

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 #4

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General comments

The paper presents observations of NO_y species together with temperature and N₂O between 14 and 31 km taken by the MIPAS-B instrument on 21 March 2003 in Northern Scandinavia. ClONO₂, N₂O and temperature observations depict the meteorological situation consistent with analysis data. Thanks to the dedicated sampling approach and to the capability of balloon observations to observe the same air masses over a longer time period, the photochemical variations of NO₂ and N₂O₅ around sunrise could be measured with high temporal resolution. The observed temporal evolution of these species is compared to box modeling runs along backward trajectories. Par-

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ticularly the latter aspect provides interesting insights about the rapid photochemical inter-conversion of NO_y species and could thus contribute to improve our knowledge of photochemical schemes in atmospheric models.

The paper is well and concisely written and it provides new results. I recommend publication in ACP after addressing the comments listed below:

1. The authors state in Section 5.3.1 that model data differences with respect to temporal evolution of NO₂ around sunrise could be caused by a too slow model photolysis. However, it is stated in Section 5.1 that the box model output is written out hourly. If the model results are then interpolated to the measurement times (i.e. every 5 minutes), it is not surprising that NO₂->NO conversion appears to be slower in the model than in the measurements taking into account that the NO₂ chemical lifetime is in the order of minutes. An additional aspect is that NO₂ photolysis rates depend strongly on the UV-vis albedo. The authors should specify whether a constant albedo (i.e. 0.4) or a variable albedo in dependence of snow/cloud cover has been applied.

2. The pronounced disagreement of modeled and observed N₂O₅ profiles discussed in section 5.3.2 and shown in Figure 12 is striking, in particular when taking into account that the box model was initialized by the observed total NO_y and O₃. A downward shift of the N₂O₅ maximum of about 5 km in the model compared to the observations can hardly be explained by erroneous O₃ overhead columns as proposed by the authors. In this case, increased (decreased) N₂O₅ abundances would be expected at all altitudes for lower (higher) O₃ overhead columns. Given that N₂O₅ formation during night depends strongly on temperature it would be useful to compare the ECMWF temperatures used in the trajectory calculations with independent measurements such as provided by satellite observations (i.e. MIPAS-ENVISAT).

3. One of the conclusion of the paper is that the currently assumed chemistry affecting the N₂O₅, and to a lesser extent to NO₂, is "too slow". Do then the authors suggest that that chemistry should be revised? If so, I think this should be explicitly

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stated in the manuscript, in the conclusion section. Although, for that end, the authors should make sure that the points mentioned above are not the responsible for the model/measurements discrepancy and they should: a) check if the actual measured temperatures are different from ECMWF and hence if they could affect significantly both the absolute N₂O₅ values and its variation during the measurements period; and b) check the potential parameters affecting the photodissociation rates such as O₃ column above, albedo, and maybe also the cross-section temperature dependence.

Specific comments and Technical corrections

p. 4694. Abstract, l. 3. Please specify which "lower altitudes"

l. 5. I would delete "along the cross section", e.g., "... reveal the dynamics through the edge ..."

l. 11-12. I suggest to delete "in terms of quantity" (if it is no indicated the contrary, one would assume it is a "quantitative" agreement, as it is specified later).

"... slightly too slow..."? Do you mean "slightly slow"?

Introduction, l. 23. Insert a "," after "BrONO₂"

p. 4695 l8. "The reformation of NO₂ after sunset is about as fast as its photolytic dissociation after sunrise." This is valid for the middle and lower stratosphere, only.

p. 4696, l.6-7. I suggest to delete "a time period of"

p. 4696 Sec. 2, last par. I would suggest to change the order of the last two sentences. I think last sentence refers to first sentence of the paragraph)

p. 4696 l24: ... adjusted RELATIVE to the position of the sun

p. 4697 l27. : Suggest to change "mesospheric" -> "thermospheric". (Thermospheric radiative contributions to the measured spectra should be significantly higher than the mesospheric contributions).

p. 4699, line 7. "that periods" -> "those periods"

P. 4699, last line and first line in the page. I suggest rewriting:

In summary, the measurements taken at altitudes between 17 km and 21 km covered the edge of the polar vortex with strong horizontal gradients while weaker gradients could be expected above these altitudes,

p. 4701. First par. I think the point made about the small difference in the temperature minimum between outside and inside the vortex, just 1 K (by the way the temperature errors are not mentioned in the manuscript) is not much relevant. The major point, I think, it is that the temperature minimum are located at significantly different altitudes inside/outside the vortex, as mentioned later. I would reduce this paragraph. In any case I would not talk about a "MORE PRONOUNCED temperature minimum" when we are talking about 1 K difference.

p. 4701, l3. "Brewer Dobson circulation" -> "meridional circulation"

p. 4702, Sec. 4.3. It would be useful to comment on how the observed NO_y partitioning compares with other observations.

p. 4702, l24. Please insert a "," after "whereas" and after "VMR peak"

p. 4704, l. 18, climatologic -> climatological

p. 4705 I understand that the total NO_y used for initialization of KASIMA model (to be used later in the box model) has been inferred from the first MIPAS B "azimuth direction" only (e.g., Fig. 6). If so, a possible spatial variability of NO_y over the observed region would not be taken into account. Could this affect the model-data comparison?

p. 4708, l. 18, Insert a "," after "measurements"

Fig 2: To underlay a PV contour at 450 or 475 K indicating the vortex boundary around 19 km would be helpful in this figure. Also drawing a line separating the night and day sides would also be useful.

Fig. 3. Are the meridians in the figure the 0ž and 30žE ones?

Fig. 6. It would be useful to add different symbols to the different species. Some colors can be confused in some printings.

Fig. 11 and 12. I would suggest to show also the modeled/measured differences by difference plots, e.g. in percentage. This gives a better idea about the discrepancies. In order to not increase the number of figures, the left panels of the figures could be removed. I think they do not contain much information.

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

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