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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The NO_x dependence of bromine chemistry in the Arctic atmospheric boundary layer" *by* K. D. Custard et al.

Anonymous Referee #1

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The manuscript by Custard et al. reports results of 0-D box model simulations which seek to unravel the manner in which NOx influences Arctic bromine chemistry. It is certainly an important and interesting topic for investigation, but I have read this manuscript several times, and am still confused as to its main points. I think it would benefit greatly by some restructuring and rewriting in places, to maintain a focus on the key results.

To start: the authors constrain the model amounts of halogens to a set of observations, then simulate a 10 day period with imposed high and low NOx amounts. But surely, since the gas phase halogens are already determined by nature, this will "twist" the chemistry in unrealistic ways in order to "keep up" with the observed Br2 and Cl2? Perhaps I am missing something ... This problem pops up in a few places in the MS, in

discussing times when observational halogen data was absent (pg 8338, lines 19-23; pg 8340, lines 18-20; pg 8341, lines 13-14).

Even accepting this limitation, I was left wondering about several of model results. The major result (not clearly stated) seems to be that there is no difference in bromine chain length between the High- and low-NOx cases. There is a brief discussion of this on pages 8337 and 8338, but I do not really follow the reason for why this is the case. Likewise, it is not clearly explained why the O3 loss rate behaves the way it does in the two model scenarios (pg 8339).

Some more minor points: On page 8337 (line14), it is stated that BeONOis not considered to be a sink for BrOx, yet in Section 3.4 it is considered in just that way.

I do not understand how Eqn 2 in obtained.

Pg 8340, lines 2-6 about the importance of BrONO2 in ODEs seems a bit of a non-sequitur.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8329, 2015.