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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.

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Authors' response to referee #1 comments for manuscript acp-2012-758:

First, we wish to thank both reviewers for their time and effort in reviewing this paper. All comments and suggestions have been taken into consideration in revisions to the manuscript, and below we address specific points and questions raised by the reviewers.

In addition to the revisions suggested by both reviewers, there are a few additional changes to the analysis that we have implemented in order to produce improved lifetimes and uncertainties: 1. Means and uncertainties are calculated in 1/tau space

consistent with the fact that the loss rates, which appear in the denominator of the expressions for tau, are the primary quantities for our analysis. 2. CRISTA-1 and 2 results are averaged first, before computing the multi-instrument mean, since these two datasets are from the same instrument with similar retrievals. 3. Burdens have been recalculated based on a mean surface pressure of 985 hPa rather than 1000 hPa. 4. A typographical error found in the MIPAS CFC-11 steady state lifetime in table 1 has been corrected.

Referee #1 - Response to Specific Comments:

1. Referee comment: Only one month of CRISTA-1 and CRISTA-2 measurements were used. However, the authors do not state why longer periods were not available for these instruments. A line or two explaining this would be helpful to readers not familiar with the details of these missions. Furthermore, might we not expect a "subsampling" error on the two lifetimes estimates derived using these instruments due to the data not fully spanning potentially real seasonal or inter-annual (e.g. QBO-related) lifetime changes?

Authors' response: The duration of observations for both CRISTA-1 and CRISTA-2 were about 8 consecutive days each, due to the limited duration of space shuttle flights. This is now clarified in the discussion of data sets, and the more general topic of "subsampling" errors is addressed in the discussion of Figure 4 (see also response #4 to comments by referee #2).

2. Referee comment: P28734, L22. A supporting reference (model–based lifetime calculations) would be helpful.

Authors' response: We added a reference to model-based lifetimes (Douglass et al 2008).

3. Referee comment: P28735, L1. I do not think Prinn et al., 2000 is the most suitable reference here because I don't think they aimed to reevaluate lifetimes in this paper.

Perhaps Cunnold et al., 1983, in which ALE data were used to estimate the lifetime of CFC-11 would be more appropriate?

Authors' response: Reference to Prinn et al has been changed to Cunnold et al 1983.

4. Referee comment: Section 3: This section is quite long with a large amount of information on a range of subjects. It might be helpful to the reader to sub-divide this into appropriate subsections.

Authors' response: This is a very useful suggestion. Section 3 has now been divided into 4 sections: 3.1 General Methodology; 3.2 CFC Observations; 3.3 CFC-11 Loss Rates and Lifetimes; and 3.4 CFC-12 Loss Rates and Lifetimes.

5. Referee comment: P28741, L16. Constant surface mixing ratios were used. Do you expect that ignoring the surface latitudinal gradient would induce a significant error?

Authors' response: The use of constant surface mixing ratios that ignore surface latitudinal gradients has a negligible impact on calculated loss, because over 95% of the loss occurs above the tropopause where mixing ratios are constrained from the satellite data. The calculated global burden is impacted by surface mixing ratios, however, because most of the CFC mass is within the troposphere. Nevertheless, the use of a global mean mixing ratio will produce the same latitudinally intergrated burden as one that is calculated using mixing ratios that include latitudinal gradients.

6. Referee comment: P8744, L15. The uncertainty analysis for the individual lifetimes is clearly described in the appendix. However, I'm confused about how the multi-instrument mean uncertainty was arrived at. Furthermore, I don't quite follow the point the authors are making when they say that errors associated with the stratospheric profiles were neglected (aren't these the largest sources of error?). These lines need to be expanded and clarified a little to make clear how these uncertainties were calculated.

Authors' response: The discussions of uncertainties in the appendix and in sections 3.3-3.4 have been revised considerably. First, multi-instrument means have been con-

verted to best-estimate lifetimes by averaging in 1/tau in order to more accurately reflect the mean of the global loss calculations (which appear in the denominator of the lifetime expressions), which leads naturally to a recomputation of uncertainties in 1/tau space. The revisions also addresses comments made by reviewer 2. We now also include the impact of stratospheric profile errors in the best-estimate lifetime uncertainties. These revisions have led to small changes in best estimates and overall uncertainties, but they do not affect the main conclusions of the paper.

7, 8. Referee comments: - P28746, L25: ...in constraining THE oxygen absorption.... - P28747, L6: ...appearing in THE loss rate calculation...

Authors' response: Grammatical errors have been corrected, thanks.

9. Referee comment: P28747, L25: " ...no averaging kernels are considered for the error analysis". Please clarify exactly what is meant here.

Authors' response: Vertical averaging kernels are not available in the ACE trace-gas retrievals (optimal estimation and averaging kernels are not used). We have added this comment and a reference to the ACE retrieval approach for interested readers.

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