

Interactive comment on “Estimating the climate significance of halogen-driven ozone loss in the tropical marine troposphere” by A. Saiz-Lopez et al.

Anonymous Referee #3

Received and published: 13 December 2011

Saiz-Lopez et al. present an interesting paper regarding an issue currently gaining attention in the literature, namely halogen-related tropospheric ozone loss. Whilst other global modelling studies, notably von Glasow et al. (2004) and Yang et al. (2005), have included effects of bromine chemistry on the tropospheric ozone column, the main advances of this manuscript are that it includes calculations of the radiative impact of halogen-induced changes in O₃ and also includes iodine chemistry. The paper is well structured and generally well-written and offers as far as I'm aware the first estimates of the radiative impact of such chemistry. I think the main weakness of the paper is that the halogen emission schemes used are premature. Further, the manuscript would benefit from uncertainty estimates of some of the calculations, and better comparisons

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



with similar work from other researchers. These points are expanded below.

1. Where is the evidence that sea-air emission of all VSL halocarbons depends on the actinic flux? Emissions of the most important VSL iodocarbons are likely to follow the opposite diurnal variation to that assumed since they will be photolysed within the surface water column. In the case of bromine species, model results indicate that bromoform emissions exhibit a minimum in summer and a maximum in early winter due to changes in the depth of the ocean mixed layer (Hense and Quack, 2008).

2. The paper states that the majority of iodine emissions are assumed to be in the form of I₂ (since previous studies shows that VSL iodocarbons are not sufficient to produce measured IO levels), yet the authors apparently have no information on the magnitude, spatial and temporal distribution of such emissions – the number used is simply an inferred flux of I₂ at one location (Cape Verde). In addition, if the main carrier of iodine was not I₂ (and with no field information on this than at present it does not appear possible to conclude either way), then the results/vertical variation of IO could be quite different.

3. Does the model include sea spray sources of BrO? There appears to be no description of this. Since sea spray is the dominant source of BrO in the MBL, it is surprising that the authors can reproduce O₃ loss at Cape Verde without including a proper treatment. This in turn brings into question the results in the upper troposphere.

4. The authors should make clear whether the modeled ozone loss includes the indirect effect of decreased NO_x levels (which should be more pronounced near coastlines downwind of major NO_x sources).

5. Pg 32012. It would be worthwhile to compare the individual effects of (a) the VSL bromocarbons, (b) the VSL iodocarbons and (c) the sea spray bromine sources (if included) on column O₃, since each of these sources may change in different ways with environmental change.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

6. Pg 32012. If VSL bromocarbons dominate the column O₃ loss then the results of this study should presumably be similar to those of von Glasow et al. (2004) and Yang et al. (2005). There is little discussion of these papers other than in the introduction, but direct comparisons of the results regarding bromine-induced ozone change should be made.

7. Finally – both the column ozone loss and the change in radiative flux need uncertainty estimates.

Minor issues/typos Pg 32010. The following sentence needs breaking up to make it clearer: “For two longer lived bromocarbons, CH₃Br and CH₂Br₂ – total lifetime (considering not only photochemical loss in the atmosphere but also uptake to oceans and soil) of 0.8 yr and local lifetime of 123 days, respectively (WMO, 2011) –, that additionally contribute to the tropospheric halogen burden, the average modelled profiles throughout the tropical troposphere are also comparable to the observations (Fig. 1).”

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32003, 2011.

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