

Interactive comment on “Vertical transport rates and concentrations of OH and Cl radicals in the Tropical Tropopause Layer from Observations of CO₂ and halocarbons: implications for distributions of long- and short-lived chemical species” by S. Park et al.

Anonymous Referee #1

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I would like to congratulate the authors on this very important and well written manuscript which contains a lot of new significant material and merits publication in Atmospheric Chemistry and Physics. The authors use observed data of CO₂ and of reactive gases to derive transport rates, OH and Cl vertical profiles and age spectra for short lived species. I have a number of comments and suggestions to the paper which are detailed below. The two major points I would like to see discussed in this paper are the effect of horizontal mixing on short lived species and significance of the

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Cl-profiles derived here. I also suggest to the authors to include discussion of some recent literature related to the topics they mention. There are a few typos or missing words, so please edit the paper once more.

Major points

Formula 1 on p.6067 does not contain a term for horizontal mixing. The authors argue on page 6072 that this has only a negligible effect based on observations of CO₂ vertical profiles in the subtropics of the NH. However, I have two questions to this:

(i) I expect SH subtropical CO₂ profiles to be different. Is there any information on this and on the possible influence on the derived ascent rates due to the effect of horizontal mixing? (ii) Even if horizontal mixing has no effect on CO₂ (due to the lack of a strong horizontal gradient, see above) this will not be the case for species with chemical loss, like the VLSL species discussed further down the manuscript. These will have significant horizontal gradients, taken that they are probably close to zero in the extratropical LMS. Therefore, even if the assumption of a negligible effect of horizontal mixing may be a reasonable assumption for CO₂, it does not seem to be reasonable for the reactive species.

My second major point is the derived vertical profile of Cl. I have significant doubts on how meaningful this profile really is. The authors state on p 607, l. 3 that the reaction rate constants with Cl for ethane and C₂Cl₄ are 200-300 times higher than those with OH. Nevertheless, OH concentrations derived here are about 3 orders of magnitude higher than Cl concentrations. This implies that the loss by reaction with OH is the major sink for both species and roughly 3-5 times more important than loss to Cl radicals. Given the uncertainty in the OH profile (note that 2.2. \pm 0.3 \times 10⁶ value given in line 10, p. 6075 is just an average and does not contain any uncertainty analysis) I think that an error propagation of the uncertainty in the OH profiles and how this propagates into uncertainties in the Cl profiles is necessary for a discussion of the Cl profile in particular but would also be important for the OH profile.

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I further suggest to include discussion of a number of other papers on vertical ascent in the tropics and on VSLS species in the tropics which have recently been published.

Ascent rates: Ploeger et al JGR, 2009., and Krueger et al., ACP 2009.

VSLS observations in the TTL: Laube et al., ACP 2008.

VSLS transport through the TTL and lifetimes of VSLS species: Hossaini et al., ACP 2010, Aschmann et al., ACP 2009, Liang et al., ACPD 2009, Gettelman et al., JGR 2009.

Minor points

Section 4.1. :I think this section would benefit from a comparison with the ascent rates derived from calculated heating rates or trajectory analysis, e.g. by Ploeger et al. and Krueger et al.

p. 6974., l. 12: I would probably call this subtropics not mid latitudes.

p. 6075., l. 15- 19.: I had to read these two sentences three times before I understood. Please rephrase for more clarity.

p. 6078., l. 22 and following: The situation for CH₂Br₂ and CHBr₃ is very different. As the main loss for CHBr₃ is photolysis, its local lifetime will not vary a lot with altitude (see Hossaini et al., 2010) and CHBr₃ remains short lived in contrast to CH₂Br₂ whose lifetime increases with altitude. So the statement that bromine from these species can reach the stratosphere certainly needs to distinguish more closely between these two species, in particular also when looking at the vertical profiles, where CHBr₃ mixing ratios are down to about zero at 16 km altitude, which is below the tropical cold point tropopause. So looking at this Figure, one would assume that hardly any bromine from CHBr₃ reaches the stratosphere in the form of source gas.

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