

Interactive comment on “Atmospheric observations and emission estimates of ozone-depleting chlorocarbons from India” by Daniel Say et al.

Anonymous Referee #2

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My main comment in the quick report was: What sets this manuscript apart from its companion paper (acp-2018-1146, Emissions of CFCs, HCFCs and HFCs from India). Both report synthetic halocarbon measurements from the same campaign which are even shown to partly correlate with each other due to similar sources. The authors responded as follows:

“Our companion paper ‘Emissions of CFCs, HCFCs and HFCs from India [revised to ‘Emissions of CFCs, HCFCs and HFCs from India based on atmospheric measurements]’ focuses on a suite of gases that are used extensively as refrigerants and foam blowing agents, whereas the primary application of the chlorocarbons discussed in this

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paper are as solvents. While the majority of gases mentioned in the companion paper are emitted predominantly from residential and mobile sources, chlorocarbons are typically associated with industrial sources such as manufacturing facilities. In addition, the analysis of chloroform in particular requires consideration of biogenic sources not shared by any of the CFCs, HCFCs or HFCs. While the companion paper is framed in the context of the Paris Agreement (emissions totals quoted using global warming potentials in carbon dioxide equivalents), this manuscript focuses exclusively on the potential significance of Indian chlorocarbons emissions as threats to the recovery of the ozone layer (emissions totals quoted in ODP Gg yr⁻¹, where possible). Hence, we feel justified in presenting the Monsoon measurements as two separate manuscripts. However, we do agree that there is the need to frequently look up details from the companion, and that this is a limitation of the current manuscript. To address this issue, we add complete experimental details to the Materials and Methods section, including a full description of the flask sampling routine and analysis, simulation of back-trajectories using NAME, assignment of boundary conditions and estimation of global emissions using the AGAGE 12-box model.”

This justification is flawed. As stated in the companion paper, CFC-113 is a solvent. Various publications have shown that emissions of HCFC-22 and HFC-23 are dominant in distinct industrial areas, see, e.g., Fang et al., EST, 2015. Several of the gases in the companion paper are therefore not emitted predominantly from residential and mobile sources. The authors themselves link DCM and chloroform to HFC-32 and HCFC-22 through chemical manufacturing processes as confirmed by their own atmospheric correlations. So the species in the two papers are heavily linked and in my opinion it still does not make sense to discuss these links in each of the companion papers. In addition, half of the species reported in the companion paper are ODSs, so their emissions are a threat to the recovery of the ozone layer, too. Their potential for that is however not even mentioned in this manuscript. The duplication of the experimental details in the Materials and Methods section only adds to these concerns. I therefore still think that the authors should revisit where to draw the lines between the two papers,

or whether there is actually a need to split this good work into two half-duplicates.

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