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Comment on acp-2022-334

Anonymous Referee #2

Referee comment on "Ozone depletion events in the Arctic spring of 2019: a new modeling approach to bromine emissions" by Maximilian Herrmann et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-334-RC2, 2022

In this manuscript, the authors expand on an earlier study on modelling Arctic spring ozone depletion. The main change to the previous study is the change in bromine source at the surface, which now is no longer just from first year ice where it was assumed to be released from an unlimited reservoir, but is separated by FYI, MYI and snow on land and connected to a limited reservoir. The model results are compared to surface ozone and sonde measurements as well as satellite BrO columns, and the results are discussed on a station by station basis, focusing on the effects of different source regions.

The manuscript is well written and fits the scope of ACP. The new approach to bromine emissions from the surface is interesting, and the results are promising as agreement with observations is improved significantly. The proposed method is computationally inexpensive and could be implemented in other models as well which makes this study relevant for the community. I therefore suggest publication after minor revisions following my suggestions below.

Detailed Comments

- In the manuscript, the emphasis is on the distinction between limited and unlimited bromide reservoirs. However, the scenario “unlimited” also differs in that it only takes FYI into account as a source while the other scenarios also include emissions from MYI and snow on land. In my opinion, this mixing of two aspects, the limitation of the reservoirs and the inclusion of additional source types is unfortunate as it complicates interpretation of the results. Adding an additional model run with a limited reservoir for FYI and no other emissions would help to better separate effects.
- As also pointed out by the authors, a number of ad hoc decisions on values and free model parameters had to be made in the model, which probably cannot be avoided but carries the risk of optimisation of results towards the stations used for validation. This includes the values chosen for the reservoirs, the distance parametrisation for snow on

land, the replenishment time and also the ozone scaling in the model.

- The exclusion of blowing snow as a source of bromine is a limitation but in view of the good agreement between model and measurements could be seen as indication for a limited importance of blowing snow for bromine release. However, this could also be linked to the choice of validation stations.
- The comparison to satellite data is less clear than the one for the ozone observations. I would suggest adding a table with correlations and differences, similar as for the other comparisons.
- The description of the new TROPOMI BrO product is very brief and vague – is that the Sihler et al. algorithm applied to TROPOMI data, or was the algorithm and the thresholds used changed? If more changes were applied to the Sihler et al. retrieval, then this should be documented, for example in an appendix.
- Why was an O_4 cross-section at the unrealistically low temperature of 203 K used?
- Are the two Pukite terms for ozone?
- Is the uncertainty discussion given for individual TROPOMI pixels?
- Line 328 – I would call this a small rate of false positives but maybe this is a matter of definition
- In tables 3 and 4, I would suggest to use bold face to highlight the best values for each comparison instead of a certain model setup which can readily be identified by its position in the table.
- line 593 – not sure what the authors are trying to say with the "chaotic component" – I assume that in the model, the initialisation of the bromine explosion is deterministic and not driven by random processes?