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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric lifetime ratio of CFC-11 and CFC-12 from satellite and model climatologies" *by* L. Hoffmann et al.

Anonymous Referee #2

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Stratospheric lifetime ratio of CFC-11 and CFC-12 from satellite and model climatologies

by Hoffman et al

Referee's report

The paper describes the application of the standard 'tracer-tracer correlation' technique to three different satellite datasets and a model to update assessments of the ratio of CFC-12/CFC-12 lifetimes or, if a standard value for the CFC-11 lifetime is assumed, the absolute lifetime of CFC-12.

General Comments

1. I assume all three satellites derive CFC concentrations from the same spectral region (850cm-1 for CFC-11, 925cm-1 for CFC-12) and use the same cross-section data available from the HITRAN web-site? Perhaps there should be some mention of this and its consequence (ie if there any significant spectroscopic errors they will be common to all 3 datasets).

2. The satellite gradient data are extrapolated to derive the slope at the 'tropopause' defined by the independently derived (and more accurate) CFC-11 surface value. (I assume there is upper tropospheric data in the satellite measurements and that this extrapolation is not intended just to cover the gap in vertical coverage). Now, one could argue that the slope should be taken from the maximum of the satellite CFC-11 & CFC-12 values, or that surface measurements should be used to provide bias corrections for both CFC-11 and CFC-12 values, but by extrapolating to the surface CFC-11 measurement aren't you effectively providing a (positive) bias correction for satellite CFC-11 without any similar corection for CFC-12?

3. Perhaps I am missing some subtlety here, but taking a simple model of photochemical decay with a constant lifetime T would suggest stratospheric concentrations given by x = x0 exp(-t/T), and hence that an alternative estimate of the ratio of lifetimes of different species is obtained from the gradient of the ln[VMR]s of the species, ie T_12/T_11 = ln[CFC-11/x0_11] / ln[CFC-12/x0_12] (where x0 are the tropospheric values). If true, this gradient in ln[VMR] would be constant throughout the stratosphere, and this seems a simpler analysis than having to extrapolate (linearly in this study) the fundamentally curved line of the gradient in [VMR] to some tropopause value.

It would have been helpful to see a plot of ln[CFC-11] v ln[CFC-12] to verify this (as well as provide alternative estimates of the lifetime ratio) but what is surprising to me (although apparently not the authors since they don't comment on it) is that the [VMR] (rather than ln[VMR]) gradient of the ACE-FTS data is almost constant with altitude (Fig 4, lower right) and the lower left panel of Fig4 seems to show CFC-12 at an almost constant 250 pptv higher than the CFC-11 concentration. This does not seem physi-

cally reasonable considering the factor 5 variations in CFC-11 values. Perhaps the a priori is having undue influence here?

Specific comments

p16873: I didn't understand the 'analysis' window width of 100ppt. If you assign the derived gradient to the mid-point of the analysis window I would not expect the gradients plotted in the lower right plots in Figs 3,4,5 to exceed (satellite maximum-50pptv), yet the points seem to be plotted right up to the satellite maximum. Did you use narrower windows as you approached the end of the range?

p16876/77: HIRDLS had a major issue with absolute calibration and, in the past, I believe that they have used MIPAS fields to provide an indirect calibration. I don't know if this is still true for the data version that you have used, but if so it removes some independence between the two results.

p16877: MIPAS operated from 2002-2014 yet you only use the 2002-2004 data. I presume this is the limit the Hoffmann et al dataset but data for the full mission are available (eg the ESA L2 products) and it seems a pity not to have used years which overlap the ACE climatology (2004-2009) which might have provide some insight into whether the differences between the two satellites could be explained by interannual atmospheric variability.

Minor/Technical corrections:

Just a superfluous comma after 'account' on p16881, line 17.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16865, 2014.