

Interactive comment on “Emissions of CFCs, HCFCs and HFCs from India” by Daniel Say et al.

Anonymous Referee #2

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The manuscript by Say et al., use a series of low altitude airborne measurements of CFCs, HCFCs, and HFCs in combination with models to estimate regional to national emissions of these compounds from India. The analytical measurements were of high quality and provided a useful data set for the emissions modeling. Though I am not really familiar with the modeling techniques used here, the approaches applied seem to be accepted practice and relatively sophisticated for this type of estimation. Overall the manuscript is well written and well organized, and I recommend publication after some revision.

My main concern about the work is the extrapolation to annual emissions of data spanning only several weeks in one limited region of India. The author's assert that the emissions should be reasonably stable over a long period of time, but provide really no evidence that this is true. If emissions are largely from manufacturing, there can be significant variations in emissions from production facilities. Also, as the authors note,

some unexpected seasonality has been observed. There are really no measurements available to check on the various assumptions of conditions during June as being representative of the annual conditions. While error analysis is a significant part of the modeling procedure, there appears to be no estimate of additional uncertainty related to extrapolation of the short and regionally limited data set to annual and national emissions. I would like to see some clearer statement about the overall uncertainty that the authors can ascribe to the national emissions from this extrapolation. Or provide some clear caveat that, “if the emissions calculated for this time period could be scaled uniformly, then the annual emissions would be” Along the same lines, it is unclear to me how uncertainties in the boundary conditions contribute to the final estimate and its uncertainty, and what might be the effect of emission plumes from beyond the Indian borders on the overall estimate of Indian emissions. My understanding is that the boundaries represent some broad regional average from a 12 box model. Would concentrated emissions from Pakistan or East Asia influence the estimates of emissions from India? Further, it was unclear how (if) the Mace Head and Cape Grim measurements were used in the model analysis, or were just used to represent “typical” NH and SH halocarbon levels. My main request for revising the manuscript would be for the authors to more clearly define how their various assumptions contribute to the emissions magnitudes and uncertainties they report.

Some other comments are given below:

P 3 , L 33, Since there may have been some contamination in a few samples, I wonder how long the samples were stored after cleaning and before use on the flights. The note about storage in rooms without air conditioning is relevant for these measurements, but evacuated or even pressurized samples in a container that could get very toasty might also lead to artifacts in canisters with small leaks. P4, L 16, Just because I am curious about statistical calculation, could you describe how you calculated and report the overall standard deviation from triplicate sample measurements? When measurement precisions are shown are these 1 or 2 std deviations? P6, L10-15. These few lines

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contain some assumptions that could contribute in some unknown way to the error of the method. As noted, I'd like to have some quantitative estimate of the error. E.g., "climate may minimize this", or "estimates are likely to be representative" or "characteristics are thought to be true". P8, L 16 – 18. Here is where I am not sure about the use of Cape Grim to represent the conditions of the southern model boundary, or the 12-box average. I wonder if the southern boundary (from either source) might overestimate the cleanliness of the regional "unperturbed" Indian background. P9, L1. And Figure 3. While there is some general correlation observed, a correlation coefficient of 0.53 is a weak argument to support common sources. There is significant variability that suggests a variety of different sources (for these and other gases), and significant variability from possibly sporadic point sources. It is this level of variability that causes me concern about extrapolation to the whole year. P9, L 29. I think the author's aren't really talking about stability of HFC-134a, but potential for leakage and artifacts, either before or after sampling (most likely before). P13, L7. I don't think that % of global emissions are expected to scale with just population, so India's 17.7% of world population wouldn't necessarily imply anything about halocarbon emissions. Data availability. I would like to be able to examine the data used in this paper, but I didn't see the data availability and source listed. Title: I agree with the suggestion of the first reviewer to include "...from airborne measurements" in the title.

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