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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CCl_4 distribution derived from MIPAS ESA V7 data: validation, trend and lifetime estimation" *by* Massimo Valeri et al.

Anonymous Referee #2

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This manuscript describes the retrieval and interpretation of a near-global data set of the atmospheric trace gas carbon tetra chloride (CCl4) from the MIPAS satellite instrument as obtained between 2002 and 2012. I consider the manuscript to be publishable in ACP after the points outlined below have been addressed, in particular the ones regarding the amount of quantitative information and the lifetime estimates. In addition I urge the authors to reconsider the excessive use of abbreviations which is limiting readability.

p1 l3. The recent SPARC report with that name should be credited here. Given that it was a very recent and international effort on CCl4 I find that report has been cited and used very little throughout the manuscript.

p1 l12. This statement and evidence for it is nowhere to be found in the manuscript.

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p1 l12-14. I disagree. This good agreement only proves that the remote sounders are producing similar results, but it is not a validation.

p1 l15-20. I would strongly recommend some more quantitative information in this section. What are the actual trends, the lowest altitudes sounded by MIPAS, and the comparability of the mixing ratios and trends, including uncertainties? Also, how do the authors explain the positive trend in the Southern mid latitudes?

p4 l20-25. Most of that section should be moved to the caption of the figure. In fact most figure captions in the manuscript need more explanation of what is shown.

p8 l 11 & 14. There are still quite a few minor English language problems in this manuscript, two examples here are “CCl4-poor” and “in the South Pole”.

p8 l11.If there is a seasonal effect it is not obvious from figure 4.Can the authors quantify this seasonality, also to prove that it is indeed statistically significant? A similarly quantitative approach would help in other parts of the manuscript too, e.g. the earlier statements on latitudinal and altitudinal gradients.

p9 figure 4 caption. “May 20117”

p11 l9-11. This is not correct. Numerous aircraft and balloon campaigns have measured CCl4 with alternative in situ techniques. Please see e.g. Volk et al., 1997 and the many papers that cite it, as well as the FTIR total column measurements from the Jungfraujoch station.

p16 l4-6.This is exactly where alternative validation methods could help.

p23 l10. A ”kind of global CCl4 trend”?

p24 l6. The smaller trend error does not take into account the biases, though.

p24 section 6. This section needs some additional work. The methodology (equation 2) is not used correctly as Plumb and Ko (1992) clearly state that a) it should only be applied to two species in steady state and b) the slope needs to be determined exactly

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at the tropopause. Moreover the method was improved by Volk et al.,1997 and Brown et al., 2013 to e.g. correct for tropospheric trends and derive steady-state lifetimes. A second problem with the lifetime estimate presented here is that it is highly dependent on uncertainties and potential biases of the trace gases involved, i.e. CCl4, CFC-11 and CFC-12. Can the authors present evidence that these uncertainties and biases have been taken into account for the determination of the lifetime and its uncertainties?

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