Response to comments from reviewer 1

The subject of the paper is very interesting. The article is generally well written and will serve as a valuable resource. The authors are quite right when insisting on the need for such a kind of studies. I suggest going ahead in a future in such a research line focusing on other toxic compounds.

Anyway, I feel the authors should include some of the related reviews, such as:

- "The mobility and degradation of pesticides in soils and the pollution of groundwater resources. Agriculture, Ecosystems and Environment, 2008, 123, 247-260".
- "A review on the fate of pesticides during the processes within the food production chain. Critical Reviews in Food Science and Nutrition, 2011, 51(2), 99-114".
- "Critical review on the environmental fate of quaternary ammonium herbicides in soils devoted to vineyards, 2013, Environmental Science and Technology, 47(10), 4984-4998".
- "A Review on the Fermentation of Foods and the Residues of Pesticides—Biotransformation of Pesticides and Effects on Fermentation and Food Quality. Critical Reviews in Food Science and Nutrition, 2015, 55(6), 839-863".
- A Critical Review About the Health Risk Assessment of PAHs and Their Metabolites in Foods. Critical Reviews in Food Science and Nutrition, 2015, 55(10), 1383-1405.
- A Critical Review About the Human Exposure to Polychlorinated Dibenzop-Dioxins (PCDDs), Polychlorinated Dibenzofurans (PCDFs) and Polychlorinated Biphenyls (PCBs) Through Foods. Critical Reviews in Food Science and Nutrition, 2015, 55(11), 1590-1617.
- State of the art on public risk assessment of combined human exposure to multiple chemical contaminants. Trends in Food Science and Technology, 2016, 55, 11-28.
- Perspective on pre- and post-natal agro-food exposure to persistent organic pollutants and their effects on quality of life. Environment International, 2017, 100, 79–101.
- Optimization of selective pressurized liquid extraction of organic pollutants in placenta to evaluate prenatal exposure. Journal of Chromatography A, 2017, 1495, 1–11.

The science presented is of a high quality with plenty of detail. This is a substantial manuscript that occupies the bounds between analytical and residue chemistry. I do express my positive opinion on the acceptance of the article to be published after minor revision.

Response: We appreciate the reviewer's positive opinion of our work. However, we are reluctant to cite the recommended papers because they are either on pesticides (#1, #2, #3, #4, #7, and #8), PAHs (#5), contamination of chemical mixtures (#6), or instrumental analysis (#9), and thus have very limited relevance to the topic at hand (human uptake of PCBs from both near- and far-field exposure routes).

Response to comments from reviewer 2

This manuscript is a very interesting and novel contribution that provides new insight into the possible historical contribution of the indoor environment to human exposure to PCBs. It is very well written and suitable for publication following minor revision.

Response: We thank the reviewer for his/her favorable appraisal and the thoughtful suggestions.

General comments

1. The exposure context for the model simulations needs to be clarified. Some Swedes lived in buildings in

which sealants containing PCBs had been used. These individuals were highly exposed via the indoor environment. Others (presumably many more) lived in buildings where such sealants had not been used and were hardly exposed at all via the indoor environment. This needs to be explained, and the authors should clarify which group they are simulating.

Response: Our calculations are for a hypothetical "average" Swede living in a hypothetical "average" building with all indoor emissions uniformly distributed therein. In other words, our prediction can be perceived as representing the average of the contamination in the two groups of individuals mentioned by the reviewer, weighted by their relative size in the total Swedish population. We calculated such an average, instead of simulating two groups of individuals separately, mostly because long-term biomonitoring data are available for the average general Swedish population. No in-depth information that would allow us to differentiate between the two groups of individuals is available to us.

In the revised manuscript, we add a few sentences for clarification:

> "…we assume that the entire indoor environment in the modeled region is a single uniform space receiving homogeneous indoor emissions. Thus, our modeled concentrations represent an "average" indoor contamination level across Sweden." (Lines 104 – 107 in the revised manuscript)
>
> "The modeled results are for an average Swede living in an average house, i.e., represent the overall contamination level of the general Swedish population." (Lines 123 – 124 in the revised manuscript)

2. I could find little empirical evidence to support the validity of the novel components of the modeling work. Most of the model evaluation evidence presented captures only the far-field exposure pathways. The exception in this manuscript is the evaluation against concentrations in humans. However, there are countless uncertainties in the model besides the absence of near-field exposure that could explain the deviation of the far-field only simulations from the observations (Figure S6). Indeed, the major explanation for the better fit of this work to the empirical data is the change in the historical PCB emissions scenario, not the addition of indoor air exposure vectors. Another parameter that would directly influence the model fit to the empirical data is the biotransformation rate in humans. For this parameter the authors acknowledge an uncertainty of an order of magnitude for PCB 28 (Table S1), which is large compared to the contribution of near-field exposure (approximately equal to far-field exposure). Hence this model evaluation cannot be used as evidence that indoor pathways were important for exposure to PCB 28; all statements / suggestions of this kind should be removed from the paper.

Response: The reviewer is raising two interesting aspects that may serve as alternative explanations for the better fit of our model results to empirical data: the updated emission estimate and the uncertain biotransformation rate.

(I) the update of PCB emission estimate

To explore to what extent the update of the PCB emission estimate is responsible for the improved modeling performance, we now include results of an additional calculation (see Figure S7 in the revised Supporting Information), assuming that all the updated atmospheric emissions directly enter the rural

environment (as in Breivik et al.). Our additional calculation indicates that, for PCB-28, the update of the emission estimate is insufficient to explain the better fit with observations, because the modeled concentrations are still only half of the observations. In other words, the addition of indoor air exposure vectors contributes at least half of the improvement in modeling performance. In fact, this contribution is also supported by Figure 6, which shows that near-field routes account for almost half of the total human exposure before the 1990s.

A brief discussion has also been included, as follows:

> "For PCB-28, the update of emission estimate is insufficient to explain the better fit with observations, because the modeled concentrations are still only half of the observed ones even if all atmospheric emissions occur in the rural environment, as assumed in Breivik et al.[11] This suggests the importance of taking into account near-field exposure routes. For PCB-153, using the updated emission estimate but moving all atmospheric emissions to the rural environment can also lead to a satisfactory agreement between modeled and observed concentrations. Whereas ignoring the indoor and urban emissions omits the contribution from near-field exposure, it also adds the part of the PCB emissions that is predicted to be retained in the indoor and urban environments (~31% of the cumulative emissions by 2015[28]) to the rural contamination." (Lines 314 - 323 in the revised manuscript)

(II) Biotransformation rate

Whereas the biotransformation rate for PCB-28 is associated with an order of magnitude of uncertainty, its geometric mean (4.36×10^{-5} h^{-1}), which is used in our calculation, is very close to the value used in Breivik et al. (5.48×10^{-5} h^{-1}). Therefore, it is not likely that the better fit in our work compared to that in Breivik et al. is rooted in the use of a new biotransformation rate.

In order to respond to the reviewer's concern, we now include results of an additional calculation (see Figure S8 in the revised Supporting Information), which is based on human intrinsic biotransformation rate constants taken from Ritter et al. (2011; representing slower biotransformation) and Arnot et al. (2014; representing faster biotransformation). The additional calculation characterizes the possible range of calculated concentrations. We add the following sentences to discuss the influence of the uncertain biotransformation rate on our conclusions:

> "However, other parameters, e.g., the human biotransformation half-life (HL_B), can be highly uncertain and variable. It is possible that equally good agreement between model and observations could also be achieved from parametrizations that do not consider near-field exposure, e.g., if the rural environment is assumed to have received all atmospheric PCB-28 emissions and a HL_B at the lower end of the known range is applied (Figure S8). Therefore, further investigations may still want to thoroughly examine possible alternative explanations." (Lines 323 – 328 in the revised manuscript)

The second option for establishing confidence in the model results is demonstrating confidence in the

sub-models that are key for the study's conclusions. In this case it is the sub-models that predict concentrations in indoor air and exposure via non-dietary ingestion that are key for the conclusions. It is unfortunate that the manuscript contains no treatment of this aspect. I had to refer to the second manuscript where I found a reference to a figure in the SI that compares measured and modeled concentrations in indoor air. However, because the submission did not include the SI for this second manuscript, I cannot judge whether it contains any convincing evidence to support the validity of the predictions of the sub-models predicting indoor air concentrations.

I therefore request that the authors: a) clearly acknowledge the inherent uncertainties in this part of the model and the resulting conclusions, and b) summarize the evidence supporting the validity of this part of their model that is given in the companion manuscript, and/or; c) present other evidence to support the validity of their exposure estimates. For instance, my understanding of the companion manuscript is that the authors have distributed all indoor air emissions among all buildings in Sweden and assumed that all Swedes are exposed. Using these assumptions, the modeled indoor exposure to PCB 28 equaled the far-field exposure. In reality, the emissions were distributed in perhaps 10% of Swedish buildings, which would make indoor concentrations 10 times higher, in which case the model would predict PCB 28 exposure of individuals living in such buildings to be 10 times higher than the exposure of individuals living in uncontaminated buildings. Measurements of the differences in human exposure between individuals living in contaminated and uncontaminated buildings could thus be used to evaluate this part of the model.

Response: We accept the reviewer's suggestions and

a) have clearly acknowledged the inherent uncertainties associated with emission estimates and biotransformation half-lives (see response to the last comment);

b) have briefly summarized the outcome of the model evaluation (in particular for the exposure-relevant indoor air compartment) contained in the companion manuscript, which has recently been accepted by *Environ. Int.*, as follows:

> "Modeled concentrations in the indoor, urban and rural environments (outputs of Blocks I and II) have been evaluated in Li et al.[28] Briefly, the modeled concentrations agree with the means or medians of observed concentrations in indoor air, as well as in air, fresh water, estuarine water and soil in the rural environment of the modeled region, with differences of less than a factor of two. In particular, our modeled peak concentrations in indoor air (~45 ng m^{-3} for PCB-28 and ~13 ng m^{-3} for PCB-153) are within the ranges (14 – 296 ng m^{-3} for PCB-28 and 1.5 – 45 ng m^{-3} for PCB-153) measured in rooms with known PCB sources.[45-47]" (Lines 252 – 258 in the revised manuscript)

c) have included additional modeled results for individual living in buildings with (10%) and without (90%) PCB contamination (see Figure S6 in the revised Supporting Information). We also discuss this in the revised manuscript, as follows:

> It is worth mentioning that our calculation is for a hypothetical "average" Swede living in a hypothetical "average" house, whereas in reality, only some Swedes lived in houses with indoor PCB sources. Figure S6 presents results of our additional calculation, which predicts that

individuals living in PCB-contaminated buildings would be 13 (PCB-28) and 3 (PCB-153) times more contaminated than their counterparts living in PCB-free buildings, if we assume that PCBs had been used in 10% of Swedish buildings in which 10% of Swedes lived. Earlier monitoring work observed that the median concentrations of PCB-28 and PCB-153 in blood of Swedes living in flats with PCB-containing sealants are 30 ($p < 0.001$) and 1.3 ($p = 0.35$) times of those of Swedes living in uncontaminated flats.[50] Furthermore, despite the remarkable difference between the concentrations predicted in the two groups of Swedes, the average concentration weighted by the relative size of the two groups is quite close to our modeled result for the hypothetical "average" Swede. This suggests that the modeled result is insensitive to the assumption as to what fractions of the Swedish population lived in buildings with and without PCB emissions. (Lines 274 – 286 in the revised manuscript)

Specific comments

Line 32. The language used here ("notion") suggests that this is not based on scientific investigation. This is incorrect. The dominance of far-field exposure is a conclusion based on measurements of human exposure during the last decades. The uncertainty arises from extrapolating this conclusion backwards in time or to sub-populations with unusual exposure situations, e.g. those that live in buildings that are still contaminated by PCBs. The insinuations here and elsewhere in the paper (e.g. first line of Abstract, the conclusions) that this is a non-substantiated claim should be removed.

Response: We accept the reviewer's advice. In the revised manuscript, the first sentence of Abstract is now changed into:

"The general population is exposed to polychlorinated biphenyls (PCBs) by both consuming food from far-field contaminated agricultural and aquatic environments, and inhalation and non-dietary ingestion in near-field indoor or residential environments." (Lines 15 – 16 in the revised manuscript)

The first sentence in Introduction section is also changed into:

"It has generally been accepted that the human population takes up polychlorinated biphenyls (PCBs) predominantly via food originating from contaminated agricultural and aquatic environments (i.e., "far-field" environments) that are mostly distant from where PCB-containing products are used.[1-3]" (Lines 31 – 33 in the revised manuscript)

In addition, the word "notion" has been replaced with the more neutral "understanding" throughout the manuscript.

52-54. The authors seem determined to prove their hypothesis before they test it. This is completely unnecessary. There are many other possible explanations for the differences between model predictions and empirical observations, and this should be acknowledged (indeed, the authors later show that a large part of the underestimation by Breivik et al. was due to an underestimation of the magnitude of emissions, not an overlooked source of exposure as the authors conclude in these lines.

Response: We accept the reviewer's suggestion and delete the whole sentence.

68. There is nothing "obvious" about this. Either this was the case, or it was not.
Response: We deleted the word "obviously".

115. "are viewed as the contamination of food"? This should be rephrased.
Response: We rephrased the sentence to be "PCB concentrations in organisms at the time of slaughter represent the contamination levels in food".

121. More information should be provided in this manuscript about Blocks I and II, in particular about how the indoor emissions were distributed in space (see General comment 1) and ventilation rate.
Response: We now include more information about Blocks I and II, in which the distribution of indoor emissions and the ventilation rate we used are specified, as follows:

> "As shown in Figure 1, the dynamic substance flow module (Block I) calculates the accumulation of PCBs in indoor and outdoor in-service products (i.e., in-use stocks), landfills and dumps (i.e., waste stock), as well as emissions from industrial processes, use phase and waste disposal. Emissions from the indoor in-use stock serve as inputs to the indoor air compartment, and the remaining emissions are inputs to the urban air and fresh water (Figure S1). Despite recognizing the fact that PCBs were used in some, but not all, Swedish buildings, we assume that the entire indoor environment in the modeled region is a single uniform space receiving homogeneous indoor emissions. Thus, our modeled concentrations represent an "average" indoor contamination level across Sweden. Next, based on the indoor and outdoor emissions, the nested multimedia module (Block II; Figure 1) calculates time-variant concentrations in various compartments (e.g., air, fresh water, estuarine water, soil, carpet, vinyl floor and organic film) of near-field (i.e., indoor) and far-field (i.e., urban and rural) environments.[28] The module considers the mass exchange between the indoor and urban environments through ventilation (based on an observed median air exchange rate of 1.23 h^{-1}), and that between the urban and rural environments through atmospheric (based on an average atmospheric residence time of 27.2 h) and fresh water advection (based on precipitation rate). It also describes permanent loss of the emitted PCBs from the environment via anthropogenic (e.g., cleanup of indoor surfaces) and natural (e.g., irreversible burial in sediments) removal processes." (Lines 99 – 116 in the revised manuscript)

Figure 1. Vegetables are missing. "Cods" and Herrings" should be "Cod" and "Herring". I also suggest that the labels "Block I" etc be added, as this is what the figure caption and text refer to.
Response: We have corrected the figure as per the reviewer's advice.

141-184. This text is largely a repeat of the ACC HUMAN model. It does not provide much new information and is not central to the conclusions of the paper. It would be sufficient to put this in the SI. The space could be better used to describe the Block 1 and Block 2 models, which are central for the conclusions of the paper.
Response: We agree with the reviewer's comment and have moved some of this text from the main paper to Text S1 in the Supporting Information. We also shortened the description of Block III in the

revised version.

Also, the authors try to restructure the description of the food web bioaccumulation model around equation 1. While I appreciate the ambition to have a more elegant presentation, some of the organisms, most particularly vegetation, do not fit into the framework specified in equation 1. Vegetation is – perhaps not surprisingly – ignored in the presentation of the model. This needs to be remedied, and the authors should reconsider their framework. Elegance should not be achieved at the cost of clarity and comprehensiveness.
Response: We regret that our description was not clear. In fact, vegetation contamination was calculated using the generic model equation.

The original ACC-Human model considers four major processes for vegetation: (i) diffusive exchange of gaseous chemical between vegetation and the atmosphere, (ii) deposition of aerosol-associated chemical on the grass, (iii) root uptake, and (iv) biotransformation. These processes are rewritten to fit into our framework as specified in equation 1:

- Processes (i) and (ii) can be collectively represented by processes of "uptake from air (IN_{air})" and "loss to air (OUT_{air})" (analogous to animals' respiration), thus can be described by an advective intake rate and a K_{OA}-dependent absorption efficiency. The K_{OA}-dependent absorption efficiency for vegetation can be derived from the K_{OA}-based Equation S10 (for foliage vegetation) in Arnot and Mackay, 2008, 42, 4648 – 4654.

- Process (iii) can be described by the process of "uptake from soil (IN_{soil})". Note that the calculation of its D-value is different from that for animals (see Text S1 in the revised Supporting Information)

- Process (iv) is characterized as it is in ACC-Human.

To improve the clarity of our manuscript, we have now rewritten the paragraph describing the generic model. In particular, we specify to which organisms each specific process is applicable, as follows:

> "As indicated in Eq.1, four uptake routes are considered (Text S1). (i) Uptake through eating prey (IN_{diet}) is applicable to species other than plankton and vegetation. (ii) Uptake from water (IN_{water}) describes either gill ventilation of estuarine water by aquatic species or the drinking of freshwater by terrestrial species. (iii) Uptake from air (IN_{air}) includes foliar mass exchange between the atmosphere and vegetation, and respiratory uptake by terrestrial animals. (iv) Uptake from soil (IN_{soil}) results from the unintentional ingestion of soil particles on the pasture or harvested feed by terrestrial species, as well as root uptake by vegetation. We assume that intake rates of food, water, air and soil are proportional to body weight (Text S1). Three absorption efficiencies are applied to convert the calculated intake to uptake: For gastrointestinal uptake (IN_{diet} and IN_{soil}), a gastrointestinal absorption efficiency (E_D) is organism-specific and calculated from the octanol-water partitioning coefficient (K_{OW}; Text S1).[31-33] For uptake from water (IN_{water}) and air (IN_{air}), absorption efficiencies (E_W and E_A) are assumed to be 100% unless indicated in Text S1. In addition, we also consider five removal processes from organism bodies (Text S1). (i) Egestion via feces ($OUT_{egestion}$) is applicable to organisms other than plankton and vegetation. (ii) Loss to water

(OUT_{water}) includes gill ventilation for aquatic species and urination for terrestrial species. (iii) Loss to air (OUT_{air}) includes foliar mass exchange between the atmosphere and vegetation, and exhalation by terrestrial animals. (iv) Biotransformation ($OUT_{biotransformation}$) is applicable to all organisms. (v) Lactation ($OUT_{lactation}$) is the most notable process for dairy cattle." (Lines 163 – 179 in the revised manuscript)

We have also rewritten the representation of individual species in Text S1 in the revised Supporting Information.

250. A summary of the evaluation of Blocks I and II is needed, see General comment 2.

Response: We have now included a brief description of the evaluation of Blocks I and II, as follows:

"Modeled concentrations in the indoor, urban and rural environments (outputs of Blocks I and II) have been evaluated in Li et al.[28] Briefly, the modeled concentrations agree with the means or medians of observed concentrations in indoor air, as well as in air, fresh water, estuarine water and soil in the rural environment of the modeled region, with differences of less than a factor of two. In particular, our modeled peak concentrations in indoor air (~45 ng m^{-3} for PCB-28 and ~13 ng m^{-3} for PCB-153) are within the ranges (14 – 296 ng m^{-3} for PCB-28 and 1.5 – 45 ng m^{-3} for PCB-153) measured in rooms with known PCB sources.[45-47]" (Lines 252 – 258 in the revised manuscript)

268. The exposure history of the "general Swedish population" is certain to have varied widely, depending on whether individuals lived in contaminated buildings or not (and even how these were ventilated). See General comment 1.

Response: We now delete the whole sentence.

306-307. This is a trivial conclusion, and inevitable given the connectivity assumed for the models. This is intellectually well below ES&T's readership. Suggest deletion.

Response: We accept the reviewer's recommendation and delete the sentence.

Figures 2 & 3. I do not understand why the contribution from non-dietary ingestion drops dramatically after the age of 10. According to the reference for soil and dust ingestion given by the authors (ref 39), the ingestion of soil and dust is independent of age between 1 and 21 years of age (equaling 100 mg/d for both vectors combined). I could not identify any other age dependent variables in the model for this exposure pathway. Could there be an error here?

Response: This is not an error. We apologize for confusing the reviewer.

In our model, the soil ingestion rate (taken from ref 39) is used to calculate the "ingestion of soil particles from the urban environment" (which is a far-field exposure route), rather than "non-dietary ingestion" (which is a near-field exposure route). Ingestion of soil particles and non-dietary ingestion are two different things. We follow the definitions by *the USEPA Exposure Factors Handbook*, in which soil ingestion refers to direct inhalation and/or swallowing of soil/dust, whereas non-dietary ingestion includes ingestion of soil/dust through pathways "other than soil and dust ingestion", mainly via hand-to-mouth and object-to-mouth contacts.

The age dependence of non-dietary ingestion rate is governed by the frequencies of hand-to-mouth and object-to-mouth contacts (i.e., contact with carpet, vinyl floor, and hard surfaces). These frequencies are taken from the *USEPA Child-Specific Exposure Factors Handbook* (for children under the age of 11) and Shin et al., *Environ. Sci. Technol.* 2012, 46, 10063-10072 (for other ages). According to the former, hand-to-mouth and object-to-mouth contacts cease after age 10 (see the screenshot below), whereas the latter assumes that these contacts do exist, but at low frequency, for teenage and adults. This explains why non-dietary ingestion "drops dramatically".

Table 4-1. Summary of Recommended Values for Mouthing Frequency and Duration

Age Group	Hand-to-Mouth				Source
	Indoor Frequency (contacts/hour)		Outdoor Frequency (contacts/hour)		
	Mean	95th Percentiile	Mean	95th Percentile	
Birth to <1 month	-	-	-	-	Xue et al., 2007
1 to <3 months	-	-	-	-	
3 to <6 months	28	65	-	-	
6 to <12 months	19	52	15	47	
1 to <2 years	20	63	14	42	
2 to <3 years	13	37	5	20	
3 to <6 years	15	54	9	36	
6 to <11 years	7	21	3	12	
11 to <16 years	-	-	-	-	
16 to <21 years	-	-	-	-	

Age Group	Object-to-mouth		Source
	Mean Frequency (contacts/hour)	95th Percentile Frequency (contacts/hour)	
Birth to <1 month	-	-	Reed et al., 1999; Freeman et al., 2001; Tulve et al., 2002; AuYeung et al., 2004; and Black et al., 2005.
1 to <3 months	-	-	
3 to <6 months	-	-	
6 to <12 months	24[a]	-	
1 to <2 years	20[b]	-	
2 to <3 years	10[c]	-	
3 to <6 years	10[c]	-	
6 to <11 years	1[d]	-	
11 to <16 years	-	-	
16 to <21 years	-	-	

359. "total uptake rate" suggests an uptake rate at a given moment in time. Line 363 suggests instead that the figure shows "aggregate exposure". Which is correct? A clearer explanation is needed of what Figure 4 shows.

Response: We apologize for the inconsistent terminology. We have now changed "aggregate exposure" to "total uptake rate", and checked for consistency throughout the manuscript.

382-393. Is the implied criticism of Harrad and Diamond valid? Two factors that influence the phenomena discussed here are the fraction of total emissions that occurs outdoors and the rate of decrease of the indoor emissions. Both of these factors can vary from chemical to chemical. I suspect that Harrad and Diamond were referring to chemicals with comparatively low outdoor emissions and indoor emissions that decrease comparatively slowly over time. I suggest that this text be reworded to avoid implied criticism of Harrad and Diamond.

Response: We accept the reviewer's advice and have rephrased our arguments, as follows:

"Our finding echoes a hypothesis by Harrad and Diamond that "over time…our main exposure route [to chemicals emitted indoors] will shift from indoor air and dust, to our diet".[53] The increasing contribution of far-field routes to aggregate exposure reflects longer lasting contamination in the rural environment than indoors, which, in the case of the PCBs, is primarily a results of extended outdoor emissions from ongoing outdoor usage.[28] The shift in the dominant route of exposure from near- to far-field is not unique. A recent investigation into the exposure of Faroese children to perfluoroalkyl substances (PFASs) also observes a remarkable shift from the dominance of near- (postulated to be dominated by non-dietary ingestion of PFASs and precursors) to far-field routes (i.e., consumption of contaminated seafood) during the past twenty years.[54]" (Lines 417 – 426 in the revised manuscript)

545. One of the co-authors names is misspelled!

Response: We have corrected the name.

Supporting Information, p.13. It is not "meaningless" to do these calculations. It may not be helpful in the context of the question being addressed.

Response: We have deleted the sentence "because it is meaningless to perform calculations at the level of individual planktonic cells".

Figures S3-S6. Why are only data up to 2005 used for the model evaluation? There are 10 more years of data available today, and this would certainly be helpful for the model evaluation.

Response: For Swedish first-time mothers' milk samples, we now include congener-specific observations from 1996 to 2016. See the newly added Figure 2 in the main text.

For herring samples, we now include congener-specific observations from 1989 to 2012. See Figure S3 in the revised Supporting Information.

Response to comments from reviewer 3

The authors have systematically investigated the importance of human exposure to PCBs from the indoor environment compared to the ingestion by consumption of food. They use a four block mechanistic model framework to simulate time-variant human uptake of two PCB congeners that differ in important physico-chemical properties. The overall goal of the paper is to identify cohort and age-dependent variations in the relative contribution of outdoor and indoor related PCB exposure routes to humans. The description of the model framework is straightforward and despite its complexity well understandable to me. However, I dislike the fact that evaluation of the two central blocks I and II is described in a companion paper that is still under review. Although this manuscript is made available to the reviewers, I feel overstrained by the implicit need of reviewing this second paper beforehand. Thus, I decided to just believe that the evaluation approach of the two model blocks was valid at all not compromising the conclusions of the manuscript to be reviewed here.

Response: We thank the reviewer for the general positive opinion on our manuscript. In the revised manuscript, the evaluation of blocks I and II from the companion paper, which has recently been accepted by *Environ. Int.,* is now briefly summarized so as to be readily assessable to readers. In particular, we describe the model's performance in reproducing the observed indoor air contamination,

as inhalation exposure discussed in this manuscript is dependent on the concentrations in indoor air. The following paragraph is now inserted into the manuscript:

> "Modeled concentrations in the indoor, urban and rural environments (outputs of Blocks I and II) have been evaluated in Li et al.[28] Briefly, the modeled concentrations agree with the means or medians of observed concentrations in indoor air, as well as in air, fresh water, estuarine water and soil in the rural environment of the modeled region, with differences of less than a factor of two. In particular, our modeled peak concentrations in indoor air (~45 ng m^{-3} for PCB-28 and ~13 ng m^{-3} for PCB-153) are within the ranges (14 – 296 ng m^{-3} for PCB-28 and 1.5 – 45 ng m^{-3} for PCB-153) measured in rooms with known PCB sources.[45-47]" (Lines 252 – 258 in the revised manuscript)

The manuscript is linguistically excellent and well-structured and the line of reasoning in the discussion is mostly convincing to me (for exceptions see comments below). The results are presented in a number of meaningful graphs and figures. However, Figures S3 – S6 are referenced several times in the results section leaving the reader with the impression that they are essential to understand the results and conclusions. Therefore, apportionment of these figures to the main text instead the supporting information (SI) should be thoroughly reconsidered by the authors. I know that ES&T limits the total space of their manuscripts, but in my opinion, the reader should be able to fully realize all results and understand the argumentation from the main text alone without being forced to look at figures in the SI. The Supporting Information is only meant to provide in-depth information for more advanced and more interested readers.

Response: We accept the reviewer's suggestion and have moved Figure S3, which is referenced multiple times in our manuscript, from the SI to main text (it now becomes Figure 2). Given the length of manuscript, we still keep Figures S4 – S6 in the SI.

All in all, I recommend the manuscript to be accepted for publication in ES&T after minor revision. In the following, I list the few points to be considered in this revision process.

The discussion of the chemical specific dominant processes (page 12 referring to Figure 2) is confusing. First, the statements made are valid only during childhood and not as bracketed in line 311 "in particular during childhood). Second, for the more volatile PCB-28 I can by no stretch of the imagination recognize that nondietary ingestion and inhalation of indoor air contribute almost equally. Even during childhood (up to an age of approx. 15 years) Figure 2a indicates much larger contribution from inhalation compared to non-dietary ingestion. However, inhalation and contribution from diet seem to be of equal importance.

Response: The reviewer is correct in pointing that our textual description does not match what is reflected in Figure 2 (now Figure 3 in the revised manuscript). The sentences are now rewritten, as follows:

> "For all ages, diet dominates the human far-field exposure to the two congeners. Inhalation of indoor air is always the single dominant route for near-field exposure to the more volatile PCB-28 (Figure 3a), whereas non-dietary ingestion contributes most to children's near-field exposure to the less volatile PCB-153 (Figure 3c)." (Lines 346 – 349 in the revised manuscript)

Insofar, the additional statement that this behaviour agrees with an earlier study (line 315) is puzzling to me. Figure

2a does not show the described behaviour of PCB-28 with respect to the importance of the two routes of near-field uptake.

Response: We regret that we had mistakenly referred to a wrong figure in that earlier study, i.e., Zhang et al. (2014). In fact, Zhang et al. (2014) presents two sets of simulation results: if a chemical has the same partition coefficients (K_{OA} and K_{OW}) as PCB-28, (i) when the chemical is assumed to be persistent, non-dietary ingestion and inhalation of indoor air contribute equally to near-field exposure, (ii) when the chemical is assumed to be highly degradable, inhalation of indoor air alone dominates near-field exposure. While we had intended to refer to the latter case, we erroneously referred to the former case. The sentence has now been corrected, as follows:

> …PCB-28 was assigned to a chemical cluster (defined by molecular weight, partitioning properties and indoor degradation half-life) for which inhalation of indoor air is of great importance in determining the total "indoor intake fraction"…(Lines 350 – 352 in the revised manuscript)

Caption of Figure 3 (lines 336-339) is confusing. Since the legend (given in Fig 3c) does not include a separate far-field route for breast-feeding induced exposure, I have no idea what is meant by "uptake rates at the age < 1 year are not completely displayed". For me, it looks as if they are not displayed at all. In case they are included in one of the illustrated categories, I wonder how and why this category should exceed the displayed scale in the figures except for 3c, which shows a steady decline from the very beginning.

Response: Breast-feeding induced exposure is included in the dietary uptake. In our model, diet is different between infants and children/adults: Infants are breast fed whereas children/adults consume contaminated food from the rural environment (Please refer to Lines 201 – 208 in our manuscript). Breast-feeding-induced exposure can be higher than food-induced exposure if breast milk is much more contaminated than the consumed food such as fish and beef, which would result in much higher uptake at infancy.

We have now rephrased the caption of Figure 3 (now Figure 4 in the revised manuscript) for clarity:

> "In Panels b, c, d and e, uptake rates at the age <1 year are not completely displayed because the dietary uptake by infants, which exceeds the displayed scale, results from breast feeding only, as opposed to consumption of contaminated food from the rural environment by children and adults."

We also include a sentence in the Method section:

> "Note that while uptake through breast-feeding is accounted for in our calculation it is not discussed in this paper." (Lines 202 – 204 in the revised manuscript)

Discussion of Figure 4 ends with the statement that "contrast between children and adults is more striking for PCB-153 than PCB-28". While this is true in terms of contribution of the different routes, I also see a large difference in total uptake between the younger childhood group (age 1) and all other age groups that is even more pronounced for PCB-28 than PCB-153.

Response: Yes, we agree with the reviewer's opinion and include this point in the revised manuscript.

"Within each panel of Figure 5, individuals of different ages live in the same contaminated environment, and the differences in the total uptake rate and the relative importance of near- and far-field exposures are attributed to the difference in human exposure factors. In general, children at age 1 have a lower total uptake rate than other age groups…" (Lines 397 – 400 in the manuscript)

Response to comments from reviewer 4

This manuscript describes a modeling framework used to estimate time-variant aggregate exposure to PCBs based on historical emissions and contamination data and modeling of PCB transport processes at work among "far-field" (i.e., urban and rural) and "near-field" (i.e., indoor) environmental compartments. The manuscript is very well-written, and the authors clearly understand and communicate their results. The information is well-organized, with the most critical material presented in the main text and additional details provided as supporting information. The results of this work will be extremely valuable for understanding and interpreting human PCB exposure data that have been collected over the past several decades. Furthermore, the methods applied here may be useful for understanding and predicting dominant sources of exposure for other types of environmental chemicals. However, in preparation for publication, the authors should consider providing clarification or additional information in response to the areas of concern listed below.

Response: We appreciate the reviewer's encouraging and helpful comments.

1. In the food-chain bioaccumulation module, absorption efficiency for PCB uptake from diet and soil was calculated from Kow based on organism-specific empirical relationships. However, on lines 169-170, it is stated that "We further assume that the chemicals inhaled with air and ingested with water are 100% absorbed by organisms." Can the authors offer some clarification regarding their assumption of 100% PCB absorption from air and water?

Response: We regret that we confused the reviewer. In fact, three kinds of absorption efficiencies are considered in this work:

(i) A gastrointestinal absorption efficiency (E_D), which is applicable to diet and soil and is assumed to be Kow-dependent;

(ii) An absorption efficiency of ingested water (E_W), which is applicable to drinking (for terrestrial species) and gill ventilation (for aquatic species);

(iii) An absorption efficiency of inhaled air (E_A), which is applicable to respiration.

We have now rephrased the sentences to distinguish the three absorption efficiencies, as follows:

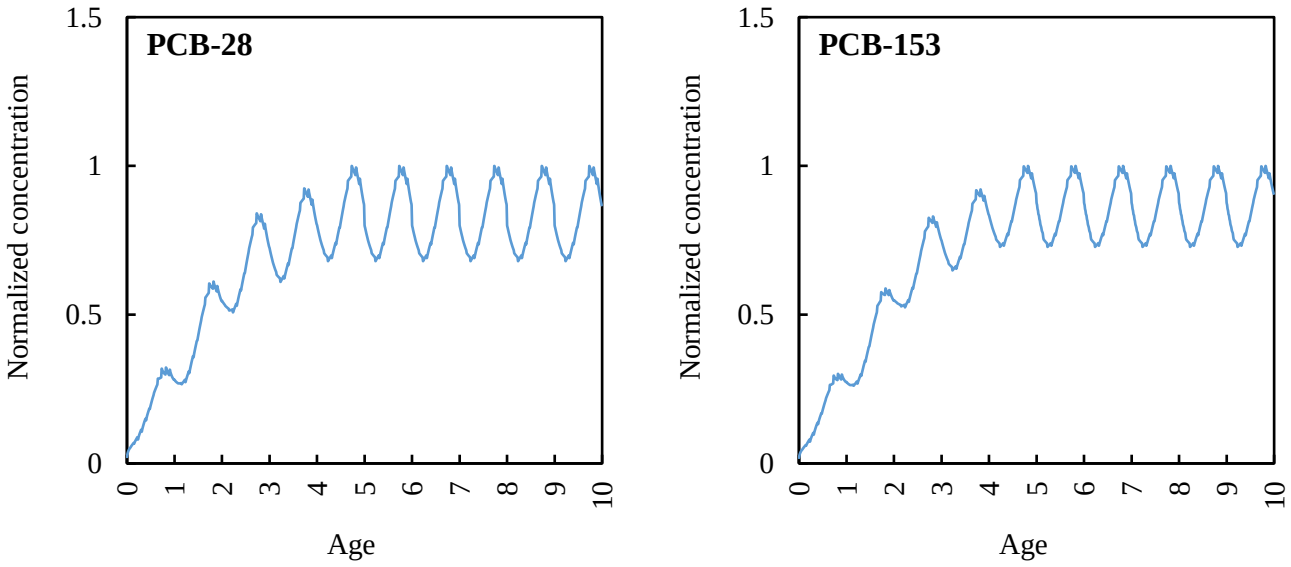
"Three absorption efficiencies are applied to convert the calculated intake to uptake: For gastrointestinal uptake (IN_{diet} and IN_{soil}), a gastrointestinal absorption efficiency (E_D) is organism-specific and calculated from the octanol-water partitioning coefficient (K_{OW}; Text S1).[31-33] For uptake from water (IN_{water}) and air (IN_{air}), absorption efficiencies (E_W and E_A) are assumed to be 100% unless indicated in Text S1." (Lines 170 – 174 in the revised manuscript)

2. On lines 181-184, it is stated that "For example, the cattle are slaughtered in the 28th month after their birth. Thus, the chemical concentration in each kind of food is calculated from its fugacity at the moment the prey dies. Contamination of milk is assumed to equal the fugacity in dairy cattle in the 28th month." What potential effect does

the assumption applied for milk contamination have on modeling results? Is the fugacity in dairy cattle highest in the 28th month (especially for PCB 153)? If so, would it result in an overestimate of milk contamination since most milk would presumably be harvested prior to the month of slaughter?

Response: In our model, beef cattle and dairy cattle are treated as two different organisms although their lifetimes are assumed to be the same. In order to respond to the reviewer's concern, we performed an additional calculation to demonstrate how concentrations of PCB-28 and PCB-153 evolve during the entire lifetime of dairy cattle if constant rural emissions are applied. As shown in the calculation results below, the fugacity in dairy cattle fluctuates seasonally because fugacities in environmental compartments fluctuate with time. Fugacity in dairy cattle does not reach its highest level in the 28th month – it is even lower than the level at the end of the second year (24th month). In other words, if milk is harvested prior to the month of slaughter, the contamination in milk could become slightly higher.

Furthermore, concentration in milk in the 28th month is approximately half of the maximum steady-state concentration (if the lifetime of dairy cattle is sufficiently long). This means that if we chose another arbitrary point near the 28th month, the uncertainty is only within a factor of two.



3. The paragraph on lines 235-238 is unclear: "Similar to the practice in the food-chain module, for gastrointestinal absorption (dietary and non-dietary ingestions, and ingestion of soil particles), we use a Kow-dependent absorption efficiency (E0) to convert calculated intake to uptake. The inhaled fraction is assumed to be 100% bioavailable." This text seems to say two different things: (1) absorption efficiency for PCB uptake from indoor air was calculated for the human toxicokinetic module as it was for uptake from diet and soil in the food-chain bioaccumulation module; and (2) absorption from air was assumed to be 100%. The supporting information file offers little clarification; absorption efficiency is not mentioned as a parameter used in the calculation of D-values for inhalation of either indoor or outdoor air. Were one or both of the methods described used to calculate PCB uptake from indoor air in the human toxicokinetic module? If both methods were used, what conditions determined which was used for a given calculation? Also, if an absorption efficiency was calculated for PCB uptake from indoor air, it is not immediately clear why this would not also be important to include when

calculating PCB uptake from outdoor air (described on lines 210-212).

Response: Similar to comment 1, our unclear expression again confused the reviewer.

For humans, we also considered three kinds of absorption efficiencies:

(i) A gastrointestinal absorption efficiency (E_D), which is applicable to dietary, non-dietary and soil ingestions, and is assumed to be Kow-dependent;

(ii) An absorption efficiency of ingested water (E_W), which is applicable to water drinking and is assumed to be 100%;

(iii) An absorption efficiency of inhaled gaseous phase of air (E_A), which is applicable to inhalation of indoor and outdoor air and is assumed to be 70% (which was mistaken for 100% in the original manuscript).

We have now rephrased the sentences to distinguish the three absorption efficiencies, as follows:

> "For gastrointestinal absorption (dietary and non-dietary ingestions, and ingestion of soil particles), we use a K_{OW}-dependent absorption efficiency (E_D)[33] to convert calculated intake to uptake. For inhaled air, the absorption of gaseous contaminants is associated with an efficiency (E_A) of 70%, and that of contaminants in the particulate phase is based on particle size-specific deposition fractions in the human respiratory system.[42] Contaminants in ingested water are assumed to be fully bioavailable, i.e. E_W = 100%." (Lines 238 – 243 in the revised manuscript)

We now also distinguished clearly the three absorption efficiencies in the SI.

4. This statement on lines 288-290 may be too strongly worded: "Our work indicates that the underestimation is due to the higher than expected emissions from the indoor in-use stock, rather than the uncertainty associated with emissions from industrial processes." The modeling work described in this manuscript may provide an alternate, more probable explanation than that previously given for underestimated PCB concentrations prior to the mid-1970s by Breivik et al. But, it is too much to say that this work conclusively indicates the reason behind that underestimation. The authors should consider more careful wording along the lines of something like "Our work suggests that the underestimation may be explained by the higher than expected emissions from the indoor in-use stock, rather than the uncertainty associated with emissions from industrial processes."

Response: We agree with the reviewer and rephrase the statement, as follows:

> "Instead of incriminating industrial processes, our work attributes the underestimation to higher emissions from indoor in-use stock in early years." (Lines 304 – 306 in the revised manuscript)

5. The authors provide a good discussion of some of the implications of their work in the paragraph from lines 405-421. Especially interesting is the text at lines 416-421: "Particularly, for PCB-28, a shift from the dominance of near- to far-field exposure, i.e., the point at which far-field contribution exceeds 50% (Figure 5), occurs in mid-1970s. Such a shift does not occur for PCB-153. For this reason, ignoring near-field exposure would cause negligible bias in estimating the body burden of PCB-153, but would substantially underestimate the body burden of PCB-28 in the early days of PCB use." The impact of this observation may be increased by expanding this discussion to comment on the expected implications for PCBs other than PCB 153 and 28. PCB-28 is relatively

short-lived compared to PCB-153, but its biological half-life is very long compared to other congeners of similar molecular weight, many of which are found at high concentrations in "near-field" environments. While a failure to address near-field exposure routes would bias estimates of PCB-28 body burden for populations exposed decades ago, would such a failure bias body burden estimates for other lower-chlorinated PCB congeners in more recent years or even today?

Response: We appreciate and agree with the reviewer's insightful suggestion. A failure to address near-field exposure routes would also bias estimates of body burden of other lower-chlorinated PCB congeners, especially those used indoors in more recent years or even today (e.g., Vorkamp et al. *Sci. Total Environ.* 2016, 541, 1463-1476 and Herkert et al. *Environ. Sci. Technol.* 2018, Published ASAP).

We now include this point into the revised manuscript, as follows:

> "Given that a number of lighter chlorinated congeners (e.g., PCBs 4, 8, 11, 47, 51, 52, 68 and 77),[55, 56] which are as volatile and degradable as, if not more than, PCB-28, are found in non-Aroclor products like household pigments currently used indoors, our result highlights the need of in-depth investigations into the near-field sources and exposure routes for these compounds." (Lines 456 – 460 in the revised manuscript)