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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "HFC-152a and HFC-134a emission estimates and characterization of CFCs, CFC replacements, and other halogenated solvents measured during the 2008 ARCTAS campaign (CARB phase) over the South Coast Air Basin of California" *by* B. Barletta et al.

**Anonymous Referee #1**

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The paper by Barletta et al. describes the halocarbon flight-measurements as a part of the ARCTAS project and emission estimates of HFC-152a and HFC-134a in Los Angeles County as well as their extrapolation to the South Coast Air Basin of California (SoCAB) and US. The study presents a new data set concerning halocarbon emissions from mega cities, and I think this is publishable in ACP. However, I have some serious concerns about the presentation of the data and its interpretation, which are explained below. I recommend publication after these points have been addressed.



1. Values of precision and accuracy for HFC-152a are missing.

2. "Chemical ratio" defined in this study is practically the same as the "tracer ratio" which is more widely used for the top-down emission estimates. I think the authors should describe the technique as "tracer ratio". If this is deemed inappropriate, then please describe how "chemical ratio" differs from "tracer ratio".

3. Please give the error range of the emission estimates from the AQM method. Since the authors conclude that the close agreement between the results from the two methods (tracer ratio method and AQM method) lends credence to the emission estimates presented in this paper, this should be important.

4. p.28038 line 7-13 — Additionally, it is particularly important to assess accurately the atmospheric levels of CH3CCl3 because this species is used to estimate global concentrations the hydroxyl radical (OH). Errors in the determination of global OH levels can be significant if emission sources in developed areas are not properly characterized. Millet and Goldstein (2004) calculate that global OH was underestimated by 7.2% for 2000 and 11% for 1997 by neglecting ongoing methyl chloroform emissions by non-Article 5 nations.—

I don't understand why the authors state this in the Conclusion section. There is no discussion on CH3CCl3 data beforehand except that the authors found above baseline concentrations of this compound. I therefore think the statement should be deleted.

5. Please state the reason why only the field measurements obtained over the SoCAB were examined, and the data from the San Joaquin Valley were excluded.

6. p.28024 line 7-10 — The original standard for the calibration of the NMHCs is gravimetrically prepared from National Bureau of Standards and Scott Specialty Gases standards (accuracy$\pm$5%). These standards are used for the calibration of the highly pressurized whole air standards (2000 psi) contained within aluminum cylinders.—

I don't know why the authors describe the calibration of NMHCs (non-methane hydro-

carbons), in spite that they report the measurements of halocarbons. This should be corrected.

7. The authors emphasize the importance of CFC data for policymakers to asses the effectiveness of mitigation strategies. I just wonder why the authors did not try to estimate the emissions of CFCs, but only focused on the two HFCs.

8. As suggested in the manuscript, the extrapolation using population data is likely to be overpredicting. I encourage the authors to consider a different approach using anthropogenic species such as CO, which should have closer relationship with HFCs than population. The authors have actually found good correlation between CO and HFC-152a in this study.

9. I would recommend to add another reference "Yokouchi et al., Geophys. Res. Lett., 2006" where the tracer ratio technique was used to estimate emission of selected halocarbons including HFC-134a and HFC-152a.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 28017, 2010.