Atmos. Chem. Phys. Discuss., 7, S7061–S7065, 2007
www.atmos-chem-phys-discuss.net/7/S7061/2007/

Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on “The impact of mixing across the polar vortex edge on Match ozone loss estimates” *by* J.-U. Grooßet al.

J.-U. Grooßet al.

Received and published: 21 November 2007

We thank Neil Harris for his critical review. In the light of this review, we improved the manuscript in the preparation for a possible publication in ACP. An error in the determination of the ozone column offset between Match performed within CLaMS and CLaMS vortex average was found and corrected. Also we have to admit that Fig. 11a was composed with a different subset of matches and therefore did not correspond to Fig. 8b. We also updated Fig. 11b, in which the time sampling was not identical to that in Fig. 3. Due to this revision and due to the reviewers' comments we rewrote the paper, particularly Section 5 of the paper. We think that the arguments are now presented much clearer.

1. *The authors' faith in the CLaMS performance and uncertainties associated with CLaMS*

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The main aim of this paper is to investigate the differences between ozone loss simulated with the model CLaMS and deduced using the Match method. It is clear of course, that the CLaMS simulations are not perfect and have deficiencies as has every simulation. To get the sense of the quality of the model, especially the ability to simulate ozone mixing ratios, comparisons with measurements were performed and presented. Especially at the end of the winter at 400 K potential temperature, the difference between simulated ozone loss and Match-derived ozone loss is of the order of 1 ppmv. From the comparison with ozone observations it is shown that the model is able to simulate ozone mixing ratios better than 1 ppmv, namely around 0.2 ppmv. This was the main motivation of this study.

Besides the comparison of ozone, another important point of model performance is the ability of the model to simulate the tracer gradients at transport barriers. This is important because the variable ozone tracer, that is used for estimating the ozone loss cannot be validated itself. It has been shown in previous studies that the strength of CLaMS is to accurately simulate tracer gradients and small filaments. Some examples of this are shown in fig. 1. Also, the comparison with the June balloon measurements shows that in the model a vortex remnant is still present as observed. If the model transport across the vortex edge would be too strong, the model could most likely not reproduce this filament.

These two points should enable us to make strong arguments, however, the reviewer is right in the point that we may have generalized this into a potentially too optimistic view of the model results. In the revised version of the manuscript, we present the model results more realistically.

2. *CLaMS tracer uncertainties and* N_2O *offset/ renitrification altitude.*

   There is some offset in the CLaMS simulation of N_2O that is visible in Fig. 7 of Grooß et al. [2005]. However, the corresponding comparison between data and the “tracer simulation” with 80 km resolution (Fig. 1) is somewhat better. It is also true, that denitrification and renitrification are not perfectly simulated most likely due to many unknown parameters, e.g. the fact that the NAT nucleation mechanism is not well known. However these aspects are not closely related with the simulated ozone. Grooß et al. (2005) showed that a complete omission of the denitrification processes would reduce the simulated ozone loss by less than 0.15 ppmv or 7% of the partial column loss between 380 and 550 K. We have rewritten the text in a way that the model can be assessed better. The most important issue in this context is the comparison with ozone observations.

3. *Little comparison/discussion is made with other model studies of that winter.*

   The aim of this paper is not a comparison of estimates of ozone depletion with other model studies. However, we have added a short paragraph on that point. There are published simulations of ozone depletion in 2002/03 from Feng et al. [2005] and from Singleton et al. [2005].

   Singleton provide not column ozone loss but report vortex (Nash) average ozone loss peaking at 1.2 ppmv in 425-450 K on March 15. Our simulation shows the maximum corresponding ozone depletion of 1.26 ppmv at 465 K. These simulations are rather comparable. The comparison with the Feng et al. simulation was already discussed by Grooß et al. [2005]. The column ozone loss (345-670 K, $\Phi_e > 65°$N, March 12-22 average) is 53.4 DU in our simulation and 57.9 DU in the simulation by Feng et al. To our knowledge these are the only published simulations of ozone loss for the winter 2002/03.

4. *Model calculated losses are on the low side and that the statements about the disagreement with Match are therefore too strong.*

   This is correct. Especially in section 5.5. one error in the determination of the ozone column offset between Match performed within CLaMS and CLaMS vortex average (about 5 DU) unfortunately led us to the assumption that we can explain the full difference between CLaMS and Match. The revised column estimate is

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

now conform with the lower error limit of the Match estimate. We have changed the manuscript accordingly.

5. *The presentation could be clearer.*

   We tried to clarify the presentation of the arguments in the revised version. Basically, we re-ordered the arguments in section 5. Now the two main points are addressed in the first subsections, mainly the method of integrating ozone loss rates (5.1.) and the comparison of Match-like derived ozone loss rates with the simulation (5.2). After that, the different effects contributing to section 5.2. are mentioned in detail. We think that this re-arrangement of text and arguments is now much clearer.

6. *It is not new that Match is less good in regions of disturbed flow; rather the authors are trying to quantify this.*

   Yes, we agree. We indeed think that the strength of this paper is a quantification of the effect. We have therefore added some more quantifications as the column ozone loss change due to the “reduced Match”.

7. *This manuscript should include a neutral summary or discussion of the Streibel et al. (2006) Match results for 2003/03.*

   We have done so in the revised version of the manuscript.

8. *The final sentence of the conclusions is not justified given the presented material.*

   This is true. We therefore deleted this sentence since the conclusions also stand without it. A repetition of this study using the 2004/05 or any other winter would in principle be possible, but it would only lengthen the paper without adding much more information.

9. *The meaning of “mixing”, transport of air in and out of the vortex, etc.*

This point was also mentioned by the other reviewer. In the ACPD paper, we have used the term “mixing” through the vortex edge where we mostly should have used “transport” or “irreversible transport” across the vortex edge. Air transported irreversibly across the vortex edge is “mixed” into the whole vortex volume, but may still maintain its characteristics for a longer time until it may be finally mixed with former pure vortex air. We did not intend to make this distinction. Important for our arguments is only that the air is irreversibly transported into the vortex. The use of the word mixing was therefore misleading. In the revised paper, we changed the wording accordingly.

10. *Figures 8/10, impact of “reduced match”*

    We have evaluated the impact of the “reduced match” vs. the original Match results as column ozone loss difference on March 16. The reduced Match ozone column loss is 1.7 DU lower. This is mentioned in the revised manuscript.

11. *The “low sun angle effect” is not important*

    We agree that this may be not the most important effect with respect to March column ozone loss. Nevertheless it was identified and quantified to of the order of 30% for one day in early January which is interesting in the context of this paper. To clarify this point, we also changed Fig. 9 such that the minimum solar zenith angle of each air mass is now also shown.

12. *English proofreading*

    The revised manuscript was proofread by the FZJ language service.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11725, 2007.