Author's response to reviews

Title: Predictors of serum concentrations of polybrominated flame retardants among healthy pregnant women in an urban environment: a cross-sectional study

Authors:

Megan K Horton (mh2180@columbia.edu)
Sabine Bousleiman (sb1080@columbia.edu)
Richard Jones (rlj9@cdc.gov)
Andreas Sjodin (zrq4@cdc.gov)
Xinhua Liu (xl26@columbia.edu)
Robin Whyatt (rmw5@columbia.edu)
Ronald Wapner (rw2191@mail.cumc.columbia.edu)
Pam Factor-Litvak (prf1@columbia.edu)

Version: 4 Date: 21 January 2013

Author's response to reviews: see over
January 18, 2013

Professor Philippe Grandjean, Editor-in-Chief
Department of Environmental Health
401 Park Drive
Landmark Center Room 3-110 East
Boston, Massachusetts 02215

Dear Dr. Grandjean and the EH Editorial Team,

We would like to express our sincere gratitude for the further thoughtful and provoking editorial comments and suggestions from your editors. We feel these have contributed to a considerably stronger and more concise manuscript. Thank you, again, for considering our submission for publication in your journal. We would like to thank the editors and reviewers for their thoughtful and constructive comments on the manuscript originally retitled “Predictors of serum concentrations of polybrominated flame retardants among healthy pregnant women in an urban environment: a cross-sectional study”. We have addressed the reviewers’ comments (in blue) and made changes in the text as appropriate. As requested, we attached a version with the changes tracked and with the final changes accepted.

We look forward to hearing from you.

Best,

Megan K. Horton, PhD
Department of Epidemiology
Mailman School of Public Health, Columbia University
722 W 168th St, Room 735
New York, New York 10032
Phone: 212-305-8303
Email: mh2180@columbia.edu
Horton, Megan  
MS: 8731561517478639

Response to reviewers

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  
Version: 3 Date: 31 October 2012  
Reviewer: Ami Zota

Reviewer's report:

Review

The revised manuscript is greatly improved and the authors have done a good job addressing some of the reviewers’ major criticisms and concerns. However, there are still areas requiring improvement, and the manuscript is too long; in particular, the introduction and discussion include a lot of information that is not directly relevant to the study. The results also need to be rewritten to include some details that are currently missing. Also, while there is a lot of emphasis on individual characteristics, there is no mention of policy measures. For example, the federal-sponsored phase out of penta and octa in the US in 2006, and many state level bans.

Major Compulsory Revisions

1. Abstract (Conclusion): The focus on exposure reduction is not well supported given the low R2s, the lack of data suggesting these actions would actually make a difference, and the fact that no other study has found an association between electronics and penta-BDE body burden. Consider adding something about chemical policy and/or your observation that PBDE levels in your study population are lower than previous assessments of US pregnant women, suggesting exposures may indeed be decreasing

   We modified the conclusion statement in the abstract to reflect these suggestions.

2. The Background section is too long (nearly 5 pages long!). Can you condense it? I think there is a lot of spurious information in there. Its almost written like a review article. For example, your discussion of dust and dermal exposure is overly detailed (on page 5) especially because this is not a household exposure assessment study. Similarly, the discussion of health effects on page 6 and 7 are overly descriptive especially since this study does not examine health effects. I
would recommend that you more succinctly describe the state of the literature.

*We reduced the background section from 5 pages to 3 pages and focused more on material relevant to this paper.*

3. Results: The section PBDE concentrations in maternal serum is overly long especially since readers can find the information in Table 2 and Figure 1. Instead of stating all of the information, can you just pick out a few findings to highlight?

*We considerably consolidated the results in this first section.*

4. Results: The subheading Predictors of PBDEs in maternal serum should be moved up to include the paragraph that begins with “Table 3 compares median and interquartile ranges…”

*We moved this subheading.*

5. Results: In general, when you discuss your findings from Tables 3 and 4, you need to include a reference comparison. For example, you list “high school or equivalent” as a characteristic that PBDE-47 was associated it, but the reader does not know if this is higher than those who did not attain high school education and/or those who went to college. Also, please state the demographic parameters associated with BDE-153. I don’t think most readers will remember the list that you evaluated.

*We added comparison categories for better understanding of the distribution of the PBDE level by category. We also re-stated the parameters for PBDE-153.*

6. Results: Please provide more information on the magnitude of effect for the various determinants as opposed to just listing which predictors were significant. Which predictors have the highest gm ratio for the various congeners? Also, please describe your results more. For example, the education effect is different for BDE-47 compared to BDE-153. It may be useful to highlight that. Also, for BDE-47, African Americans seem to have the highest levels but in the results section you just point out that whites have the lowest levels (were differences b/w Hispanic and AA different?). Lastly, you use a lot of real estate summarizing the R2 for each congener. You could summarize the four sentences into one, by saying” Adjusted R2 ranged from 4.6% for BDE-99 to 24.5% for BDE-153. And for BDE-153, once again, please provide more information about the direction and magnitude of the associations observed rather than just stating which predictors were significant.

*We present the results to help highlight the differences between predictors for each congener. We also add information on the magnitude of effect, as suggested. Again, we*
considerably reduced the text and rely more appropriately on the tables and figures.

7. Discussion: In the first paragraph, you say “higher levels of education appear predictive of PBDE – 47, -99, and -153. This is not quite true for BDE-153 since those with some college had higher levels than high school grads. I think your discussion needs to reflect the nuisances of your data.

In the first paragraph of the discussion, we include a slightly longer but accurate description of the data, reflecting the different patterns for race/ethnicity and education.

8. Discussion: In that first paragraph, you also mention “electrical appliances”, but I do not see this variable in your tables. Was this part of the household electronic variable or separate? Please clarify.

We corrected this statement in the discussion. Electrical appliances (ie refrigerators or stoves) were not included in this variable.

9. Discussion: In the section Levels and patterns of PBDEs, it seems important to mention the state ban of PBDEs in New York and the federal phase out that occurred b/w some of the other studies and your own, and this is a likely reason for the decline compared to Herbstman and Woodruff papers.

We added a discussion of the New York legislation and national abatement policies that may explain the lower levels of PBDEs in our cohort.

10. Discussion: You say nothing about your findings around the demographic variables such as education and race. These were important determinants and it would be useful to compare these findings to other studies. This may be more important than the paragraph on age since age was not a significant variable in your study nor has it been an important predictor in other PBDE studies.

We removed the discussion of age and added further discussion of education and race/ethnicity, in particular highlighting the similarities and differences from the Windham (2010) and Rose (2010) studies.

11. Conclusions: Both your conclusion and your abstract would be strengthened if you said something about the fact that while exposure was widespread, the overall levels were low compared to other US populations of pregnant women.

We added this comment to the conclusion of the discussion as well as the abstract.

12. Also, you say “reducing exposure may require increased regulatory restrictions”, but PBDEs have already been phased out and banned in many
states. So can you be more specific on what type of policy measures you recommend? What about saying something about the fact that once these chemicals are introduced, they are hard to eliminate from consumer products, the environment, and human bodies so there should be further regulatory screening and action before chemicals are introduced into commerce.

We added text to discuss the conclusion to reflect that current policies appear successful at reducing exposures, though exposure continues though dietary and consumer products.

13. Your recommendations regarding exposure reduction should be tempered and/or caveated. Your R2 values were quite low, so do you think changing diet and electronics consumption would actually reduce PBDEs? There is little to no evidence suggesting that individual actions can reduce PBDE exposures. Also, do you think in particular, its realistic for individuals to reduce electronics? Why not say something in your conclusion about how your results can be used by risk assessors and epidemiologists to identify potentially highly exposed subpopulations. Or something more generally, such as our research further supports the hypothesis that indoor environments and diet are important contributors to PBDE body burden.

We changed our recommendations to reflect these suggestions.

Minor Essential Revisions

Abstract

1. Results: Some of the language used to describe the results is a bit awkward and vague. For example, what does “appear predictive” mean? I would suggest using more standard language such as “were associated” since that is really what you can say from regression analysis. Also in the results, you should state whether these are for bivariate or multivariate regression analysis. (The “appear predictive” language should be changed throughout the manuscript since predictive modeling is different than regression analysis).

We made these suggested changes.

Background

2. Pg. 5, line 4. What does “high levels” mean? You should use another word or provide more context. High compared to what reference?

This reference has been removed from the text.
3. Pg. 8, There are two errors in first paragraph on page 8. At the end of the first paragraph, you discuss recent study of “California toddlers”, and the Stapleton et al. study actually studied toddlers in North Carolina. Also you say “BPDEs” in that paragraph instead of “PBDEs”.

We removed this text.

Results

4. The number of significant digits you use to represent percentages fluctuates throughout the results section. In the first paragraph, you round percentages (27%), while in paragraph two you sometimes round percentages, sometimes present one number beyond the decimal point (98.4%) and sometimes two decimal points (7.73%). You should be consistent throughout your text and tables. Presenting percentages to three or four significant digits seems unnecessary. It is more customary to use two or three significant digits (e.g. 27% or 98.4%).

We edited the percentages to make them consistent.

5. Table 2. You have “Median LOD” column twice with two different numbers. Is one of the columns mislabeled?

In Table 2, we originally presented “Median LOD ng/g serum”, “Median LOD ng/g serum lipid” and “Median ng/g serum lipid”. We deleted the “Median LOD ng/g serum”.

Discretionary Revisions

Abstract

1. Consider stating in your abstract that your cohort is multi-ethnic and predominately low-income.

We added these phrases to the abstract.

Methods/Results

2. Do you have any further information on race classification of your Hispanic women? Are there Hispanic Black or Hispanic White? Seems like an important piece of information that is missing. If you have that information, it would be useful to add to Table 1.

We do have that information (please see table below). As we did not stratify on these categories for analyses due to small sample size, we selected not to present them in the
<table>
<thead>
<tr>
<th>Race/Ethnicity</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Black/African American</td>
<td>20</td>
</tr>
<tr>
<td>Black Hispanic</td>
<td>1</td>
</tr>
<tr>
<td>Black Non-Hispanic</td>
<td>19</td>
</tr>
<tr>
<td>White</td>
<td>73</td>
</tr>
<tr>
<td>White Hispanic</td>
<td>5</td>
</tr>
<tr>
<td>White Non-Hispanic</td>
<td>68</td>
</tr>
</tbody>
</table>

Discussion

3. The discussion would be stronger if you started it off with your major findings as opposed to more review of the literature.

We think with the current revisions reflecting the reviewers suggestions, the discussion reads much clearer and stronger.

4. Indeed the comparison to the Herbstman paper is quite interesting (lower BDE-47 but higher BDE-153) and could be further highlighted.

5. The paragraph on page 21 is quite long and some of this information is already discussed in the introduction. Perhaps you can condense.

This section has been condensed.

6. In the section entitled Predictors/sources of exposure. You go into quite a lot of detail on the Herbstman study. I would not include all this detail unless you are going to directly compare your findings on BDE-153, which you currently do not. It would also be useful for you to include one brief sentence on why your findings on predictors of 153 differed so much for Herbstman paper.

We reduced the discussion of the Herbstman paper, and included some comparison of our findings.

7. The discussion provides a lot of information on dust exposure assessment, which could be condensed since your paper did not include indoor measurements.

We shortened this paragraph (on page 22) considerably.

8. Discussion: I am still perplexed by the consistent finding with # of electronics. Does this association hold in both income brackets (below and above poverty).
Do you think this is a proxy for something else?

We are also perplexed by this finding. We have attempted to examine whether number of electronics is a proxy for a sociodemographic or other characteristics. We added a brief discussion in the text to further describe the relationship between electronics, race/ethnicity and income. The association between PBDE-153 and electronics is highest among subjects in the lowest income category and highest among Hispanics. The association between PBDE-47 is highest among white subjects and Hispanic subjects and consistent across all 3 income strata.

Level of interest: An article whose findings are important to those with closely related research interests.

Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I have no financial competing interests.
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

Version: 3 Date: 3 December 2012

Reviewer: Alicia Fraser

Reviewer's report:
Re-review of manuscript by Horton et al. for Environmental Health

Date: 12-2-12

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

Summary: The authors revisions have improved the manuscript considerably. I’ve identified a few small items, listed below, that should be addressed. Otherwise, the manuscript should be ready for publication.

Minor Essential Revisions:

1. The revised statistical methods section is helpful. However, the text and footnote now say $p < 0.1$, but the authors’ response to my previous comment says $\alpha < 0.20$. Please be sure to check which is correct and present the correct number in the paper.

   We check the numbers and confirm that the footnote is correct, we used $p < 0.1$ for the cutpoint.

2. Table 3 needs a footnote to explain what “***” means.

   We added this footnote to Table 3.

3. Background, 1st paragraph: There is an incorrect reference to Table 1. I believe it refers to a table that was removed during revision.

   We deleted this text based on suggestions from the other reviewer to condense the background section.

4. Results, 2nd paragraph: I believe the reference to Table 3 should actually be
to Table 2.

We corrected this error.

Level of interest: An article whose findings are important to those with closely related research interests

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.