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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A closer look at Arctic ozone loss and polar stratospheric clouds" *by* N. R. P. Harris et al.

Anonymous Referee #3

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The paper addresses the empirical relationship between column-integrated Arctic Ozone loss and the volume of polar stratospheric cloud volume which is inferred from meteorological analyses. It is an extension of a previous article and contains some important points which will be of interest to the scientific community. The paper is well written and I would recommend publication in ACP after some modifications and further investigations.

General Comments:

The scope of the manuscript is a more detailed analysis of the empirical relationship mentioned above compared to the author's previous paper in the International Journal of Remote Sensing (Harris et al., 2009, H09). To my mind this aim has not been achieved at all. In particular, the discussion of the effects has been rather qualitative

(e.g. Chapter 5.4) and repeates the findings of the H09 paper (discussion of Fig. 4 and 7, by the way: Fig. 4 is nearly identical with Fig. 2 in the H09 paper).

Some examples:

Comparing Fig.1 of H09 with Fig. 2 of this manuscript, it is obvious that both VPSC calculated from different ECMWF data analyses and O3 loss differ with consequences for the slope of the empirical relationship. The reason for this as well as the consequences for future Ozone loss in a changing climate has not been addressed in the submitted manuscript.

In the discussion of available chlorine and vertical redistribution of NOy due to de-/renitrification model results are shown in Fig. 8 and Fig. 9, respectively. Neither in the discussion nor in the figure caption the set up of this simplified box model is given (e.g. initialisation, simulation period, real or idealized trajectories, heterogeneous activation still possible during the model run?, . . .) which limits the usefulness of this calculations. In addition, the discussion of interannual variations in transport should be more quantitative with respect to horizontal mixing as well as to subsidence which determines the amount of available chlorine, also.

It is mentioned that SLIMCAT heating rates has been used to adjust the vortex average descent. How does these heating rates fit to the ECMWF analyses used for the VPSc calculation and what is the sensitivity of the results on these data?

In order to improve the manuscript I would like to suggest to pay more attention on the quantitative explanation of the model runs and in particular on their sensitivity to the assumptions made (e.g. What is the zonal dependence of the results using the idealized trajectories?, What is the sensitivity of the results on the H2O and HNO3 vmr for calculating the NAT threshold?, Where does the initial HCl:ClONO2 ratio comes from and how does the results (O3 loss) depends on this ratio?).

Specific Comments:

- Which type of PSC are shown in Fig. 1?

- The Newman and Rex (2007) paper is not included in the reference list.

- Page 6683, line 29: Please add "under sunlit conditions" after "... being converted ..."

- ClOx is differently defined in the paper (page 6683, line 29 vs page 6689, line20)

- As in the discussion it is stated that "At 400K, the relationship is found to be slightly more significant when cold aerosol activation is assumed in the place of NAT" it is necessary to discuss this more explicitly in Chap. 3 (page 6689, line 7 ff.).

- Please add a "k" in "bacground" (page 6689, line 11)

- OH is mainly formed due to the O(1D) + H2O reaction which should be mentioned at page 6693, line 5.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6681, 2010.