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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cyclone-Induced Surface Ozone and HDO Depletion in the Arctic" *by* Xiaoyi Zhao et al.

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To Referee #1:

Xiaoyi Zhao et al., focus on two cyclone-induced surface ozone depletion events (ODE) at Eureka, Canada and make connection between ODE and HDO depletions by using ground-based, satellite, and reanalysis datasets. They explain the formation of bromine-enriched SSA from blowing snow processes. In addition, they compare modelling results with their measurements and use FLEXPART to find the potential emission sources. In general, this study is relevant for ACPD/ACP and can help to improve our understanding of ODE process in the Arctic. Making a better connection

between SSA and ODE in the introduction could be useful.

Thank you to referee #1 for your helpful comments. We have revised the manuscript based on your suggestions. Please note the page and line numbers in our response are referring to the numbers in the "changes tracked" revised manuscript (but the referee's comments refer to numbers in the ACPD version).

Aerosol aloft is necessary, but not sufficient for BrO to distribute vertically (see Simpson et al 2017, ACP). The authors explained the ODE and Br reactions, but it can be interesting if they can add more information about Ozone depletion and the chlorine

Thanks for pointing out this relevant work. We include a reference to Simpson et al. (2017) to explain this issue (see p.11, lines 11-16). We agree that the aerosol layer aloft is necessary but not sufficient for bromine aloft. In short, the events (ozone depletion aloft) we reported in this work were due to a different dynamical process (cyclone) compared to the event observed by Simpson et al. (2017) (convection due to the opening of a large sea ice lead). Thus, to distinguish the origin of these different aloft events, multiple measurements (lidar, radar, sondes, etc.) are necessary.

Regarding information about ODEs and chlorine, unfortunately, we do not have any chlorine measurements during the two events studied. In the future, chlorine measurements (such as HOCl and ClO) should be included to facilitate the studies of ODE and mercury deposition.

Equation (2): Are ???? changes in δD and ???? changes in water vapour mixing ratio?

The symbols in the referee's review are not displayed properly. We guess the question

was: Are $d\delta$ changes in δD and dq changes in water vapour mixing ratio?

Yes. We have included this description in the paper (see p5, lines 2-3).

Page 8 line 9: What is the linear relationship between each pair of the three variables? Can you mention that?

As pointed out in the manuscript, TCO is not only governed by tropopause height, but also by other factors (e.g., chemical loss). However, in general, there is a simple inverse relation between those three variables. For example, when tropopause height reaches a maximum in summer, TCO is at its minimum value, while δD is at its maximum value. In the winter, as the tropopause height decreases, TCO reaches its maximum value, while δD is at its minimum value. This general comment has been included in the manuscript (see p.8 lines 2-5).

For fig. 1, showing the height in the vertical axis is better.

Fig. 1 has been modified as suggested.

In fig 1a: what are δD values for 3rd and 4th of March?

For 3 and 4 March 2007, the δD values were -529‰and -551‰. These numbers were included on p.8 line 14.

In Fig. 1a, there are no data points for the 3rd or 4th of March 2007. This is because the Brewer did not have any TCO measurements in early March, and so there are no paired data points (TCO, h, δD) for those two days. Thus in Fig. 1b, Brewer TCO was replaced with MERRA-2 TCO, to obtain paired data points for the 3rd and 4th of March

as displayed. This was explained in the manuscript (p.8 lines 11-15).

Explain fig 5 more: Is it for 2007? Can you add a plot for 2011 too?

Fig 5 was for the March 2007 event. We have added ozone profile plots for the 2011 event in the revised Fig. 5.

Also, why do the model and measurement data disagree in fig 4?

The UKCA model did capture the 2007 shallow surface ODE, as also shown in Fig. 5. However, the aloft ozone depletion layer was not simulated by the model. This could arise from few factors, such as dry/wet deposition velocity of aerosol or model vertical dynamic transport. This disagreement between the model and measurements indicates that improvement are needed for the UKCA model.

On the other hand, pTOMCAT performed worse than UKCA. This is also true for the 2011 event, and is probably due to the models' dynamics. As discussed in Sections 2.2.3 and 3.1.1, UKCA has a more advanced boundary layer dynamics compared to pTOMCAT. Future comparisons between these models are needed to evaluate and improve their performance, but this is beyond the scope of this study.

Have you measured snow salinity to report here?

No, we don't have measurements of snow salinity. The snow salinity used in this work for both UKCA and pTOMCAT was the latest surface snow salinity data obtained for the Weddell Sea. The snow salinity used in the model was described in the manuscript (p.7, lines 19-23).

Page 2 line 29: ice-covered

Corrected.

Page 13 line 10: extremely

Corrected.