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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.

Anonymous Referee #2

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The paper by Laube et al. analyzes a series of high altitude trace gas measurements to infer stratospheric lifetimes, fractional release factors, and ozone depletion potential for several halocarbons, CFCs, and HCFCs. Though there was apparently some minor problem with the analyses for a few compounds in a limited number of samples, the data appear to be of very high quality with good comparability between labs and with the NOAA ESRL surface network. The theoretical framework for the analysis of lifetimes and the procedure to derive lifetimes from tracer-tracer correlations follows that discussed in Volk et al. (1997). It is noted that care must be used to properly apply the correlation method to observations, and the authors appear to take the necessary precautions to arrive at their lifetime estimates. From the calculated lifetimes, and re-

lated fractional release factors, the authors estimate ozone depletion potentials for the studied gases and find some differences to current estimates that have implications for the rate of stratospheric ozone recovery. The material presented in the manuscript is significant for assessing stratospheric ozone recovery, and the new lifetimes will be of interest to the stratospheric chemistry community. The paper is well-written and discussions are thorough, and can be published with only minor changes.

Some minor comments:

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.

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

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".

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