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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Uncertainties in modeling heterogeneous chemistry and Arctic ozone depletion in the winter 2009/2010" *by* I. Wohltmann et al.

Anonymous Referee #1

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In the current version of the manuscript, Wohltmann et al. use a chemical transport model ATLAS to study the stratospheric chemistry and denitrification for Arctic winter 2009/2010. They have also done a lot of sensitivity experiments to investigate the impact of some uncertainty parameters of chlorine activation and denitrification on the model results. These results are also compared with some measurements made during the RECONCILE campagin. They concluded that "Even large changes in the underlying assumptions have only a small impact on the modeled ozone loss, even though they can cause considerable differences in chemical evolution and denitrification". So the question is that these uncertainties in the limitations in modelling the complex processes in Chemistry and Transport Models (CTMS) remain an important

issue (Line 22-24)? In other words, does that mean CTM model is not so sensitive to the parameter you choose for the sensitivity studies?

The major comments:

1) It seems to me that the ATLAS CTM did not do a good job in simulating the ozone loss (Figs 8. and 9). The model gives the ozone loss at very high altitude (600 -700 K). From previous work, it showed that the maximum ozone loss occurrs at around 475 K for other Arctic winters. The authors need to double check why modelled Arctic ozone loss for 2009/10 occurred at higher altitude than winters (the MLS also show the maximum ozone loss occurrs around 550 K for Arctic winter 2009/10)?

2) It would be better to change some plots using log-scale which will be clealy seen the difference or using the relative difference?

3) Can you highlights some results in the abstract? The current conclusion in the avstract is too general and no real results are highlighted.

4) Page 26249 Line 15-17, why there is no correlation between chlorine activation and the formation of PSCs in MLS data? Is that true?

Some minor comments:

1) Table 1. Can you add the photloysis and gas phase reaction rate coefficient used in Table 1?

2) Page 26250 Line 18, it should be 12, not 14.

3) Can you specific the data of H2SO4 used in the model? Is it from SAGEII?

4) Page 26252 Line 16. The model use the hybrid pressure-potential temperature coordinate or sigma-potential temperature coordinate?

5) Model setup. It seems that there is no spin-up time has been used for your CTM simulations after you initialise the chemical species on 1 December?

6) Page 26253 Line 10 and Page 26281. For the reference run, you have already had 10 times of supersaturation, so the MORE-SUPERSAT should be 300 times of standard supersaturation. Is that true? or I misunderstand it here?

7) Chlroine activation. Why does the model overstimate the chlroine activation compared with MLS?

8) Page 26260 Line 5, maybe change "identical" to "similar".

9) Page 26263 Line 5, change "pace" to "rate"?

10) Page 26266, Line 11-12, if you use "excellent agreement", why you mention "no major problmes" here?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26245, 2012.