Atmos. Chem. Phys. Discuss., 12, C12221–C12223, 2013
www.atmos-chem-phys-discuss.net/12/C12221/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observation-based assessment of stratospheric fractional release, lifetimes, and Ozone Depletion Potentials of ten important source gases" *by* J. C. Laube et al.

J. C. Laube et al.

j.laube@uea.ac.uk

Received and published: 1 February 2013

Referee comment

1) Table 1 presents a comparison of measurements "near the tropopause" vs. hemispheric mean mixing ratios found from the NOAA ESRL network. It is unclear exactly what data is used, and what the authors are intending to show by this comparison. Perhaps it is just to demonstrate a good level of comparability between calibration scales. Otherwise, there are a number of reasons that the comparison would show differences. One might expect slightly lower values "near the tropopause" compared to a hemispheric mean due to mixing with stratospheric air. Also, one could expect

some difference due to difference of 1 - 2 months of transport time from the surface to the tropopause. Finally, the hemispheric mean contains values near the tropics, which should show some lower values compared to the mid-high latitude tropopause.

Author response

As an initial remark we would like to acknowledge the work of the anonymous reviewer which has further improved this publication. As for comment 1) the data is indeed shown to demonstrate good comparability. The text was modified to make that clearer: "After correcting for these offsets we further evaluated the comparability of the data sets by comparing the NOAA NH mixing ratios with our upper tropospheric values."

Referee comment

2) It would be useful to see a map of the sample locations from the different campaigns.

Author response

As a more quantitative approach all sampling locations (longitudes, latitudes, altitudes, and pressures) and the respective observed mixing ratios were added to the supplement (acp-2012-791-su.xls).

Referee comment

3) The authors spend some time discussing the differences between FRFs reported by Schauffler et al and Newman et al compared to the current study. There are clearly some differences. The authors might also consider if there could be a potential impact on the FRF from the fact that the age spectrum of older air from the data discussed by Newman et al. and Schauffler et al. may include air from before, during, and after the turn over in Cl abundances. I am curious, too, about how much difference could be explained by small analytical differences, eg. offsets or non-linearity at the low mixing ratios at old ages, between the two sets of measurements.

Typo: P28529, line 11: Should be "where" not "were".

Author response

We agree with the referee, that there is a potential impact on FRFs from changing trends. This has already been explained in detail in the manuscript: “Therefore the FRFs derived here can not be considered as equilibrium quantities and are only valid as long as the tropospheric trends of the respective compounds do not significantly change. This could also explain the differences between our FRF-mean age correlation and that of Newman et al. (2006), especially for HCFC-141b; as growth rates became more linear the shape of this correlation changed significantly. In 2009-11 it had become very similar to that of HCFC-22, which has been growing more linearly since the 1990s (WMO, 2011).” Small analytical differences are unlikely to be influential as a) such differences would not look similar for different compounds and more importantly b) both data sets underwent thorough quality checks. Finally, the typo has been corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28525, 2012.