

## ***Interactive comment on “Sensitivity of stratospheric Br<sub>y</sub> to uncertainties in very short lived substance emissions and atmospheric transport” by R. Schofield et al.***

**Anonymous Referee #2**

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This study uses a simple conceptual model to investigate the sensitivity to key parameters controlling the contribution of very short lived substances (VSLs) to stratospheric bromine loading. This is an interesting new approach compared to previous studies and provides important contributions to the ongoing discussion in this field. Overall the paper is well written and I recommend publication in ACP after a few, mostly minor, changes.

### General Comments

While I really like the approach used here, I feel that some of the assumptions made need to be better motivated, discussed and a bit more detail should be given. Table 2

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with the VSLs scenarios is really useful, but I am slightly uneasy with it as there is the risk that future studies may refer to it, without fully recognizing its limitations. Therefore I suggest to give a bit more information how these numbers were extracted from the references given (Yokouchi et al. and Kerkweg et al.) and to discuss in a bit more detail the uncertainties in these estimates. E.g. Carpenter et al. (2009) give quite different values, possibly indicating that there is a larger uncertainty in these numbers than the difference between Scenario 1 and 2 suggests.

A critical value for the CPT washout is the fraction of soluble versus insoluble bromine gases. Here a reference to the modeling study of Yang et al. (2005) is given, stating that 85% of inorganic bromine in the TTL is in the form of HBr and HOBr. However, I couldn't find this result for the TTL in the Yang et al. paper (maybe I just overlooked it) but I remember having seen other model results that showed much less HBr and HOBr in the TTL. (However, unfortunately I can't find any published figures right now.) While this is not really critical in the current study as any uncertainty here can be lumped into the washout efficiency gamma, one should clearly avoid giving numbers that future studies may use in a wrong context.

### Specific Comments

p.24177,l.4: please be specific if you mean detrainment rate or divergence. Both may have the same units, but they are not the same.

p.24179, Sect. 2.4: I'm having problems with the "concentration weighted lifetime". It is not obvious to me that such a step is equivalent to calculating the evolution of the individual gases.

p.24179,l.24: "CH<sub>3</sub>Br is usually treated as a long lived species within chemistry-climate and transport models": What does that mean? Isn't CH<sub>3</sub>Br explicitly modeled in such models?

p.24180,l.16: Hossaini et al. show that the difference in lifetime for CH<sub>2</sub>Br<sub>2</sub> is huge

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between the mid-troposphere and the TTL! How would the much longer lifetimes in the TTL affect the conclusions of the current study?

p.24185, l.25: "Changing the BL to outflow convection efficiency": This is an awkward phrase.

p.24194, Table 2: Are the numbers given ppt of CH<sub>2</sub>Br<sub>2</sub> and CHBr<sub>3</sub> or ppt of bromine? I.e., are these numbers already multiplied by the number of bromine atoms?

Technical Corrections

p.24173,l.2: "i.e." -> "e.g."

References

Carpenter, L. J., Jones, C. E., Dunk, R. M., Hornsby, K. E., and Woeltjen, J., Air-sea fluxes of biogenic bromine from the tropical and North Atlantic Ocean, *Atmos. Chem. Phys.*, 9, 1805.1816, 2009.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 24171, 2010.

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