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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global and regional emissions estimates for HCFC-22" *by* E. Saikawa et al.

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We would like to thank the reviewer for taking the time to read and review the paper. Below we write the answers to the questions/comments raised in the review.

> This manuscript presents work on estimating global and regional emissions of HFC22, a potent greenhouse gas and an ozone depleting substance. The authors use the latest data from 8 NACP tower sites, AGAGE, ESRL/GMD, and NEIS networks, they employ MOZART model and an Bayesian inverse methodology to infer the sources. The manuscript is clearly written and very easy to follow. Both the global and the regional studies are interesting, although the global one seems to just confirm what the earlier studies have found already. In the regional study, the sensitivity of testing smaller and larger division into regions is a nice way to test the aggregation error. All

the figures, except as noted below, are very clear and helpful.

Thank you for the nice comments.

> I am not very familiar with the manipulation of flask measurements and was surprised to read about the ratio being applied. Is this a standard practice? It would be helpful to see a reference.

Thank you for this question. Because it is superior to use combined data from multiple networks, this methodology is used as a standard practice and we have now included references (Chen and Prinn 2006; Rigby et al., 2010) in the revised manuscript on p. 9.

> It would be expected to read at least a few sentences of background on STE in MOZART and stratosphere in general in MOZART: do we expect any more uncertainty?

Thank you for this great comment. We have revised the description of the model in the revised manuscript on p. 11:

The global three-dimensional chemical transport model, MOZART version 4 (Emmons et al., 2010) is used to simulate the three-dimensional HCFC-22 atmospheric mole fractions between 1995 and 2009. MOZART v4 is a model for the troposphere, has updates over the previous MOZART version 2, and is built on the framework of the Model of Atmospheric Transport and Chemistry (MATCH) (Rasch et al., 1997). Previous studies have found too strong stratospheric flux in the model using the reanalysis meteorology (Holloway et al., 2000; Van Noije et al., 2004; Xiao et al., 2011) resulting, for example, in errors in the tropospheric ozone budget as well as in the ozone mixing ratios in the upper troposphere (Emmons et al., 2010). We believe that this is not a major problem in our analysis, however, as the main loss mechanism for HCFC-22 is by tropospheric OH.

> It would also be useful to read some more justification of using quite old OH fields.

ACPD

12, C7818–C7821, 2012

How do they compare to the other OH fields others have used since then?

Thank you for this point. This OH field has been used widely, and was also used in the recent TRANSCOM study for CH4 (Patra et al. 2011). We have not conducted any comparison between different OH fields, but we agree that there is uncertainty related to OH. We have inserted the following in the revised manuscript on p. 24 as follows, as another way to improve the emission estimates:

Fourth, reducing the uncertainty in the OH and O1(D) (especially OH) fields involved in the loss of HCFC-22 will allow us to more accurately model atmospheric mixing ratios and improve our inversion results.

> In Eq. 1, if "y" is the difference between measured and modeled, then shouldn't "x" be the difference between priori and optimized?

Thank you for this point. Yes, x is the difference as well, and we have rewritten that in the revised manuscript as follows to make it clearer on p. 14:

x is the vector of the difference between the prior and the optimized emissions.

> P18262, line 22: suggestion to replace "uncertainty reduction increases" with "uncertainty decreases". That sentence is a bit confusing and it's hard to tell what the second "increase" means.

Thank you for this suggestion. We have changed the phrase as you suggested. It now reads as follows on p. 21:

The uncertainty decreases in 2007 in Canada/Alaska, US Midwest, and US West, and it decreases in 2008 in US East when we include the NOAA tower measurements.

> P18265, line 16-19: will higher resolution models help if there is so little data?

Thank you for this question. There are currently 4 stations in North East Asia (Hateruma and Ochiishi in Japan, Shangdianzi in China, and Gosan in South Korea). As our regional inversion results illustrated, tower flask measurements contributed sig-

ACPD

12, C7818–C7821, 2012

Interactive Comment

nificantly to constraining the regional emissions within the US. We can expect the similar result with higher resolution models that are able to simulate pollution events better than the coarse-resolution models for this region. To clarify, we have modified the sentence as follows in the revised manuscript on p. 24:

More research using a finer spatial and temporal resolution model that allows for a better simulation of the pollution events as well as a direct comparison of the regions is needed to resolve these differences.

> Figures 6-10: Please increase the fonts in the figures, especially the labels are hard to read

Thank you very much for this suggestion. We have changed the figures to make them more readable. Please see revised Figures 6a-e in the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18243, 2012.