Reviewer’s report

**Title:** Pre-pregnancy predictors of hypertension in pregnancy among Aboriginal and Torres Strait Islander women in North Queensland, Australia: a prospective cohort study.

**Version:** 1 **Date:** 15 November 2012

**Reviewer:** SANJIB GHOSH

**Reviewer’s report:**

The manuscript titled, “Pre-pregnancy predictors of hypertension in pregnancy among Aboriginal and Torres Strait Islander women in North Queensland, Australia: a prospective cohort study” by Campbell et al. is quite interesting, however while going through the manuscript, I came across some serious pitfalls, which I would like to discuss:

**Major Compulsory Revisions:**

1. Did the authors obtain a written informed consent from each of the patients? At least I could not see any such mention in the manuscript, although it is absolutely essential requirement.

2. How was the level of physical activity in the study subjects categorized? In my opinion using GPAQ (global physical activity questionnaire) designed by WHO, would have been a better idea.

3. I feel the patient inclusion methodology for this study was sound and justified, however for statistical analysis, the authors could have used Logistic Regression Analysis (Univariate/Multivariate) as the results of this test are easy to interpret and also very reliable to test the association between study variables/predictors and the primary outcome of the study.

4. The authors have discussed that BMI is an important risk factor for preeclampsia. I sincerely agree, however to establish the association, they should have analysed dietary intake, family history of obesity, and level of physical activity in the patients, which they have not. I feel this a lost opportunity on this excellent topic they have chosen for the study. Moreover many authors have emphasized on the fact that BMI is not an accurate measure of obesity, rather body fat distribution is a good surrogate marker of obesity. The authors should have mentioned this in the limitations of their study.

5. If the authors are trying to establish these factors as pre-pregnancy predictors of preeclampsia for effective screening, then sensitivity and specificity assessment is a must, which they have not mentioned.

**Minor Essential Revisions:**

1. The authors should have mentioned a sample size calculation, which would
have justified the number of patients included in the study.

6. The authors should have cited the pioneering works of Powers et al. in analyzing the association of obesity and preeclampsia, in their study and compared their findings.

2. Other risk factors as mentioned by the authors like previous h/o preeclampsia, nulliparity, increased maternal age etc. are well known risk factors for any population group and I feel there is no need for a separate study to establish the same.

Overall I feel that although the authors were honest in their approach, yet the final outcome is disappointing, as the study fail to introduce any new and convincing methodology regarding preeclampsia screening. However this manuscript can be improved, if the authors consider the deficiencies and rectify them accordingly, keeping in mind the following points:

a) Whether a combination of the factors mentioned is effective in screening, or they are effective individually.

b) They should highlight the point that these factors are more economical than any biochemical/biophysical marker to assess the risk in preeclampsia. Also this assessment does not require highly skilled personnel.

c) If they are showing BMI as a risk factor, then they should categorically mention which is the high risk group: overweight or obese or morbid obese?

d) Citation of recent relevant world literature is required.

**Level of interest:** An article of limited interest

**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