**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.

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

Major comments

- 1) We have added discussion on the reasons of the discrepancy between 500 K and 650 K in Section 3.4. The effect of the discrepancy on the column is small. The discrepancy in mixing ratios is at most 30%, and if you take ozone (and not ozone loss), it is at most 10%. The life time of ozone between 500 K and 650 K at

the end of March is of the order of months, so the discrepancy can be caused by chemistry on longer time scales or by transport effects. Correspondingly, there are two plausible explanations for the discrepancy in ozone:

a) The discrepancy could be caused by differences in dynamics between model and reality. There is a corresponding discrepancy between modelled and measured N2O: N2O is overestimated in the model. The discrepancies in ozone and N2O appear when the major warming sets in at the end of January. Both the ozone and the N2O discrepancy could be explained if there was more mixing over the vortex edge in the model (i.e. in ERA Interim) compared to reality during the major warming. The gradients of N2O and ozone over the vortex edge in this altitude region would be compatible with that. Differences in subsidence can be excluded, since more ozone in the model would imply more subsidence in the model than in reality. However, that would mean less N2O in the model, and not more N2O, as actually modelled.

b) Additionally, part of the discrepancy in ozone could be caused by chemistry: There is an overestimation of HCl and an underestimation of active chlorine in the model in January just in the altitude range where the discrepancy in ozone appears. That does not explain the discrepancy in N2O, however.

The discrepancy noted here is well within the performance of other stratospheric CTMs in general (see e.g. papers like Sinnhuber et al., 2011 (doi:10.1029/2011GL049784), Feng et al., 2011 (doi:10.5194/acp-11-6559-2011) or Grooß et al., 2005 (cited in the paper)). Note the discussion on model shortcomings in Section 3.6 and 4 (particularly 26268, line 27 to 26269, line 23), where possible reasons are discussed.

Most importantly, the ozone loss below 500 K is well simulated by ATLAS (see Figs. 9 and 10).

- 2) We have considered the suggestion, but we cannot see a plot where a logarith-

mic scale would provide additional useful information. That would imply that the values would span several orders of magnitude, which is not the case. Similarly, in our opinion, relative differences would not provide additional information.

- 3) The abstract has been revised to include more results from the paper.

- 4) The statement is true. This is a citation of a result from a different paper (Wegner et al.). As discussed in the abstract and in the introduction, one of the main results of our paper is that chlorine activation on the background aerosol is sufficient to explain the observed magnitude of chlorine activation (in the model). The results of Wegner et al. suggest that this may in fact be the case in reality, at least for the case studies discussed in the paper.

Minor comments

- 1) The numbers are missing intentionally. It is noted in the text that all reaction rate coefficients are taken from the JPL catalogue (Sander et al.), which is freely available. The numbers are hard to interpret intuitively and give no additional insight into the topics discussed in this paper.

- 2) 14 **is** the correct number. You counted the reactions in the first column, which is not the total number of reactions. Note that a reaction with the same net chemical formula is a different reaction on different types of surfaces. The number 14 refers to the **additional** reactions compared to the last published model paper.

- 3) Yes, the data are from SAGE II. We fitted the calculated vortex mean surface area densities of the reference run to the CCMVal surface area density climatology. This was discussed in the paragraph on page 26251, lines 12–18.

- 4) The model uses a "pressure-potential temperature" coordinate (page 26252, line 16). There is no reference to a "sigma-potential temperature" coordinate in the paper.

- 5) The initialization of the longer-lived species has been deduced from observations and therefore reflects the state of the atmosphere as well as possible. Over the timescales relevant here, there is no inconsistency with the model dynamics. There is no significant spin-up time needed for the shorter-lived chemical species. The species which are not initialized normally have lifetimes less than a day.

- 6) The supersaturation in the reference run is 10 and the supersaturation in the MORE-SUPERSAT run is 30. Both values are absolute values.

- 7) The model does not overestimate chlorine activation compared to MLS (see Fig. 3 of the discussions manuscript).

- 8) The term "identical" expresses what we want to say here.

- 9) Done.

- 10) An excellent agreement implies no major problems.

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

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