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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of deep convection and dehydration on bromine loading in the upper troposphere and lower stratosphere" *by* J. Aschmann et al.

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We would like to thank Robyn Schofield for her careful reading and the helpful comments. In the following, the original text of her review is in *italics*.

A very novel aspect of this study is the sea surface temperature linkage (ENSO) for Bry, and how Bry might be expected to respond in a changing climate - though perhaps the authors may wish to reiterate this component more in their conclusions (i.e. providing some outlook).
We will add a paragraph to the conclusion to further discuss this subject.

Page 126, line 11 an appropriate reference for the entrainment or convective dilution of the boundary layer component is [Romps and Kuang, 2010] where they discuss how most detraining air contains maximally a 30% boundary layer component. In general the discussion in the paper as it stands reads as though detraining air contains mostly boundary layer air (affected slightly by convective dilution) but this is not the current "state-of-science" and maybe the ERA-Interim entrainment rates (i.e. boundary layer versus free troposphere) could also provide some support for the your choice of 1 ppt etc (otherwise some qualifying statements should be added).
Thank you for pointing out this important issue. In fact our phrasing in the mentioned paragraph is misleading and we will clarify this section for the upcoming revision and include the suggested reference. The detrainment mixing ratios in our modeling approach are essentially free parameters, since our model does not contain the lower troposphere which makes the explicit application of the ERA-Interim entrainment rate impossible. To justify the choice of our parameters we refer to the recent study of Liang et al. (2010) who presented a compilation of observations of bromoform and dibromomethane from various aircraft campaigns. These measurements support our assumption, that a detrainment mixing ratio of 1 pptv for both CHBr3 and CH2Br2 is an appropriate approximation, even if a major part of convectively transported air is entrained in the free troposphere.

Page 126, line 14, the assumption is made that detrained air is saturated with respect to the ambient conditions. However, since the convectively detraining air is likely to be cooler it would in reality be sub-saturated with respect to ambient conditions. Conversely, as ice lofting also is thought to occur in deep convection (then evaporates) this is likely to balance the sub-saturation making the 100% RH assumption valid, maybe some discussion of this assumption could be added here.
We follow here the assumption of Dessler and Sherwood (2004) that convective updrafts contain significant amounts of ice which is lofted upwards and drive the

detrainment region towards saturation with excess ice sedimenting out. They in turn refer to Alcale and Dessler (2002) who measure ice particles in deep convection using a precipitation radar.

The performance our water vapor treatment was already discussed in our previous study (Aschmann et al., 2009) that introduces this approach. The results generally agree with observations thus the assumption of saturated detrainment seems appropriate. However, there are indications that our approach tends to inject too much water into the stratosphere, however, it is not clear whether this is a effect of saturated outflow or a general problem caused by the simplified representation of convection. In fact we did some sensitivity calculations where we varied the detrainment mixing ratio of water in the range between 70% and 130% of the saturation mixing ratio but the results were not very sensitive to these changes so we finally stuck to the 100% assumption.

Page 126, line 16, is it correct that the model does not allow supersaturation? Since we know that in the TTL supersaturated air parcels do exist, i.e. up to 130-160% RH [Gettelman et al., 2000; Koop et al., 2000] how might this assumption affect the results presented here? I can imagine that ice may form too quickly in this model, perhaps it has no overall bearing on the results presented but has this been tested for?

It is correct, our model does not allow supersaturation therefore we likely overestimate the impact of dehydration in our model. However, this would only affect the results of our idealized approach where we assume total solubility, which is in itself an unrealistic assumption only made to determine the maximal possible effect of dehydration. In contrast, the results of the full chemistry approach will be hardly affected, since the impact of dehydration on stratospheric bromine loading is already negligible in this setup.

Page 123, line 15, here in general (i.e. seasonality if any) and in the discussion

page 138 first paragraph, discussion of modelling studies relevance for washout Bry ranges, the recent study of [Schofield et al., 2011] would add to the ranges given here, and the discussion.

We will include the suggested reference in the revised version.

Page 125, line 7 the slow ascent and convection being super-imposed is rather typical of most recent trajectory modelling studies, not A distinctive feature of our model i.e. [Dessler et al., 2007] , perhaps a qualifying statement if you are referring to CTMs only?

We apologize for the unclear phrasing. We are referring to isentropic CTMs where this approach is still uncommon. We will rephrase the sentence.

Page 128, line 6, After one model timestep all dissolved and adsorbed species are released . . . I am unclear in what this means is this describing the evaporation of sedimented particles?

Yes. As we mentioned in the reply to the first review we will clarify this paragraph in the upcoming revision to make this more clear.

References:

Liang, Q., Stolarski, R. S., Kawa, S. R., Nielsen, J. E., Douglass, A. R., Rodriguez, J. M., Blake, D. R., Atlas, E. L., and Ott, L. E.: Finding the missing stratospheric Bry: a global modeling study of CHBr3 and CH2 Br2 , Atmos. Chem. Phys., 10, 2269–2286, doi:10.5194/acp-10-2269-2010, 2010.

Aschmann, J., Sinnhuber, B.-M., Atlas, E. L., and Schauffler, S. M.: Modeling the transport of very short-lived substances into the tropical upper troposphere and lower stratosphere, Atmos. Chem. Phys., 9, 9237–9247, doi:10.5194/acp-9-9237-2009, 2009.

Dessler, A.E., and S.C. Sherwood, The effect of convection on the summertime extratropical lower stratosphere, J. Geophys. Res., 109(D23), D23301, DOI: 10.1029/2004JD005209, 2004.

Alcala, C.M., and A.E. Dessler, Observations of deep convection in the tropics using the TRMM precipitation radar, J. Geophys. Res., 107(D24), 4792, DOI: 10.129/2002JD002457, 2002.

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