

## ***Interactive comment on “Long-lived halocarbon trends and budgets from atmospheric chemistry modelling constrained with measurements in polar firn” by P. Martinerie et al.***

### **Anonymous Referee #1**

Received and published: 8 March 2009

Review of ACPD-2008-0632, Long-lived halocarbon trends and budgets from atmospheric chemistry modelling constrained with measurements in polar firn, by Martinerie et al.

General Comment: This is a comprehensive, well thought-out, carefully constructed, and well written manuscript on the subject of obtaining 20th century atmospheric records of halogenated gases from polar firn air. It is a particularly useful endeavor in that these gases were not measured in the atmosphere until at least the 1970s, which is several decades after the onset of emissions for most of them. At least two of the gases have questions about the possibility of their having natural sources. The

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authors work to reduce bias in their findings by making use of several data sets for both atmospheric and firn air measurements, identifying biases among them, and incorporating the most up-to-date approaches in developing emission estimates and modeling atmospheric records. It is, I believe, the first effort to couple a 2D atmospheric model with models for firn air profiles, thus allowing evaluation of lifetime evolution for gases destroyed mostly in the stratosphere.

Specific Comments: My comments are mostly minor in that I do not feel the authors have made any major errors in model comprehension or data treatment.

p. 994, lines 4-5 (Introduction). The authors note here the assumption that these gases are totally anthropogenic. While this assumption may be correct, I would feel more comfortable stating “predominantly, if not totally” anthropogenic. This suggestion is mainly to cover CCl<sub>4</sub> and SF<sub>6</sub>.

p. 994, line 18 (Intro & elsewhere). The authors often refer to a “recent” slowdown in SF<sub>6</sub> emissions. The word “recent” should be clarified. Production records and emission estimates suggest that around 1997 there was a sharp reduction in SF<sub>6</sub>. Since that time, however, the growth rate, and hence deduced emissions, of SF<sub>6</sub> has been reasonably constant (e.g., <http://www.esrl.noaa.gov/gmd/hats/insitu/cats/conc/brwsf6.html>).

p. 995, lines 3-4 (Intro). The NOAA Climate Monitoring and Diagnostics Laboratory has been subsumed into NOAA’s Earth System Research Laboratory since 2006 (<http://www.esrl.noaa.gov/gmd/>). The authors should note this at least here in the introduction, so that those seeking links to data can be properly directed.

p. 996, line 11 (Intro). Change “to discuss” to either “us to discuss” or “the discussion of”.

p. 999, lines 408 (& p. 1002, lines 15-26, p. 1003, lines 3-9). Though the authors overall have made good use of earlier firn-air studies, they may find additional value in

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the Battle et al. 1996 study, which provides N<sub>2</sub>O profiles through firm air at the South Pole.

p. 1010, line 21 (Section 6.6.2). I'm not sure I would characterize CFC-113 as a minor CFC. Indeed, in terms of contributing chlorine to the stratosphere, it is considered one of the major CFCs.

p. 1013, line 16 (Sec 6.2.3). The statement are not inconsistent with, though correct, understates the possibility that there could be a small natural source of CCl<sub>4</sub> and no observation has yet ruled that out. This possibility comes into play in later discussions and another comment.

p. 1013, line 20, (Sec 6.2.4) and p. 1014, lines 1-4. As above, note what recent is.

p. 1016, lines 16-22 (Sec 6.3). It's here that we should be reminded that the absence of evidence is not evidence of absence. The ocean sink cannot be simply discarded and maybe the less well constrained land sink should not be either. These additional sinks may be reconcilable (at least in part) if there is an additional source not accounted for in the anthropogenic emissions. Further, because any land sink would be concentrated in the NH and the oceanic sink appears to be evenly distributed over the ocean, which would imply a larger SH contribution, the two may offset one another, thus obviating the meridional gradient argument put forward by the authors. I don't think the authors should make a big deal of this in this paper, but they should toss a bone to the possibility that there is an additional small source of CCl<sub>4</sub> or that balancing partial lifetimes for this gas is a challenge.

p. 1020, change to translate to us to translate; or the translation of;

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 991, 2009.

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