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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Factors affecting the atmospheric occurrence and deposition of polychlorinated biphenyls in the Southern Ocean" *by* C. J. Galbán-Malagón et al.

Anonymous Referee #1

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The Manuscript "Factors affecting the atmospheric occurrence and deposition of polychlorinated biphenyls in the Southern Ocean" presents air concentrations of PCBs in the Antarctic and based on the presented and other earlier published results the Authors estimate partition of PCBs to aerosol, air-water exchange fluxes, temperature dependence of gas-phaseconcentrtions and other relevant factors. The authors have a very strong theoretical background and thus their results elaboration is very solid. Some comments though follow hereafter.

Specific comments 1) On the structure: a) Personally I found the manuscript a bit difficult to follow and it would benefit from a small restructure. The reason is that the

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reader should always remember what each acronym means (ICEPOS, ATOS etc) and to which sampling sites and periods they refer to. b) I believe that all equations and explanations of the nomenclature should go to the methodology part. That way the flow and understanding of what the Authors did is not interrupted.

2) On the analytical part: I don't find very clear the presentation of the LODs/LOQs. The Authors mention that they divided the standard of the lowest mesureable concentration with the air volume of the samples. However, the air volumes in each campaign were different. In addition, if they have divided with the air volume, probably the units should always be "pg/m3" and not just "pg" (both can be foud in the text and tables". For the blanks, the Authors need to add if the blanks used were only the PUFs, or filters too.

3) On the results: Personally, I find the discussion on "ΣPCBs" and not on individual PCBs as not appropriate. Especially when the Authors themselves mention that the comparisons with other periods/studies might not take into account the same number of congeners. I believe that an effort to compare the same congeners could be taken. In addition, in my opinion, when the authors compare with studies of 20 years ago, they should do it bearing in mind that the air concentrations of PCBs in America and Europe and prssumably all over the world have shown a net decline during these last two decades. So, the fact that we still today find similar concentration levels, might require some extra thoughts/insight.

Regarding the comparison with other studies, the Authors could add a recently published paper by Kallenborn et al. published in ACPD, 13, 6219–6246, 2013.

4) At a certain point the Authors mention that "There are no significant differences between the PCB concentrations measured in air samples influenced by the three characteristic air masses (Fig. S7 in Annex IV)." The way this is written, it appears that in gneral the different air mass origins/directions will not affect the PCB concentrations. However, a close look at the results shows that bi variations can take place even within two consecutive air samplings. For example: samples GA3 and GA4 in table S6,

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present differences in air levels of up to 10 times (see for example PCB52 - 2.90 vs 0.29 pg/m3). Where would the Authors attribute these big differences?

This particular sample (GA3), if I am not wrong, is also one of the samples presented in aerosol-gas phase partition (Fig 3). It would be nice to have some more insight about these inter-sample differences.

Regarding the gas-particle partition, the concentrations of PCBs in aerosols for ATOS II, in Table S8 are named as A1-A4. Do these correspond to GA1-GA4 of Table S6? And then again, the aerosol sampling lasted longer than the gas phase. Do these results really correspond to each other, or was there overlapping of one aerosol sampling event with more gas-phase samplings.

Technical comments: Some very minor errors in the use of English.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18779, 2013.