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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling the transport of very short-lived substances into the tropical upper troposphere and lower stratosphere" *by* J. Aschmann et al.

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We thank reviewer 2 for her/his comments and remarks. In the following, the original remarks of the reviewer are in *italics*.

1.) The authors claim in the title of the article to investigate the transport of very short-lived substances, but they refine themselves to two species, namely CH_3I *and* $CHBr_3$*. I would recommend either to change the title from "very short-lived substances" to "*CH_3I *and* $CHBr_3$*" or to add some concluding remarks in the discussion session about what the results of this study imply for the other very short-lived substances and for the total amount of bromine in the stratosphere.*

Our main intention here is to present a new modeling approach with first applications to a set of idealized VSLS. Our modeling approach can easily be applied to other VSLS in addition to bromoform and methyl iodide. We will include the suggested remarks concerning the implications of our results for other VSLS in the discussion of our manuscript.

2.)The authors often refer to a "ground level source of 1 ppt" (e.g. p18514 l.23). But, I expect a source to have a unit including the information "per time interval". I suspect you simply assume the mixing ratios at the ground level to be 1 ppt all the time. Can you be more precise?
The latter is correct. We assume the ground level mixing ratio or, to be more precise, the detrainment mixing ratio, is set constant to a particular value, e.g. 1 ppt. We will modify our text to avoid the misleading word "source" in this context.

3.) p. 18517, ll. 2-3: It is not clear to me if instantaneous or 6 hourly averaged updraft convective mass fluxes are used. It would be desirable to get an estimate about the error this includes. (A strong short-lived convective cell might have an unrealistic impact, is this unimportant in the statistical mean? By using 6 hourly instantaneous data I would expect to overemphasize certain regions where convection is usually high at this time of the day. By using averaged data I would assume that the averaged updraft convective mass fluxes are not strong enough.)
The ECMWF detrainment rate is available two times a day (12 and 00), but only as forecast with 3 hours interval, e.g. 12+3, 12+6, 12+9, 12+12. The particular forecasts are integrated over the timespan, that means for example that dividing the 12+3 forecast by 3 hours will give the average detrainment rate from 12:00 to 15:00. So in fact we are using a 3 hourly averaged detrainment rate in our model, not an instantaneous value.

4.) Section 3.1.2: a) Only the comparison to two SHADOZ stations is shown here. I think the comparison to the other SHADOZ stations is also important. The authors give no reasons for their choice of the two stations. I suggest to provide the pictures of the

comparison to the other SHADOZ stations within an electronical supplement.
b) The authors state that the model contains a linearized ozone mechanisms. A discussion of the errors implied by this simplification would be useful to correctly classify the results of the comparison.
a) Our main intention was to show that the transport processes, in particular the large-scale vertical motion via diabatic heating and fast localized convective transport, are realistically represented in our model. One way to show this was comparing ozone profiles at two stations where the first is located in an area with strong convective activity (Java) and the other in an area with fewer convection (San Cristobal). As shown in the manuscript, our model is able to reproduce the main differences between the ozone profile at these two stations. As the chemistry and detrainment mixing ratio in our model is identical between these two stations, the modelled differences are due to transport.

For completeness, we added the other stations in a supplement to this comment. You can see that our model is able to reproduce the observed profiles as long as the ozone concentration in the free troposphere is similar. Thompson et al. (2003) showed that there exists a "wave-1" structure in free tropospheric ozone with enhanced ozone over South America, the Atlantic and Africa (between approximately 50°W and 50°E), possibly due to ozone precursor emission from biomass burning or other sources. Since in our model calculations we use only a single (longitudinally constant) ozone detrainment mixing ratio we do not reproduce these features in the free troposphere.

b) In the upper TTL and tropical lower stratosphere the ozone chemistry is dominated by production due to O2-photolysis, that we believe is accurately represented by the linearized chemistry. Catalytic ozone destruction due to HOx, NOx, ClOx and BrOx is included to a first order approximation. However, additional ozone production by precursors that could be important in the free troposphere are not included here and could effect the comparison in the upper troposphere, see the discussion in the previous paragraph.

5.) p.18523 ll. 4-8: It took me some repeated rereading of this sentence to understand, what the authors wanted to say. I understood, that 1.2-1.3 ppt of the 1.6 ppt Br_y *above the cold point are formed above the cold point, i.e. the insoluble* $CHBr_3$ *was able to crossed the cold point whereas the soluble* Br_y *would have been dehydrated during the vertical transport. Did I understand correctly? The sentence should be rephrased to be easier understood.*
The interpretation is correct. We will try to make this sentence more clear in the upcoming revision.

6.) Section 3.3 Even if the focal point of this study is the tropical region it would be interesting to see the relative contribution of different source regions to other region, e.g., what are the contributions to the midlatitudes or even the polar region? Is there are dominant source region for Africa, North America etc.? I know this is beside the point of this paper nevertheless it would be interesting.
In the supplement to this post we added plots of the relative contribution of the individual source regions to TT20 (idealized bromoform tracer) abundance in different areas. They confirm our result that the major pathway for VSLS into the stratosphere is the West Pacific region which contributes approximately 50% of bromine uptake regardless of the observed area.

Typos etc.:
[. . .]
Thank you for your careful reading. We will fix the typos accordingly.

References:

Thompson, A.M., J.C. Witte, S.J. Oltmans, F.J. Schmidlin, J.A. Logan, M. Fujiwara, V.W.J.H. Kirchhoff, F. Posny, G.J.R. Coetzee, B. Hoegger, S. Kawakami, T. Ogawa, J.P.F. Fortuin, and H.M. Kelder, Southern Hemisphere Additional Ozonesondes (SHADOZ) 1998-2000 tropical ozone climatology 2. Tropospheric variability and the zonal wave-one, J. Geophys. Res., Vol. 108 No. D2, 8241, doi:

10.1029/2002JD002241, 31 January 2003.

Please also note the Supplement to this comment.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 18511, 2009.