

Interactive comment on “Continued increase of CFC-113a (CCl₃CF₃) mixing ratios in the global atmosphere: emissions, occurrence and potential sources” by Karina E. Adcock et al.

Anonymous Referee #2

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This paper updates and advances our understanding of CFC-113a in the global atmosphere, its lifetime, regions contributing emissions, and potential sources. The continued increase is interesting and important to document and understand, especially given the accelerated increase that appears to be continuing after being initially documented in an earlier paper. It is nice to see the broad range of measurement locations and information they supply. The paper is mostly sound, although there are a few sections where some reconsideration of results is warranted and where some improvement in the writing is needed. But after these issues are addressed, the paper likely would be appropriate for publication.

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Issues to consider: As the authors note, the impact of these emissions on the ozone layer to date is minor. Suggesting that more CFC-113a might make it to the stratosphere than is indicated by surface means is a conclusion whose importance can only be speculated about (line 309-312). It is not a conclusion based on data presented here so doesn't seem appropriate to include. Geophysica results from the stratosphere are indicated as starting at background levels and decreasing above; in other words, entirely consistent with background mole fractions at Earth's surface.

Related to this, it seems important to mention in the text that a constant emission of 2 Gg/yr for a chemical with a 50-yr lifetime yields a steady-state global mole fraction of 5 ppt (15 ppt of Cl for CFC-113a). This helps the reader to objectively understand the significance of these results compared to the contribution of other chemicals including CFCs in a much more meaningful way than a comparison of cumulative emissions since 2007, for example (lines 206-209). Undoubtedly CFC-113a emissions could increase, but the potential for this is constrained by the cause of the increasing emissions and, for that fraction associated with HFC production, the Kigali Amendment.

Potential sources for CFC-113a emissions should be considered in light of the fact that emissions were fairly small until 2010, and then increased to a new value and have been essentially constant since. It is my view that this step change in emissions is primarily why this paper is worth publishing in ACP. It provides a strong hint as to which process likely caused this step change (at the least it reduces the likelihood of some causes) and is important to consider in gauging the likelihood of emissions increasing in the future. At the present time this section (4) rambles a bit and would benefit from significant tightening.

On the upper lifetime limit derived for CFC-113 based on the observed rate of change of CFC-113. Some consideration or discussion of steady-state lifetimes vs lifetimes at zero emissions is required here before such a conclusion is made. These are two different quantities that have different values. For CFC-113, I expect its lifetime in the presence of zero emissions to be slightly shorter than one derived at SS. See papers by

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Prather on this topic, and consider calculating the difference in your model to determine if an upper limit to a SS lifetime is inconsistent or not with the observationally-derived value upper limit (assuming $E=0$).

Line 438-440. A suggestion that new regulatory mechanisms might need to be added to the Montreal Protocol is made in the conclusion. This statement diminishes the objective nature of the data and discussion included in the paper. Policy is made with consideration of a broad range of costs and benefits, and you cannot begin to cover this complex and multifaceted discussion in a paper about atmospheric changes. If you want to comment on policy, consider doing it with an "if...then" construction. And in this case, if policy-makers wanted to require absolutely zero emissions of CFCs, then they might consider doing x,y, and z.

The discussion of section 4 in the supplement is not useful without explicitly considering the changes over time in tropospheric entry values. Without this, the section adds little to the paper.

Other items. How was calibration consistency maintained throughout time and across the different missions?

Figure 3, mention blue solid and dashed lines in caption.

p. 2, line 89-91, uncertainties are mentioned, but these are not the uncertainties used in the modeling, which are discussed in the supplement but not the section on modeling. I'd suggest that this appear somewhere in the main text.

On uncertainties in CFC-113 calibration arising from co-elution of CFC-113a. Consider doing the atmospheric measurement community a favor by discussing the relative magnitude of interference that an analyst might have in measuring CFC-113 given co-elution of CFC-113a at the different ions these chemicals have in common (perhaps a simple table in the supplement?). This would be very helpful, and easy to add, I imagine, given that you are in a unique position to supply this important information

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that to first order would be independent of mass spec instrument being used.

Lines 133-136 and 150-152. This doesn't make sense. Fitting well data at CGO wouldn't say much about the accuracy of and emission distribution in the lower SH and throughout the NH.

Lines 217, Confusing phrasing. Ultimately, global emission magnitudes derived from observations depend on the lifetime used, and you used different lifetimes than others.

Paragraph starting on line 251. First part: make this a discussion of variability in mole fractions and not just mole fractions. This makes your point valid and will help later when you are discussing differences in trends vs short-term variability in the UK vs Cape Grim. Second point: mention the HCFC-133a lifetime.

Para starting on line 265 (also line 291). Assertions are made that are not valid here or that extend limited results to broader context without justification (was Tacolneston sensitive to emissions from all UK source regions? Why would results from this site be representative of the NH? They might be proportional to that quantity, but not necessarily quantitatively the same). These are weakness to the paper that aren't needed and could be easily avoided.

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