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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal variations of reactive chlorine and nitrogen oxides observed by MIPAS-B inside the January 2010 Arctic vortex" *by* G. Wetzel et al.

Anonymous Referee #1

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This paper focuses on temporal variations of ClOx and NOx concentrations around sunrise to noon (over 3 hours) in the polar stratosphere. An overestimate and an underestimate of ClO (22-24 km) and NO_2 (32-34 km), respectively, are obvious around sunrise in a model calculation (CTM) compared to an observation (MIPAS-B). The reason of these discrepancies (in terms of earlier increase in ClO or earlier decrease in NO_2 in the model) could partly be explained by an exclusion of the effect of clouds on photolysis calculations in the model. The result presented here is so important for further improvement of modeling studies. However, a detailed sensitivity test by other group [Suminska-Ebersoldt et al., ACP, 2012] concludes that the effect of clouds on the photolysis of ClO dimer is not significant at SZAs considered here (90-94°). My

concern is whether or not the effect of PSC on the photolysis calculations could help reduce the discrepancy. More discussion referring adequate literatures is required to understand uncertainties associated with the photolysis calculations. A role of temperature dependence of (R1) should also be discussed more detail, since the observation was conducted in cold temperatures. I suggest that some concerns listed below should be addressed prior to acceptance of this manuscript.

1. The effect of tropospheric clouds at 8 km.

Tropospheric clouds will virtually always lead to enhanced radiation in the stratosphere, even at very high SZA$>$90°, due to reflection of diffuse radiation [Suminska-Ebersoldt et al., ACPD, 2011]. If the CTM includes the effect of the clouds, larger ClO values, in qualitative sense, would be expected between 7UTC and 8UTC at altitudes of 20-24 km where nighttime ClOOCl values are large. In page 4877, lines 13-15, this is refer to the direct sunlight. But, in these times and the altitude region, a photolysis frequency of ClOOCl, J(ClOOCl), is dominated by the diffuse radiation [Suminska-Ebersoldt et al., ACP, 2012]. Thus, the exclusion of the effect of tropospheric clouds on the photolysis calculation in the model seems to be not a reason for the discrepancy between the observation and the model (earlier increase in ClO). It should be discussed more whether or not the effect of PSC on the photolysis calculations could help reduce the discrepancy. For instance, Uhl and Reddmann [ACP, 2004] suggested that, at high aerosol conditions (or may also be at PSC conditions), the impact of some parameters (atmospheric refraction and its induced divergence effect) on the photolysis calculations at large SZAs ($>$90°) should be quantified by special calculations. I suggest to add some more discussion referring adequate literatures on uncertainties associated with the photolysis calculations.

2. Figures showing temporal (and SZA) variations of ClO or NO_2 and J(ClOOCl) or J(NO_2) at specific altitudes of interest.

In order to see the key results clearly by eyes, I suggest to add a line plot that shows

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a temporal (and SZA) variation of ClO at 23 km both from the observation (Fig. 4) and the model (Fig. 5). In addition to that, a line plot of J(ClOOCl) from the model would also be helpful. Similarly, I also suggest to add a line plot that shows a temporal variation of NO_2 at 33 km and J(NO_2) or k(NO_2 + O(^{3}P)) from the model.

Minor points:

1. page 4871, line 4: "collisional decomposition". "thermal decomposition" is more adequate (e.g., the JPL2011 uses it).

2. page 4876, lines 5-8: Is this not based on Williams et al. [ACP, 2006]? If not, how degree the difference is?

3. page 4876, lines 23-25: Is it enough for (almost complete) chlorine activation that temperatures below T(NAT) last 2 days? How large was the surface area density (SAD)?

4. page 4877, line 14: What is a reference for the calculation?

5. page 4880, lines 9-11: Again how large was the SAD in the model? I think the gamma value of this het. rxn. is well determined so far, so that the SAD seems to be too large in the model.

References:

Suminska-Ebersoldt et al., Atmos. Chem. Phys. Discuss., 11, C9542-C9546, 2011.

Suminska-Ebersoldt et al., Atmos. Chem. Phys., 12, 1353-1365, 2012.

Uhl and Reddmann, Atmos. Chem. Phys., 4, 1399-1405, 2004.

Williams et al., Atmos. Chem. Phys., 6, 4137-4161, 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 4867, 2012.