Author’s response to reviews

Title: Seasonality, temperature and pregnancy oral glucose tolerance test results in Australia

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Author’s response to reviews:

Thank you to the reviewers for their detailed assessment of our manuscript. We have addressed each point as below:

Emmanuel Cosson (Reviewer 1): The reviewer thanks the authors for improvement of their manuscript, which is very interesting.

I have still the following major issue:
Why limiting to correlation Cord C measurements and temperature at birth? You could show us the mean (SD) of C-peptide and glucose values according to the seasons at birth (as in Table 2 for OGTT data). I asked for this in the previous review as I do think that it would reinforce (or not) your new results. I do not understand how these data could be unavailable, as you needed them to make the presented data.

ES: We have provided this data as table 6 as suggested and added text in the Results section to reflect this.

I noticed some minor issues:
Table 1:
SD should be written as a foot note, such as "data are mean (SD) or %"
I am not a statistician but how can a p be > 1?

ES: Footnotes added. Apologies for the statistical misrecording. All stats have been double checked with SPSS to ensure accuracy and validity.
Table 2 and 3
Idem a footnote would be preferred

ES: Footnotes added.

Table 3: global comparison is performed but no comparison between each season for 1h and 2h glucose values (when globally statistically different) was performed

ES: Seasonal comparison for 1h and 2h glucose values have been added as table 3. Statistical calculations performed with independent t-test with Bonferroni correction.

Line 137: prevalence of GDM based on fasting VPG tended to be greatest in the winter (p=0.055)

ES: We have commented on this in-text, although not statistically significant.

Table 4: why « % » after HbA1c?

ES: Removed.

Line 145: « to correlate » to be deleted

ES: Removed.

MARY Amoakoh-Coleman, MD, MPH, PhD (Reviewer 3): Very interesting concept to investigate

Statistical analysis for the paper is satisfactory, especially with the previous reviews done.

Of concern (needing clarity and consistency in write up) to me are:

1. What is the temperature in reference to? Is it average weather temperature during the time of the OGTT? If so this will be clustered for a month for participants taking the OGTT during that month. The clustering effect should then be factored in the analysis. If it temperature per time of test then the current analysis is fine.

DM: Temperature is by month of OGTT, we could potentially include the monthly clusters in the analysis, but I am not sure that would help. We have tried testing by hours of sunshine or mm of rain and those climatic factors did not make a difference.

2. What conclusions are drawn with the results is not clear to me. What is the rationale for this study and what recommendations are to be made with respect to the seasonality and the outcomes studied? This does not come out strongly in the Discussions. Beyond the fact that this seasonality for GDM has been reported but under-reporting during summer not established, why is this this study conducted.

ES: The rationale for this study is to potentially validate the findings of previous reports in this area using the HAPO cohort, which is unique as it is a large, centralised cohort with a high level of quality control. To add to this conversation, our data further addresses how this seasonal variation may affect neonatal parameters. Our conclusion is that seasonal variations do exist in GDM diagnosis, and while effect size is small, do correlate with previous findings. We found that this variation is further correlated in neonatal parameters. This may imply a need for seasonal adjustments to OGTT values, but
further research is needed given the small effect size and the post-hoc nature of this study. We have added to our discussion to better reflect this.