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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of chlorine in tropospheric chemistry" *by* Xuan Wang et al.

Anonymous Referee #2

Received and published: 27 November 2018

This paper by Wang et al. presents an extensive modelling study of chlorine chemistry using GEOS-Chem. The updates to the GEOS-Chem chemical mechanism are much needed. I think the paper is suitable for ACP, after the authors have addressed the following points.

MAJOR COMMENTS

As a general comment, I think it would be useful to have a more detailed discussion of the difference in the results between the version of GEOS-Chem with the previous mechanism and this new version. As it stands, the reader is mostly referred to previous papers. Figure 9 provides some of this information but it is presented as a sensitivity study, so it is not really a comparison of the old and new mechanisms.

Another general point is that the choice of datasets to compare with the model seems somewhat arbitrary. I understand it is not possibile to use all the available datasets, but

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you have to explain why you selected certain datasets and not others for this work.

On page 8: you say that the BrCl concentration in this work is lower than in previous modelling studies and you attribute this to a change in the chemical mechanism. Please add a note here that this point is further explained in section 5.2. Also note that BrCl measurements are discussed in the papers by Lawler et al. (see below). You may want to add that to the discussion in section 4.1.

On page 10 there is a brief discussion about the results by Lawler et al. (2010). First of all, it is either the 2009 or 2011 paper, please check which one you are referring to or add both. Both papers (2009 and 2011) reported high concentrations of HOCl and Cl2 when air was coming from continental Europe, not from Northern Africa as stated here. Please correct your statements. Those papers propose that high HOCl and Cl2 may be caused by aerosol acidification during long range transport and/or slower conversion of HOCl to Cl2 in the aqueous phase. These points are also investigated in Sommariva and von Glasow (2012), which you may want to take into consideration. Is the high HOCl and Cl2 still unexplained in GEOS-Chem if you take into account the findings of these papers?

On page 11: note that the results by Roberts et al. (2008) indicate that the reaction of ClNO2 + Cl- -> Cl2 is relevant only on very acidic aerosol. It may be that aerosol pH during the WINTER campaign was not low enough, which may explain why removing the reaction improves the agreement with the model. Can you please comment? Also, I think the reference is wrong in the bibliography.

MINOR COMMENTS

Page 3: the title of 2.1 should be "GEOS"

Page 7, line 30: add space in "HCl +OH"

Page 12: is the lower Cl* in this work driven by lower BrCl, as mentioned on page 8?

Table 5: there are more observations of ClNO2 available than those listed, especially

in Europe and Asia. Either expand the table or explain why those were chosen. Also, it is not obvious in which order they are listed (year, season, region, concentration?).

Please tidy the references list, there are many mistakes.

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