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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Changing trends and emissions of hydrochlorofluorocarbons and their hydrofluorocarbon replacements" *by* Peter G. Simmonds et al.

Anonymous Referee #1

Received and published: 12 January 2017

This is undoubtedly a very good and timely work updating trends and emissions of 8 trace gases of importance to stratospheric ozone deplation and/or global warming. I consider it to be of sufficient quality and importance to be published in ACP. There are however some concerns that need to be addressed beforehand. Two of them stand out:

1. In section 4.1. atmospheric mole fractions and growth rates are discussed but there is no attempt to compare any of these with existing published data, e.g. from the multiple articles cited in the introduction. There is only a discussion for emissions, which leaves the reader in the dark as to whether emission estimates might agree for the wrong reasons (e.g. model differences). 2. As also detailed in the comments be-

low, some of the uncertainties presented are not very well explained. It is for instance unclear to the reader why global atmospheric mole fractions and growth rates are so much less uncertain than global emissions. A brief explanation of the major contributing factors to these uncertainties would help.

In addition there are a substantial number of (partly equally important) comments on specific parts of the manuscript that require attention prior to publication: 1. Line 1-2 and 19-20. Consider including "HCFCs" and "HFCs" in the title or "hydrochlorofluorcarbons" and "hydrofluorcarbons" in the abstract.

2. Line 27-28. This reduction is not significant even within 1 sigma, so it is rather questionable to call it a reduction. And how can "all quoted uncertainties in this paper" be 1 sigma if it is stated later that some represent a complicated mix of "uncertainties due to the observations, the prior and the current best-estimate lifetimes of these compound. . ."? A clear and ideally brief explanation of what different uncertainties stand for and how these were calculated is needed.

3. Line 29. The second "HCFC" is not needed.

4. Line 33. "increasing annual growth rates" presumably refers to mole fractions, not emissions? I find it somewhat surprising that the abstract does not include any numbers for mole fractions, especially since the first two words of the title are "Changing trends". 5. Line 42-50. The first paragraph is not backed up with any references whatsoever.

6. Line 51. It is worth noting why they have been introduced as replacements for the HCFCs and CFCs.

7. Line 52-53. I recommend citing Table 1 here.

8. Line 68. Please name these stations or cite Table 2 here. I also find it rather surprising that no station data is shown anywhere in the manuscript or the supplement. Perhaps it would be more appropriate to add "global" to the title if regional trends and

emissions are not covered?

9. Line 74. These are not just lifetimes but steady-state atmospheric lifetimes.

10. Line 77-78. The most recent estimates of ODPs and GWPs are published in the last WMO Ozone Assessment (2014) as well as the last IPCC report (2013) and I urge the authors to cite the appropriate chapters from these assessments as well as to not use outdated numbers in their calculations.

11. Line 81. Please explain what is meant by “tuning”.

12. Line 97-108. Again, none of these statements is backed up by any reference.

13. Line 109. Why are these “non-zero” ODPs given as “0” in Table 1?

14. Line 117-120. I suggest changing the title of this paragraph to “AGAGE sites” or similar, and to mention “coordinates”.

15. Line 146. Please explain the technique of filling standards “cryogenically”. Have any differences been observed between standards filled with the two concurrent techniques?

16. Line 176-178. This is rather opaque. Please explain the methodology of calculating these uncertainties.

17. Line 232-234. Please clarify whether “2015” means the end of that year or the annual average.

18. Line 235-238. I suggest moving this paragraph to after the end of the HCFC mole fraction and growth rate discussion.

19. Line 308-309. I disagree. From the data shown in Figure 1 and 2 it is not clear whether HCFC-141b growth rates where at a maximum in 1998 as this is where the data starts. It might also be worth mentioning somewhere in this manuscript that data exists for earlier years but is not being focused on (and why).

20. Line 320. I find this nomenclature confusing as there is only one supplementary material file.

21. Line324. Surely there are some EDGAR emissions for HCFC-22. Why are these not included or discussed?

22. Line 325. How can emissions be derived from 1995 onwards using global mole fractions that start in 1998?

23. Line 335-336. 63 $\pm$ 6 Gg/yr and 31 $\pm$ 6 Gg/yr are not similar. Also, the minimum occurred during 2004-2005, not the decline.

24. Line 342-343. This is quite a striking difference. Can the authors offer any explanation?

25. Line 344. Please check the number for HCFC-141b.

26. Line 388-398. The quality of this figure is much worse than the others.

27. Line 477. How do these emissions compare with previously published estimates in O’Doherty et al., 2009 and 2014?

28. Line 480. Why is HFC-125 listed first here and in the figure?

29. Line 495-497. This should probably be “in agreement with” instead of “in spite of”.

30. Line 499. Please clarify whether “most rapid” refers to relative or absolute changes.

31. Line 514. Have the authors considered that Velders et al., 2009 also included HFC-152a, HFC-245fa, and HFC-365mfc in their calculations?

32. Line 550. Why was Velders et al., 2009 data “rescaled”?

33. Line 555-556. This sentence seems to disagree with the previous one.

34. Line 591. Can the authors present any evidence for this claim?

35. Line 595. It would help to include the starting point of that rise in order to illustrate

it.

36. Line 598. “at al.”

37. Line 604-607. This is implied to be a main point of the manuscript as it is included in the abstract and the conclusions (lines 635-637 and 647-649). I strongly recommend either moving this analysis to the main manuscript or removing the respective statements.

38. Line 620-623. This is quite surprising. The HCFC-22 growth rate has dropped from around 8 pmol/mol to less than 4 pmol/mol in that period, yet the global emissions have not been affected much? I think many readers would be interested in an explanation for this apparent disconnect.

39. Line 625-626. I thought the CDM had expired?

40. Line 650-652. Please explain why only above-linear HFC growth should be related to a deficit in HCFC emissions.

41. Line 654-658. Why are the HFC results only compared to Velders et al. (2009) and not the updated and improved projections from Velders et al. (2015)?

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