Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-936-RC3, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the vertical transport of $CHBr_3$ and CH_2Br_2 over the Western Pacific" *by* Robyn Butler et al.

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Received and published: 12 January 2017

Butler et al. (2016) presented a modeling analysis of the transport of very-short-lived bromocarbons from the surface to the UT/LS over the Western Pacific. This analysis is based on the GEOS-Chem simulation of CHBr3, CH2Br2, age of air tracer and aircraft measurements from the CAST and CONTRAST campaigns. After reading through the manuscript, I have to say that I share similar concerns with the two other reviewers. Here, I am not going to repeat many of the issues raised by the other two reviewed, but just stating the major issues with the current model design and approach:

1. The use of Liang et al. (2010) emissions. I don't understand why there were emissions over the land, as the Liang et al. emissions scheme only specifies emissions from Open Ocean and coastal regions. While the original inventory was derived on 2x2.5

horizontal resolution, I provided a refined emissions inventory on 1x1 degree resolution to the GEOS-Chem group. Could it be possible when the 1x1 degree emissions were regrided to 2x2.5, emissions appeared to occur over the island landmasses as a result of coarse resolution? Whatever the reason was, the use of land tagged emissions tracers for CHBr3 and CH2Br2 and the reference of terrestrial sources of these gases throughout the manuscript, in my view, are not accurate and lead to wrong impression that land could be a source of these oceanic-originated compounds. Second, the Liang et al. (2010) emissions inventory was originally derived for stratospheric bromine budget purposes (therefore without much attention to fine-tuning the surface emissions details, e.g. longitudinally invariable and simple treatment of Open Ocean vs. Coasts), with no observations over the western Pacific to constrain surface emissions in that region. As shown by Hossaini et al. (2013), the Ziska bottom-up inventory is a much more skillful and a more appropriate choice of emissions for the Western Pacific region. Quantifying the relative importance of open ocean emissions vs. coastal sources using the Liang et al. (2010) emissions scheme for the Western Pacific region, which is one of the main focus of this paper, does not provide a credible estimate.

2. Page 8, Line 29 – Page 9, Line 9. I have to say I don't see the meaning of the use of modeled profiles of CHBr3 and CH2Br2 by applying a vertical uniform correction of model biases to quantify SGI and PGI.

i) First, the estimated injection of PGI based on model corrected profiles is not correct. Why use the model? The model, even after correction, still shows low biases for CHBr3 and CH2Br2 at 10-12 km. In fact, shouldn't the observation-based organic Br be the true PGI value?

ii) While it was not explained in the text, my guess is that the authors use the difference of Br value at the surface and that at the TTL to calculate PGI. This is not a correct approach in my view. As show in Liang et al. (2010) , a significant fraction of the inorganic Br produced from CHBr3 and CH2Br2 degradation are removed by large-scale precipitation in the lower troposphere and never makes to the UT.

3. Same as the other reviewers, I also find the use of idealized age of air, in particular the results presented in Figure 11, hard to interpret.

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