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Interactive Comment

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 #1

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This is an interesting set of results from a region of the globe not well characterized for these compounds. The results appear to be of high quality, given the consistency derived in the background atmosphere compared to other NH locations. The mixing ratio enhancements make sense with respect to input from regional sources, given the wind rose analysis. It is clear that this type of data will be invaluable for discerning the contributions of an important region of the global to atmospheric changes. Despite the very nice experimental work, however, I think the interpretations provided in this manuscript need improving before this paper is publishable.

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Interactive Discussion

Discussion Paper

Correlation in the observed mixing ratio enhancements above background for HFCs and PFCs relative to CO are used to derive Chinese emissions of halocarbons. The measured correlation slope can provide an estimate of halocarbon emission given an independent estimate for CO emissions. This approach has been used in the past by a number of investigators, but better descriptions about assumptions that are made and the accuracy of the information (e.g., CO emissions) are needed. Firstly, HFC emissions are derived as total Chinese emissions, and they are derived from a total Chinese CO emission based on a 2006 estimate and a growth rate for the intervening years of 3.4%/yr. What is this rate of increase in total Chinese CO emissions based upon? Multiple estimates of Chinese CO emissions estimates from the mid-2000s are given, yet no indication of their consistency or accuracy is presented. The authors assume an uncertainty of 10% on the total Chinese CO emissions, but no discussion is presented on how this number was derived. Secondly, issues of spatial scale are not considered. Are the measurements at Shiangdanzi representative of all of China? Or must some assumptions be made with regard to extrapolating the results from this site so as to be representative of emissions from all of China? What fraction of CO emissions from China is from industrialized regions (and/or processes) likely to be associated with emissions of HFCs? My guess is that this ratio is smaller than for more broadly industrialized nations where ratios to CO have been studied, and this may substantially influence these emission estimates and seems appropriate to consider. Thirdly, the description of the tracer ratio method needs some improvement, what is an "inherent relationship"? The tracer ratio method requires many important assumptions (source co-location, etc.) that aren't mentioned here. No indication of how background mixing ratios of CO are derived. Was this also with the statistical method?

Atmospheric increases based on the observations over a 1-year period are presented in the paper. No indication is given about the robustness of these trends or their uncertainty. I agree that communicating how the atmospheric composition changed in background air at this site is a useful thing to do, but some indication of the potential accuracy of derived trends is needed. A close inspection of Figure 2, for example,

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suggests that the application of the statistical filter to isolate the background data has the potential to bias the derived trends (e.g., would a slightly different statistical filtering give substantially different trends for some compounds?). Furthermore, defining a metric related to annual changes is difficult to derive from only one year of data. A simple sinusoid has a non-zero slope and a correlation coefficient of 0.5. Consider quoting the May-to-May change for all gases at least, though a close look at Figure 2 suggests only a very small amount of data are available during May 2011. In the text the authors rightly suggest that these rates are quite uncertain, but this caveat needs to be expressed throughout (abstract in particular) the paper.

The comparison of trends among different studies (Table 3) is also problematic, as only one of the other results included in the table are for concurrent periods. What are we to take away from this comparison? That the measurements are done well or that growth rates at Shiangdianzi are anomalous? I'd expect these growth rates would change over time, but this isn't considered. If kept, the trend comparisons would benefit from consideration of some more recent data (including updates to data originally published in Greally et al and other studies) that appear in Chapter 1 of WMO(2011, Table 1-15) Ozone Assessment report.

Clarity issues: English usage is OK, but needs some improvement in a few places.

Abstract lines 15-16, "small local contributions..."? Without a definition of the term “loadings” as it is used in the abstract, it seems likely to be misinterpreted.

Minor issues: I'm a bit surprised that PFCs are mentioned as being substitutes for CFCs and HCFCs in the introduction. I hadn't consider them as such, especially for all three PFCs considered here despite the text in the introduction (Mulhe et al., do indicate C3F8 as being a refrigerant and perhaps some enhanced use a few years ago so maybe the comment is true for this gas only). Certainly they haven't taken the place of any significant fraction of CFC and HCFC past usage.

p. 11155, lines 23-24, pollution events for PFC-318 occur fairly often and range from

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2-3 times background levels, seemingly inconsistent with the text. Some clarification would be useful.

Section 3.2, para starting on line 14. It would be useful to indicate that these larger differences are expected as they are typically observed for all long-lived gases emitted in substantial quantities from human activities.

Section 3.2, para starting on line 19 and 23. These paragraphs might be reconsidered. The approach is not communicated clearly especially in the latter para... also, given the extrapolation required from 2004 data (with a trend apparently determined in that study from 1 year of data) it would seem the only appropriate conclusion is that mixing ratios of HFC-32 have increased since then. Updated HFC-32 data are included in the WMO (2011) report, Chapter 1.

Loadings section 3.3. Indicate if this analysis was performed on the full set of measurements, not just background or polluted results. The authors also might reconsider the definition of loadings provided (line 26, p. 11157), given that the unit used is ppt hr and not ppt/hr.

p. 1158, line 19, Palmer misspelled.

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

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