Reviewer’s report

Title: Effect of Body Mass Index on pregnancy outcomes in nulliparous women delivering singleton babies

Version: 1 Date: 13 April 2007

Reviewer: Gordon C Smith

Reviewer’s report:

General

-----------------------------------------------------------------------------------

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

-----------------------------------------------------------------------------------

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. I am slightly concerned about the fact that the data span 30 years. There must have been a very significant change in the number of key outcomes over that period of time and, in particular, rates of caesarean section. I note that the authors comment, lines 345 – 347, that adjusting for year had no effect. I note that they divided year into 10-year categories and included these as categorical variables in the logistic regression. The use of 10-year categories is quite broad. Assuming that the changes were linear over the period time there is no reason why they could not include year in the model as a continuous variable. It is critical that all associations are adjusted appropriately for year given the fact that it is positively associated with obesity and positively associated with some of the outcomes.

2. There is little information in the Methods section about the procedures for coding or quality assurance of the data. This is referred to in lines 327 – 329. However, some reference to the quality assurance information would be helpful.

3. There is no description of how the study cohort was selected. They must have had some inclusion/exclusion criteria, for example births before a given week of gestation and multiple pregnancies. It would be useful if they could identify how they selected their study cohort. I note that there is no information at all on missing data. It is virtually impossible to have a dataset that is 100% complete. If this study is the rare exception this should be explicitly stated. Otherwise they should discuss whether they excluded cases with missing data or how they treated missing values in their analyses.

4. Lines 280 – 288. The authors discuss the association with pre-term birth. However, they pool all pre-term birth. They confirm the very striking association between obesity and pre-eclampsia. Pre-eclampsia is the major single reason for elective pre-term delivery. I think they should refine this discussion to discuss the distinction between elective and spontaneous pre-term birth. Moreover, I think the analysis would be benefited if they could examine the association between obesity and these two different types of preterm birth. This has been addressed in the recent study which they cite (reference 14).

5. The authors in a number of places confuse a negative finding with lack of statistical power. For example, in lines 300 – 302 they comment that there was a reduced risk of having a live birth amongst women who were obese but not those who were morbidly obese. However, if you examine the odds ratios in Table 3, the point estimate was 0.5 for obese women and 0.4 for morbidly obese women. That is, the point estimate was actually lower in the morbidly obese group. However, due to the rarity of the outcome and the relatively small number of women who were morbidly obese, this was not statistically significant in multivariate analysis. In reality, they have too few morbidly obese women to make any statement about the association with stillbirth.

6. The authors use odds ratio as the measure of association. The odds ratio is a good approximation to the relative risk when the outcome is rare but tends to be further from 1 when the outcome is common. A number of the outcomes they study are relatively common, such as caesarean section. Therefore the odds ratio is something of an overestimate of the relative risk. In general, this could be addressed by a note in the Discussion that the odds ratios would tend to be an over-estimate of the true relative risk. However, they
express the risk of stillbirth as the odds ratio for a live birth. Because live birth is very common, the odds ratio becomes quite difficult to interpret. I would much prefer that the relationship between obesity and perinatal mortality is expressed as the odds ratio for stillbirth rather than the odds ratio for live birth.

7. In Table 3, the authors should not use "NS" for associations that are not statistically significant. This relates to the previous point regarding statistical power. They should include all point estimates and 95% confidence intervals, and then the reader would be able to assess for themselves whether a clinically meaningful relationship could be reliably excluded by the confidence intervals or whether the study simply lacked statistical power.

8. There is a problem in the labelling of Table 3. The authors say that normal BMI was the referent category. However, in the column labelled ‘overweight’, has in parenthesis underneath "BMI = 20 – 24.5". I assume that this is an error. Curiously though, 20 – 24.5 does not reflect any of the classifications that they list in page 5. Similarly in this table, morbidly obese is defined as a BMI of >35 but in page 5, they define morbid obesity as being >40. There seems, therefore, both to be something of a mislabelling of columns in Table 3 and also the appearance of a slightly different classification system. The errors need to be corrected in the Table 3 and they need to confirm that they used the same method of classification throughout the paper.

9. Line 304 – they should avoid the use of the word ‘undernourished’. BMI is a proxy measure of body fat and it would be wrong to say that all women with a BMI of <20 were undernourished.

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions
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