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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of stratospheric Br_y to uncertainties in very short lived substance emissions and atmospheric transport" *by* R. Schofield et al.

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We thank the reviewer for improving our manuscript with their questions and insightful review. We have included the original review text here as bold text, and our responses we provide as normal text below.

This manuscript presents a trajectory based estimate of transport of VSLS into the stratosphere through the TTL, focusing on the sensitivity to various parameterized processes. The manuscript is well thought out and well written, and sould be publishable in Atmospheric Chemistry and Physics subject to minor revisions. My only major substanstive concern is that the authors could explore

a little more the realism of the ERA-Interim convective detrainment rates, since they identify this as a key uncertainty.

General comment:

The authors note that there is not an easy way to validate the ERA- Interim convective detrainment rates. I think however that there might be some simple ways to check the realism of these rates. One way would be to use something like the vertical strucutre of CloudSat cloud fraction to compare to the vertical structure of detrainment rates. This would not provide a quantitative comparison, but might be able to provide gradient information on whether the vertical and horizontal structure of detrainment rates are reasonable, as well as checking some of the seasonal assumptions.

Cloud fraction and detrainment rates are not directly comparable (as the reviewer notes only a qualitative comparison would be possible). There is literature evidence that the cloud fraction in ERA-Interim performs well [Tompkins and Janiskova, 2004], as realistic cloud and precipitation rates is one of ECMWF's core objectives – therefore we do not attempt to repeat this comparison. Within the (relatively large) uncertainties of an attempt to "validate" convection in ECMWF, the convective parameters (winds, divergence etc) are shown to be acceptable and there is extensive literature doing exactly these type of comparisons (e.g. the observational cloud cover profiles compared with ECMWF forecast runs [Tompkins and Janiskova, 2004]). With ERA-Interim being a state-of the art forecast model, and we do not question its ability to reproduce clouds but whether the detrainment is representative. The TTL Review paper of Fueglistaler et al., [2009], explored this to some extent and concluded there is no obvious simple way to "validate" detrainment rates. To the extent that they can be validated we point to Aschmann et al., [2009](following Björn-Martin Sinnhuber's comment on this discussion), where the zonal mean detrainment rates were inverted and favourably compared with observed turnover rates from O3 and CO [Dessler, 2002]. Thus providing some evidence that the detrainment rates (at least on average) are realistic. We propose rather

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that an observationally based comparison study using the seasonality of O3, CO and HCN (and radon if possible) in the TTL would provide a truer test of the seasonality (or rather lack thereof) seen in the ERA-Interim detrainment rates, clearly such a test is beyond the scope of this paper.

Detailed comments:

P24174, L11: is the analysis dependent on what theta surface the parcels are started on?

In starting the trajectories at 400K we assume that the last dehydration occurs before this level, starting higher up would have no implications other than a slightly different ratio source:product. However, we know from other studies (see also the study of Liu et al., [2010]) that dehydration can occur up to, say 430K or so occasionally in the model calculations. Hence, the reviewer is correct that there may be some sensitivity of the statistics to the starting level - specifically the washout of soluble species could be underestimated, with a starting level of 400K - but given that the bulk dehydration occurs below 400K, it is safe to assume that this uncertainty is second order compared to those discussed in detail in the paper.

P23174, L20: knowing these fractions and how they varied would be interesting.

These have been added to the text - 66% of all trajectories traverse the TTL in JJA and 71% in DJF.

P24175: Figure 2 could use another sentence of explanation: it is just the initial values from table 2 with the lifetime applied right?

Correct – we add another sentence and follow the next suggestion of adding panels to figure 2.

Also, could you show (maybe just for initial lifetime, maybe in 2 more panels) the alternate source gas distribution from Kerkweg?

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This has been done for land, coast and ocean emissions.

Also: for table 2: can you show the sum of total Br_y for each distribution set?

This has been done - also following the suggestions of reviewer 2 table 2 footnotes have been created.

P24177,L19: see general comment above about a way to try to evaluate the detrainment.

We did not conduct a cloud fraction comparison of the ERA-Interim data because this has been studied elsewhere.

P24184, L3: does this fraction of trajectories matter for the subsequent entry distribution of Br_y?

Indeed, we only address the incoming airmasses – and do not account for the background stratospheric Br_y concentration (which is actually less well constrained i.e. 3 – 8 ppt VSLS compared to the distributions coming from our model runs).

Do you need to account for it using your method (it seems like it might dilute the air you are trying to quantify).

To keep things 'clean' we don't do this, but do include a cautionary note to this effect.

P24184, L8: The description of an 'hourglass shape' did not make sense to me in these horizontal maps. Please rephrase.

This has been changed.

References

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