Reviewer's report

Title: A cross sectional survey of determinants of serum concentrations of polybrominated flame retardants among healthy pregnant women in an urban environment

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Reviewer: Ami Zota

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Horton et al., Predictors of PBDEs in New York City Pregnant Women

In this cross-sectional study, Horton and colleagues examine a rich dataset of a unique and important study population of diverse, urban pregnant women to examine predictors of serum PBDEs (mostly penta-BDEs). They find a variety of significant predictors including socioeconomic status (education, income), diet (such as processed meat), and use of electronics. The current manuscript, however, is lacking some key variables such as country of origin and years living in the US that would help to deconstruct their finding. Additionally, the discussion needs to be revised to include a more careful and robust discussion of the observed findings including possible explanations for some of the anomalous findings. For example, why would use of electronics (which is largely treated with deca-BDE) be associated with components of penta-BDE? Additionally, the discussion includes several important factual inaccuracies (see below) that need to be corrected.

Major Compulsory Revisions

Introduction

1. Discuss uniqueness of your cohort, and evidence suggesting non-white populations may face higher exposures to PBDEs (see Windham et al., Zota et al. 2009, Stapleton et al. 2012)

2. The authors should state that this is a unique opportunity to examine social determinants of PBDEs explicitly.

3. They should also list housing characteristics in their list of important factors (See Rose et al. ES&T)

Methods

1. Do the authors have any data on country of origin and years lived in the United States? Given the CHAMACO study findings on the importance of these variables in determining PBDE exposure among Latina women, this is likely to be an important predictor of exposure in the present study population, and possible confounder for some of your observed findings.

2. Did you consider examining interactions between race and diet? Did including sources of PBDEs in the model attenuate race or income coefficients?
Results
3. Can you breakdown the Hispanic category into country of origin (Mexican-origin vs. Dominican)?
4. Please include pre-pregnancy BMI in Table 1
5. Any information on housing characteristics (e.g., number of bedrooms, square footage, any proxies for air exchange or ventilation) [If not, this should be listed in limitations]

Discussion
1. Last paragraph on page 24. The authors state, “para-Hydroxylated metabolites have not been detected in human serum [39]”. This statement is inaccurate and needs to be revised. 5-OH-BDE47 is as an example of a para-OH-BDE that has been measured in pregnant women (see Zota et al., 2011 and Qiu et al., 2009).
2. If you do not have country of origin data, please address this in the study limitation section and discuss the potential implications for not having this covariate (for example, how it may have affected race/ethnicity findings).
3. Please address the finding with education and how it compares to other populations? What are potential reasons for this finding?
4. Please address the finding between congeners of penta-BDE and electronics use. Why do you think you see an association between penta-BDE congeners and electronics which are primarily treated with deca? While BDE-153 may be a breakdown product of BDE-209, I am less convinced that BDE-47 comes from deca?
   a. Specifically, you need to substantiate the plausibility of this result. Have other studies found an association b/w penta-BDEs and electronics? If so, please discuss these studies in the discussion. Also, since you measured BDE-209, why don’t you see if those who used more electronics were more likely to have measured levels of BDE-209 (do a 2 by 2 table with <BDE-209 below and above LOD by high versus low electronic use).
5. The section on “Levels and patterns of PBDE exposure” (page 18) needs to be revised to be more specific. For example, the statement, “levels of PBDE measured in the women enrolled in our urban cohort are slightly lower than those measured in these other studies conducted in North America (US and Canada)”.
   a. Notably, the samples in the current study were
collected several years later than those in the other North American cohorts.” This is also not true. The present study population was sampled between 2009-2010. Zota et al. (2011)’s cohort was sampled between 2008-2009 and Stapleton et al. (EHP2011)’s cohort was sampled between 2008 -2010. Please revise and make the appropriate comparisons.

b. The authors continue, “Further, the U.S populations represented in the National Health and Nutrition Examination Survey (NHANES) is a non-pregnant population.” This is also not true since NHANES does include pregnant women, and Woodruff et al. (2011) examined chemical exposures in US pregnant women from NHANES including PBDEs. It would be useful for the authors to compare their levels to NHANES pregnant women.

6. The discussion and data on congener distribution seems overly long and unnecessary since it is not adding much to the literature. Can you make greater use of your BDE-209 data?

7. In the following sentence on page 21, “In one recent study of a largely Mexican immigrant population of pregnant women living in California, PBDEs concentrations increased with increasing years residing in the U.S. and with the number of pieces of stuffed furniture in the home [42].” You have cited the wrong article. This was not a finding by Zota et al. but from the CHAMACOS group (Bradman et al. )

8. On page 18, the authors say, “Consistent with other studies, the distribution of PBDEs in our cohort is log-normally distributed”. If this is true, why did the study authors use a gamma regression analysis instead of log-transforming PBDE concentrations and do multivariate regression analysis? Were the results different between the two methods?

Discretionary Revisions
1. Why did you choose an 80% cutoff, that seems high?
2. Table 1 is redundant and has been shown in many other publications. I would remove this and use the space to further examine the data.
3. Page 4: reference 7, is this appropriate reference?

Level of interest: An article of limited interest

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