

Interactive comment on “Quantifying the vertical transport of CHBr₃ and CH₂Br₂ over the Western Pacific” by Robyn Butler et al.

Anonymous Referee #3

Received and published: 6 January 2017

The manuscript investigates the vertical transport of very short-lived halocarbons over the Western Pacific based on model simulations and aircraft measurements. Bromoform and dibromomethane observations from two aircraft campaigns are linked to simulations of tagged tracers and age of air. The study is in general of interest to the readership of ACP. However, the analysis is not presented clearly and major uncertainties and assumptions are not discussed appropriately. Moreover, the discussion of the results is confusing in many places. I suggest publication after major revisions addressing the comments listed below.

1) Results in section 4.2 regarding the tagged-VSL model output depend very strongly on the chosen emission scenario. Most of the information presented here (i.e., the amount of coastal versus open ocean emissions contributing to upper air mixing ratios) could be quite different for another emission scenario. This aspect is not addressed or

discussed at all in the manuscript. Given the large differences between the different emission scenarios and existing research investigating those differences and the implications for atmospheric mixing ratios (Hossaini et al., 2013; Hossaini et al., 2016) a proper discussion is required. Ideally, the study should be carried out based on at least one more emissions scenario in order to understand the uncertainties resulting from the assumptions made here.

- 2) The choice of the emission scenario is not discussed. Why top-down and not bottom-up? Which scenario is thought to be the most realistic in this region? Why is this scenario used if the simulated surface mixing ratios show large deviations to the observations? Could these deviations be minimized for a different (lower) emission scenario?
- 3) What would cause land sources of CHBr3 and CH2Br2? In the introduction, only marine sources are discussed, but later the reader is confronted with the land tagged tracer and its contribution to the observed mixing ratio.
- 4) The discussion of the model evaluation (section 4.1) needs to be improved. How large are the relative deviations between model and observations. If the bias is mostly a result of the emissions used, than the relative differences should stay constant with height. If however, the relative differences increase or decrease with height this would indicate errors introduced by the transport scheme of the model. Even though, there are six panels used to discuss the comparison such conclusions are currently not possible.
- 5) Please provide the model resolution. At the moment only the resolution of the meteorological input data is given. Is this the same as the model resolution and the resolution of the output data? How would this quite coarse resolution ($2^\circ \times 2.5^\circ$) impact the results? In particular, how would this impact the model-based analysis of the observations?
- 6) Please improve description and discussion of Figures 9, 10 and 11. It is difficult to understand what has been done and why some of the statements are made. See also

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detailed comments further below.

Minor comments

Page 5, line 4-12. Please explain Figure 2. Are the tagged tracer regions shown or are the tracer regions combined with the emission scenario shown? How do you end up with 20 tagged tracers? Seven for CHBr3 and seven for CH2Br2 and the rest for total and background?

Page 5, line 15. Please explain what 'de-seasonalized monthly means'? Are you using annual means? Or interannual anomalies plus mean values?

Page 5, line 26. This sentence makes no sense. You use age of air simulations because you have no reliable emission inventory? But then the other half of the analysis is based on one emission inventory? Furthermore, should this sentence suggest that only the bottom-up inventories are unreliable while the top-down are not?

Page 6, line 21-22. Please explain how the amount of explained variability is estimated.

Page 7, line 17. How were those percentage contributions calculated? Transform numbers from Figure 5 into relative numbers and then apply them to the observations? Here and at other places, the methodology is not clear and the reader has to guess what exactly has been done.

Page 8, line 5. I don't understand how the discussion of Figure 9 (which shows age of air as a function of source region but no emissions or mixing ratios) allows such a statement. Or is here information from other earlier analysis used? Same for line 8.

Page 8, line 12. The text says that 53% of what reaches the TTL comes from the open ocean? From other parts of the manuscript, I had the impression that the large majority comes from the open ocean? Please clarify.

Figure 9: Comparing the lines for ocean, open ocean and coastal ocean, I wonder if the coastal and open ocean together should give the ocean age of air? However, the

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total ocean (blue line) shows the youngest age of all. Please clarify.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-936, 2016.

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