Author's response to reviews

Title: Physical activity, depressed mood and pregnancy worries in European pregnant women who are obese: results from the DALI study

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Reply to reviewers comments

*Physical activity, depressed mood and pregnancy worries in European pregnant women who are obese: results from the DALI study*

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Reviewer Sarah Holton

*General comments*

I think the use of ‘people first language’ throughout the paper would be appropriate. For example, ‘women with high BMI’ or ‘women who are obese’ instead of ‘obese women’.

We have tried to use ‘people first language’ as much as possible in the revised version of the paper. For example, we changed the title.

The term ‘lifestyle’ needs to be defined as it has been used differently throughout the paper (eg lines 60 and 65).

We have defined the term ‘lifestyle’ where appropriate and now used it consistently in the revised paper.

*Abstract*

Line 60: should ‘lifestyle’ be ‘physical activity’?

We have changed the text as suggested.

*Introduction*

Lines 64: define ‘sufficient’

We have deleted the word ‘Sufficient’ from this sentence, since in the next paragraph it is explained what sufficient physical activity in pregnancy is.

Line 65: what is meant by ‘lifestyle’ in this sentence?

We have deleted it from this sentence, since it is not needed, and only confused the sentence.

Line 78: delete ‘the’. Which ‘women’? please include more details eg country where this data was collected etc.

We have changed the text as suggested and added some information about the weight status of the study participants and about the country where the studies were conducted.

Line 79: define the terms ‘overweight’ and ‘obese’. Change ‘were’ to ‘have been’.

The definitions have been added to the sentence.

Line 84: state why and how obesity is a public health and personal issue.

We have extended the sentence and explained why it is a personal and public health issue.

Line 86: include ‘the’ before ‘factors’. Are you referring to all pregnant women in this sentence or just those with high BMI?
We have added ‘the’. We are referring to all pregnant women here, and tried to clarify that by adding ‘all’ before ‘pregnant women’.

Line 89: a reference needs to be included for the statement ‘people with depressive symptoms are likely to be less active’.

We have included a reference here.

Line 107: why would it be ‘reasonable to assume that obese pregnant women ... ’. Is this statement based on the existing evidence?

We have rephrased this sentence into: ‘Similar to other populations, obese pregnant women with worse mental health (i.e. depressed mood and pregnancy-related worries) might be less physically active, which may further increase their risk for adverse pregnancy outcomes [17].’

Methods

Line 131: please state why a pre-pregnancy BMI>29 kg/m² was the cut off for inclusion in the study. Is this based on a particular classification system? The WHO classification regards BMI #25 as overweight and #30 as obese. Also, please state how pre-pregnancy BMI was measured. Was it self-report?

We used this cut off for inclusion, because the risk of developing GDM increases significantly with this BMI. Since the risk of developing GDM and not BMI per se was relevant for the study, and inclusion of obese pregnant women is difficult, we chose to use this cut off and not the more commonly used BMI > 30, in order to increase the number of women eligible for the study. This BMI was based on self-reported prepregnancy weight and measured height. This information has been added to the text.

Line 138: Please state which language(s) the instruments were administered in? Was each instrument validated for use in each country included in the study?

The WHO-5 was validated for each language, the CWS was translated and not validated, except for English. This information has been added to the paper.

Results

Line 243: please state how many minutes women with a depressed mood spent in MVPA.

This information has been added in the sentence.

Discussion

Line 274: Please discuss how your finding that 27% of women have depression compares with the proportion of pregnant women of normal weight and women with high BMI of a similar age etc.

We have added some more discussion on this topic in the revised version.

Line 292: please state the country Claesson et al conducted their study.

This information has been added.

Line: the limitations and strengths of the study should include acknowledgement and explanation of the small sample size – overall and from individual countries; the limitations of the use of self-report weight
(see Gorber et al); why women with high BMI were not compared to women of normal weight; whether or not pre-pregnancy depression was assessed; a comparison of activity levels with those pre-pregnancy to determine if pregnancy is also a reason for decreased activity not just obesity (these may be areas for future research?).

We have added acknowledgement of the small sample size, self-reported data for BMI and lack of data on prepregnancy physical activity and mental health to the discussion.

Line 364: please discuss why ‘behavioural activation’ is the best treatment option. Also, please include more discussion of the ‘so what’ of clinical implications of the findings of the study in the Discussion.

We have deleted the sentence about behavioral activation in the revised manuscript, and have added more discussion on the implications of our findings.

Textual suggestions

Line 66: ‘on’ should be ‘of’ and delete ‘a’.
Line 71: insert ‘women’ in ‘These guidelines advise women to …’.
Line 80: Change ‘while’ to ‘yet’
Line 82: change ‘in’ to ‘of’
Line 92. The second sentence should start ‘most studies have either had a small …’.
Line 101: change ‘were’ to ‘have been’.
Line 90: delete ‘effectively’. After ‘in’ include ‘previously’ and change ‘persons’ to ‘people’.
Line 108: change ‘might’ to ‘may’
Line 114: insert ‘to’ in ‘this allows insight into the association’.
Line 126: change ‘from’ to ‘with women from’
Line 182: include ‘such’ in this sentence ie ‘aspects such as parity’.
Line 248: include ‘an’ in as an independent variable’.

We have changed all these sentences as suggested.
Reviewer Diane Ehlers

Major Compulsory Revisions
• While the introduction provides adequate justification for studying depression and worries in obese pregnant women, more specific detail may improve the significance of the study. For example, specific statistics related to activity levels in pregnant and overweight/obese pregnant women (lines 77-80) would be helpful. Additionally, specific reference to citation #17’s aim and take home message would be nice. The authors provide some general statements (lines 90-94), but a little more detail will provide the reader with a more vivid picture of the problem. I think more explicit reference to literature related to depressed mood during pregnancy and pregnancy related worries would also help. As written, the reader is required to make liberal deductions between depression and mood and anxiety etc. (lines 97-105) and pregnancy-related worries. Be explicit with terminology and literature review to make these connections.

We have restructured and rewritten parts of the introduction, and made the terminology more consistent and accurate according to what was reported in literature.

• Actigraph GT1M is not triaxial (lines 141-142). Also, please note use of multiple accelerometers as a limitation to the measurement of physical activity in this study. GT1M and GT3X are comparable when both are initialized to measure movement in the uniaxial mode (see Kaminsky & Ozemek, 2012). This is adequate rationale for use of both as long as uniaxial data were used in estimating activity. Please provide reliability/validity data on the actitrainer and its correlation with GT3X and GT1M (140-141).

The reviewer is completely correct and we have added this information to the paper.

• Reference #28 does not validate the Actigraph in pregnant women – it validates an Omron pedometer against the Actigraph. Please provide validity/reliability on these accelerometers in pregnant women (or justification as to why we would not expect activity estimates to be different in pregnant v. non-pregnant women) (lines 141-142).

Two studies assessed use of Actigraphs in pregnancy in laboratory conditions (Connolly et al. 2011; DiNallo et al. 2012). No validation of Actigraphs in pregnancy in free living conditions is available as yet. Therefore, we have deleted reference to validity of Actigraphs in the paper.

• Why was the right hip used? Are three days of data valid for the estimation of regular activity in pregnant women? General guidelines for accelerometry recommend 4+ days of data (3+ for older adults) to validly estimate activity. Please provide justification for right hip and 3 days guidelines (Lines 145-146).

The right hip was used since it is the standard place for wearing the Actigraph accelerometers for adults. Indeed, 4 or more days are often used. In the paper of Trost et al (2005) between 3 and 5 days are recommended. Given practical limitations we were not able to have the women wear the accelerometers for more days. We have provided justification of our choices to the paper.
Are Freedson cutpoints valid or widely applied in pregnant women (lines 149-152)? Please provide this information. Good that this was cited as a limitation.

Freedson cutpoints have not been validated in pregnancy, but have been used widely, also in this population. We have added some more information to the methods section about the cut points.

I would suggest looking at some of Kelly Evenson’s work, especially Evenson & Wen (2011), which uses US surveillance data from NHANES.

We are familiar with the work of Kelly Evenson, and have referred to a number of her papers.

Why was 480 minutes used as the cutoff for a valid day? General guidelines recommend at least 600 minutes per day. Please provide evidence-based justification for 480 minutes.

The cut off for a valid day was set at 480 minutes, since in our experience in pregnancy (especially at the end of pregnancy) many women do not reach the usual cutoff of 600 minutes per day. Too many participants have to be excluded then based on the 600 minutes criterion.

At the moment no evidence based justification for this cut point is available. However, we did not have to exclude many women because of it, and the average wear time was about 780 minutes/day. Therefore, for the purpose of this paper the lower cutoff of 480 minutes will not have influenced the results. However, since within the DALI study we will also analyze the accelerometer data at the end of pregnancy, using the same criterion also in this paper makes data within the whole DALI study more consistent.

In the study of Evenson & Terry (2009) in pregnancy, the same 480 minutes/day were used as cut off for a valid day.

Identify the exact factors considered in the model as confounders (Lines 213-216).

We have added which confounders were considered as confounders to the text.

Why were the WHO-5 and Worry Scale tested separately? There is some covariance between these scales (as indicated by correlation of 0.44 and results in Lines 235-236), so orthogonalizing them and entering them into the model together may provide a better indication of the unique contribution of each to women’s activity levels (Lines 206-207).

We think this is a very good suggestion and have reanalyzed the data with both mental health aspects in the model together. In essence, the results do not change, but it does indeed give a better indication of the unique contribution of depressed mood and pregnancy worries.

Provide citation for definition of confounding in lines 216-217.

We have added a reference to the text.

Please provide more detail on the statistics used to represent the linear regression results. It appears in Table 3 that you used odds ratios and mean difference. This is relatively confusing. Please provide more detail related to these statistics and your interpretation of the linear regression results with log transformed MVPA used as the dependent variable. Please be clear in lines 210-211 what was used as the dependent variable – log transformed minutes per day of MVPA or log transformed counts per minute?
We have added information on the dependent variable and how to interpret the results.

• Why wasn’t ethnicity considered as a possible confounder (Lines 253-255)? It was related to depressive mood status, so may need to be considered. Additionally, approximately half of the sample was pregnant before the current pregnancy. It might be interesting to see how this affects women’s MVPA (Table 1).

Ethnicity was considered as possible confounder, but did not appear to confound the relationship between physical activity and mental health in this study. Also parity did not influence this relationship.

Physical activity levels were not different between primiparous and multiparous women.

• Were any interactions tested? Given some of the relationships observed between covariates and depressive mood, there may be some important interactions among independent variables.

We have tested for interaction with age, ethnicity, parity, smoking, alcohol consumption, but no interactions were found. We have added this to the paper.

• Did you control for time in pregnancy? What was the range? Inclusion criteria include <20 weeks gestation, and this captures half of a pregnancy. There are so many changes that occur throughout a pregnancy, with some symptoms more prevalent during certain parts of a pregnancy compared to other parts. There are changes even just between the first and second trimesters.

We do agree that many changes occur within this time window in pregnancy, and therefore we checked whether controlling for gestational age was necessary in the analyses. However, it did not change the associations, and therefore we have not added it to the models presented in the paper. The range in gestational age was from 9 to 20 weeks.

• There are a lot of holes in relation to the reporting of the methods, statistical analyses, and results. I think some major revisions to the presentation of each section are needed in order for the reader to better understand, not only the methods and results, but the scientific underpinning of the analyses and study as a whole. Several variables were measured as part of this study. I would suggest making it very clear as to why these things were measured and how/why they were included in the analyses, followed by concise, but detailed reporting of results. As written, the methods and results are not extremely focused.

We have tried to make it more clear how we assessed confounding, and why the HAP A factors were added to the models. Also elsewhere, we added information on the approach.

• Make some of the wording throughout the manuscript more consistent. For example, the abstract and introduction reference “mental health status” but the first sentence of the Discussion uses “emotional well-being”.

We have checked the manuscript for consistency, and have used the term ‘mental health’ when both depression and pregnancy-worries were referred to, and otherwise used the specific terms ‘well-being’, ‘depressed mood’ and ‘pregnancy-related worries’.

• No measure of pregnancy-related symptoms was included. This is a limitation, as symptoms, such as morning sickness or fatigue, may have contributed to women’s low levels of physical activity. It is important to know the confounding influence of these factors.
We have acknowledged this in the discussion as possible limitation.

- No measure of pre-pregnancy physical activity (even by self-report). Research shows that pre-pregnancy physical activity is a major predictor of activity levels during pregnancy. This is an important covariate that may need controlling. This is a limitation of this study.

We have added this to the limitation section in the discussion.

- I think the limitations discussed in lines 346-352 are extremely important and really limit the significance of this research. I understand that the DALI study was an intervention; therefore, prospective observational work is not possible. However, due to the changes associated with pregnancy and their influence on activity levels, a cross-sectional view of activity at ~15 weeks gestation is less meaningful. I think if the authors can do a better job of emphasizing the significance of this research, despite this limitation, this study will have more importance to field.

We have emphasized the relevance of the study and the implications of the findings in the discussion.

- The writing is acceptable; however, I don’t feel that the authors elucidated the really important findings of this research in their discussion. I think that the results of this study could potentially be important for future research, but this take home message was not evident in the discussion as written. I think a more parsimonious, but comprehensive statistical model will help with this. Consider revising the statistical approach to better uncover the important variables driving the relationship between depressive mood and physical activity in obese pregnant women.

We have emphasized the relevance of the study and the implications of the findings in the discussion.

We feel the take home message is now much clearer in the revised version.

Minor Essential Revisions

- Include reference to research on physical activity and depression in pregnancy. Authors mention “mood” (line 91), but not depression. You should be able to use reference #7 for some of this.

We have added additional references.

- Define “mental health status” in line 111 and abstract. A parenthetical including “(i.e., depressed mood and pregnancy-related worries)” should be sufficient.

We have done as suggested.

- Were women 18 years and older or older than 18? Clarify this in lines 130-131.

The women were 18 years and older. This has been clarified.

- Spell out body mass index in first use (line 131). Why was a BMI of 29 chosen? Obese is defined as BMI of 30 or more. A BMI of 29 is certainly at risk of obesity, but provide evidence-based rationale for this inclusion criterion (line 131).

BMI has been spelled out. We used this cut off for inclusion, because the risk of developing GDM increases significantly with this BMI. Since the risk of developing GDM and not BMI per se was relevant for the study, and inclusion of obese pregnant women is difficult, we chose to use this cut off and not the more commonly used BMI > 30, in order to increase the number of women eligible for the study.

- Spell out International Association of the Diabetes and Pregnancy Study Groups in first use (line 134).
We have spelled this out in the revised manuscript.

- Include a space between 10 and mmol/l (line 135) for consistency.

We have changed as suggested.

- The lifestyle factors assessed – were these in reference to pre-pregnancy or current behaviors (Line 182)? This is clarified in Table 1, but add to Methods as well.

We have added this information to the text in the Methods section.

- Is HAPA validated in pregnant women (Line 190)?

This health behavior change theory has not been developed or validated for this target group specifically, but is a widely applied model applicable for many target groups. There is no evidence as yet showing that processes of behavior change are different in pregnant women.

- Add the word “to” after both instances of “pertained” in lines 193-194.

- Change “less” in Line 243 to “fewer” or simply “approximately 13”.

- Change MPVA in Line 255 to MVPA

- Line 294 – “use” to “used” and “less” to “fewer”

We have changed as suggested.

- Note small sample size and imbalanced group sizes as limitation – loose reference in lines 344 and 359-361. Explicitly note this limitation.

We have added this limitation to the text in the discussion, and deleted the references.

Discretionary Revisions

- Line 71 – change “preferable” to “preferably”

We have changed as suggested.

- Consider rewording sentence in lines 79-82. The use of a semicolon and the word “yet” in line 80 (as opposed to “while”) may help.

The sentence has been reworded as suggested.

- Provide an example or two of pregnancy-related complications (line 81).

- Provide justification as to why GDM is “most important” (lines 81-82). Why is GDM a more significant health concern when compared with other pregnancy-related health concerns, such as hypertension, preeclampsia, excessive weight gain?

- Be careful in lines 86-88 of using inactivity and low physical activity interchangeably. There are differences between determinants of inactivity versus determinants of physical activity. I would suggest changing “inactivity” in line 86 to simply “activity”.

- Sentence in lines 109-110 – add why. The evidence presented up to this point does not explicitly suggest why this might be.

- Consider referencing the most recent version of the Compendium (2011) (Line 155).

- Has the WHO-5 been used in pregnant women and is it a valid measure of depressed mood in pregnant women (Lines 161-167).
• Line 210 – “positively skewed”?
  We have changed as suggested.
• Remind the reader of the definition of “likely depression” in Line 233. <50 on WHO-5? Additionally, based on the WHO-5, is it valid to state this is likely depression, or simply as “depressed mood” (as discussed in the Methods section)?
  We have added the cut off again in the sentence as a reminder, and used the term ‘depressed mood’ here.
• Does score of <50 on WHO-5 signify “likely depression”? If previous data support this, please provide a reference. Otherwise, I would be careful using this term.
  We have replaced this term throughout the manuscript with ‘depressed mood’.
• Surprised that diabetes status and smoking behavior were not significantly different based upon depressive mood status. Address magnitude of these differences and possible implications.
  We have not taken up this suggestion, since it would confuse flow of the discussion, and distract from the main points. But we fully agree that it is surprising that these factors are not significantly different between the groups.