Atmos. Chem. Phys. Discuss., 12, C4074–C4078, 2012
www.atmos-chem-phys-discuss.net/12/C4074/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "In-situ measurements of atmospheric hydrofluorocarbons (HFCs) and perfluorocarbons (PFCs) at the Shangdianzi regional background station, China" *by* B. Yao et al.

Anonymous Referee #2

Received and published: 27 June 2012

This manuscript presents a high quality data set of measurements of radiatively important trace gases over a period of one year. The data has been collected in globally important source region where in addition very little data has been published previously on these compounds. In addition the manuscript is well structured and – with the exception of some minor language problems – appropriately written. For these reasons I believe this to be a potentially very valuable contribution to the literature. There are, however, a number of problems mainly connected to the data analysis and interpretation that should be addressed first. The most important issue is the estimation of

emissions in section 3.4. I agree with referee 1 that using carbon monoxide as the reference tracer here is highly questionable for several reasons. Firstly, the estimate of the Chinese emission inventory of CO is based on a single study (rather than the several cited) and a growth rate of 3.4% both of which is not explained or justified. Secondly, although showing a wind rose, the authors do not show or discuss any variations in wind speed or trajectory calculations. Without this information it is very hard to assess the actual source regions. Even if air masses arrive from SW they might well have been influenced by other sources in S or SE Asia, maybe even South Korea or Japan. The air mass origins in 2010 and 2011 are not necessarily similar to those presented in Vollmer et al., 2009. Moreover, it is very likely that for the halocarbons high concentration events are dominated by sources in or close to Beijing and thus not necessarily representative for all of China. On the other hand the CO part of these correlations could well be influenced by a variety of sources not all of which are halocarbon sources (e.g. biomass burning). Again, considering trajectories could help here. It would be very recommendable to show these correlations against CO to corroborate such a crucial part of this manuscript. In case of considerable scatter a revision of the estimated uncertainties might be advisable. Currently these emission estimates are in my opinion not comparable to Chinese emission estimates from other studies.

Specific comments

P3, l2: How do you define "large"? The GWPs of these compounds have a considerable range.

P3, l9: Only HFCs

P3, l15: Should be "HFC and PFC measurements". Similar problems with plural can be found throughout the whole manuscript.

P4, l18: 10 metres is hardly a "tall tower".

P4, l24-25: No precision, accuracy or reference given.

P5, l1-2: This sentence is irrelevant unless some of these measurements are used in the manuscript.

P5, l5: What does “closely tied” mean? Also, references should be given for the calibration scales. Have all of them been reported in the literature before? If not, information is needed on their accuracy and how they were defined.

P5, l17: Ruckstuhl et al., 2010 has only reached the discussion stage in AMTD so far. The reviewers have requested some changes to this manuscript. It would be good to specify the exact method and criteria used here.

P5, l22-24: Not only the mixing ratio difference is of interest here but also the changes relative to background mixing ratios, which are much larger for some of the PFCs. Also, Fig. 2 does not support the interpretation of pollution events being “rare” for any of the compounds.

P6, l8: Uncertainties should be given to demonstrate that these mixing ratios are indeed “consistent” with each other. It would also be recommendable to compare growth rates.

P 6, l24: How was this estimate carried out exactly?

P6, l27-28: Only when assuming that the growth rate has not changed since then. Also “growth rate” should be used throughout the manuscript.

P7, l3-4: All other compounds? Also, why were different approaches used for different compounds?

P7, l5-9: Again it would be interesting to see the relative changes. Also, the use of small words such as “the” and “by” is incorrect in various places.

P7, l11: What are the criteria for “big differences”?

P7, l14-15: Which changes? And would they not also have the potential of greatly altering any emission estimates? Adding a reference might also be advisable here.

P7, l21: “year” instead of yr.

P7, l22: Which measurements were carried out? These should also be added to section 2.2.

P7, l23: Was the wind data measured at the surface or at the air inlet at 10m?

P7, l27: Should be “previous studies”

P8, l4 and Figure 4: What criteria are being used to identify “distinct emission sources”? It would also be good to put these anomalies loadings in context by comparing to other literature.

P9, l2: The variable “p” is not explained.

P9, l10-11: Is this a repetition of l2-3 or are both “all measurements” and “enhanced mixing ratios” significantly correlated? If so, which correlation was used?

P9, l16: How is the 10% uncertainty justified?

P9, l21-23: If the correlation is weak, then its scatter may well be used to infer a more realistic uncertainty range.

P10, l4-5: Which facts corroborate the hypothesis of previous projections overestimating growth rates?

P10, l27: Should be “Our”.

P11, l4: It would be good to be more quantitative here and to name the compounds again. Also, very little context is given in this section.

P16, Table 1: CO is missing.

P18, Table 3: This comparison is questionable as very different times and time ranges

are compiled.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 11151, 2012.