Author’s response to reviews

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

Authors:

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Author’s response to reviews:

Below is a point-by-point response to each of the reviewer’s comments.

Reviewer #1: This paper makes use of a good data set, and asks a pertinent question. However, it feels as though the whole things has been retro-fitted around the one significant finding (the association between the consumption of soda and post-partum weight retention (PPWR). The background section does not detail why soda might be a particular issue for PPWR, and the aim of the study is stated as being "to evaluate which dietary variables … predict PPWR … in women with gestational diabetes". And yet the title and discussion are entirely focused on soda and SSB consumption.

We greatly appreciate this suggestion. The aim of the paper was to identify what available dietary variables were linked to PPWR. We have modified the entire manuscript to be more comprehensive and match the aim of the study. We have included analyses of all food and beverage variables measured and both soda and fried food intake were linked to substantial PPWR. We have revised the abstract, introduction, results, and discussion to reflect this change.

I feel that the paper needs to be refocused - please provide more background for the rationale for looking at the chosen food and drink categories, and discuss all the results more fully - including the lack of associations for other foods.

Agree, and we have refocused the entire manuscript to be more comprehensive as suggested.
At the moment it is not clear which food variables have been analyzed and why. This list given in the methods (page 8, lines 17-29) is not the same as that presented in table 2. The variables presented in table 2 represent a sub-set of the food variables collected in PrimeScreen. The methods claim that variables were selected if the validation study (reference 19) showed reasonable comparability with a semi-quantitative FFQ. But looking at the results of the validation shows that there were other food variables with equally good comparability that are not included here (e.g. whole milk dairy, low-fat dairy, eggs, margarine, pasta, fish/seafood, processed meats and deep fried foods). Why are these variables left out? The last two in particular maybe important for weight retention.

We appreciate this suggestion and have included analyses of ALL food variables from the PrimeScreen. Chi-square analyses also found significant differences between weight retention categories and whole eggs (P=0.03), processed meats (P=0.03), and fried foods (P=0.0005). Therefore, logistic regressions were also run with these variables and the only two variables that remained significantly related to substantial postpartum weight retention were high soda intake and high fried food intake. We have revised the entire manuscript to reflect this change.

It would also be interesting to know if and how the consumption diet variables relate to each other.

We ran spearman correlations between the different dietary variables and found that soda intake was highly correlated with fried foods (r=0.12; p=0.0004) and have adjusted all logistic models for one another. These two variables were also the only two diet variables that were related to PPWR after controlling for covariates.

There does not seem to be any measure of socio-economic status - this has the potential to be a confounding factor in the association observed, and should be discussed as a limitation of the study.

We apologize for this oversight. There were significant difference in maternal education level and WIC benefit eligible between PPWR groups (P=0.0003 and P=0.0008) and we have added these variables to Table 1 as suggested. Given these variables are highly correlated, we controlled for maternal education level in all subsequent logistic regression models. We have modified Table 3 accordingly.
What is the justification for stratifying by gestational weight gain? Is there an a priori reason for this? Was the interaction tested first? It seems like GWG may be a confounder rather than an effect modifier.

We have removed stratification by GWG, as we agree that was somewhat tangent from the original aims. However, given that GWG is a known predictor of PPWR we have included it as a covariate in logistic regression models.

Minor edits:

Page 3: Line 16/17: "Women who were overweight or obese pre-gestational…" should be "pre-gestationally" or "pre-gestation" Line 16/17: "...as well as those with excessive gestational weight gain are leading risk factor for developing GDM" needs re-wording.

We have reworded this sentence.

Line 33/34: the abbreviation GWG has not been defined

Agree, we have added the definition to the sentence above.

Page 5: Lines 43-46: What is the Women's Infant and Children special supplemental nutrition program?

We apologize for this mistake, it now reads “special supplemental nutrition program for Women, Infants, and Children (WIC)”

Page 6: Infant feeding: the first sentence is a bit odd - saying "breast feeding intensity and duration were assessed… by asking mothers to record the amount of formula fed using a weekly diary" maybe separate out the descriptions of breast and formula feeding?
We have modified this sentence to be easier to understand.

Page 8: Please give the original categories of regularity of consumption (of foods and drinks), as well as how these were collapsed.

As mentioned above, we have revised the manuscript to include analyses of all food groups in the PrimeScreen.

Page 9: Line 11/12 - This sentence should start " Differences in participant characteristics between weight retention categories…"

We have made this change.

Page 10: Line 33/34: Should be "…at baseline had a 71% increase in the odds of having substantial PPWR."

We have made this change.

Page 11: Line 50/51: "Moreover, growing evidence to shows…" remove the "to".

Done.

Table 2: The categories used for food consumption are not inclusive - what happens to people who eat 5-6 servings per week?

The response category on PrimeScreen was actually “nearly daily or daily”, which we simplified to ≥1 serv/d, but you are absolutely correct that this response category was also intended to capture those who consumed 5-6 serv/wk. In addition, there was one additional response
category, which was “twice per day”, but the frequencies were so low in this category, that response was combined with the $\geq 1$ serv/d. However, we will revise this category to be $\geq 5$ serv/wk.

Reviewer 2: 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.

Reviewer 1 had a very similar comment and we have revised the entire manuscript to reflect that all available dietary variables measured were examined.

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.

We agree and the title has been changed. We have also revised the entire manuscript to reflect this.

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.

We agree, and have revised the hypothesis to be more inclusive of other dietary variables that may have affected PPWR. We have also reworked the introduction.
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 we were to stratify the table by all of the independent dietary variables, this table would be huge. We wanted to assess what if any characteristics (demographics, infant feeding, GWG) were significantly different between PPWR groups so that we could then include those variables as covariates in subsequent models.

If these are characteristics at baseline, how could a mother have had 10 months of breast feeding? Please clarify the time frame for these covariates.

We apologize for this confusion. The infant feeding was assessed throughout the 1-year postpartum period and then the transition to DM was also assessed at 1-year. We have modified the table as suggested.

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.

We respectfully disagree. Performing the crude statistics helps us see if dietary variables are related to PPWR and if significant then we wanted to test to see if this relationship remains significant after adjusting for other potential confounding variables. There were significant differences between weight retention categories and whole eggs (P=0.03), processed meats (P=0.03), fried foods (P=0.0005) and soda intake (P=0.005), therefore logistic regressions were run to see if these variables predicted PPWR independent of known confounders.

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.

As mentioned above, we have analyzed the relationship of all measured dietary variables with PPWR and only soda and fried food intake was significantly related after controlling for confounders. We have updated Tables 2 and 3 to reflect this change. In addition we have modified the title, abstract, background, results, and discussion to reflect this change.

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.

We apologize for not including the interaction term in the original manuscript, as it was in fact significant. However, we feel that stratifying by GWG was somewhat tangent from the original aims of this paper, thus we have removed this from this version of the manuscript. Given the strong relationship of GWG with PPWR, we will control for GWG in all logistic regression models.

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.

We completely agree and have examined the affect of ALL available dietary variables from the PrimeScreen on PPWR and modified the entire manuscript accordingly.

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?

We analyzed all vegetable and fruit categories as they were measured on the PrimeScreen, which included dark leafy vegetables, cruciferous vegetables, carrots, and other starchy vegetables. The
way the PrimeScreen was developed it would be impossible to combine the vegetable and/or fruit servings into a composite score. We could calculate the percent of participants who reported any vegetable intake or any fruit intake in frequency categories, but this would not reflect the sum. In addition, the PrimeScreen was not validated to examine the data this way. Therefore, we have left the vegetable and fruit categories separate.

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.

We double-checked the analyses with whole grains and it is not significant ($P=0.52$). We also decided to list the column percentages (instead of the row percentages), that way the differences between the groups is more obvious. For whole grains, 46.7% of normal PPWR consumed whole grains $\geq$5 more time per week compared to 51% of those subjects with excessive PPWR.

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.

We appreciate this comment and did find a significant association with fried foods and soda intake and have included both in all adjusted models. We have added this to the manuscript as suggested.

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.

Our new findings show that both fried food and soda are linked to substantial PPWR and have revised our results and discussion to mention both of these. We have also included a brief discussion on the null results as well.

Please do not begin a sentence with a numerical value.
We have fixed this.


We have included this reference in our introduction as suggested.

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.

We appreciate this suggestion and have included more information on the other RCT that have been conducted.

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.

We have revised this section.

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

Case counts were moved to the results section as suggested.
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.

While we appreciate this suggestion, given our small numbers of subjects who consumed high amounts of fried foods and soda, excluding women with T2D at 1 year would reduce this sample size even further. We did however control for this as a confounder in the analyses and while the results were attenuated with soda intake, the results remained highly significant for fried foods.

Avoid the term "effects" in observational studies.

We have removed the term “effects” throughout as suggested.

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.

Yes, non-caffeinated soda was included in the soda intake. We have specified that in the method section.