Reviewer's report

Title: Prenatal polychlorinated biphenyl exposure is associated with decreased gestational length but not birth weight in a prospective cohort study.

Version: 1 Date: 11 March 2012

Reviewer: Wilfried Karmaus

Reviewer's report:

Major Compulsory Revisions:
1. Is the question posed by the authors new and well defined?
   a) It is not clear why the author strongly distinguish between shortened gestational age and reduced birth weight. Both outcomes are not independent from each other, but shortened gestational age --> reduced birth weight. One should not separate one etiologic chain into separate pieces and claim that there is conflicting evidence.
   However, there is one subgroup of reduced birth weight not linked to shortened gestational age: “small for gestational age” (SGA).
   b) Indeed, the authors – when controlling gestational age on birth weight (page 11) – assess SGA and not just birth weight.
   c) The authors need to better clarify, define, and justify their analytical approach. Explain the etiologic chain. In particular, it is necessary to estimate the effect of PCB and its congener (controlling for DDE and all confounders) on birth weight (not SGA).
   d) It is understandable that they want to consider whether there is intrauterine growth retardation above and beyond shortened gestational age. However, shortened gestational age and birth weight are not independent effect and distinguishing between these two is hairsplitting and does not contribute to an etiological understanding.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
   a) Most of the methods are well-defined and demonstrate state-of-the-art approaches. However, whenever intervening variables are addressed, the methods are not appropriate. (In addition, I suggest not using the word mediation (page 8) but intervening variable. Mediation assumes a two-sided process (such as mediation in conflicts); however, the authors only estimate “b” as an intervening variable: a # b #c, not as recursive models).
   b) The authors need to use structural equation models or path analysis (e.g., CALIS in SAS or similar programs) to assess intervening effects. When assessing a reduction of the original regression coefficient once an ‘intervening’ variable is included in the linear model (page 8) does not exclude all types of intervening effects. This approach more or less excludes “perfect intervening”.

3. Are the data sound and well controlled?
   a) I understand that the authors like to compare birth weight and length of gestational age one-by-one and using the same scale (Table 4). However, gestational age is not normally distributed; hence linear regression analyses are not justified and appropriate. Take gestational age out of Table 4 and use the information of Table S2a instead. Same for table S4.
   b) The log-transformation of organochlorine concentration (as predictor) needs to be justified. There are two approaches, (i) reduce the impact of values outside the distribution (as chosen in this manuscript) or (ii) address the impact of values outside an expected distribution as those with a higher risk. The authors need to justify that (ii) is not the case.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   a) Yes

5. Are the discussion and conclusions well balanced and adequately supported by the data?
   a) In the introduction, there is a factual errors on page 4. Karmaus and Zhu (reference 11) looked at 168 offspring. Their sample size was not fewer than 30 subjects.
   c) The critique in the introduction that studies did not measure specific PCB congeners becomes an interesting twist, since finally in this study, total PCB becomes significant since all congeners seem to have the same effect (Table 4, Figure 1, see also the first para in the Discussion section). This needs to be discussed since the approach of older studies taking total PCBs is supported.
   d) I believe that it is worth mentioning that concentrations of PCBs did not decline with parity (Table 3) as some other publication have suggested.

6. Do the title and abstract accurately convey what has been found?
   a) The abstract had some information on the various groupings on PCB, which became irrelevant after their careful analyses, since all PCB congeners were related to gestational age. I suggest removing this from the abstract, since this is not relevant, but emphasize that all congeners were tested.

7. Is the writing acceptable?
   Yes

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

**Quality of written English:** Acceptable
**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**
No competing interests.