

Interactive comment on “Southern hemispheric halon trends and global halon emissions, 1978–2011” by M. J. Newland et al.

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I read your manuscript with interest. While most parts of the manuscript were clear to me, I found a few parts where I was confused and I am hoping that you could clarify these parts. They are mainly related to the measurements.

It isn't very clear to me if the entire CGAA sample record was re-analyzed or if some of the results were taken from Fraser et al., 1999. It would be good if you could clarify this.

Part of the confusion is the unclear distinction between 'measurement dates' and 'samples dates'. If you could be more specific about that throughout this paragraph, that might help a lot. E.g. l. 5 p. 29293, 'Up until 2006, the ...'. Does this mean samples

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collected up to 2006, or does this mean that analysis date 2006? Here it suggests that this year refers to when measurements were made (of the flasks), but that leaves unclear if this is coincident with the sample collections. It becomes more confusing later (l. 15) where then it is mentioned the 'to reanalyse selected samples from before 2006'. Do you mean 'selected samples collected before 2006' or, 'selected samples, which had been analysed before 2006'?

Is the assumption correct that the switch to the updated GC-MS and the switch to the Agilent GasPro column occurred at the same time? Would this mean that the individual effects of these two rather major changes could not be investigated (as the changes were simultaneous)?

'During the 1978–2005 period, several different alumina-PLOT columns were used ... This suggests that you already started measuring these samples in 1978, but this is hard to believe. Comparison of the 'seven samples'. Is it correct, that these selected samples were analyzed on a Chrompack PLOT column, then on an Agilent PLOT column, and later on an Agilent GasPro column (with the Autospec MS), so three times analyzed? If so, which two sets do we exactly see in the suppl figs? In that sense, the caption to the figs in the suppl. material could be a bit more detailed. You mention 'seven' samples, but it looks like more than seven samples on these plots.

l. 27: suggest to replace '..H-2402 data which was ...' by '... H-2402 samples, which were ...' if this is what you'd like to say. Are the mean precisions 1 or 2 sigma, and how were they determined (through repeated Cape Grim sample analysis or perhaps differently)?

Calibrations: Please state the accuracies for the NOAA-2006 scales for H-1301 and H-1211. Please do the same for H-1202 and H-2402 and explain what defines the UEA scale, how the primary material was prepared etc, how many primary standards etc, or refer to potential publications that may detail this. Is it correct that for each compound, so far (since Fraser et al., 1999) there has been one single UEA calibration scale, and

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that the UEA calibration scale was never revised since (except that for H-1301 and H-1211 the UEA scale is not used anymore in this paper)? Is the description in Fraser et al., 1999 still accurate?

My main concern is that I cannot reproduce your scale conversion from UEA to NOAA-2006, when I compare the results in Fraser et al., 99 (UEA scale) to the present ones (NOAA-2006), which presumably the fitted CGAA results in Table S1 are reported on (It would help to mention in the caption that these results are on NOAA-2006 for H-1301 and H-1211, and on UEA scale for the other two compounds). Your statement on p. 29294 l. 10 ff is confusing. 'The ratios of the UEA volumetric scale, used in previous work, to the NOAA-2006 gravimetric scale is 1.13 for H-1211? Is it not the other way around? As is written now, I would think that mole fraction reported on UEA are higher than those reported on NOAA-2006. That is clearly not the case when comparing the data sets. It may be less confusing if rephrased to something like 'the conversion between the two scales is such that mole fractions, previously reported on UEA must be multiplied by 1.13 to convert to NOAA-2006. But then comes the next confusion, e.g. take Fraser et al., 1996 values (2.0 ppt on UEA), and try to convert to the (fitted) new 1996 values of 2.45, that is clearly not a factor of 1.13 (or the reverse of it). This needs an explanation. The conversion also doesn't seem to be correct for H-1301 although there it is a bit more difficult to compare the measured flask values and the yearly means from the fit. For H-2402, the results in Table S1 (present work) and Table 1 (Fraser et al.) are both on the UEA scale, and the measurements for samples younger than 1998, which are not affected by the small nonlinearity issue, should then be identical within the measurement precisions of the two data sets. Is this correct? If not, could you comment on agreement/disagreement. Same for H-1202.

Is it possible that not all samples shown in Fraser et al., Table 1 were re-analyzed. It looks like the current data set before about 1990 looks much more sparse, particularly for H-1301.

While I recognize the very valuable listing of yearly Cape Grim mixing ratios from fitted

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data (Table S1), it is really a pity that you do not publish your measurement results of the CGAA, and I'd like to encourage you to do so in an additional table (given that the samples were really re-analyzed, which is not becoming clear to me when reading the manuscript). Such a table would allow for a comparison with (yet unpublished) CGAA halon measurements by other groups.

Are the numbers reported in para 4 those listed in Table S2? If so, this should be mentioned. Some of these numbers (e.g. p. 29300 I.4 , 3.0 Gg), don't seem to match those in Tables S2. Also, the HTOC cumulative emissions for H-1301 1979–2009 seem to be 97 (not 99) Gg if you define 1979–2009 by subtracting the cumulative emission reported for the year 1979 from the year 2009.

Reference Douglass et al., 2008 seems to be missing. The citation Montzka and Reimann (2011) is not consistent, see e.g Table 2 caption.

One last comment that might help improve the figures of the supplementary materials. Presuming that the purpose of the figs S1 to S4 is to demonstrate the degree of agreement/disagreement between various measurement techniques: Then it would be much more illustrative to not show GasPro vs AI-Plot but to display the difference GasPro–AI-Plot on the y-axis while keeping the AIPlot on the x-axis. This difference could be plotted in ppt or as percentage, or both. That would show much more details on potential variability. Also, it should be stated that the fits were forced through 0/0, if that is the case, and if the fitting routine has taken both delta-x and delta-y into account, or perhaps only delta-y.

I hope you can address some of these issues thereby clarifying some of the apparent confusions. Thank you.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 29289, 2012.

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