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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A quantitative analysis of the reactions involved in stratospheric polar ozone depletion" *by* Ingo Wohltmann et al.

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Dear reviewer,
thank you for reviewing our manuscript and your helpful comments.

General comment on HCl

- We have now considerably expanded the discussion of the HCl discrepancy. There is additional discussion in the introduction and more references. The appendix was moved to the main part of the paper as an additional section. The uncorrected runs are now shown in a supplement and differences are discussed in the main text.

Specific comments

- Page 2, line 3: Results for additional pressure levels (70 hPa, 42 hPa, 32 hPa) are available, which were not shown in the manuscript to limit the paper to a reasonable length. We have now included results for additional pressure levels in a supplement and added discussion of the dependence of the results on altitude in several places.
- Page 2, line 7: We assume you refer to Page 3, line 7? Added "dehydration parameterization" to the sentence.
- Page 2, line 15, 16: We agree that it is important to know how dependent the results are on the choice of the winter. Results for two additional winters are available, which were not shown in the manuscript to limit the paper to a reasonable length. The winters are Antarctic winter 2011 (as suggested by you) and Arctic winter 2009/2010.

  We have now added the results for the two missing winters in a supplement. We have added a new section discussing in how far the results can be generalized and discussing the notable differences between the winters.

  Actually, the choice of the northern winters was guided by the fact that a large range of possible meteorological conditions would be helpful for interpretation, and the winters 2004/2005 and 2009/2010 were chosen as a colder and a warmer winter.
- General comment: Moved the paragraph about the Polar Stratospheric Clouds to the model overview.
- Page 3, line 24: The value for the supersaturation in the chemistry module and the dehydration module can be set independently. We have added a note to the manuscript. The value of 0.7 was chosen to give the best possible agreement

between modelled and measured water vapor. The value of 0.35 was obtained by comparison of MLS water vapor and temperatures. We agree that it would be helpful to have the same values here. The value for the chemistry module is of secondary importance however, since most of the activation normally occurs on STS clouds in our model setup. Thus, it is probably acceptable not to do the model runs again.

- Page 4, line 8–9: We have to admit that this 10 % change was not really necessary. We used an initialization file from an earlier study here (Wohltmann et al., 2013) and did notice this too late (after finishing the runs). Since this 10 % change does probably not significantly change the results or the agreement to observations, we think it is acceptable not to do the model runs again or to discuss this choice in the paper.

- Page 5, line 1–4: We have now added a supplement with additional pressure levels, see above. In addition, we have changed the title of the paper.

- Page 6, line 6: Done. Thanks for pointing us to this obvious omission.

- Page 11 and 12: Thanks for pointing us to these obvious omissions of important references. We have now added references to these studies in several places. In addition, we have increased the number of citations in the text and of the referenced papers considerably.

- Page 15, line 18: Changed to "HO_x chemistry". "HO_x production" was not quite correct here. Added a reference to Figure 8. Extended discussion on the $HOCl + HCl$ reaction considerably (see next comment).

- Page 21, line 14–15: The differences between the $ClONO_2 + HCl$ and $HOCl + HCl$ reaction in timing and between hemispheres are a quite interesting point and we have expanded the discussion of this issue in this section considerably.

First, there is a misunderstanding in your comment. The value of 70% refers to an integrated value over all months (i.e. the integral of the colored area for $HOCl + HCl$ in Figure 13 compared to the colored area for $ClONO_2 + HCl$). We have added "integrated over the winter" to the sentence to make that more clear.

The reason for the increased importance of $HOCl + HCl$ is the deactivation of chlorine by the reaction $Cl + CH_4$ (caused by the low ozone values), which provides HCl and produces HO_x (see Portmann et al., 1996, Crutzen et al., 1992). This can clearly been seen in the rising HCl levels in September in the southern hemisphere (Figure 11) and in the increased HO_x levels (Figure 9). In contrast, deactivation is into $ClONO_2$ in the northern hemisphere, keeping HCl levels low. Since the southern hemisphere is more denitrified, activation by $ClONO_2 + HCl$ is hindered. We have added discussion of this to the text.

Your statement that $ClONO_2$ is higher in mid-September and October is not quite correct. $ClONO_2$ mixing ratios are near zero from June to November due to the denitrified conditions (see Figure 11, also compare with values in the northern hemisphere). Thus, no explanation is necessary why $HOCl + HCl$ is still more important than $ClONO_2 + HCl$ on 1 October.

We added figures in the supplement showing that that the 70/30 ratio does not depend on altitude and added a general statement that the results for the chlorine species do only moderately depend on altitude at the beginning of the section.

- Page 21, line 27: Added reference.

- Figure 19: Actually, vortex means of ozone from MLS and ATLAS compare relatively well in this time period (Figure 22, note that we changed the plots from showing values at 475 K to 54 hPa to be consistent with the rest of the paper). The difference is only slightly larger than the accuracy of the MLS ozone measurements (added bars for the accuracy of the MLS measurements to the figures). The reason that the MLS ozone values slightly differ from the model may

be related to the fact that there is a notable overestimation of N_2O in ATLAS compared to MLS in the southern hemisphere in 2006 in October and November (we have now added plots showing N_2O to the manuscript). The overestimation of N_2O points to an overestimation of mixing across the vortex edge, possibly caused by problems in the ERA Interim data. This is supported by an underestimation of HCl. This would however lead to a faster increase in ozone mixing ratios in ATLAS compared to MLS, which is not observed. An alternative hypothesis would be strong upwelling in ERA Interim not existing in reality. In summary, the discrepancies remain an unresolved issue.

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