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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimates of anthropogenic halocarbon emissions based on its measured ratios relative to CO in the Pearl River Delta" *by* M. Shao et al.

Anonymous Referee #2

Received and published: 28 March 2011

General comments: This is a very useful and important paper. It provides an observationally based estimate of halocarbon emissions from the Pearl River Delta region of China, an issue that has attracted significant interest. However, there are shortcomings of the paper that must be corrected before publication in ACP. Relatively major concerns include the following:

1. The method of estimating emissions that is developed Eq. (1) and applied in Section 3.2 implicitly assumes that the emissions of the halocarbons are correlated with the emissions of CO. The relatively low correlation coefficients presented in Table 4 indicate that the halocarbons and CO are not necessarily emitted from the same sources,

or even uniformly enhanced proportional to their emissions in polluted air masses. Nevertheless, the method can still give a useful (although uncertain) estimate for the halocarbon emissions if the line fit to the data pass through a point that represents the global backgrounds of both CO and the halocarbon. Figure 2 suggests that this situation holds for many of the halocarbons, but in other cases the intercept of the CO background (approximately 100 ppbv) is at a halocarbon concentration that is elevated above its background (e.g. HCFC 22, CHCl3, CH2Cl2, CCl2=CCl2 and even methyl chloroform). This behavior suggests that halocarbon concentrations can be elevated by local emissions even when CO is close to the global background (i.e. not elevated by local emissions). In such cases a more accurate emission estimate would be obtained from a linear fit that is forced to pass through the point that represents the global backgrounds of both CO and the halocarbon. The authors must clearly consider their estimates of the background concentrations, whether the lines fit to the data are consistent with the background estimates, and clearly discuss the implications for the uncertainty of their emissions estimates.

2. The English language use in this paper must be improved throughout the paper. There are many minor misusages of English, and in some places the content of the paper is unclear. I suggest copy editing by a native English speaker.

Less significant concerns include:

Specific comments:

1. I do not understand the significance of the rectangle inset in Fig. 1. It is never discussed, so I suggest that it be removed.

2. The authors employ an orthogonal distance (ODR) linear regression. However, the slope, intercept and their confidence limits derived from such a regression is strongly dependent upon the weighting selected for each of the variables. This weighting should be clearly discussed.

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3. The notation in Eq. (2) is inconsistent with that in Eq. (1). CO2 under the square root should be ECO2. Also the sentence following the equation has the definitions of the two uncertainties switched.

4. Line 25 and elsewhere: Inventory numbers should be reported with a number of significant figures consistent with their uncertainty; e.g. 5900 Gg (in 2000), 8700 Gg (in 2006). A similar comment applies to the estimated emission ratios; e.g. on pg 2965 the X/CO ratios, should be reported as 0.71 ± 0.13, 0.12 ± 0.02, and 0.44 ± 0.07 pptv ppbv-1 for DCM, PCE and TCE, respectively.

5. In lines 25-27 on pg. 2958, the specification of the confidence limits of the CO emissions are poorly described. The term $\pm185\%$ makes no sense to me, as that would include large negative emissions, which are not physically reasonable. It would be better to indicate the uncertainty by a factor, e.g. uncertain within a factor of 1.85, if that is indeed the uncertainty that the authors wish to convey. This same approach should be taken for the other uncertainty estimates given in this section.

6. Line 11 on pg. 2960: I think that the relevant statistical results are given in both Table 1 and Table 2.

7. Lines 16-20 on pg. 2960: The magnitudes of relative standard deviations (RSD) are a function of loss processes as well as emissions. Very long-lived species have very small RSDs, other factors being equal (see for example, Jobson et al., Trace gas mixing ratio variability versus lifetime in the troposphere and stratosphere: Observations, Journal of Geophysical Research, 104 (D13), 16091-16113, 1999.) The sentence on these lines is incorrect as written.

8. Line 29 on pg. 2960: In the Sentence "Moreover, the median emission values of . . ." I think the authors mean ".. median measured concentrations of . . ."

9. Line 2 on pg. 2961: In the Sentence ". . . suggesting long-term sources of emissions for . . ." is not correct. The greater concentrations certainly suggest emission sources,

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but do not indicate that they are “long-term”.

10. Line 20 on pg. 2961: The phrase “statistically positive relationships” is not clear. Do the authors mean “statistically significant”? If so, this statement should be supported by statistical significance tests, which are not given. Do the authors mean “positive correlations”, i.e. the halocarbon increases with increasing CO concentration? The wording needs to be clarified.

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