Author's response to reviews

Title: Maternal obesity and postpartum haemorrhage after vaginal and caesarean delivery among nulliparous women at term: a retrospective cohort study

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Version: 2 Date: 23 September 2012

Author's response to reviews: see over
To the editor 17 Sept 2012

Please find below a point by point response (highlighted) to the reviewers comments as requested. The manuscript has been revised accordingly.

Reviewer's report 1
Title: Maternal obesity and postpartum haemorrhage after vaginal and caesarean delivery among nulliparous women at term: a retrospective cohort study
Version: 1 Date: 4 September 2012
Reviewer: Sohinee S Bhattacharya

Reviewer's report:
Major compulsory revisions:
Nil
Minor essential revisions:
1. Please mention total number of women in each BMI category in the abstract. If word limit is a problem, the total n=11,363 can be omitted. Done

2. The authors conclude that PPH in overweight and obese women following vaginal delivery is due to uterine atony and recommend active management of third stage. However, they also mention that active management of third stage is the norm in their hospital, which implies that PPH occurred despite active management. Please clarify/amend.
   The reviewer is correct that active management is our standard of care and that PPH occurred despite this. To clarify this we have removed comment suggesting that active management should be used in obese women (and emphasised that this is the standard of care for all).

3. Using the same cut off value of 1000ml blood loss for both vaginal and caesarean delivery should be justified with reference. Many more caesarean deliveries would be expected to lose 1000 ml of blood than vaginal deliveries. Our data has indeed demonstrated that the percentage of women with EBL ≥1000mls is higher after CS than vaginal delivery. However, the physiological effects of blood loss (whatever the mode of delivery) have similar impact. In addition our comparisons for BMI and blood loss are within each group of mode of delivery.

4. BMI measured at booking visit is used as proxy for pre-pregnancy BMI. This can be justified provided all women booked before 16 weeks. It would be good to present the distribution of weeks of first ANC visit by BMI status. This information is unavailable.

5. Please discuss whether the tertiary nature of the setting is likely to have introduced bias in the study. This will not have introduced bias in the study but results may only be generalisable to similar populations. We have described the population which is studied, we can elaborate on this further if at editors request if necessary.
6. As I understand this retrospective cohort study used maternal overweight and obesity as exposure variables - how then, can BMI be a confounder in multivariable analyses?

We have reworded the methods section to remove the confusion over terminology. Multivariable logistic regression for major PPH was performed adjusting for potential confounders identified after an extensive literature review. The following variables of hypothesised interest or potential confounders were hence included in the model: BMI, maternal age; ethnicity; smoking; maternity care provider; antepartum haemorrhage; diabetes; hypertension; induction of labour; epidural anesthesia; duration of first, second and third stages of labour; mode of delivery; perineal trauma; retained placenta and birthweight.

7. It is not true that the association of APH with PPH is a novel finding (Harley et al, 2008. Idiopathic bleeding during the second half of pregnancy as a risk factor for adverse perinatal outcome. J Mat Fetal Neonatal Medicine 21: 331-335.). In this quoted paper PPH was only significant in the univariate analysis of perinatal complications associated with APH. PPH is not included as a significant finding in logistic regression. Our finding of APH (excluding placenta praevia) was an independent risk factor for PPH when adjusting for confounders.

8. The authors say that one of the strengths of their study was the ability to adjust for many risk factors for PPH. For a variable to be a confounder, it needs to be associated with both exposure and outcome. Did the authors test for this?

We agree that to be a confounder the variable would be associated with the exposure and the outcome. What we have done, as is common place in epidemiological studies, is control for potential confounders, i.e. those identified previously in the literature as related to the outcome. We have not specifically tested associations between exposure and confounder as many of the potential confounders are in their own right hypothesised to be related to the outcome.

Discretionary revisions
1. Please change N to n in various places throughout the document. As requested
2. Please present the increase in PPH risk with unit increase in BMI as this is more relevant to the current analysis than birthweight.

<table>
<thead>
<tr>
<th>Risk factor</th>
<th>Adjusted OR</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vaginal delivery BMI ↑ 5 units</td>
<td>1.27 (1.15-1.42)</td>
</tr>
<tr>
<td>Caesarean section BMI ↑ 5 units</td>
<td>1.19 (1.09-1.30)</td>
</tr>
</tbody>
</table>

We are happy for the editors to include this data if you wish, however, it may be more difficult to adapt to clinical situation.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests.

Reviewer's report 2

Title: Maternal obesity and postpartum haemorrhage after vaginal and caesarean delivery among nulliparous women at term: a retrospective cohort study

Version: 1 Date: 6 September 2012
Reviewer: John Morrison

Reviewer's report:
I think this is an interesting manuscript on a complex problem. The current data available in the literature are conflicting in relation to the true association between maternal BMI and the risk of postpartum haemorrhage. This study however is focused entirely on nulliparous women. I have outlined below some issues to be addressed in relation to this manuscript.

1. The introduction does not adequately deal with previously published reports pertaining specifically to PPH risk and BMI. The introduction deals with obesity, and PPH separately, but simply states at the end of the first paragraph that BMI has not been considered much as a risk factor for PPH, and that the data are inconsistent, citing 3 references. This is the central point of the paper, and should be adequately addressed in the introduction, including recent references e.g. Arrowsmith et al 2011, Bloomberg 2011, some of which are cited later in the paper.

The first paragraph has been adjusted to report past literature on these subjects and the reference of Bloomberg included and discussed. Arrowsmith has not been referenced as the study only included a small subgroup of women (IOL for postdates) and as such is not applicable to the populations being considered.

2. I think the comment speculating that obese women who give birth vaginally are likely to have normal myometrial contractility, presumably in comparison to those who have a caesarean delivery, is rather simplistic and not well founded. The causes of caesarean section in obese women are poorly understood and often multifactorial.

We agree this is rather simplistic, however, it is explanatory in how we reached our initial hypothesis. We have made some minor adjustments to reflect that this is hypothetical only.

3. The main issue I have with this study is that the rates of PPH > 1000mls are extraordinarily high - 16.2% overall in women having a caesarean section, and 24.2% in obese women having a caesarean section. These are much higher rates than those published in other papers, and in relation to general hospital statistics. I find them hard to accept, but also appreciate that this is what was observed. It may require a comment or explanation on how the volume of PPH was assessed. The authors argue in the discussion that this high rate was is consistent with one other study when parity is taken into account.
In the discussion we highlight that when a definition of >1000mls (rather than ≥1000mls) is used our rates of PPH after CS is only 8.2% and overall 5.1% (and this is documented in the text – we have added additional clarification). We have added a comment in the methods section regarding our method of estimation of blood loss.

4. It is my view that PPH with retained placenta should be analyzed separately. It represents a definite cause and is different to the concept that atonic PPH may be more prevalent in obese women due to some type of dysfunctional contractile performance. It would be interesting to see if this has any bearing on the results, i.e. if cases of PPH for this reason are analyzed separately. The paper has already commented on this – discussion section end of paragraph 3, where we report results of subgroup analysis showing when women with retained placenta were excluded there was no significant difference in effect. We are happy to move this to the results section if editorial request.

Level of interest: An article of importance in its field
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