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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling atmospheric transport of persistent organic pollutants in the Northern Hemisphere with a 3-D dynamical model: DEHM-POP" *by* K. M. Hansen et al.

Anonymous Referee #2

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Review of paper:

Modelling atmospheric transport of persistent organic pollutants in the Northern Hemisphere with a 3-D dynamical model: DEHM-POP

by K.M. Hansen, J.H. Christensen, J. Brandt, L.M. Frohn and C. Geels

The submitted paper is a contribution to the understanding of the atmospheric transport and fate of a pesticide (a-hexachlorocyclohexane) in the Northern Hemisphere. This study complements past modelling efforts, as undertaken by other research groups. The paper is well written and well structured, and the discussion of results is generally transparent and clear. In particular, the section evaluating model predictions against

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monitoring data is important. In general, the paper holds an adequate standard, and it is believed that the results should be of interest to many readers of this journal. Still, the authors are kindly requested to consider some mainly minor issues that are detailed below. In particular, some firm statements should be reconsidered, and some more literature research on detailed topics may be desirable as detailed below.

Title: The paper has a title that refers to this as a modelling study of persistent organic pollutants (POPs) [In plural]. In reality, only one single chemical is addressed in the paper (a-HCH). The authors should therefore consider changing the title of the paper by replacing POPs with a-HCH. This may seem like a minor issue, but there is no guarantee that the model will work equally well for other POPs (e.g. those POPs that are more sorbed to particles in the atmosphere).

Page 1340, lines 19-22: An additional criteria that is frequently used and referred to is toxicity.

Page 1340, line 24 until page 1341, line 2: Strictly speaking, the environmental fate is also strongly affected by the environmental conditions (OC content etc) as well as temperature in addition to the physical-chemical properties listed.

Page 1341, line 10: Please consider; POPs may have adverse health effects

Page 1341, line 12; The term lipophilic is not very good as most organic chemicals are fairly equally soluble in fat and other organic phases. However, their hydrophobicity may be very different as expressed by Kow. Please, consider to change lipophilic with hydrophobic.

Page 1341, line 14: Potential harmful effects?

Page 1341, line 17: It is claimed that the atmosphere is the major pathway of POPs to the Arctic. This is a very firm statement, which I doubt is correct for all POPs in general, and it should therefore not be kept in the paper without references. See e.g. Li et al 2002. Sci Total Environ 291: 229-246.

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Page 1344, lines 6-10: Please consult Li et al 2003, Environ Sci Technol 37: 3493-3498 for more recent information. The authors may also want to consider emphasising: Technical HCH is historically the most used insecticideĚ

Section 2.2.2 (page 1345): Atmospheric reaction is clearly a key process in this model. I miss a clear statement if spatial and temporal variability of OH-radical concentrations are taken into account, and if temperature dependent degradation rates have been considered. I think that the issue of atmospheric reaction of a-HCH deserves to be discussed in some more detail in the paper, e.g. referring to the data presented by Brubaker and Hites, 1998 Environ Sci Technol 32: 766-769.

Degradation rates in soil (p 1347, line 18) and water (p 1348, line 12): It is unfortunate that the authors simply adopt these parameters as used in the earlier modelling study by Strand and Hov without any further attempt to discuss the literature on the subject and discuss these input parameters in some more detail.

Page 1349, line 13 versus line 26. First a-HCH is described as a compound having great persistence, and next the relative short lifetime in soil are emphasised. This is somehow contradictory.

Page 1352, line 21. Seasonality is discussed in terms of re-volatilisation processes, but the possible impact of seasonality of emissions is not mentioned in this paragraph.

Page 1354, line 10. The important assumption of uniform emissions throughout a year should have been explicitly mentioned already in section 2.4.2.

Page 1355, line 11. This is wrong. There have been other dynamical models used to study a-HCH as dynamic may be interpreted as non-steady state, thus facilitating an analysis of the environmental response to changes in emissions. Additional examples are e.g. Wania et al 1999 and Toose et al 2004 as listed in the paper, as well as e.g. Breivik and Wania, 2002 Environ Sci Technol 36: 1014-1023.

Page 1355, line 4-5. It is claimed that the results in this paper are better than the results

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from previous models (assuming that this refers to previous attempts using multimedia box models in section 4.1). The term "better" is certainly a relative measure as different models have been developed and used to answer different questions. If the authors really want to keep this firm statement, they should be really cautious and critically compare and contrast how their overall modelling approach and specific results may be considered superior to past studies. As stated by Wania (1999) it is important to note that the low spatial and temporal resolution of multimedia box models often is a deliberate restriction rather than a regrettable shortcoming. This is based on the belief that the predictive capability of numerical models of environmental POP behaviour is not limited by the resolution of atmospheric transport processes, but rather by the uncertainties inherent in emission estimates, physical-chemical properties, degradation rates, and air-surface exchange descriptions of POPs (all of which are correctly recognised as uncertain parameters and processes in this paper). Adding complexity in the presence of such uncertainty does not necessarily improve a model nor make it better. It would therefore probably be better to regard this study as complementary to past studies, rather than better?

Wania, F., 1999: Differences, Similarities, and Complementarity of Various Approaches to Modelling Persistent Organic Pollutant Distribution in the Environment. WMO/EMEP/UNEP Workshop on Modelling of Atmospheric Transport and Deposition of POPs and Heavy Metals, Geneva, Switzerland, 16 -19 November 1999. http://www.wmo.ch/web/arep/reports/gaw136_vol1.pdf

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