General comments:

Huang and co-authors used their GOES-Chem CTM model to examine the influence of blowing snow SSA as a source of bromine on springtime bromine activation and ozone depletion in the Arctic. Comparing to previous modelling work, this work has some significant progresses, e.g. in parameterising bromine flux from sea ice surface and reproducing enhanced BrO events in spring. Moreover, they pointed out the importance of multiyear sea ice as a source of bromine and recycling of deposited bromine to BrO seasonality. This is a decent modelling work which adds novel information to our understanding of polar bromine chemistry and certainly will benefit following on modelling simulations. The manuscript is well-written, and I only suggest a minor revision.

Specific comments:

Abstract (lines 34-36): I doubt your suggestion of inclusion of snowpack Br activation will definitely fix the model difficulty in reproducing local ODEs. I cannot see how you derive this conclusion, as in your manuscript (lines 366-374 and lines 518-520), you pointed out several potential factors that could cause model failure in reproducing local ODEs, which are (1) coarse resolution, (2) lack of chlorine chemistry, (3) difficulty in modelling boundary layer processes and (4) lack of snowpack emission. The model horizontal resolution used in this study is 2X2.5 degree, which is quite coarse. We know that model cannot capture any sub-grid scale events. If we assume a mean wind speed of 20 km per hour, then any transported event with a lifetime of around or less than ~10 hrs will be smoothed and cannot be reproduced by the model. In which case, it demands a high resolution model simulation. In addition, the poor model ability in reproducing shallow boundary layer, especially in polar regions (which is a common issue in most current models) is another killer. As the air exchange rate at the top of the boundary layer largely determines near surface ozone level. Therefore, if the model cannot reproduce well the dynamical process of the boundary layer, it will not reproduce well near surface ozone. For these reasons, I recommend the authors to discuss model limitations, rather than pointing to other possibilities.

Line 67: ‘Four’ or ‘three classes’ ?

Line 333-334: A recent modelling work (by Rhodes et al., 2017) focusing on SSA simulation also showed a similar conclusion that when multiyear sea ice is considered as a source of SSA, modelled SSA has the lowest mean standard deviation across the Arctic sites comparing to other experiments.

Rhodes, R. H., Yang, X., Wolff, E. W., McConnell, J. R., and Frey, M. M.: Sea ice as a source of sea salt aerosol to Greenland ice cores: a model-based study, Atmos. Chem. Phys., 17, 9417-9433, https://doi.org/10.5194/acp-17-9417-2017, 2017.

Lines 526-628, you mentioned that ‘… in the shallow boundary layer (~50-250m) over the springtime Arctic direct snowpack halogen activation dominates Br release and is responsible for the most severe ODEs’, do you have any in situ data to support this statement? In my point of view, the snowpack mechanism (as a dominant source of bromine in ODEs) is still a hypothesis without direct in situ data to support. If ODEs are long-distance transported in association with synoptic systems, why cannot the observed events in the shallow boundary layer be transport-related?