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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.

Anonymous Referee #2

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The authors present an original method to tag along spatial and temporal variations of the NOy species at high latitude using balloon observations. This is done from long-duration float altitude balloon observation (about 4.5 hours) by the MIPAS-B instrument which is one of the rare instruments capable of measuring the stratospheric vertical profiles of the whole NOy family with a nice accuracy. It must be pointed out that this instrument has been used routinely in numerous validation campaigns at various latitudes. To me, when obtained over a wide temporal range such rare observations deserve to be published. The study is completed by simulations using trajectories coupled with a zero dimensional (0D) model. The MIPAS-B observations are valuable since they allow modellers to constraint their model with the correct amount of total

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NOy, a process known to reduce significantly discrepancies between models and measurements regarding the partitioning of the various NOy species. This is a strong point in the paper.

I have however two main points that I would like to be addressed by the authors before publication in ACP. The first one deals with the hypothesis of spatial homogeneity of the atmospheric layers using such a limb-sounding technique. Do the authors have ensured the validity of the spatial homogeneity hypothesis especially in observation conditions close to the vortex edge? Secondly, the modelling part a bit suffers from lacks of quantitative investigations about uncertainties in the trajectory and the 0D-model calculations. Details are given below.

Major comments:

- The authors summarize the MIPAS-B measurement method and vertical profile retrieval and provide all the adequate general technical references. Some specific conditions of observations, such as those presented in the paper, require further explanations/investigations. In particular, this is cautiously done by the authors regarding the retrieval of the NO species which appears to be tricky in the studied conditions. MIPAS-B observations consist of limb-sounding using atmospheric emission. It is assumed that the scanned stratospheric layers are homogeneous. However recent work has shown that vertical profile retrievals of stratospheric species from remote sensing techniques are likely to be biased when the homogeneity conditions are not valid (see Swartz et al., ACP, 6, 1843-1852, 2006 and Berthet et al., JGR, doi:10.1029/2007JD008699, 2007 for ozone and NO2 respectively). Of course these authors use different observation techniques than MIPAS-B but I have concern about the consistency of the mixing ratio vertical profiles of NOy obtained by MIPAS-B for each given azimuth angle in particular for lines of sight crossing the vortex edge below 21 km with strong horizontal gradients and possible signatures of both vortex air and mid-latitude air (for example I am wondering about the pertinence of the ClONO2 mixing ratio values below 21 km for the lines of sight crossing the vortex edge; between

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3:15 and 5:03 in Fig. 8). 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.

- 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 N2O5: 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 N2O5 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 N2O5 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.

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.

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- 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).

- 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).

- Please mention briefly why you do not present comparisons between the measurements and modelling of HNO3 (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.

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