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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nitrogen compounds and ozone in the stratosphere: comparison of MIPAS satellite data with the Chemistry Climate Model ECHAM5/MESSy1" *by* C. Brühl et al.

C. Brühl et al.

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1 Response to referee #4

We thank referee #4 for his detailed suggestions for improvements. Evaluation of the model is one aim of the paper (as written in the abstract). Another aim is, to use the combination of satellite data and model simulations, to check the sensitivity to new laboratory data and model simplifications concerning reaction products. We will point this out more clearly in the abstract or the introduction.

1.1 Major points

1. Additional figures on the percentage of different N-species to NO_y for day and night will be given in an electronic supplement. For this only a subset of data where all N-species are available at the same time and location in the satellite data can be shown. We will expand the text concerning that in the interpretation of Figures 3 and 4.

2. The text on Figure 1 concerning the diurnal cycle will be improved. The diurnal cycle will be discussed in more detail in the following section by analyzing data for NO, NO_2 and N_2O_5 separately for day and night using the solar zenith angle in the satellite data as criterion. The panels for NO, NO_2 and N_2O_5 in Figures 3 and 4 will be replaced by panels for day only or separate panels for day and night, maybe with one set in the supplement. We are also thinking of showing day and night data of NO, NO_2 and N_2O_5 in the scatter plots of Figures 5 and 6 in different colors.

3. The introduction will be expanded. The dynamics of the vortex split is not the main subject of the paper but the referee is right that more references should be given on that. We are aware of the special issue in Journal of Atmospheric Sciences and some papers in Journal of Geophysical Research. Concerning MIPAS a rather preliminary version of data was used in some of these studies which has to be pointed out also as justification for our new study on NO_y-chemistry. Concerning dynamics, we will expand the text in the way the referee suggested.

4. We will provide an additional panel using model data only for the satellite data points. The referee is right that Figure 8 gives not much additional information and will be skipped or moved to the supplement. It was included only because some collegues were interested in integral quantities.

5. As already replied to the other reviews, we will expand that part for more clarity.

6. Some frames of Figures 3 and 4 might go the supplement, maybe together with the expansion of Figure 2 by more species.

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1.2 Minor points

1. The quality of MIPAS NO data has been considerably improved since the Funke et al., 2005 publication. A publication on the retrieval improvements is in preparation.

2. Unfortunately there are not enough NO data available for other periods yet (except for a few individual orbits or days in winter 2003/2004).

3. We have done a detailed analysis on that presented at the recent AGU and EGU conferences (it is not allowed or meaningful to refer to that, or a paper in preparation in ACP(D)). As mentioned in section 2 there is a high bias (7%) in tropical MIPAS N_2O-data (will be repeated here). We will expand the discussion in the paper.

4. The text and the figures will be improved.

5. This will be analysed in more detail, the text will be improved (the model has a resolution of 2.8^o, the satellite data 5^o, the closest point has been selected). In the meantime we found by a sensitivity study that the difference is partly due to an overestimate of $J(NO_3)$ in the model for stratospheric conditions.

6. This will be done, see above.

7. We will try to include the location of simulated wind maximum in the figures (at least in Figure 2).

8. We meant the two vortex lobes in the SH, this will be corrected.

9. At 50hPa simulated N_2O points to not enough descent inside the vortex, however, too much mixing cannot be excluded. The text will be modified.

10. See point 3. Checking other long-lived species and the tape recorder of water vapor makes it unlikely that underestimated ascent in the tropics is the reason.

11. We checked also the individual days. The position of the vortex does not change so much that it prevents showing quasi synoptic maps. We prefer to use the 3 con-

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secutive days together for statistical reasons and to have better coverage. This would not hold if further days are included. Pixels for satellite and model data are always at approximately the same time and position. Using trajectories here would not help much for interpretation, it would introduce only further uncertainties from the ECMWF or ECHAM5 models (which should be taken?) to be used for the trajectory calculation.

12. This will be done. The range in the original figures included also the latitude bins not shown in the paper.

2 Response to referee #1

We thank reviewer #1 for his suggestions. However, we think that a species-wise discussion would not improve the manuscript. Our intention is to discuss the model's ability to reproduce observed distributions of the NO_y species and to understand which properties of the model succeed or fail to reproduce them. The inter-relationship between the species is a valuable indicator towards the relevant model features; therefore we prefer to discuss properties of the model/distributions on basis of a joint view of all the species. As mentioned in the previous section, some frames of Figures 3 and 4 may go the supplement.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9899, 2007.