Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-334

Anonymous Referee #1

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-RC1, 2022

The paper details the use of 3D modelling (WRF-Chem) to reproduce Arctic bromine chemistry and assoicated impacts (ODEs). The source of the bromine is snow within sea ice regions and within 300 km of the coast and in contrast with other studies this is a limited, replenishable, source rather than an infinite one. The effectiveness of the model is evaluated using BrO measurements from TROPOMI, in-situ ozone measurements at various Arctic sites and ozone sonde data. Importantly, unlike many other 3D modelling papers, this paper does not consider a "blowing snow" source. The authors find the new parameterization improves agreement with observations. The extent of this improvement varies from site to site depending on the sea ice conditions prevalent at those sites. The work is an interesting contribution and merits publication, however I would like to see the authors motivate their chosen snowpack bromide values using the larger data sets available rather than the limited dataset of Pratt et al 2013.

Specific Comments:
The salinity assumption detailed in line 218 is odd. Pratt et al 2013 is not a comprehensive snow data set for the Arctic and has a very limited number of samples. Krnavek et al 2012 would be a much better resource as the sample size is much larger and more likely to be representative. Then the value from Pratt et al is discarded in favor of a value that seems to be chosen rather arbitrarily. Kranvek et al as well as Peterson et al 2019 also have values for bromide in different ice regions which would be useful to improve your values of initial releasable bromide. Assuming the values for MYI regions and land are related to the first year ice region value seems like another arbitrary choice, particularly when measurement data are available. The paper would be improved if the authors motivated their chosen values using existing more comprehensive snowpack bromide measurement datasets.

Line 166: Why were observations at other sites not used?

Line 268: This statement concerning upward migration is only true if the snowpack is sufficiently shallow. In deeper snowpacks, the surface salinity is likely not impacted by brine migration and thus the bromine source would be limited. Domine et al 2004 suggest this point is about 17 cm.

Figures 9 and 12: It is hard to get what I am supposed to take away from these figures. Perhaps it might help to plot to difference between the modelled and satellite observations which would allow you to reduce the number of panels as well as allow the reader to more easily spot the areas of agreement/disagreement.

Technical Corrections

Line 240: Delete "the" between full and value

References:

Krnavek, L., Simpson, W. R., Carlson, D., Domine, F., Douglas, T. A., & Sturm, M. (2012). The chemical composition of surface snow in the Arctic: Examining marine, terrestrial, and atmospheric influences. Atmospheric Environment, 50(0), 349–359. https://doi.org/10.1016/j.atmosenv.2011.11.033

Peterson, P. K., Hartwig, M., May, N. W., Schwartz, E., Rigor, I., Ermold, W., Steele, M., Morison, J. H., Nghiem, S. V, & Pratt, K. A. (2019). Snowpack measurements suggest role for multi-year sea ice regions in Arctic atmospheric bromine and chlorine chemistry. Elem Sci Anth, 7(1), 14. https://doi.org/10.1525/elementa.352

Domine, F., Sparapani, R., Ianniello, A., & Beine, H. J. (2004). The origin of sea salt in snow on Arctic sea ice and in coastal regions. Atmospheric Chemistry and Physics, 4(9/10), 2259–2271. https://doi.org/10.5194/acp-4-2259-2004