Reviewer's report

Title: A cohort study of the association between secondary sex ratio and parental exposure to polybrominated biphenyl (PBB) and polychlorinated biphenyl (PCB)

Version: 1 Date: 31 March 2009

Reviewer: Daniel Smith

Reviewer's report:

- Major Compulsory Revisions

This report represents a unique look at the question of PBB and PCB exposures and sex ratio at birth, based on a unique cohort. The findings of an increased sex ratio with parental PBB exposure is interesting, and deserves to be disseminated in the literature and discussed. The several comments below are intended to have the authors elaborate certain aspects of their study, and provide a more clear presentation.

The statistical tests should be better described. For the crude analysis on page 11, I get $p=0.039$ rather than $p=0.02$ for a two sided test of 754/1392 vs. $H_0 = 0.514$. Two-sided tests are the appropriate ones here given that the authors are concerned about both higher and lower sex ratios.

The authors loosely use the term 'significant' to apply to statistical significance at the alpha = 0.05 level. For example, the second sentence on page 12 implies that a 20% decrease in male births is not important, when they mean that wide confidence limits include one.

For Table 2, the authors assert that the proportion of males where both mother and father had high PBB (0.60) is statistically significantly increased, but compared to what? To the national population figure quoted earlier (0.514)? Or relative to the 19 births in the category of low PBB for both parents? In either case, when attempting to create the actual numbers under the proportions in Table 2, I cannot reproduce their statistically significant results with a chi-square test.

The authors find an association in a multivariate model when both mother and father have high PBB levels, but not when either parent alone has
high levels. These are the main findings of the paper and ought to be emphasized. I would place all three of these results in Table 3, so they can be displayed in one place and compared (both parents high, father only high, mother only high). The authors unfairly dismiss the odds ratio of 1.58 for fathers alone because it does not reach the conventional p-value, even though it is consistent with (even slightly larger than) the odds ratio of 1.5 when both parents are high.

Also, the results in Table 2 are expressed in terms of proportions, while the results in Table 3 are odds ratios, making it difficult to compare the adjusted and unadjusted analyses. A male proportion of 0.60 for both parents high vs. 0.53 for both parents low works out to an odds ratio of 1.3, so it appears that the multivariate analysis has strengthened the estimate of the effect.

- Minor essential revisions

Figure 1 needs to be labeled better. The axes are apparently log base e of PBB and PCB, and a legend should be provided for the symbols.

At the top of page 7, the authors mention that they had to exclude births outside of Michigan. How many births are these, and is there any potential bias? This could be included in the discussion.

- Discretionary Revisions

Dates for the feed contamination (1973-74) are given in the abstract, but should be in the Background section as well.

**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:**

I declare that I have no competing interests.