caffeinated soda included in this study? Mothers often receive advice to avoid excess caffeine while breastfeeding, so there may be higher than normal caffeine free beverage intake in this population, leading to some measurement error if ignored.
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.

Unable to assess

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:

Needs some language corrections before being published

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