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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability and long-term changes of brominated VSLS at the tropical tropopause" *by* Susann Tegtmeier et al.

Anonymous Referee #1

Received and published: 10 July 2019

This manuscript analyses the variability in CHBr3, as one of the major VSLS Bromine species that can be transported to the stratosphere by strong convection in the tropical latitudes. They have used a representative surface emission pattern at monthly-mean time intervals, and transport-chemistry parameterised using FLEXPART, an widely used modelling system for short-lived species simulation. The manuscript is generally very well written, and the subject of this research is important. However, I have some strong reservations which have to be addressed before manuscript can be accepted for publication in ACP.

Major comments. I am very concerned with the model simulation over the South Asia region. The pattern of transport of VSLS species from the surface layers appears unrealistic (Fig. 8), meaning the location of the anticyclone in JJA appears to be vastly

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Discussion paper

misplaced for both CHBr3 and Transit time - based on our knowledge of global (Eulerian) chemistry-transport models and observation of tracers, e.g., Park et al. (JGR, 2007) or Chandra et al. (ACP, 2017). The upward motion over the India monsoon domain is limited to northern India rather than in the tropical areas.

Which raises a wider question about the validity of the FLEXPART model framework implemented in the work. Can there be some independent tracer simulation to validate the model transport, say using Radon-222 or SF6 or CO for which we have better knowledge of model transport (Forster et al., 2007 could be using older version of the model, is your version same that work?). I am aware that the FLEXPART works reasonably well for the horizontal transport at regional scale but not sure whether suitable for such global model simulation.

I am also very disappointed in the ways the model and measurements are compared. For a reasonable evaluation of the model simulations we need to first sample the model at the time and location of measurements.

Minor comments. p.3, l#4: could you put a timeline here, as in the WMO/SAOD, 2018

p.3, l#13: may be cite - Hossaini et al., ACP, 2016

p.9, l#1ff: this is quite a bit of simplification of the loss of SGs, a proper treatment using a 3D OH field would have been useful to simulate tropospheric distribution of the SGs

p. 11 : Figure 1 and associated text: the southern spread of this anticyclone as appears from your figure looks too wide, is there a possibility to show these plots for summer and winter (arrange a 2x2 panel figure). The panel b doesn't provide sufficient information.

Also I would like you to add the surface fluxes for the two seasons, making it 2colx3row figure. Eventhough the emissions are taken from Ziska et al., the readers cannot directly assess results without showing here again.

Figure 3 and associated text: Observations are much less than the model values

How to explain the spotty features in the model simulations???

Observations are higher over the gulf of Mexico persistently, hinting towards low emissions?

Figure 4 and associated text: I am not very happy with this comparison. I believe that the model simulations should be first sampled at the measurement points and then compared with measured values.

This plot makes me interpret that the model simulation is out of phase with the measurements during June to Aug! I do not know if this problem comes from not sampling the model properly or the model itself is wrong.

p.21, l18: Again, good to have shown the emission maps in Fig. 1, for winter and summer

Figure 7 and associated text: This again reiterate why are you not showing one-to-one comparison, given that the transport varies

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