

Interactive comment on “Polar boundary layer bromine explosion and ozone depletion events in the chemistry-climate model EMAC v2.52: Implementation and evaluation of AirSnow algorithm” by Stefanie Falk and Björn-Martin Sinnhuber

Anonymous Referee #2

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This paper presents a new implementation of the mechanism of halogen activation presented in Toyota et al., 2011 in the model EMAC (ECHAM/MESSy Atmospheric Chemistry). I am supportive of this work being eventually published in GMD as it is well within the scope of the journal and presents a new model tool that can be used by the community to understand halogen activation and its role in ozone depletion events (ODEs). However, there are a number of issues that need to be addressed before publication. I largely agree with the first reviewer's comments and have in addition

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some major and minor comments, detailed below, that should be addressed prior to publication.

Major comments:

1. How is bromine recycling on aerosol treated? Is this important to sustain halogen activation and does it contribute to ozone depletion events?
2. The authors should look deeper into the literature as to how the understanding of halogen chemistry in the Arctic & Antarctic has developed over time. Papers such as Barrie et al., 1988 and Abbatt et al., 2012 should not be omitted from the reference list. In addition, Simpson et al., 2007 provides an excellent overview of how our understanding of halogen chemistry and ODEs has developed.
3. A clearer discussion of how snow contributes to halogen activation is needed, as discussed by Pratt et al., 2013 and Thomas et al., 2011.
4. A list of the reactions that are included to describe the halogen cycle is needed either in the paper or in the supplement, including a short discussion of how heterogeneous reactions on aerosols are treated.
5. In general, I find the discussion of the results too short. Major features of the figures are not really described, which leaves the reader a bit lost as to what the model validation section means. For example, why is the surface ozone so low in the model compared to the measurement sites in Antarctica (Neumeyer and South Pole Stations)? If the model is so poor at predicting background ozone, does it make sense to evaluate the contribution of halogens to ozone depletion events in this region?
6. In the Antarctic, another source of bromine activation that has not been included here may be more important (from sea-salt aerosols formed from blowing snow, Yang et al., 2010). The authors should discuss more clearly the implications for not included this mechanism, which may be included in a future study.

Minor comments:

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1. Abstract – “Most likely, they are related to events of boundary layer enhancement of bromine.” This statement doesn’t accurately reflect our understanding of boundary layer ozone depletion events, suggest to take out “Most likely”.

2. Page 1 – Line 13: “Events of near-complete depletion of polar boundary layer ozone are observed frequently during spring-time over both hemispheres (Oltmans, 1981; Bottenheim et al., 1986, 2002, 2009)”. I expect to see Barrie et al. as a main reference in this reference list.

3. Page 5 - Line 28: This sentence should be combined with next paragraph to avoid having a one sentence paragraph.

References:

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