Atmos. Chem. Phys. Discuss., 9, C1012–C1015, 2009
www.atmos-chem-phys-discuss.net/9/C1012/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source regions of some Persistent Organic Pollutants measured in the atmosphere at Birkenes, Norway" *by* S. Eckhardt et al.

Anonymous Referee #1

Received and published: 20 May 2009

Review of: Source regions of some persistent organic pollutants measured in the atmosphere at Birkenes, Norway

Paper by: S. Eckhardt, K. Breivik, Y.F. Li, S. Manø and A. Stohl

20 May 2009

1 Summary

This paper describes the application of a Lagrangian transport model, FLEXPART, to interpret four years of new data on concentrations of PCB-28 and γ-HCH at the EMEP site in Birkenes, Norway. The paper is interesting and novel in that the authors apply the model to link geographically resolved emission estimates for these substances to observations at the Birkenes site, which is relatively remote from most sources. This type of applcation of transport modeling nicely addresses the scientific questions that underlie international regulation of chemical substances, such as under the UNECE CLRTAP convention.

It is difficult to assess all of the technical aspects of this paper because the functioning and assumptions of the model are not presented here in detail. However, the results

and the authors interpretation is for the most part sensible, and the work thus appears to have been competently performed. My main criticisms of the paper are, first, that the authors make a clearly erroneous assumption about the temperature dependence of emission rates of the semi-volatile chemicals considered here, and second, that the work is placed within the context of a very narrow review of earlier research in this area.

My most fundamental problem with the paper is the author's assumption that emission fluxes in each grid cell are linearly dependent on temperature (Equation 5). Presumably, emissions vary with temperature because of variability in the vapor pressure of PCB-28 and γ-HCH. And, the ideal gas law dictates that the logarithm of vapor pressure varies in direct proportion to 1/T, with the proportionality constant being the heat of vaporization. Therefore, in assuming that emissions vary linearly with temperature, the authors have failed Environmental Chemistry 101!

A second problem with the paper is that the work is interpreted within only a very narrow review of other research that addresses similar issues. The most glaring omission, in my opinion, is the lack of references and discussion of the Potential Source Contribution Function (PSCF) modeling work that has been done to interpret long-term monitoring data from the IADN sites in the Great Lakes. One reference to this work is: Hafner, W.D., and Hites, R.A. *Environmental Science* & *Technology*, 2003, 37, 3764-3773. Like this approach, PSCF modeling also combines long-term monitoring data with a trajectory model run in reverse to estimate the location of source regions. I think it is necessary for the authors to acknowledge this previous work and state differences of their approach. I suspect that the PSCF approach is exactly what the authors of this paper have in mind in their outlook in the last sentence of the paper prior to the acknowledgements.

2 Detailed Comments

Page 3, "Depending on temperature and the concentrations..." The "potential" to undergo reversible exchange is not a function of temperature or concentration.

Page 3 (last paragraph): In this paragraph the authors use a very narrow survery of the literature to justify their modeling approach. Their approach is novel and interesting, but there are other tools available that are capable of something similar. For example, multimedia box models have evolved a long way since MacKay, 2001, and there are now models that consider meteorological data in their parameterization, most notably the BETR-Global model (MacLeod et al. *Environmental Science* & *Technology*, 2005, 39(17): 6749-6756. This study is novel and interesting because of the close marriage of modeling and monitoring data, not because the model is somehow superior to other models.

Page 5: Here, the ES and EC concepts are introduced. These are important for interpreting the results that are presented, and I would like to see this section revised to try to communicate these concepts more clearly. Again, I believe there a precident in the literature for these ideas that is not cited in the manuscript. Don Macky (*Environmental Pollution*, 2008, 156, 1196 - 1203 1182 - 1189) has introduced the concept of "distant residence time" as the mass of contaminant in a distant region divided by the emission rate. I believe this is the same concept as the "ES" that is introduced here, and at least a reference is warranted in the paper.