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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.

Anonymous Referee #2

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General comment

The submitted study by Gong et al, is an important and significant contribution to the understanding of the global transport and fate of PCBs. This study clearly represents a major research effort, in order to develop and parameterise the model. It is particularly noteworthy for its attempt to evaluate model predictions against observations from major air monitoring stations in the northern hemisphere. In general, the discussion of results is transparent and clear, and it is anticipated that the study would be of significant interest to those that are interested in the global fate of Persistent Organic Pollutants. However, I miss more information on the model parameterisation (and evaluation) for

the southern hemisphere as detailed below.

Specific concerns

According the description on page 3405-3406 (section 3.3), initial seawater concentrations were 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? 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. Besides, it would be interesting if the authors could please expand on the assimilation technique used as well (p3406, lines 3-4). 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? Finally, 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.

Minor issues

Abstract, line 10: Please delete “are” or rephrase.

Page 3398, line 20: Dioxins are not waste but commonly termed (unintentional) by-products of combustion.

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

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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”.

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 parameterised 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.

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

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

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.

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 focussed on the year relevant to this study, i.e. 2000).

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

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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?

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

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

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.

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

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