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

Anonymous Referee #3

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General: This paper presents an analysis of satellite data and model simulations of CFC-11 and CFC-12, and applies a tracer-tracer correlation method to assess the ratio of steady-state atmospheric lifetimes. It is an important topic - and one has been the focus of several recent papers and a SPARC evaluation. The paper is generally clear and concise and the figures and tables are all useful and complete. There is one major recommendation for the Discussion and Conclusion section, along with minor comments for revision below

Major: Most of the discussion centers on Table 1, which is an excellent summary and comparison of results. More clarification and discussion is needed, however, for a complete comparison.

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1. Differences with Brown et al. are much larger (16%) than one might expect given that the same satellite data set was used. Three possibilities are mentioned (different time period, linear vs. quadratic fit, satellite vs. surface burdens). It would be most useful to other researchers if this paper quantified the impact of each effect. For example, it would be helpful to know whether using 2005-2010 vs. 2004-2009 really makes that much of a difference, or whether the method is really that sensitive to using a linear vs. quadratic function to extrapolate the slope.

2. There is no discussion for the large ($\sim$25%) disagreement with Laube et al (2013). That study used aircraft and balloon data. Can all of the difference be attributed to different data sets, or are there large differences in the analysis techniques?

3. There is no discussion of the large difference between the EMAC/CLaMS model and the Chipperfield et al (2013) model results. The latter was based on a multi-model analysis. Does this imply that the EMAC/CLaMS model is doing something significantly different than most of the other models used in CCMVal? It will be important to identify the reason or reasons for the difference.

Minor: p. 5, line 22: The tropopause is not entirely below the sink region. Up to 2-4% of CFC-11 loss occurs in the tropical upper troposphere (e.g. Chap 5 of SPARC Lifetimes Report). It would be useful to note this and to quantify the impact of neglecting this loss on the tracer-tracer slope method.

p. 7, line 23: The given value for alpha depends on B_1 as representing CFC-11 and B_2 as CFC-12, I think. However, this is not explicitly stated (or it was not obvious to me).

p. 8: It is made clear that the analyses are based only on midlatitude data, but not so clear whether annual means are used until pp. 11-12. It might be useful to point this out early on p. 8, but more importantly in connection with the tracer-tracer slope technique, it should be discussed why the analysis is not restricted to the fall-winter-spring season. In Volk et al (1997) it was pointed out that for this method, vertical

gradients should be derived from data taken during the winter half-year that dominates net transport.

p. 14, lines 10-11: A more complete description is needed for the ClaMS simplified chemistry scheme and its treatment of photolysis rates and O(1D) (and thus, ozone) chemistry since these are the main loss reactions for CFC-11 and CFC-12. For example: the input UV solar spectral irradiances, the treatment and parameterization of oxygen and ozone absorption, and whether the modeled ozone distribution is realistic in comparison to observations.

Figs 4-7: Suggest terminating the red line at CFC-11 = 100 ppt, since the linear fit to the slope is restricted to values of CFC-11>100 ppt.

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