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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric loss and atmospheric lifetimes of CFC-11 and CFC-12 derived from satellite observations" *by* K. Minschwaner et al.

Anonymous Referee #2

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This paper uses available satellite observations and an ultraviolet radiative transfer code to evaluate the lifetimes of CFC-11 and CFC-12 during the 1992-2010 time period. The updated lifetimes are generally similar but somewhat longer than those cited in the latest WMO ozone assessment report. The paper presents an extensive analysis of the related uncertainties and technical details involved; for example, there is good use of different cross section expressions to achieve the optimal representation of the original laboratory measurements at a wide range of wavelengths. There is also a very nice summary of historical background work, providing good motivation for the current study.

The paper is relevant to ACP by reporting this important CFC lifetime information derived from observational data sets. Overall, the paper is mostly well written, and should be published with minor revisions and improving the clarity in a few places (see specific comments below).

Specific comments: P. 28739, lines 4-6, in regards to using the SORCE solar flux data for one month (March 2004). Recent work has indicated possible problems with long term changes in the SORCE data (Lean and Deland, 2012, J. Climate, p. 2555; Swartz et al., 2012, ACP, p. 5937), although I assume this will not be an issue with using just the one month of data. However, will there be much of an impact on the computed lifetimes if instead the solar flux data corresponding to the specific times of the CFC measurements is used (eg, from the NRL solar spectral irradiance reconstruction - Lean, 2000)? For example, recent unpublished model calculations indicate that with everything else fixed, the CFC-11 lifetime changes by 1-2 years for solar max vs. solar min conditions using the Lean NRL solar flux (with a 2-3 year change for CFC-12). These changes are on the order of 2-3%, so this effect appears to be small, but the authors should at least add a sentence or two mentioning the potential impacts on the lifetimes of the solar flux variability.

P. 28739, lines 22-25, "Additionally, there is a shift. . .... the peak in ozone mixing ratio." This sentence has useful information, but as written is somewhat long and hard to follow. Please re-word/clarify, perhaps dividing into two sentences.

P. 28742, lines 1-3, ". . ..no corrections have been applied. . ." – I don't quite understand this sentence since lines 13-15 on the previous page states that the tropospheric mixing ratios have been adjusted to match the WMO, 2011 values. Please clarify. Also in regards to lines 10-11, ". . .MIPAS mixing ratios are larger by 10-20%...", I assume this is mainly a measurement bias, and is not due to the 4% TD change in surface mixing ratio (WMO, 2011)? Please clarify.

p. 28743, lines 14-17: While the seasonal variation in the loss rates can be accounted

for, some of the data sets have less than 1 year of coverage (e.g., 1 month for CRISTA-1, -2) so the seasonal variation in the constituent distributions will likely cause significant seasonal biases in the computed lifetimes. For example, unpublished model calculations (the same as noted above) indicate a seasonal variation of +/- 10 years for CFC-11 and +/-20 years for CFC-12 (due to the seasonal variations in both the constituent distributions and loss rates). There will also be a substantial QBO influence on the constituent distributions as well. Of course these are the limitations of the data sets and can't really be avoided, but the authors should include a statement or two noting that there are probable seasonal and interannual biases. Further to this point, I'm not sure if doing an equal-weighted mean lifetime in Table 1 is the best way to go. I would think giving the MIPAS and ACE data more weight (perhaps accounting for the number of months of data available) would be more appropriate for CFC-11.

P. 28745, lines 2-4, this is a bit confusing. Doesn't the smaller mixing ratio from CRISTA-2 (above $\sim$20 km) imply a shorter lifetime compared to MIPAS via eqn. 2? I would think the burden contribution to the older lifetime in CRISTA-2 vs. MIPAS is due to the larger burden in CRISTA-2 below $\sim$19km in Fig. 6? Please clarify this.

Technical corrections:

1) p. 28736, line 5, change to: ". . ...the assessment of CFC-11's potential. . .."

2) p. 28740, for the leading factors in eqns. (3) and (4), change "2" to "4" since this should be the area of the earth (to get 5.1e18 cm2).

3) Reference on p. 28753, line 10, change to "tropical"

4) Fig. 5, right panel: it would be good to include contours here since the point is made (p. 28743, lines 7-8) that the O1D loss contribution at 26-34 km (4-8%) is small but not zero. It's very hard to discern this in the color-only figure as is.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28733, 2012.