

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

Anonymous Referee #1

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General Comments.

This work derives the “quantitative analysis of chemical reactions involved in polar ozone depletion in the stratosphere, and of the relevant reaction pathways and cycles”, for one model, the ATLAS CTM driven by ECMWF meteorological fields. In this work, comparisons to observations are not shown. The authors did a nice job of reviewing the literature on polar chemistry, however, I have made two suggestions on additional references that should be added. There is also an adjustment to HCl STS solubility by applying a -5K offset to the calculation of the Henry constant of HCl. The impact of this offset on reaction pathways and cycles was not discussed. Overall I found this to be an interesting paper and worthy of publication in ACP.

Specific comments.

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Page 2, Line 3. Why 54hPa? You may want to add a sentence for justification on this choice. That is, 54hPa is not the core of the ozone depletion in the SH polar region. According to ozonesonde data, it is closer to 80-70hPa. See Solomon et al., Four decades of ozonesonde measurements over Antarctica, JGR, 2005 Figure 12. The NH depletion tends to peak lower in pressure (higher in altitude).

Page 2, Line 7. You mention the ATLAS denitrification module, but not dehydration module. If you are going to mention denitrification you should also mention dehydration in the Model overview section.

Page 2, line 15, 16. You picked years 2004/2005/2006 for this study. Why? E.g., 2010/2011 would have been a better year since both the NH and SH had very cold temperatures and significant ozone depletion.

General comment on model setup discussion. You start by describing details regarding the model (domain, met fields, simulation period). You then have a model description (STS, NAT, ICE details). Finally, you talk about the initial conditions (i.e., back to the model setup). It seems to me you should have a model description first (i.e., you call this a model overview), then discuss the model setup.

Page 3, line 24. You state: “For ice particles, a supersaturation of 0.35 is assumed. . .”

Page 4, line 2. You also state: “. . . removes ice above a given supersaturation, which is set to 0.7 here”. These statements seem to contradict each other?

Page 4, line 8-9. Based on the discussion of HCl later in the paper, it seems to me you should explain why you increased ClONO₂ by 10% at the expense of HCl – not just give a reference.

Page 5, lines 1-4. All of what you say is true, however, isn't the issue with quantifying as the title states “reactions in the stratospheric polar ozone depletion” dependent on the region of the vortex and dynamical mixing and vertical descent is an important component of this. The procedure you are following with the vortex tracer approach

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possibly justifies a new paper title? That is, you are only looking over a narrow pressure range near the core of the vortex.

Page 6, line 6, “the chemistry of polar ozone depletion can be divided into several phases (Solomon, 1999)”. Instead of Solomon, 1999, I would recommend you reference the paper that to my knowledge, first defined the phases (i.e., Setup, Activation, Maintenance/Further Act, Termination). Portmann et al., JGR, 101, 1996. It seems appropriate to use these “phase names” in your paper?

Page 11 and 12. The discussion on ClONO₂ was outlined in detail in Portmann et al. 1996. I should be referenced by this work. You should also reference: Douglass, A. R., M. R. Schoeberl, R. S. Stolarski, J. W. Waters, J. M. Russell III, A. E. Roche, and S. T. Massie (1995), Interhemispheric differences in springtime production of HCl and ClONO₂ in the polar vortices, J. Geophys. Res., 100, 13,967–13,978, doi:10.1029/95JD00698.

Page 15, line 18. ... related to the fact that reactions like Cl + CH₄ and HOCl + HCl play a role in HO_x production. You previously stated that Cl + CH₄ is a production process. HOCl + HCl is a loss process (line 12). Please clarify what you mean by “play a role in HO_x production”.

Page 21, lines 14,15 “.. In the SH, the HOCl + HCl reaction accounts for about 70% of the HCl activation by heterogeneous reactions. . .” Based on the previous sentence, this statement is only correct for Aug-Sept (where ClONO₂ is low). However, I would expect this not to be the case in mid-September and October (where ClONO₂ is higher). This doesn't seem to be what is shown in Figure 13. In your model results, can you please explain why HOCl+HCl is still » ClONO₂+HCl on 1 October?

Is this 70/30 ratio true only at 52 hPa? If not, please add a sentence to discuss this ratio at other pressure levels (mainly pressures >52hPa). Also, can you say what the role of denitrification is in setting this heterogeneous rate partitioning?

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Page 21, lines 27. Should reference Douglass et al. 1995.

Figure 19. Why is ozone not recovering in late October through November? If you plot Aura MLS Ozone at 82S (max latitude for MLS), ~ 50 hPa, you would see recovery in the observations. Is this an issue with the model's dynamics/transport not replenishing ozone in November?

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