

Interactive comment on “Spatial Distribution of the Persistent Organic Pollutants across the Tibetan Plateau and Its Linkage with the Climate Systems: Five Year Air Monitoring Study” by Xiaoping Wang et al.

Anonymous Referee #1

Received and published: 1 April 2016

Review of “Spatial distribution of the persistent organic pollutants across the Tibetan plateau and its linkage with the Climate systems: five year air monitoring study”

This is a well written and interesting paper reporting a relevant sampling effort of the POP concentrations in the Tibetan Plateau. The data set is novel and merits to be published. The limitations of the manuscript come from the fact of using passive samplers integrating the concentrations over one year. The authors make a poor job quantifying the uncertainty that this sampling approach introduce in the results. This is my only major comment to this manuscript, and it needs to be solved before publication. The

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other minors issues can be found below.

- Page 2, Line 6. “climate interactions” is mentioned when the abstract does not deal with these!
- Page 2, Line 15. As the text says “are still ubiquitous”, I suggest to update these references that are 10-17 years old!
- Introduction. Climate or weather fluctuations? Or both?
- Figure 1. There is no contrast of the bars over the map. I recommend a re-design of the map and figure in general.
- The passive samplers were deployed for 1 year! The authors argue that the obtained concentrations are the annual average. Is this true? Atmospheric concentrations of POPs do show a high variability depending on wind directions, season, etc. If we focus on the influence of temperature, it is well known that gas phase concentrations show an inverse correlation with temperature. The gas-XAD partitioning coefficient is also temperature dependent. The influence of temperature is not linear (it is logarithmic), and therefore a passive sampling cannot get the average concentration over a long period of time with important temperature fluctuations. The influence of other variables can also be non-linear. The authors need to address these important issues, make the appropriate corrections, introduce an assessment of error propagation in their concentration estimates, and then discuss accordingly to the new results.
- Page 5, line 19. Storing XAD at -20C is a bad practice. Freezing will allow the humidity in the pores of XAD to break the XAD and introducing a blank problem.
- Re-assess the uncertainties, as 35% is too low for the sampling + estimation of sampled volumes + chemical analysis.
- Page 7, line 21. What is good agreement? Quantify this.
- Pages 7-8. To compare among sites and different studies in ng/sample does not make

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any sense, unless the authors first demonstrate that the exposure time, the temperature and wind speeds are similar among the different samplers. I suggest to remove all this comparison, as it is useless. The only comparison possible is qualitative, and maybe on patterns.

- Page 9. The statements on the comparison of patterns should be supported by the appropriate statistical tests.

- Page 10. Which is the uncertainty on the fugacities in soil?

- Page 11. If air and soils are close to equilibrium, then it means that they are coupled in terms of concentrations. This means that the atmosphere supports and controls the soil concentrations of POPs, but that the soil support and control the atmospheric concentrations of POPs. This is obviously a secondary source. Equilibrium does not mean a zero flux, it just means that the volatilization flux is similar in magnitude to the depositional flux. A close air-soil coupling has also been observed using methods with lower uncertainty (Cabrerizo et al. EST 2013, Degrendele et al. EST 2016).

- Page 11. The lack of correlations between soil and air fugacity do not show anything, as the air concentrations are pseudo annual averages and not measured simultaneously with the soil samples. I would remove this paragraph.

- Obviously, the authors can not identify the seasonal variation, but as it may occur, does it affects the discussion?

- Page 14, line 21. I disagree that PAS are cheap and simple. It is simple to obtain a value for ng/sample, but very difficult to obtain a value for ng/m³ with an acceptable uncertainty that allows you to discuss certain patterns and processes. Therefore, both the "results"/cost and "new knowledge"/cost ratios may not be favorable neither.

- The paper relies too much on tables and Figures in the supplementary material. Sometimes is difficult to follow.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-89, 2016.

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