Reviewer's report

Title: Low birth weight: comparison of two birth cohorts in Sao Luis, Northeastern Brazil, 1997/98 and 2010

Version: 4 Date: 28 August 2013

Reviewer: Cynthia Ferre

Reviewer's report:

I appreciate the attempts the authors made to address several of the methodologic concerns this article has. However, they have not addressed methodologically the major flaw in their analysis: the 2 cohorts cannot address the changing rates of low birth weight and of stillbirth that the trend data show. The cohorts were not designed to investigate changes in low birth rates: several key variables are missing; there is no review of medical records; still births were not included; and they do not have any cohort data for the years when the low birth rates were higher. Thus the article remains confusing and misleading to the reader. I still strongly recommend limiting the analysis to that of the 2 cohorts or the SINASC trends, but not both together.

The comments below are offered to improve the coherency and interpretability of the analysis.

The statements in the 1st paragraph of the discussion regarding the low birth weight and preterm birth rates remaining stable during the study period are incorrect. They were not significantly different *at the 2 time points* for the cohorts. The authors present no data on how PTB rates may have varied in between these 2 time points.

The authors have not clearly described what this article contributes to the public health or perinatal epidemiology literature, nor even why it is important for the local community. The findings - low birth weight has a multiple risk factors which change over time - are not new. The finding of a ‘fetal shift’ is also not new. In the discussion, the authors go over a list of associations they found and confirm that others have found these associations in other studies. Moreover, the authors cannot address several crucial risk factors such as race/ethnicity, changes in maternal pre-pregnancy BMI, or maternal morbidity (which they have acknowledge in this recent revision). There is no discussion of the implications of the findings for public health practice in the local community. The 2 highest ORs for LBW in 2010 are for no prenatal care and for smoking. A couple sentences should be devoted to the need to continue improving access to prenatal care and smoking cessation.

The article continues to have problems with word and paragraph flow within each section. For example, in the Methods section, exclusions are listed in the 5th paragraph however an additional exclusion (births less than 500 g) then appears in paragraph 7. Acronyms are misspelled in several places (IURG in the abstract; LBE in the discussion). References to Tables are missing.
Please provide the total number of births for each time period before sampling. Please state that the study design is a retrospective cohort and trend study. Please provide a reference for the prenatal care index in paragraph 8 in the Methods section.

The statement about newborns being weighed at birth is not needed since the authors removed the language on babies being weighed at interview. The use of “birth weight” is sufficient.

Please include the percentages that were imputed, not just the numbers, in the methods section.

Results line 1: this p-value (0.742) does not agree with Table 1 (p=0.847)

The statement “Later on possibly medical care improved further still leading to simultaneously decreasing stillbirth and LBW rates” is conjecture. Not clear what ‘improved medical care’ means. The statement about the lower LBW rate in 1997 in the SINASC data being due to missing birthweight is also conjecture (no data presented).

The paragraph on cesarean section in the discussion needs to be reordered and clarified. Was there an assessment of potential effect modification between c-section and other maternal variables? Several sentences in this section do not make sense: “although unnecessary cesarean section deliveries were still performed they may now be less associated with iatrogenic prematurity and IUGR”. Not clear how c-sections for iatrogenic PTB and IUGR are unnecessary. Not clear how “percentage of births at 37 and 38 weeks . . . provides evidence in this respect”. Figure 3 indicates that there was a shift to this gestational age range from the greater gestational ages (reduction in weeks 40-42) but the authors do not present data showing c-section or induction rates by gestational age and how these may have changed over time. C-section was associated with increased low birth weight in the first cohort and not in the second.

Cannot read the legend in Figure 2. The columns on %LBW in Table 5 repeat data from Tables 3 and 4.

Supplementary Tables 3 and 4 are not necessary. It is sufficient to say in the text that a sensitivity analysis was done and the imputation did not affect the findings.

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

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.