



Interactive
Comment

Interactive comment on “Global stratospheric fluorine inventories for 2004–2009 from Atmospheric Chemistry Experiment Fourier Transform Spectrometer (ACE-FTS) measurements” by A. T. Brown et al.

Anonymous Referee #1

Received and published: 16 August 2013

General comments

This manuscript reports about the recent global evolution over six consecutive years (2004–2009) of fluorine in the Earth’s atmosphere. The authors combine observations from the ACE-FTS satellite instrument with simulations from the 3D CTM SLIMCAT model, for a significant number of source and reservoir species of the fluorine budget. They carefully compile and provide the partitioning of the 18 F-bearing gases with respect to altitude, in four latitude regions and further establish total stratospheric trends,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



in terms of mixing ratio and GWP-weighted annual changes. This is an important contribution within the framework of the Montreal and Kyoto international treaties and the subject is clearly in the scope of ACP.

There are however a few changes that I would recommend before publication in ACP. This study involves model results (for 10 species out of 18) and combines them with observations to derive a global fluorine inventory. If the quality of the ACE-FTS measurements is evaluated towards some balloon or satellite experiments, we have no idea of the reliability of the model results included here. Is it only stated that “it gives a realistic stratospheric circulation”. I think the reader would know more about the reliability of the model simulations: are they in agreement with e.g. balloon, aircraft observations, for specific intercomparison exercises involving the present targets? And what about the ability of the model to reproduce observed trends? Moreover, I would recommend discriminating between the observed and modeled species in the Tables A1-A4 (e.g. by adding a title line with ACE-FTS for the first eight columns, SLIMCAT for the next 10 entries); the authors should also quote the respective contributions of the observations and model inputs to important derived numbers, such as the mean stratospheric volume mixing ratios (in Table 4) or trends. In other words, the reader should be able to evaluate the impact of the model results on the conclusions drawn.

Finally, the reference to section numbering is completely wrong throughout the paper. This is particularly annoying and disappointing. The authors and/or the journal should definitely have revised with care the proofs before online publication.

Specific comments

- The title should mention the non-negligible model contribution to this study; otherwise it's misleading. Also but of less importance, I don't see why “inventories” is in the plural form. I would suggest to change the title to “Global stratospheric fluorine inventory for 2004–2009 from Atmospheric Chemistry Experiment Fourier Transform Spectrometer (ACE-FTS) measurements and SLIMCAT model simulations”

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- Page 16888, line 15: I would refer to Kohlhepp et al. (ACP, 12, 2012)
- Page 16888, line 24: “long-term changes” is an overstatement for a six year time period! Replace “long-term” e.g. with “recent”. Btw, it is unfortunate that more recent ACE-FTS results are not included in this study
- Page 16890, line 14: what is the origin of the CF4 lifetime value mentioned here (>2300 yr)? A reference is needed. In WMO-2011, it's > 50000 yr.
- Page 16894, line 14: the COF2 molecule is also a target of the ground-based FTIR network and evidences of its recent upward (but weak) trend are also presented in Duchatelet et al. (ACP, 9, 2009)
- Page 16896, section 5: the latitude bands have been selected such as to extend up to 70°N and 70°S. Hence the question of the impact of winter vortices on the ACE-FTS measurements and their significance in peculiar dynamical conditions is open. The authors have used the MAD statistic to sort out the data. We could wonder if derived meteorological parameters would not have been more appropriate for the data selection. Could the authors comment on that?
- Page 16898, line 21: the authors should refer to relevant articles dealing with the age-of-air
- Page 16904, line 15: this last sentence seems incomplete
- Table 1: the authors could consider adding a column with the lifetime of the various species, to made available useful information about the source gases at a glance
- Table 4: could the authors comment on the larger values determined for the slope of total fluorine profile, e.g. in 2007 for both extra-tropical subsets? Do they have any significance? And why are the slopes significantly lower in the tropical regions?

Technical corrections

- Page 16889, line 7: “Section 3” instead of “Section 2” and so on throughout the

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

manuscript. . .

- Page 16891, line 1: “This study found an agreement” (remove “that”)
- Page 16891, line 15: I think that the Mark IV instrument is abbreviated MkIV instead of Mk-IV
- Table 2: replace CHF₂Cl by CHClF₂
- Table 6: reword “contribution which contribute”

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16885, 2013.

ACPD

13, C5916–C5919, 2013

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C5919

