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

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**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.

We obviously disagree. The current manuscript looks at a number of factors not addressed in Harris et al. (2009), in particular the importance of the vertical dimension, including a previously unpublished empirical analysis. It is also more thorough in analysing other potentially important factors. In addition it presents for the first time the relationship calculated using ERA Interim re-analyses rather than operational products, i.e. the meteorological analyses are now self-consistent for the first time.

** In particular, the discussion of the effects has been rather qualitative (e.g. Chapter 5.4) and repeats 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).

See response to Referee 2. Briefly, we are trying to identify that main counter-acting mechanisms are of the right magnitude; we are not trying to reproduce the observed relation. Three dimensional models already reproduce that. Neither the observed or modelled relation have been analysed in as much detail as the current study.

**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.

The main differences between H09 and the current figure are (a):Vpsc: the VPSC/O3 loss figure inH09 is based on operational ECMWF analyses, that in the current paper is based on the internally consistent ERA interim reanalyses; (b) Ozone loss: H09 shows partial column loss between eTheta of 360 and 550K (comparable to Fig. 7 in the online aux. material of Rex et al., 2006). Note that Fig. 3 in Rex et al. (2006) and Fig 4-13 of WMO (2007) show 380-550K and are therefore slightly different from H09. Figure 2 of the current paper shows 380-550K to maintain consistency with Figure 3 of Rex et al. (2006). Hence, the ozone loss values are slightly different from those shown in H09. This is the main reason for the slightly smaller slope (ozone loss from a slightly smaller vertical region); (c) even so the difference in the slopes is only $\sim$10% which is within the statistical uncertainties.

**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.

We have included more description of the model runs in the manuscript. The basic chemical initialisation is now given in section 2 and we identify where other assumptions are made. The basic trajectory was already described in Section 2 (final paragraph starting "Idealised trajectories. . ."). This has been expanded and its role later in the paper made clearer.

**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.

We disagree for the reasons given above: 3D models are the best way to address this, not trajectory models. These factors are important for Arctic ozone loss, and we think they contribute to the observed scatter, as we have not identified counteracting mechanisms which would lead to a linear and compact relation.

** 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?

The approach that has been used to establish the basis for this paper (i.e. the ozone loss results) has a long literature trail and is traceable to the literature. This particular paper deals with explaining the compactness and linearity of the ozone loss / Vpsc relation rather than with repeating that work which led to the ozone loss results. It would generate substantial overlap and redundancy with the existing literature if all aspects of deriving the underlying ozone loss data would be repeated here.

**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?,

A paragraph about the zonal dependence is now included toward the end of section 4. This text additionally serves to explain why a zonally varying trajectory was chosen. We have put in numbers in a number of places to strengthen our arguments.

**What is the sensitivity of the results on the H2O and HNO3 vmr for calculating the NAT threshold?

Again, we think is outside the scope of this paper. We use VPSC as a proxy for the formation of polar stratospheric clouds and do not claim that all PSCs are formed. As shown in this paper, the VPSC/O3 loss results are not sensitive to the definition used. For our photochemical modelling, we are using idealised trajectories and are only presenting activation at temperatures of 194K. We have investigated the effects of lower temperatures on heterogeneous activation rates, but not for PSC formation.

** Where does the initial HCl:ClONO2 ratio comes from and how does the results (O3 loss) depends on this ratio?).

The initial ClONO2/HCl ratio is 0.33 (0.75 ppb: 2.25 ppb). This is consistent with the ratio of the satellite data in Fig. 1. A sensitivity run has been made with with ClONO2/HCl = 0.5 (1 ppb : 2 ppb). The qualitative picture remains similar to Fig. 5. Quantitatively, the higher ClONO2/HCl ratio leads to more efficient chlorine activation. This is now mentioned in the discussion of R1-3. A higher ratio also leads to larger O3 loss (1.2 vs 1.0 ppm), as expected from the discussion in section 4.2.

Specific Comments:

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

The PSCs shown in Figure 1 are Type I and are inferred from the meteorological analyses. This is now stated in the caption.

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

The reference is to the Polar Ozone chapter in the WMO 2006 assessment, and it was in the list. The way the reference is written has been clarified (as has the Newman and Pyle one) to be clear and follow the guidelines laid out in that assessment.

3. Page 6683, line 29: Please add “under sunlit conditions” after “. . . being converted. . .”

Done.

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

The definition of ClOx is now given just once as ClOx = ClO + 2*Cl2O2. The values in Figure 1 are actually Cly minus HCl + ClONO2 and so include Cl2 and HOCl. This is made clear in the text and the caption.

5. 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.).

We now do so. We were actually surprised how little difference was found using a cold aerosol formulation made, so we do not want to stress the slightly more compact relation at 400K too much.

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

Done.

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

The referee’s statement is true for much of the atmosphere. However, for the conditions considered here (polar spring) the HNO3 photolysis produces more OH than the reaction O(1D) + H2O -> 2 OH by a factor at least four according to our model. We now state this more clearly in the text.

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