

***Interactive comment on “Mesospheric N<sub>2</sub>O  
enhancements as observed by MIPAS on Envisat  
during the polar winters in 2002–2004” by  
B. Funke et al.***

**B. Funke et al.**

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We thank Reviewer #1 for his/her very helpful comments and suggestions that have significantly improved our manuscript. The "Reviewer Comments" are noted first and then we give our "Reply:" to the comment. We are submitting a revised manuscript that includes all the actions noted below.

The figures in the originally submitted version were much larger than the ones in this final version. Unfortunately, this change resulted in the current figures being extremely difficult to read. I had to magnify my file to about 150% (250% for Figure 1) to read the information in the figures. Thus, I strongly recommend that the authors modify all of the figures to make them more legible.

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Reply: We fully agree with the reviewer. We were expecting most figures to appear as "two column" figures, which would make them larger and more readable. With respect to the final version, we will take care that the figures are at their original size included in the submitted manuscript.

Page 10566, line 21: The authors note that regularization effects in the retrieval might lead to an underestimate of the altitude of peak N<sub>2</sub>O mixing ratio. Because of the very low vertical resolution, could the peak altitude also be overestimated?

Reply: The application of MIPAS averaging kernels to CMAM N<sub>2</sub>O model simulations during the "Halloween" SPE (see Figure 6 of Funke et al., 2008) showed that the retrieved N<sub>2</sub>O peak altitude is lowered by approximately 10 km. This is caused by the regularization having a stronger effect at higher altitudes. Thus it is very unlikely, that the N<sub>2</sub>O peak altitude is overestimated by MIPAS.

Page 10566, line 25: The authors conclude that an anti-correlation between N<sub>2</sub>O and CH<sub>4</sub> suggests that a dynamical origin of N<sub>2</sub>O enhancements is unlikely. I am confused by this assumption. If, for instance, the N<sub>2</sub>O observed near 60 km were produced near 90-100 km and then descended, wouldn't the enhanced N<sub>2</sub>O correlate with low CH<sub>4</sub>? So in this case, the enhancement would have a dynamical origin? If I am just misinterpreting what is meant by "dynamical origin", I recommend that this be clarified. This reasoning is seen again on page 10568, lines 14-16, so I assume I am just missing part of the logic or misinterpreting some of the words.

Reply: The reviewer is right that this statement is misleading. Our intention was to express that the dynamical redistribution of N<sub>2</sub>O produced by the "standard" tropospheric source wouldn't lead to an anti-correlation of N<sub>2</sub>O and CH<sub>4</sub>. Thus, instead of stating at page 10566, line 25, that a dynamical origin of N<sub>2</sub>O enhancements is unlikely, we state now that the observed anti-correlation between N<sub>2</sub>O and CH<sub>4</sub> cannot be explained by dynamics without invoking an upper atmospheric chemical source of N<sub>2</sub>O. In this sense, we have also rewritten I5-6 of the abstract, where a similar state-

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ment was included. At page 10568, lines 14-16, we have changed "of chemical origin" to "related to an upper atmospheric source".

Page 10567, line 23: The authors describe changes in CH<sub>4</sub> that were induced by transport. They go on to describe "transport-generated N<sub>2</sub>O enhancements", but note that these appear later and at lower altitudes than the CH<sub>4</sub> changes. I do not understand this; if the N<sub>2</sub>O changes are transport generated, why do they not coincide with the CH<sub>4</sub> changes?

Reply: The reviewer is right in assuming that transport-generated N<sub>2</sub>O enhancements should coincide with CH<sub>4</sub> changes, and so they do. However, these N<sub>2</sub>O increases are less pronounced than those of CH<sub>4</sub> (and thus hardly visible in Figure 2) at higher altitudes due to the non-linear N<sub>2</sub>O-CH<sub>4</sub> relationship. In order to avoid any misinterpretation, we state in the revised version "are less pronounced at altitudes above 50 km" instead of "appear later and at lower altitudes".

Page 10569, line 3: While the MIPAS data only showed the N<sub>2</sub>O descending to 45 km, I think it should be noted that observations ceased at this time; so any descent below 45 km would not have been observed.

Reply: This is a good point. We now have added the sentence: "Unfortunately, a further evaluation of the N<sub>2</sub>O temporal evolution was not possible since MIPAS observations ceased on 26 March 2004 due to an instrumental failure."

Figure 6. This figure would be much easier to understand if the months were spelled out on the horizontal axis (e.g., labeling months 1-12 of each year), rather than having continuous months since June 2002. This point was apparently recognized by the authors, who have parenthetically cross-referenced the months in the figure to the months in the text, but it would be much more straightforward if this were not necessary.

Reply: We have changed the x-axis labels of Figure 6 such that months and year are spelled out.

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Page 10573, line 25: It is stated that the MEPED measurements are compromised by the presence of protons; please provide a reference for this. Reply: We have added the corresponding reference to Evans and Greer (2000) where they stated: "The electron detector telescopes are also sensitive to protons entering through the collimator with enough energy to pass through the nickel foil and into the detector...Care should be taken in interpreting observations from the electron detector telescope at times when the proton telescope sensor response indicates large fluxes of protons in the  $>200$  keV energy range."

Page 10573, line 26: Please also provide a reference for the statement that SPEs are thought to be associated with elevated electron fluxes inside the polar caps.

Reply: The distortion of the magnetosphere due to solar energetic particles during the October - November 2003 SPE, leading to enhanced electron precipitation, is discussed for instance in Baker et al. (2004). We have now included this reference.

Page 10576, line 14: Should this say "4 ppmv" instead of "4 ppbv"?

Reply: The reviewer is right. We changed the text accordingly.

Page 10580, line 2: The reference to Semeniuk et al. 2007 is not in the reference list.

Reply: The Semeniuk et al. 2007 paper was still in review when submitting our manuscript. For this reason, the reference was provided by a footnote on page 10563 in the ACPD version. Since this paper has been recently accepted for publication in JGR, we have now included Semeniuk et al. (2008) in the reference list.

Page 10580, line 18: "...most elevated fluxes of  $a > 100$  keV electrons"; What is "a"?

Reply: The "a" should be omitted. Since the "a" was not included in the submitted version, it seems to be related to a copy-editing error during the html-conversion. We will take care of it during the galley proof reading.

Conclusion: I would love to see a sentence added here (and to the abstract) that gives

an estimate for the total fraction of stratospheric N<sub>2</sub>O that comes from the mesosphere. I assume this must be very small, but I really don't have a very good idea of the number. I think this should be possible to estimate from the MIPAS data and analysis done here. This is also relevant to the issue of whether, and over what altitude/latitude regions, N<sub>2</sub>O can be used as a tracer.

Reply: A direct evaluation of the total fraction of stratospheric N<sub>2</sub>O produced in the mesosphere from MIPAS observations is rather difficult since the mesospheric N<sub>2</sub>O which has descended into the middle or lower stratosphere during the winter can hardly be separated from the much higher background N<sub>2</sub>O abundance. Instead, we have chosen an indirect way to assess this number: Given that at 50 km, none of N<sub>2</sub>O and NO<sub>y</sub> is produced in situ by EEP and none of them show significant photochemical losses during polar winter, their ratio should remain constant while being transported to lower altitudes. In consequence, we can apply the N<sub>2</sub>O/NO<sub>y</sub> ratio observed at 50 km to the EEP-generated stratospheric NO<sub>y</sub> amount, which is much easier to estimate from the observations (e.g. Funke et al., 2005). In this way, we have estimated N<sub>2</sub>O depositions of around 0.05 Gigamole during the "strong" polar winters (Antarctic 2003 and Arctic 2003/04), which makes about 0.2-0.7% (4-8%) of the total stratospheric N<sub>2</sub>O above 20 km (30 km) inside the polar vortex. However, the total fraction of EEP-generated N<sub>2</sub>O to the stratospheric N<sub>2</sub>O on a global scale is negligible. In this sense, we added the following paragraph to the conclusions: "The total fraction of stratospheric N<sub>2</sub>O produced in the upper atmosphere is difficult to assess directly from MIPAS observations since the EEP-generated N<sub>2</sub>O which has descended into the middle or lower stratosphere during the winter can hardly be separated from the much higher background N<sub>2</sub>O abundances. However, it can be estimated indirectly from the deposition of EEP-generated NO<sub>y</sub> into the stratosphere (Funke,2005), assuming a constant ratio of N<sub>2</sub>O and NO<sub>y</sub> of upper atmospheric origin below 50 km. This is justified since at these altitudes none of N<sub>2</sub>O and NO<sub>y</sub> is produced in situ and none of them show significant photochemical losses during polar winter. The estimated fraction of EEP-generated N<sub>2</sub>O to the total stratospheric N<sub>2</sub>O inside the polar vortex above 20

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km (30 km) never exceeds 1% (10%) during the 2002 - 2004 winters. Compared to the global amount of stratospheric N<sub>2</sub>O, the EEP-generated contribution is negligible." The last two sentences have also been included into the abstract.

Technical Corrections: All typos have been corrected. Thank you very much.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10561, 2008.

**ACPD**

8, S6130–S6135, 2008

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