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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Soil-air exchange controls on background atmospheric concentrations of organochlorine pesticides" *by* A. Cabrerizo et al.

Anonymous Referee #2

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The manuscript reports on the seasonal and spatial variability of soil-air partitioning of organochlorine pesticides (OCPs) in Europe. It is concisely written with detail statistical analysis and graphical presentation. The finding of soils being secondary source of OCPs to the atmosphere is not novel, but being able to carry out such experiments in multiple sites and different times of the year is rare and adds valuable information about soil-air partitioning. Publication is recommended after consideration of the following comments.

General comments:

Did you see breakthrough of HCB while sampling in the summer months? This may

worth to mention in the analytical section, as the loss of HCB may affect the data interpretation. HCB does not showed seasonality like other pesticides. Is there a reason for this?

p. 6 line 13. The recoveries of for some of the chemicals were low, i.e. ∼50% in both air and soil. Are these the same chemical? Do you think this would be a problem if one chemical's recovery is not consistent, e.g. say in soil is 50% and in air is 90%?

p. 6, last sentence. Why is altitude an appropriate proxy for organic matter (OM) quality? I don't see the importance of altitude here in the paper. I think it is a very indirect factor which other variables such as soil organic carbon or nitrogen content, pH, vegetation type, organic carbon aromaticity etc. may be a better proxy. Can you provide some reference to show the relevance of altitude.

p. 9. It is interesting to see ppDDE/ppDDT<1 at some sites. What is the ratio in the air? In fact, I would like to see the authors compare the isomer ratios (e.g. a/g-HCH ratio) between air and soils. It would be interesting to see if the isomer ratios of soil and air agree during net volatilization. Chiral analysis would also be a good tool to study the soil-air exchange but unfortunately it is not within the scope of the study.

Specific comments:

In Section 2.1, first line. It says "ten" sites, but I can only find nine (see Table S1). Please clarify.

p. 7 lines 8-13. This part sounds contradictory. At first it says the OCP concentrations showed seasonality, and then the next sentence says that they do not change dramatically as the PCBs. Suggest to re-phrase the statements.

p. 7 line 10. Please specify which "legacy pesticides". Do you mean the OCPs in the current study?

p. 7 line 16. Růžičková, not RůŽičková

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11, C12314–C12316, 2011

p. 7 line 22. NOOA should be NOAA - National Oceanic and Atmospheric Administration.

p. 8 lines 5-11. The fugacity of air is described and presented here. However, the authors do not provide any context or interpretation of the data. I wonder if this is the appropriate place to bring this up. It might be better if it is described in the latter section (e.g. section 3.5) where fs/fa is discussed.

p. 8, line 9. To be consistent with the notation of fraction, use ng m-3 instead of ng/m3.

p. 9, line 4. Delete bracket before “Najera”

p. 12, equation 3. Please provide unit for Ksa.

p. 12, line 4. References for Su et al. (and others) report temperature correlation with HCHs in air rather than Ksa.

Figure 1. Caption for b-HCH, the R2 and p-value both equals to 0.59. Please check whether there is a typo for the p-value.

References are not consistent. Some references have issue no. but some don’t. Please check.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 25937, 2011.