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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.

**Anonymous Referee #4**

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Brühl et al. present a comparison of MIPAS data for chemical constituents with a CCM simulation for the period of the sudden Antarctic stratospheric warming in September 2002. The main aim of the paper seems to be the validation of the model, although it is not explicitely written. The focus is on ozone, $N_2O$ and $NO_y$ compounds. Day-night changes and $NO_y$ partitioning are investigated and $NO_y$ production is derived from the MIPAS data set. The main scientific message however remains unclear.

Especially it should be worked out more clearly which of the model-data differences are caused this particular model setup and which inconsistencies seem to point to possibly inaccurate laboratory work. The paper is rather difficult to read and a lot of points are confusing and mis-leading. They are listed in detail below. I recommend publication only after a major re-write in which these points have be accounted for.

## Major points:

1. $NO_y$ partitioning: The authors show all single $NO_y$ compounds in figures that are composed of daytime and nighttime data. I would understand the term "partitioning" rather like a percentage of a single species with respect to $NO_y$. This could be easily provided from the data but it is rather complicated to be done in the readers imagination.

2. The authors claim to show "diurnal cycles" of chemical species, especially for $NO_y$ compounds. However, these arguments can almost not be confirmed by the shown figures. They show e.g. zonal averages that are a mixture between daytime and nighttime data. (Fig. 1). This is not really meaningful for the short-lived species NO, $NO_2$, $N_2O_5$. "Spikes" appear at locations where nighttime data are missing, however this is no proof of a correct representation of the diurnal cycle. They would also appear, if e.g. the nighttime observations would be off by a factor of 2. Therefore a claimed day-night consistency can only be shown if both daytime and nighttime data agree. Moreover, with the presented data set they can only show the day-night differences (10h and 22h local time) but not a full diurnal cycle. But even this is hard to judge from the presented figures. Figures 2-4 are a composite of day and nighttime data and it is not easily possible for the reader to distinguish the daytime and nighttime satellite traces.

3. There is already a large discussion of the Antarctic ozone hole split in 2002, especially the March 2005 issue of J. Atmos. Chem. All these papers explaining dynamics and chemistry (besides Glatthor et al.) are not mentioned in the paper although at least some of them should.



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The coupling between stratosphere and tropospheric dynamics are investigated in great detail, e.g. the planetary waves forced in the troposphere (Newman and Nash; Krüger et al.,...) The fact (p.9904, 21ff) that the major stratospheric is reproduced well by the model (that is is nudged below 200 hPa to ECMWF data) is not surprising. It only shows that dynamical strat-trop coupling is represented well in the model.

4. The idea to derive the $NO_y$ production rate from MIPAS observations and some model quantities is new and interesting. These data are for 10:00 local time. However, it is also not clear what the reader learns from it when it is compared with model diurnal averages. There is qualitative agreement, however, the difficult test would be the direct quantitative comparison. Also, it is unclear what can be learned from figure 8.

5. p.9909, 10: The authors conclude that there is need for more laboratory work. The amount $N_2O_5$ inconsistency is, however hard to see due to the points made above. Some reactions (p.9908, 3) seem to reduce the discrepancy, but it is not clear whether these reactions are included in the presented model version. It is only said that they are not included in Jökel, 2006.

6. The large nuber of figures (and sub-panels) could be shortened to those figures that confirm the presented findings. Or some of them could be shown as a supplement.

## Minor Points

1. MIPAS NO data: There used to be difficulties in the retrieval of NO data during daytime. Have those problems been solved in the current data set?



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2. p.9903, 11ff: The zonal average is shown for the period of the vortex split where (as mentioned) south of about 50 degrees S the results should not be interpreted as typical. This comparison could be replaced by a similar one a few weeks earlier, where also the polar cap contains rather zonally symmetry. Jökel (2006) do not show all the nitrogen compounds. Or is this the only period where all $NO_y$ compound data are available?

3. p.9903, 18ff: Too low $N_2O$ can be seen in the figure but not a shift of the subtropical barrier. The latter may be a possible explanation for the low $N_2O$, but it is not clear, if there are other explanations.

4. p.9903, 25ff: The anti-correlation with $NO_y$ visible, but it is more difficult to recognize in the single $NO_y$ compounds.

5. p.9904, 15: mismatch between model and observations: Does that mean that the model is not evaluated at the observation location (tangent point)? Or is this an issue of model resolution? The exact location of the terminator should be not be problematic to model.

6. p.9905, 10: Yes the maps appear to be noisy. Therefore I would suggest to show daytime and nighttime separately.

7. p.9905, 14: The location of the vortex edge in figures 2-4 is not trivial to find, especially during this time of the vortex split. To follow this argument, it would be helpful to have the vortex edge over-plotted as a line.

8. p.9905, 17: denitrification in the vortices: denitrification is only seen in the Southern vortex in September, of course.

9. p.9905, 22: Too much mixing at the vortex edge: Indeed it looks like too much mixing through the vortex edge. The too diffusive vortex edge would imply that

not only $N_2O$, but also the $NO_y$ species are questionable inside and around the two vortex remnants.

10. p.9906, 5ff: The $N_2O$ difference is also visible at the equator in both shown altitudes. Could this be due to an under-estimation of ascent in the tropical air masses?

11. Figures 3 and 4: The figures receive the impression of a synoptic map. However, the shown locations of the observations are the tangent point locations for all data within 3 days. Especially during the chosen time with rapidly changing vortex location this is not ideal. The locations of airmasses change significantly during 3 days. I suggest either plotting the data points at "synoptic location" determined by a trajectory (as done in other publications) or to show only one day of data.

12. Figures 5 and 6 (especially 6) : Some of the scatter plots could be better, if the x and y range of the axes were adjusted to the parameter range. (fig5: NO; fig6: $HNO_3$, $NO_2$, NO, $N_2O_5$, $NO_y$). The x and y axes should have the same range but it should not necessary have zero as minimum. Probably interesting could be the scatter plots for [x]/[$NO_y$] to see how the model reproduces the $NO_y$ partitioning.

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