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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatiotemporal variations of NO_y species in the northern latitudes stratosphere measured with the balloon-borne MIPAS instrument" *by* A. Wiegele et al.

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We would like to thank the referee for the valuable comments which we answer in the following (referee comments are inserted *in italics*).

Major comments

I think that a short explanation is at least required about how the authors take into account effects of possible spatial mixing ratio inhomogeneities along the MIPAS-B lines of sight (it may be part of what the authors call 'LOS errors' I guess). An investigation could be done from an analysis of the trend of the slant column densities profiles versus elevation angle.

First of all it is important to stress that the observation scenario was set up such

that inhomogeneities along the line-of-sight were minimised for species that are SZA-dependent by looking orthogonal to the direction of the sun rays. Concerning inhomogeneities imposed by crossing the vortex edge, these effects have been studied for $ClONO_2$ which has revealed the largest gradient across the vortex edge. We have performed test retrievals assuming a strong horizontal gradient of $ClONO_2$ of 1 ppbv per 100 km. The results show that, thanks to the well defined averaging kernels with narrow peaks of the contribution function around the tangent altitudes, horizontal gradients generate only small variations of up to 1% in the retrieved volume mixing ratios. Therefore, these errors can be regarded as minor for the reported observations. We now have addressed the effects of horizontal gradients in Section 2.1.

The analysis of the model results and of the discrepancies with the observations is a bit too qualitative. Did the authors perform sensitivity tests on the trajectories such as greater backward time integrations (from 3 to 10 days), uncertainties on the trajectory position (see Canty et al., JGR, doi:10.1029/2004JD005035, 2005) or ECMWF temperature uncertainties along the trajectories that could partly explain the reported discrepancies between observations and simulations? Specifically, for N_2O_5: Differences could be due to uncertainties in the ozone climatology used to calculate the photolysis rates. ECMWF temperatures should be compared to observations whenever possible (vertical soundings, MIPAS-B observations) and their impact on the N_2O_5 modelling could be quantified. Finally, another source of error could result from simulations of heterogeneous processes. Dufour et al. (ACP, 5, 916-926, 2005) present a sensitivity test of different liquid sulphate aerosol surface area densities on the NOx/NOy profile obtained from balloon measurements. Even in the summertime high-latitude conditions presented in this paper (low quantities of N_2O_5 due to long-time sunlit conditions), the impact on the N2O5 amounts and NOx/NOy is not negligible. Incomplete knowledge of stratospheric aerosol content is a topic currently under investigation (see SPARC report N_4, Assessment of Stratospheric Aerosols Properties, WCRP124, WMO/TD N_1295, 2006). Therefore, I suggest the authors to mention in the text (in part 5.3.2) this possible source of uncertainty.

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In order to investigate the discrepancies between box model results and observations more quantitatively, we have added a new chapter where we compare the NO_2 J values used in the box model runs with NO_2 J values deduced from the MIPAS-B measurements around sunrise. In addition, sensitivity runs with the radiative transfer model ART have been performed to characterise the influence of different ozone columns and different albedos on the NO_2 J values shortly after sunrise. It has turned out that the ozone column only has a small influence on the NO_2 J values.
Differences between ECMWF temperatures and the temperatures measured by MIPAS-B are typically in the range of 2 K or below. They do not show a significant bias and the standard deviation of the differences is in the order of the MIPAS-B temperature errors. Model calculations have shown that a temperature variation of 2 K alters the N_2O_5 mixing ratio by less than 5% after a built up of 12 or 24 hours in the altitude region between 15 and 31 km. We now discuss these findings in the paper and also mention the importance of liquid sulphate aerosols surface area densities on the NOx/NOy partitioning and their possibility to act as a source of uncertainty for N_2O_5.

Minor comments

To help the reader, it would be nice to provide information about the position of the vortex edge (using small arrows fro example) at the 3 altitudes for each limb-scan represented in Fig. 2.
We have modified Figure 3 such that the PV maps for 475 and 550 K also show the location of the tangent points at 19.5 km and 22.5 km altitude, respectively, as well as the vortex edge according to Nash et al.

Page 4706; Line 2: What do you mean by 'points' (tangent altitudes? Mean position corresponding to the vertical profiles?)? This is a bit confusing when you mention individual trajectories ending at each tangent altitude (described line 10).
We mean tangent points and we have changed the text accordingly (replacing “points” by “tangent points”).

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I do not see why you do not use only the individual trajectories ending at all the tangent altitudes (maybe a question of time calculation?) for the model-measurement comparisons (as done in Rivière et al., JGR, doi:10.1029/2002JD002087, 2002 for example).
We also did model calculations using individual trajectories for each azimuth direction. A comparison between the two model runs showed that for the high tangent altitudes, which are important in terms of photolysis, the differences were small. At these altitudes, the different tangent points are rather close, and therefore, not much difference in the chemical composition is expected. In the model, however, individual trajectories lead to small discontinuities between the azimuth directions due to the different initialisation. Therefore we decided to present the results from the synthetic trajectories, as we stated on page 4706, lines 9 ff.

Please mention briefly why you do not present comparisons between the measurements and modelling of HNO_3 (I guess it is because it does not vary very much over the considered period) or specify shortly in the text the results of these comparisons.
Indeed, the variation of HNO_3 during the considered period is rather small. Since photolysis of HNO_3 is almost negligible in the lower stratosphere (as stated in the introduction, p. 4695, line 2), the temporal evolution of HNO_3 is not in the scope of this publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4693, 2008.