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Comment

## ***Interactive comment on “Measurement and modelling of reactive halogen species over the tropical Atlantic Ocean” by A. S. Mahajan et al.***

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Mahajan et al. present interesting measurements and model results of tropospheric halogen chemistry at Cape Verde. I recommend publication of the manuscript in ACP after making some changes as described below.

### **1 Major comments**

- I only have one point of scientific criticism: I don't find the explanation very convincing that the missing source of iodine is  $I_2$  resulting from ozone deposition. Can you please address my concerns regarding your hypothesis:

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- How can you exclude that iodocarbons which were not measured contribute significantly to the iodine source? As far as I understand, several individual iodocarbons were measured but not the sum of organic iodine.
- Ozone deposition does not require sun light, thus I cannot see why it should show a strong diurnal cycle.
- You suggest photochemical production of  $I_2$ . However, the photolysis of  $I_2$  is very fast. Thus I expect photochemical destruction during the day rather than photochemical production.

## 2 Minor comments

- Title and first line of abstract: I suggest to replace “reactive halogen species” by “reactive tropospheric halogen species”. Otherwise, this might also be a paper about stratospheric halogen chemistry.
- According to the IUPAC Recommendations (Schwartz & Warneck “Units for use in atmospheric chemistry”, Pure & Appl. Chem., 67(8/9), 1377-1406, 1995), the usage of “ppb” and “ppt” is discouraged for several reasons. Instead, “nmol/mol” and “pmol/mol” should be used for gas-phase mole fractions. I suggest to replace the obsolete units.
- Abstract and elsewhere in the text: The phrase “total ozone depletion” usually refers to polar ozone depletion events. I think what you are referring to here could be better described as the “sum of all ozone sinks”.
- Page 24284, line 22: Change “IO was first detected at Mace Head” to “Atmospheric IO was first detected at Mace Head”.
- Page 24286: Out of curiosity, why do you use  $NO_2$  spectra from two different references for retrieving IO and BrO?

- The physical properties “mixing ratio” and “concentration” are sometimes used as if they were identical. This is not the case! (for details, see <http://www.mpch-mainz.mpg.de/~sander/res/vol1kg.pdf>) Please check all occurrences of the word “concentration” in the text and check if it should be “mixing ratio” instead.

- Page 24288: As a non-native speaker, I am not entirely sure what you mean by “top-hat variation”. Since you explain that it “is due to the removal of BrO via the reaction with HO<sub>2</sub>”, I assume that a diurnal variation with a top-hat shape automatically implies a local minimum at noon. Is that correct?

Regarding the model studies that you cite here: Although the results of Vogt et al. and Yang et al. show similar diurnal cycles of BrO, I think that von Glasow et al. 2002a (which you already cite elsewhere) were the first to explain this shape.

- It is difficult to fully understand the model study without knowing the reaction mechanism. I suggest to show the complete reaction mechanism with all rate coefficients in the electronic supplement.
- The observatory is at 25° W, thus local time is different from GMT. Could you indicate in the figures that use GMT where the local noon is?
- Page 24293: The sentence  
“If the observed levels of halogens are now included in the model (1.5 ppt IO and 2.5 ppt BrO)”  
suggests that IO is prescribed in the model run. Can you please clarify if this is the case? Or is the model producing mixing ratios around this value based on the emissions of iodine?
- Section 4.6: Several reactions of I<sub>2</sub>O<sub>3</sub>, I<sub>2</sub>O<sub>4</sub>, and I<sub>2</sub>O<sub>5</sub> are mentioned here briefly. Iodine chemistry is quite complex and not very well-established. Thus, again, it would be good to show the complete iodine reaction mechanism in the supplement.

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Comment

- Section 4.6: The discussion of the iodine oxide particle (IOP) formation is very interesting but I am missing one aspect of it: When you switch on IOP formation in the model (but leave the iodine emissions unchanged), how does this affect the concentration of IO and thus ozone? I think it would be very interesting to mention this.
- Page 24297: I suggest to change “The figure shows rather graphically...” to simply “The figure shows...”. How else can a figure show something, if not graphically?
- Page 24303, line 6: Add the Umlaut dots to the author “Hönninger”.
- Table 1: The rate coefficients for the reactions  $\text{DMS} + \text{XO}$  ( $\text{X}=\text{Br}, \text{I}$ ) have been measured by different groups with different results. Which rate coefficients have you used to obtain the data presented in Tab. 1?
- Figure 2: I think the slopes are quite meaningless at these low correlation coefficients. I suggest to remove their values from the caption and only show  $R^2$ .

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24281, 2009.

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