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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of NO_x in the polar middle atmosphere from October 2003 to March 2004: vertical transport versus local production by energetic particles" *by* M. Sinnhuber et al.

Anonymous Referee #2

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General Comments The authors investigate the impact on the atmosphere of energetic particle precipitation during the October 2003 to March 2004 time period using MIPAS observations of NOx and CO. They conclude that the NOx enhancements observed in the Arctic upper stratosphere and lower mesosphere during January-February of 2004 were caused by descent of NOx produced in the upper mesosphere and lower thermosphere by low (e.g., auroral) energy electrons, not by solar protons or higher energy electrons. This contradicts previous reports that direct production of NOx below 70 km was responsible for a significant portion of the observed Arctic NOx enhancement.

The paper is well written and organized. Improved understanding of the atmospheric impact of radiation belt electrons, and how this compares with lower energy electrons and solar protons, is an important contribution to the science community. I recommend publication after the following, mostly minor, comments are addressed.

Specific Comments p6, line 1. For people who wish to repeat any of this work, it would be helpful to cite a reference for the averaging kernels.

p10, line 8. Will transport and mixing really act on CO and NOx in the same manner? Doesn't this depend on vertical and horizontal gradients in the two constituents, which are not the same?

Figure 6. I suggest adding color scales that denote the dates of the various colors in the two panels.

p10, lines 16-21. On the basis of the CO/NOx relationship, the authors conclude that the high values of NOx below 70 km in late January are most likely due to subsidence of air, rather than direct production. I believe that the rationalization is that since the NOx values in air that had descended to 70 km were already high by 19 January – before the storms – there is no need to invoke a direct production source to explain them. This was not obvious at first reading, though, so if I am correct, a more explicit discussion would be helpful. Particularly important is to point out that even though the high CO / high NOx correlation *by itself* cannot rule out direct production (since the production could have occurred in air that had already descended), the fact that high NOx existed in the descended air prior to the geomagnetic storms is sufficient justification for the conclusion. (Of course, if that is not what is meant, the discussion should be modified accordingly).

p10, last paragraph. If photochemical loss of CO moves the NOx-CO pairs to the left, can't this result in the appearance of points above the secant line? Related to this: What do the vertical "error" bars in Figure 6 denote? Also with regard to Figure 6: The red and orange data (at least; some other points are hidden) have a very peculiar

shape – I think the authors should comment on this, if only to ensure that they are not overlooking some important physics that would affect their conclusions.

p11, lines 19-22. The most significant conclusion from this paper is that the NOx enhancements observed in the lower mesosphere in Jan-Feb 2004 were not caused by direct production of NOx, as suggested previously based on an analysis of GOMOS data. This conclusion relies not only on the evidence from MIPAS, but also on the ability of the authors to show that the previously published interpretation of GOMOS data was incorrect. The authors suggest here that had the GOMOS analysis taken into account sampling biases with respect to the "moving wave 2 structure", an NO2 enhancement would not have been found. Because contradicting the GOMOS data interpretation is fundamental to their overall point that the enhancements were not caused by direct production (e.g., that the current interpretation of MIPAS data is more correct than the previously published interpretation of GOMOS data), I think the authors should support their speculation more strongly. In particular, they should show that the GOMOS sampling in Jan-Feb 2004 would indeed have led to a bias that would have resulted in a misinterpretation of the data.

p12, first paragraph. The authors should be more precise when discussing the problems with analyzing NOx: What is meant by "cross-talk"?

p13, lines 1-3 (and perhaps on previous page). This refers to NOx and the lower left panel of Fig. 8, which only shows NO. The text should be clarified.

p14, lines 13-14. The authors refer to the upper panel of Figure 10, but there is only one row of panels in Figure 10. Since Figure 10 pertains to the same situations as Figure 8, it would be convenient if both figures were formatted the same.

Finally, I was surprised that several references on the 2003-2004 winter, which I believe are relevant to the current paper, were not cited. I recommend that the authors consider the following papers:

1. Hauchecorne, A., J.-L. Bertaux, F. Dalaudier, J. M. Russell III, M. G. Mlynczak, E. Kyrölä, and D. Fussen (2007), Large increase of NO2 in the north polar mesosphere in January–February 2004: Evidence of a dynamical origin from GOMOS/ENVISAT and SABER/TIMED data, Geophys. Res. Lett., 34, L03810, doi:10.1029/2006GL027628.

2. Pancheva, D., et al. (2008), Planetary waves in coupling the stratosphere and mesosphere during the major stratospheric warming in 2003/2004, J. Geophys. Res., 113, D12105, doi:10.1029/2007JD009011.

3. Randall, C. E., et al. (2005), Stratospheric effects of energetic particle precipitation in 2003–2004, Geophys. Res. Lett., 32, L05802, doi:10.1029/2004GL022003.

4. Semeniuk, K., J. C. McConnell, and C. H. Jackman (2005), Simulation of the October–November 2003 solar proton events in the CMAM GCM: Comparison with observations, Geophys. Res. Lett., 32, L15S02, doi:10.1029/2005GL022392.

5. Seppälä, A., M. A. Clilverd, and C. J. Rodger (2007), NOx enhancements in the middle atmosphere during 2003–2004 polar winter: Relative significance of solar proton events and the aurora as a source, J. Geophys. Res., 112, D23303, doi:10.1029/2006JD008326.

Minor Technical Corrections

p3, l28. Add "and" before "by Clilverd. . ."

p5, l25. Add "and" before "about CO. . ."

p6, l12. "combining"

p6, l15. "available" instead of "provided"

p6, l22. Remove the comma after "21".

p9, l2. "off in late. . ." not "of in late. . .".

p9, l5. "on the order. . ." not "in the order. . ." [and anywhere else this appears]

p9, l6. "sunlit" (no hyphen) [and anywhere else this appears]

p9, l8. "lifetime" and "sunrise " (no hyphens)

p9, l25. "development" not "developement"

p10, l10. "observed by the MIPAS NOM data". I'm not sure what exactly was meant here, but it can probably be changed to just "observed by MIPAS".

p10, l19. "seems" rather than "turns"

p10, l21. Depending on how you handle the above comment, "NOM" should be defined here.

p11, l12. "continuously"

p11, l20. "stellar" instead of "star" (and no hyphen)

p11, l21. Remove "a" before "NO2"

p12, l14. Add "and" before "NO2".

p14, l24. Either "Exceptions are" or "An exception is"

Figure 4 caption. "rate" not "ratef" in the last line

Figure 9 caption. "violet" not "violett"

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