Reviewer’s report

Title: Secular trends of macrosomia in southeast of China, 1993-2005

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Reviewer: Emily Oken

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Review of Lu et al. Biomedcentral

This paper presents trends in macrosomia among births in southeast China, based on data from a pregnancy monitoring system. The results suggest that macrosomia rates and birth weight increased from 1993-2000, then declined somewhat and leveled off from 2001-2005.

Overall this paper is a valuable contribution to the emerging literature suggesting that birth weight trends have shifted; whereas birth weight rose steadily over the 2nd half of the 20th century, it appears to have begun to decline in the early part of this millennium.

Major compulsory revisions:

1. The writing is generally clear though written by non-native English speakers, but in some places (especially the discussion) the language interferes with my ability to understand the points the authors are making. A review by a native English speaker would improve the clarity throughout.

2. The authors argue that the increase in macrosomia before 2001 was attributable to rising maternal BMI and GWG, whereas the leveling off thereafter results from an increase in elective csections that offsets the effects of higher maternal weight. However, the trends in maternal weight/weight gain and csections do not seem to follow this pattern, as in fact maternal weight increases much more quickly in the later years, whereas the csection rate is stabilized (though higher) by that time. An alternative explanation is that fetal growth really is declining. An analysis that accounts for gestational age, e.g. by using LGA as an outcome, or looking at birth weights within each week of gestation, would enable the authors to see whether the decline is entirely related to a shift in the distribution of gestational ages, or whether both are declining. See Donahue SM et al. Obstet Gynecol. 2010.

3. Page 4: Please clarify who recorded the information that was recorded into the pregnancy monitoring system and who recorded the information on the cards.

4. It is curious that fewer infants over time were born at 37 weeks; the authors should discuss why that may be.

5. I am quite concerned about the large drop off in the number of births included in 2001, compared with other years (a 40% drop compared with 2000, e.g.), which is also when the largest drop in birth weight/macrosomia was observed. I suggest the authors take a close look at their dataset to ensure no births were
missed in that year, or provide possible explanations for why there would be so few births in that year compared with all other years.

6. Table 3 – since macrosomia trends were not linear, it is not appropriate to use a linear model.

7. The discussion needs more about the limitations of the study.

Discretionary revisions:

1. Page 5 middle paragraph – I don’t understand how they used the paper by Alexander to estimate the number of missing births – seems they combined two types of data checks into one sentence.

2. Please explain why you did not classify GWG adequacy according to maternal BMI as recommended by the US IOM and other guidelines.

3. Page 5: please explain why only 12 of the 19 sites were chosen, and on what basis they were selected

4. Page 7: The first sentence is run-on and confusing.

5. Page 10 – The sentence beginning “these findings” needs some revision.

6. Page 11, first paragraph – I am having a hard time following the logic here.

7. Table 1 – please add units for each exposure (e.g. meters for maternal height)

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**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