

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 #3

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General

Wiegele et al. present MIPAS-B measurements of NO_y compounds for several profiles within about 5 hours covering a sunset in the polar vortex on 21 March 2003. The presented data are a valuable data set, however, the interpretation of the shown data should be improved before this paper is acceptable for publication in ACP. Especially the figures should be improved. Also, the shown discrepancy between the model and the observations must be investigated and explained better. If the points below have been addressed, especially major point 4, this will be a valuable paper.

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Major points

1. page 4701, line 18ff, subsidence of about 1 km: Seen in the figure is an altitude shift of the N_2O contour by 1 km between the first and the last observed profile. It would correspond to subsidence only if (i) the first profile is well outside the vortex and (ii) if no mixing across the vortex edge would have taken place. The observed N_2O contour should be interpreted as a **lower limit** for the subsidence in the polar vortex.
2. page 4701, line 25ff, mesospheric intrusions: Müller et al., (JGR, 2007) clarified, that the air with the lowest N_2O mixing ratios is rather unmixed air originating from the upper stratosphere. The mesospheric origin of the air masses is visible higher up ($\approx 24\text{-}27$ km).
3. page 4705, line 7ff: Is the normalisation factor close to 1 or significantly different? If it would deviate from 1, non-linearities may become important.
4. The discrepancy between observation and model NO_2 and N_2O_5 is only interpreted qualitatively. The statement that the “model chemistry is too slow” (abstract, l. 14 and p. 4709, l. 4) is very vague and should be quantified and explained better. Most important for the NO_2 decomposition at sunrise is the O_3 mixing ratio and the NO_2 photolysis rate. Model O_3 could be compared with the MIPAS-B observations and NO_2 photolysis is also rather constant with altitude (see e.g. Stolarski, 1995, Scientific Assessment of the Atmospheric Effects of Stratospheric Aircraft, NASA Ref. Publ. 1381, 1995, or Becker et al., J. Atm. Chem., 37, 217-229, 2000.). Thus in principle, the NO_2 decomposition at sunrise should be easy to model and reasons for discrepancy should be provided in a study like this. In the rather un-complex model like the used model, sensitivity studies with respect to the uncertainty of the relevant kinetic parameters would be a good way to investigate this discrepancy.

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Minor points

1. page 4695, line 1: The words “fast” and “slow” in a scientific publication are only meaningful if compared to a certain value.
2. To me it was confusing to read that the MIPAS-B flight was on March 21, 2003 that is one day after also a MIPAS-B flight are published (e.g. Engel et al., 2006). It seems that these are different data. Please confirm that the given date is not a typo and mention the other flight. It would be interesting to see how the two flights compare.
3. p. 4702, l. 24ff: It is not clear how “vertical NO_y redistribution” can be seen from this plot.
4. p. 4704, l. 16ff: Not much is said how the photolysis rates are interpolated to the altitude and zenith angle of the trajectories. This detail may be important as during sunrise the photolysis rates change quickly over orders of magnitude and a not sophisticated interpolation may cause errors especially near sunrise and sunset.
5. Figure 1: Also important are the thermal decomposition of N_2O_5 and HO_2NO_2 . The main product of ClONO_2 photolysis is NO_3 , not NO_2 .
6. Figure 3: It would be elucidating to see the 7 tangent point locations of the observations at the nearest corresponding altitude over-plotted, not only the location of Kiruna. With that the reader would get a better impression of which data are inside or outside the vortex.
7. Figures 4/5 and 7-10: The horizontal gradient in the figures is difficult to read from the color scale. It would be better to complement these figures by a time series (quantity vs. time) for a chosen interesting altitude, e.g. 20 km. Would it

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be possible to add vortex edge after Nash et al. similarly as the sunrise line or PV or equivalent latitude?

8. The above argument also holds for figures 11 and 12. It is very difficult to judge over agreement and disagreement in a quantitative way from these figures, since differences may be hidden in the color contrast or may appear exaggerated depending on the choice of the color scale.

Technical Corrections

1. Abstract line 2: change to “spatio-temporal” or “spatial and temporal”
2. p. 4704, l. 17/24: At this paragraph, it is not yet clear what trajectories or trajectory levels are, since this is explained in the following section.
3. p. 4704, l. 8 (and other places of the paper): change “sunlit” to “sunlight”
4. p. 4705, l. 18: change “with” to “from”
5. p. 4706, l. 2: change to “. . . ending at the tangent points. . .”
6. p. 4706, l. 19: change to “. . . to include the box model results for the simulated period.”

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

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