Reviewer's report

Title: Birth weight and air pollution: investigating the association using complementary exposure metrics

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Reviewer: Inmaculada Aguilera

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This paper investigates the effect of air pollution exposure on birth weight through different exposure assessment methods. The manuscript is very well written and in general is well balanced (see specific comments below). Besides, it addresses an important topic in air pollution and perinatal epidemiology, i.e. the potentially different health effects associated with various exposure metrics that capture different aspects of exposure.

Main comments

1. My main concern relates to the validity of some exposure metrics back in time, taking into account the large time frame of the birth data (1997-2006).

LUR models were developed based on several monitoring campaigns conducted in 2006, 2007 and 2009 (according to Li et al., Atmos Env 2012). In a previous publication where authors examined the effect of the same exposure metrics on preeclampsia and preterm birth (Wu et al., Environ Res 2011), they discussed that “the assumption of a stable spatial surface used to temporally adjust the LUR – as is commonly done for most existing LUR models – may have been inappropriate”. I suppose this is the reason why they have not included temporally-adjusted LUR estimates in the present study (which, by the way, should be mentioned in the methods section and/or the discussion, as it is strange not to report temporally-adjusted exposures in a study on birth outcomes). However, the same reasoning can be used to argue that extrapolation of LUR estimates back in time until late 90s may be inappropriate as well, as this implies the assumption that small scale spatial contrasts in pollution remain stable over a long time period. This is an important limitation of these LUR estimates, which must be addressed in the manuscript.

Regarding exposure estimates from CALINE 4, it is not clear which was the time period of the input data. If input data were not obtained for the whole study period but only for the last years, then CALINE-based estimates are similarly affected by the limitation stated above. Finally, the same applies for traffic intensity data, which I assume has suffered large variation over the whole study period.

2. The conclusions of the present study are very different from the previous study on preeclampsia, preterm birth, and very preterm birth. Of course the outcomes are different and the potential biological mechanisms involved in the observed associations may not be the same. But given that both studies have used almost
the same exposure metrics, it would be interesting to discuss the present results in comparison with the study by Wu et al. 2011 in terms of exposure assessment methods.

Other comments

3. Methods, individual variables. Authors say that gestational age was estimated using the date of last menstrual period and ultrasound dating. Please be more specific about the procedure, e.g. was LMP the date used by default, and corrected by ultrasound dating only if they differed in more than x days?

4. Methods, exposure metrics. Although the calculation of the different exposure estimates has been previously published, some additional information would be helpful for the reader:
   - Please specify the number of monitoring stations used for each pollutant in the whole study area
   - LUR models: include the years when passive sampler measurements were performed as well as the predictor variables of the final models.
   - Dispersion model: justify why you chose a cut-off distance of 3 km to predict traffic emissions.

5. The discussion section is too long, particularly the paragraphs about confounding factors (page 15), which can be shortened while still making the same arguments.

6. I don’t know whether a limited number of bibliographic references are allowed, but even if there is no limit, I suggest reducing the number of references when possible.

Level of interest: An article of importance in its field

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