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Atmospheric Chemistry and Physics Discussions

Interactive comment on "GEM/POPs: a global 3-D dynamic model for semi-volatile persistent organic pollutants – 1. Model description and evaluations" *by* S. L. Gong et al.

S. L. Gong et al.

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We would like to thank the anonymous reviewer for the detailed review of our manuscript which gives us the opportunity to clarify some points. In the following we quoted each review question in the square brackets and added our response after each paragraph.

Specific concerns

[According the description on page 3405-3406 (section 3.3), initial seawater concentrations was derived from the output of a (northern) hemispheric model (MSC-East). How could this be used to provide initial seawater concentrations for the southern hemisphere in the GEM/POPs model?]

Oceanic concentrations of PCBs from the MSC-East hemispheric POP model (Gusev et al., 2005) were used to spin up the GEM/POPs model. After two years spinning up, seawater concentrations for the southern hemisphere were built up and these oceanic concentrations of PCBs were used to provide the required spatial distributions of initial seawater concentrations.

[Did the authors do any attempt to compare the predicted seawater concentrations (output from the MSC-East model used as input to GEM/POPs) with real monitoring data? Please elaborate on this in the manuscript.]

We compared the predicted seawater concentrations (output from the MSC-East model) and found they were underestimated. After the two years' spinning up of our GEM/POPs, the sea water concentrations match with the observations (Sobek and Gustafsson 2004) very well for PCBs 156 and 180 with an over-estimate of the PCB28 on the oceans. A new figure 1b is added to show the comparison of model results with measurements on a cruise ship track.

We used these concentrations as the initial oceanic concentrations of the GEM/POPs for this study.

A paragraph was added:

"Initial oceanic PCB concentrations were also evaluated against available observations. After two years of spin-up from the MSC-East model outputs, the sea water concentrations match the observations (Sobek and Gustafsson, 2004) very well for PCBs 156 and 180 (Fig. 1b) along the cruise ship track. Magnitudes and trends of these two PCBs along the latitude direction were reasonably reproduced. For PCB 28, the latitude trend was produced but a 6 time over-estimate for the oceanic surface concentration was produced (Fig. 1b)."

[Besides, it would be interesting if the authors could please expand on the assimilation technique used as well (p3406, lines 3-4).]

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The method to combine the MSC-East model and the observed distributions of soil concentrations is through a data-assimilation method (3D-Var). The main idea is to minimize the difference between the observations and the modeling results to achieve a more reasonable soil distribution than the original soil concentrations from the MSC-East model. We will expand the methodology in the paper.

[Finally, the authors compared their model predictions with results from many different sites (and hence also chemical laboratories). How could possible differences in sampling and analytical methodologies have affected the overall interpretation of results?]

We don't think sampling and analytical methodologies will affect the overall interpretation of results. However, the measurement methodology applied at various sites and network is different in terms of their sampling methods (e.g. frequency) and laboratory analysis. These differences will have an impact on the way the modeling results are extracted and compared.

[It would have been nice if the authors had included some tables with correlation coefficients (model vs observed) to support the discussion around model performance.]

We have added a figure (Figure 4) to show the correlations between model and observation results and added a paragraph in the text:

"Figure 4 shows the comparisons for the three PCBs between the modeling results from GEM/POPs and the observations averaged over IADN stations for year 2000. The predictions for PCB180 are well within a factor of 2 from the observations. However, for PCB28 and PCB153, there are some over-estimates by GEM/POPs but majority of the results are within a factor of 3. The over-estimate is especially evident for PCB153, indicting some systematic bias in the model."

Minor issues

[Abstract, line 10: Please delete "are" or rephrase.]

Done.

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[Page 3398, line 20: Dioxins are not waste but commonly termed (unintentional) byproducts of combustion.]

Changed.

[Page 339, lines 10-11: I would strongly suggest that authors are a bit more cautious in their critique of the multimedia models. Specifically, I partly disagree with the statement that these models fail to accomplish the detailed spatial and temporal distribution of a compound. For example, these models are typically characterised by their ability to predict the (long-term) temporal distribution of POPs, reflecting the life-time of these compound in the environment (which is often of key interest), although they typically lack a high spatial and temporal (short-term) resolution as the 3-D models. Please S618 consider to rephrase the statement, e.g. "these (multimedia) models are less suitable to predict the detailed spatial and short-term variability in the distribution of a compound".]

According to reviewer's suggestion, this sentence has been replaced by:

"However these (multimedia) models are less suitable to predict the detailed spatial and short-term variability in the distribution of a compound"

[Page 3399, line 17: The authors claim that the 3-D models are more accurate in terms of describing transport, reactions and removal. Although I agree that it should be correct with respect to transport, it may be less correct in terms of chemical reactions and removal because the latter processes are still not very well understood and parameterized in the first place, irrespective of which models that may have implemented these processes. Thus, a model that has a high spatial and temporal resolution of uncertain processes may not necessarily be more accurate, although it could possibly be claimed to be more detailed in its description of these processes.]

This is a very good point. We agree with the comments. The 3D models are generally better in the transport and removal processes as they use more realistic winds and pre-

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cipitations than the multimedia models. However, the chemical reactions and removal processes may not be well understood, which will generate some uncertainties for both 3D and multimedia models.

[Page 3399, line 19: It does not seem to me that Sahsuvar et al. 2003 is an appropriate reference for PCBs (rather HCHs).]

We think her thesis was primarily on the fate of PCBs in the atmosphere with a focus on gas-aerosol partitioning.

[Page 3401, line 7: In addition, emission fields would probably be needed to simulate other POPs?]

This sentence has been modified as:

"To apply GEM/POPs to other POPs, the physiochemical properties, chemical reaction rate constants and emission fields of a specific POPs are needed."

[Page 3405, line 17: In general, it is unfortunate to refer to personal communication in a scientific paper. Instead, it would be better to cite previously published studies providing similar insight (e.g. Wania and Daly, 2002 Atm Env 36: 5581-5593; Wania and Su, 2004 Ambio 33: 161-168, Meijer et al. 2003 ES&T 37: 667-672, Hung et al. 2005 Atm Env 39: 6502-6512) and remove the citation to personal communication.]

This statement is deleted and replaced by:

"According to Wania and Daly (2002) the maximum emission data is better to reflect the reality than mean and minimum emissions and is therefore used in this study."

[Page 3406, lines 17-19: It seems irrelevant to discuss the sampling frequency before 1994 when this study refers to the year 2000. It would also be desirable and appropriate if the authors could cite some key reference to the IADN data discussing the monitoring results for PCBs (papers focused on the year relevant to this study, i.e. 2000).]

Due to the large variation ranges of PCB observations from IADN, we are using the

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observational data from 1993 to 2003 to obtain the range of observations which is used to illustrate the range of agreement of model and observations.

[Page 3407, line 17: Presumably, the authors suggest that observed concentrations at Kosetice were not captured by the model (not simulated). Secondly, the authors (quite reasonably) suggest that this may have something to do with potential local PCB sources not accounted. However, I also assume that the model is not designed to reproduce possible pollution gradients within a 2 by 2 deg grid?]

That is correct. There are two reasons for the large discrepancy at this location. One is the local emissions and the other is the model resolution of 2x2 degree that is hard to capture the very large pollution gradients.

[Page 3407, lines 19-21: Sentence needs to be rephrased and clarified ("a large gaseous PCB153 and PCB180" does not make any sense).]

Changed to

"On the contrary, the GEM/POPs predicted higher concentrations of gaseous PCB153 and PCB180 at Burnt Island than the IADN observations."

[Page 3408, line 4: Please be more specific about what is meant by "several years around 2000". For example, plus/minus x years.]

The sentence is modified as: Wherever possible, the measurement data for several years around 2000, observation data between 1994-2004 for EMEP sites, 1992-2000 for Alert site and 1993-2003 for IADN, have been used to generate a vertical box to show the variation range of measured PCBs (gray box with whiskers).

[Figure 2: The table with site information needs to be checked in column "country". Please also include site information for the Alert station, as it is also discussed in the associated figures.]

Revised. Alert Station information: 82.50 N / 62.33 W was added.

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[Figure 1 and 5: Is it possible to publish maps in colour in the final version?]

Yes. We will request to the editor's office to publish them in colour.

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