The Editor,  
BMC Public Health

Dear Sir/Madam,

Please find attached revised version of manuscript – “Effect of Body Mass Index on pregnancy outcomes in nulliparous women delivering singleton babies”. We have taken the reviewers’ helpful comments on board and believe that the manuscript is much stronger as a result. We have dealt with the reviewer’s comments as follows:

Reviewer: Patricia M Dietz

Minor Essential Revisions:

1. Abstract classifies morbidly obese as >35 BMI whereas methods, line 167 classifies it as > 40 BMI. These need to be consistent.  
   This has now been changed in the text to be >35 (line 174)

2. line 204, % is missing from 1.9
   This has now been added

3. The authors use stillbirth in text and in table 2, but in table 3 use live birth. It would be easier for the reader to model stillbirth instead of live birth and report those findings in table 3 (see line 301).
   Livebirth has been changed to stillbirth in table3 and this has been changed in the text

4. line 290, use of IUGR isn't appropriate as the variable was birth weight <2500 grams, which includes IUGR and preterm infants appropriate for gestational age. I would recommend using <2500 grams consistently through out the paper.
   This has now been done.

5. There is a typo/extra word on line 331.
   This has now been corrected to read ….."the ideal time to record the baseline height and weight of a pregnant woman is before the occurrence ……."

6. Table 3 -- why is NS used some of the time? It would be better to present all the results.
   Where the crude Odds Ratios included 1, the variables were not entered into the logistic regression model as adjusted Odds Ratios were felt to be redundant.
   These have now been changed in Table 3.

Major Compulsory Revisions

1. Methods: more detail is needed on the definitions/ICD-9 codes used to define variables. For example, was height and weight measured or was it based on self-report? This information was in the discussion but belongs in the methods. How was emergency c-section identified and defined? What was gestational age based on (e.g. LMP, ultrasound, clinician's best guess)
   Suggested changes have now been made throughout the methods section. (Line161, 164, 182,186)
2. line 234, the model for predicting preterm delivery did not include adjustment for pre-eclampsia. This adjustment would give information whether the effect of obesity on preterm delivery was through the increased risk of pre-eclampsia. The model did include pre-eclampsia, but due to an oversight, this was not mentioned before. This is now specifically mentioned in the footnote of table 3.

3. line 280-288. The differences in findings regarding preterm may reflect differences in what was adjusted for in the models. Adding this information to the discussion would help to understand differences in study findings. This has now been added (Line 251)

4. line 292-294. The authors state that after adjustment, morbidly obese was not associated with macrosomia, yet on Table 3, the results show that it was, even after adjustment. This has now been corrected in the text (Line 261, 305)

5. line 338, values measured at a prenatal visit may be less accurate than those based on recall of height and weight before pregnancy, but they are unlikely to be biased. Self-reported weight and height are more likely to have bias. We agree with the reviewer on this point; that is why we mention this to be a major strength of our study where height and weight are routinely measured and recorded in the database.

Reviewer: Gordon C Smith

Minor Essential Revisions:
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. The year of delivery as a continuous variable has now been adjusted for in the models.

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. Two references (Ref. 17 and 27) have now been included.

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. Both these issues have now been addressed in the methods section. (Lines 154-164)

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).
We have now done a subgroup analysis to calculate the risk of spontaneous preterm delivery in the different BMI groups.

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.
This statement has now been made explicit in the discussion section (line 314)

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.
This has now been changed to stillbirth in both the text and table 3.

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.
Where Crude Odds Ratios included 1, the variables were not entered into the logistic regression model; but we have now substituted the real adjusted Odds Ratios with 95% Confidence Intervals in table 3.

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.
This has now been corrected both in table 3 as well as in the text.

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.
This has now been corrected.

We hope that the reviewers find that the revised paper answers their comments satisfactorily. Please do not hesitate to contact us if there is anything else that we can do.

Yours sincerely,

(Sohinee Bhattacharya)