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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Iodine's impact on tropospheric oxidants: a global model study in GEOS-Chem" *by* T. Sherwen et al.

Anonymous Referee #2

Received and published: 5 November 2015

General comments:

The manuscript by Sherwen et al., discusses the implementation of tropospheric iodine chemistry into the GEOS-Chem global chemical transport model. It compares simulated iodine species and ozone (O_3) with measurements from aircrafts, ground monitoring stations and ozonesondes. The modelled distribution of iodinated compounds and the impact of iodine on O_3 and OH is also presented. Several sensitivity studies, including the inclusion of inorganic iodine emissions, heterogeneous loss and cycling, photolysis rates and ocean surface iodide, are also presented in this manuscript. This paper shows that the role of halogen needs to be included in climate and air quality models.

The paper is well-written and scientifically relevant. I recommend publication to ACP, after addressing the specific and technical comments listed below.

Specific comments:

This is a long manuscript and provides a lot of information. I would recommend to re-write some parts of this manuscript with clear organization. However, saying this - there is potentially some really good useful science here, it is just lost beneath the overload of figures and tables.

Starting with the introduction section, 5 pages. It needs to be shortened. For example, the first paragraph could be a general explanation of the chemistry of the troposphere, including the main oxidants in the troposphere, OH and O_3, and NO-NO_2-O_3 systems in the presence of organic compounds. The second paragraph, could start with the halogen chemistry and how it affects the ozone concentration.

Page 20962, line 28: Moreover, the recent sea-to-air flux climatology of Ziska et al. (2013) could be also cited in the global organic halogen emissions part.

I also agree with reviewer R. Sander that the model runs need to use concise names. Then section 2 and 6 would be clarified.

Section 2, page 20966, lines 15-16: The model is run for "two years, (2004-2006)" (this is three years!) and use "the final year 2005"? Something seems wrong with this part.

I understand that in all the analysis, the model resolution is 2° x 2.5° and only the sensitivity studies are run with the model resolution at 4° x 5°. I suggest to explain both model resolutions, 2° x 2.5° and 4° x 5°, in section 2.

In section 2.1 the parametrization for the inorganic iodine compounds from Carpenter et al. (2013) is briefly described. I suggest to briefly describe how these monthly emissions of Ordóñez et al. (2012) are calculated. For example, in the abstract of Ordóñez et al. (2012) "Ocean emissions of (...) parametrized by a biogenic chlorophyll-a (chl-a) dependent source in the tropical oceans (...)"

In the last paragraph of section 3.1 is discussed the model overestimation of I_2 concentrations. However, the parametrization for the inorganic iodine (I_2, HOI) from Carpenter et al 2013 is not mentioned in this section. Could be any limitation in this parametrization that also contribute to the overestimation of I_2? This parametrization depends on 1/wind speed. What happen at low wind speeds? I also agree with reviewer R. Sander that the emission fields need to be included as a supplement to this article.

Table 7 gives a lot of information, however only a few results are discussed in section 5.2. In addition, I couldn't find the global tropospheric O_X loss of "184 Tg yr-1 from bromine chemistry" in Table 7. I suggest to rewrite this section in a clear way and thinking that the reader needs to find easily the information from Table 7 in Section 5.2.

Boxplots in the manuscript are confusing. For example lets take a look at Figure 4. From first glance its appears that the model and observations data are made at different latitudes or times stamps, and I assume this is not the case. The same for Figure 5 with the altitude. I suggest to plot both model and observations at the same time and latitude, instead of one next to the other, or explain in more detail what these boxplots show us.

I suggest to give more information about the axis of each figure. Figures 5 and 11 miss the names of the species "IO" and "O_3" in the x-axis. In addition, I suggest to use the same structure in the axis. For example in the axis of Fig. 4 " IO concentration (pptv)" and in the axis of Fig. 10 " O_3 / ppbv".

Technical corrections:

Page 20960, second paragraph: I suggest to add after "to produce two OH radicals" that this process is dominated by the tropics.

Page 20966, line 3: Are there a lot of species that have linearized chemistry in the stratosphere as an upper BC for the troposphere? If there are only a few I suggest to specify these species.

Page 20966, line 19: "spun-up" Change to: spin-up

Page 20967, Section 2.2: The last sentence in the first paragraph is repeated in the second paragraph.

Pages 20969-20970: Surface mixing rations of IO, IO, OIO, HI, IONO and $IONO_2$ from Figures 1 and 2 are not discussed in the document. It might be useful some discussion in sections 4.1 and 4.2.

Page 20970, lines 13-14: "Concentrations of CH_2ICl appear to be better simulated (Fig. 3)". We could say that CH_2ICl is better simulated in the MBL. However, there is a lack of observational data above the MBL. A more detailed discussion is lacking here.

Page 20970, line 22 : "over estimate" Change to: overestimate

Page 20971, line 7: "over estimate". Change to: overestimate

Page 20974, line 8: "tropspheric" Change to: tropospheric

Page 20976, line 23: the word "O_3" needs to be added after "Global tropospheric burdens of"

Page 20977, line 7: "fractional diurnal fractional". Something seems wrong with this sentence.

Page 20980, first paragraph: The "I2O_x exp. X-sections" simulation is not defined in the manuscript.

Figure 11: The pressure needs to be turned in these plots. 1000 hPa should be in the surface and not in the top of the atmosphere.

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