

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

### **Anonymous Referee #2**

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This manuscript provides updates on measurements of HCFCs and HFCs from a global sampling network that provide a global picture of the transition being made as a result of the Montreal Protocol. Results are provided and discussed in terms of atmospheric changes and inferred emission rates. Comparisons are made to emissions derived previously on a mass basis and are considered also on the basis of CO<sub>2</sub>-equivalent emissions for individual gases and for classes of gases. The paper presents high-quality measurement data that add to our understanding of recent atmospheric changes stemming from the Montreal Protocol. I found some sections in need of additional consideration before publication in ACP would be appropriate.

On uncertainties: It's not clear that the change derived for aggregate HCFC emissions

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from 2010 to 2015 is accurately characterized as a decrease given the substantial overlap in the stated uncertainties. The two different estimates are 483 +/- 70 and 444 +/- 75 (this decrease is mentioned in multiple places in the text). Same point can be made for the 1.4% difference in cumulative emissions over the two five-year periods (lines 553-559). This needs more careful consideration and an accurate description. The "increase" in aggregate HFC emission values also need considering, as there is substantial overlap there too. I also find it surprising that the uncertainties on global values provided in Figure 1 and 2 aren't dependent on mole fraction or the number of sampling stations used to derive the values (2 sites in early years with the ADS and more sites recently with the updated Medusa instruments). Why isn't this observed? Were the early measurements from two sites much more precise?

On implications for compliance with the Montreal Protocol: The text in the abstract (lines 29-33) and on lines 584-595 can be read to suggest that usage of HCFC has increased after 2013 despite the global cap on production and consumption. Text on lines 589-591 suggests developing country emissions have increased in spite of the 2013 cap on production. These seem to be fairly significant statements with important implications but no evidence is supplied to back them up. I don't doubt that HCFC emissions and use increased prior to 2013 in developing countries, but what evidence suggests that use and emissions increased after 2013 from these countries?

On comparisons with emission estimates presented previously: Emission estimates for many gases and many sources (Figure 3). It's great to see the authors provide emission estimates from previous work for comparison of derived magnitudes and trends. Although I'm not sure it is surprising that CO<sub>2</sub>-e emissions of HCFC-22 are larger than the four HFCs, has this not been obvious from earlier work and WMO assessments? Regarding figure 3, it would be more useful for the reader if it were clear which results were derived independently from the AGAGE data (from different observations and model), which were derived independently from the AGAGE 12-box model but with AGAGE data, and which were derived from inventories (e.g., what are Velders et al.,

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results derived from?). Also, a quick look at the Montzka et al., 2015 paper shows emissions derived and presented for HCFCs and HFCs for many years, not just 2012 (only 2012 results are plotted in this manuscript). This comes across as a bit misleading, but more importantly, the authors miss a significant opportunity to determine if the two measurement networks provide similar conclusions regarding the unusual inter-annual changes in emissions for these gases (particularly the uneven changes for HCFCs).

Unusual insertions in the text: 1) The first mention of HFC-23 is in the conclusion section. This seems out of place and, I'd suggest, inappropriate given that none of the information provided about HFC-23 is derived from data or analyses of observations presented in this manuscript and the points made aren't closely relevant for this manuscript. 2) The discussion of HFCs being released predominantly in blends seems out of place and unusual. This is a straightforward conclusion based on uses of these gases by industry and it is not clear how the atmospheric data add to this discussion. There is a related point made in the conclusion about results not agreeing with some from Montzka et al (2014?), but there is no indication given as to the reason for this difference. Is it because the derived emissions disagree or is it because more information was brought to the analysis in the present manuscript than was available previously that refines our knowledge?

Details: Citation seems important but is lacking on line 50.

Lines 61-65. Have no HFC results been reported by NOAA since 2004?

Precisions are quoted on lines 151-154 as single numbers, but I would guess that they have changed over time with different instruments and as atmospheric mole fractions have increased from v small levels. Does typical = median?

Results and Discussion: How comparable are the model output mole fractions to the actual results? No indication of this is presented or mentioned.

Are growth rates quoted (line 237-238) based on some time interval, or just the mea-

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sured change during 2015?

Line 309, reconsider text. HCFC-141b growth rate isn't reported before 1998, so it doesn't seem accurate to suggest that emissions peaked in that year.

Consider units on increasing emission rates as per yr per yr.

Line 499 and 561-562. I believe this is correct only if you refer to relative rates of increase.

Line 565. "emissions of HCFC-22 represent 79% of the global cumulative HCFC burden..." doesn't make sense. Is the percentage relating to mole fractions or emissions?

WMO reports are appropriately cited by lead coauthor names; consider doing that as recommended in the reports.

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