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Atmospheric Chemistry and Physics Discussions

Interactive comment on "HOCl chemistry in the Antarctic stratospheric vortex 2002, as observed with the Michelson Interferometer for Passive Atmospheric Sounding (MIPAS)" *by* T. von Clarmann et al.

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General comments:

As the title suggests, the paper presents a dataset of stratospheric HOCl observations over the Antarctic winter 2002 made from the MIPAS instrument on the European research satellite ENVISAT. The paper is well presented and clearly structured. The description of the measurement technique and data retrieval in the paper is very brief, but all the relevant information has been presented in earlier publications that are prop-

erly referenced. Together with ClO and $ClONO_2$ measured by the same instrument, the HOCl data provide insight in the evolution over the winter of ClO_x and its partitioning in two different altitude regimes. Our current understanding of the chemical processes governing the levels of important chlorine species is described in the introduction, and is tested via comparison of the observations to model simulations in the main part of the paper. One important conclusion (in agreement with one earlier paper by *Kovalenko et al.*) is that the currently recommended rate constant for reaction R1, ClO + HO_2 = HOCl + O_2, seems to be too low to explain the observed levels of HOCl. This means that the laboratory measurement of this rate constant and/or our understanding of the chemistry governing HOCl in the atmosphere may need revision.

Because the paper presents valuable new data that are used to address relevant scientific questions, I favor publication in Atmospheric Chemistry and Physics. Prior to publication, the following issues should be addressed.

Specific comments:

Major issues

1. **PSC occurrence (Page 18970, line 23; Figure1):** It is good to include a figure showing the presence of PSCs. However, the observations made for just one day (13 September) are not the most helpful information for the discussion of chlorine chemistry over a long time period that follows. I can understand that, because MIPAS can actually measure PSCs, the authors prefer to use this information (and the Höpfner et al. reference) rather than PSC probabilities based on temperature reanalysis. However, a time-altitude color map of possible PSC existence over the entire vortex (an example of such a plot can be found in Figure 8 in *Tilmes et al., ACP 4, 2181-2213, 2004*) would be much more useful than Figure 1. A short statement comparing the possible PSC existence based on temperature information to the actual PSC existence based on MIPAS data for one or more days (e.g. 13 September) could then be added.

ACPD

8, S9276–S9279, 2008

2. **Vortex evolution and position:** The presented data are averaged over the vortex, raising issues such as inhomogeneous air masses and irregular sampling (this is of course discussed in the paper). Also, the effect of increased solar irradiation due to vortex displacement on HOCl mixing ratios is mentioned several times. I think that an additional figure with a set of potential vorticity maps that show vortex extent and position on the dates of the measurements would be of great help to follow the arguments presented in the text.

3. **Evolution of Cl species in heterogeneous chemistry regime:** The observed ClO_x levels and partitioning in the heterogeneous chemistry regime (Figure 6) are partly attributed to irregular sampling (page 18977). I agree with the authors, but a more detailed discussion in the context of a rather inhomogeneous vortex in this time period should be added. For example, there is evidence that low O_3 mixing ratios in some air masses led to deactivation mainly into HCl, while in other air masses with less O_3 loss, deactivation into $ClONO_2$ was more important. This will obviously affect the levels and partitioning of ClO_x, and averaging over an inhomogeneous vortex may have different effects for different chlorine species with respect to the timing and magnitude of their concentration changes. The very detailed study of chlorine chemistry and ozone loss in this Antarctic winter by *Grooß et al.* (*J. Atmos. Sci. 62, 860-870, 2005*) may be useful in this context.

Minor issues

1. **Page 18970, line 14:** The last sentence of the introduction could be more elaborate, e.g. state what kind of model runs/comparisons you do and why you do it.

2. **Page 18971, line 25:** "...procedure *similar* to that described by *Nash et al. (1996)*...". Please specify what you mean by "similar".

ACPD

8, S9276–S9279, 2008

3. **Page 18972:** For the present study, why do you not simply use the diagnostic KASIMA model for all altitudes?

4. **Page 18973, line 7 + page 18976, line 9 + page 18980, line 6:** What is the reason for not using *Sander et al., 2006* (as opposed to 2003) in all model runs?

5. **Figure 2** Some of the panels are very busy. As the data are grouped into 4 different time periods, maybe one could go one step further and display averages of these time periods rather than sets of daily averages.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18967, 2008.