

Interactive comment on “Atmospheric trends of the halon gases from polar firn air” by C. E. Reeves et al.

Anonymous Referee #1

Received and published: 8 April 2005

General:

This work represents a useful addition to our understanding of past atmospheric composition, especially regarding halons, which are potent ozone-depleting gases.

It extends previous studies because lower mixing ratios were found in older air. This reduces the possibility of any significant natural contribution to halon amounts observed in today's atmosphere.

The authors also use the results from the different hemispheres to provide constraints on emission distributions over time. Emphasis on inferred emissions tends to diminish the paper in some instances.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

Specific:

I find the description of methodology confusing in the abstract, on p. 940, lines 10-15, and p. 942, lines 7-15. Why the emphasis on emissions here, which unnecessarily confuses and convolutes the issue? Wouldn't it be clearer and more accurate to state in each of these places specifically that atmospheric trends were derived fundamentally from archive air measurements in the SH, and these results were extended in time and space (across the globe and to earlier years) with a 2-D model calculation. These derived trends were used as input to a forward firm model, and the calculated depth profiles were compared to those measured. Along a similar vein, the fact that a measured depth profile can be reproduced with a calculated history and firm model shows that the history is consistent with the firm measurements. To suggest that the consistency "shows that the trends in global emissions are consistent with these firm measurements" is much more of a stretch, especially for H1202 (p. 945, lines 1-3).

On potential model transport scheme errors and uncertainties: Model tests on CFC-11, -12, or methyl chloroform don't test the validity of model-calculated photolysis rates at longer wavelengths, which are relevant to H1202. The model-inferred measurement differences in the NH are assumed to be entirely the result of emissions distributions though significant uncertainties in H1202 lifetime likely exist. How sensitive are your conclusions regarding emissions distributions of this halon, in particular, to the calculated photolysis rate of this gas? Would an increased photolysis rate of 20% negate your conclusion on the emission distribution of H1202, or is your conclusion robust? I think the authors would agree that the lifetime of H1202 has considerably more uncertainty than the other gases, hence some investigation into its influence on their conclusions seems warranted.

The authors should consider showing the updated atmospheric trends used in the forward firm model (else change the title to mention emissions), even if they are similar to those shown by Fraser et al (1999). How else are to we evaluate the extent to which the "firm measurements provide constraints on the atmospheric concentrations

Interactive
Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

in both hemispheres” (argued as being made possible by this work in the abstract). Perhaps this would allow you to open the discussion and help the reader understand, for example, why the measured mixing ratio in the deepest Devon Island sample has a mixing ratio <0.001 despite a CFC-12 mean age that encompasses years (1930-1977) in which H1202 emissions were non-zero

Technical items

Text indicating “concentrations are zero” would be more accurately and appropriately stated (at least initially) as “mixing ratios were below detection (<0.001 ppt)”. I would think it useful to mention that this represents ~2% of the modern day atmospheric abundance of H1202 and much less for the other halons.

p. 941, lines 13-21. Calibration scales change over time, is there a time stamp on the intercomparisons that could be mentioned here (or AGAGE scale date)? Or is this information based upon the same intercalibration activities that were discussed in Fraser et al., 1999 (if so, reference)?

p. 944, line 15. Results from convective zones not included in plots—OK, but please indicate the depths excluded at the different sites (in text or figures caption).

Section 3.3 and Figure 6 caption would be better labeled as “Total organic bromine from halons” and “Historical trends of organic bromine from halons at (a)”

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 937, 2005.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)