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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancement of N_2O during the October–November 2003 solar proton events" *by* B. Funke et al.

Anonymous Referee #2

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General Comments: This paper discusses observations from the MIPAS instrument that show enhanced levels of N2O in the upper stratosphere and lower mesosphere after the solar proton events (SPEs) of late October/early November 2003. Using calculations from the Canadian Middle Atmosphere Model (CMAM), the authors conclude that the N2O was formed by reaction of N(4S) with NO2. The paper is well written and addresses a timely topic that is important for our understanding of the stratospheric odd nitrogen (and thus ozone) budget. I have just a few minor comments and a concern about the quantitative interpretation of the results.

Specific Comments: Page 4670 near line 25. It would be appropriate to add a reference to the ACE data here [Rinsland, C. P., C. Boone, R. Nassar, K. Walker, P. Bernath, J. C. McConnell, and L. Chiou (2005), Atmospheric Chemistry Experiment (ACE) Arc-

tic stratospheric measurements of NOx during February and March 2004: Impact of intense solar flares, Geophys. Res. Lett., 32, L16S05, doi:10.1029/2005GL022425].

Page 4673, top: The retrievals are done in a sequential manner, with N2O and CH4 retrieved last. I would like to see a comment about the species separation errors that might be significant for N2O - what is the magnitude of the errors in the retrievals of other constituents, and what errors do they lead to in the N2O retrievals, particularly if a bias is expected? This might be discussed in Glatthor et al., but it is directly relevant to this paper, so a summary would be helpful. Along similar lines, a short statement defining the "total error" (line 11) would be helpful.

Page 4673, line 23. Please state why two different versions of NO2 are used (_9 and _11) - do these pertain to different time periods, for instance?

Page 4675, near line 10: The authors assert that the spatial correlation between N2O and NO2 supports their explanation of N2O being produced by the reaction of NO2 and N(4S). Certainly a qualitative comparison of figures 1 and 2 supports this conclusion. I fail to see a strong correlation in Figure 3, though, so I question the value of this figure. I would prefer to see a quantitative analysis of the data in Figures 1 and 2 - even something as simple as a scatter plot, with a correlation coefficient reported, and some discussion of locations or times when the correlation is perhaps not as robust (e.g., 20031031 and 20031111).

Page 4675, line 25: Here the authors speculate that there is an indication of aurorally enhanced N2O in Figure 4 on 26 October. I assume they are referring to the region of light blue/green extending above 55 km near the pole, narrowing to around 52-55 km near 60 deg latitude. I agree that this is beyond the focus of the current work, and that it is appropriate to include just a brief speculation about the cause. I question their speculation, however, since the enhancement, if it is an enhancement over normal conditions, occurs at such low altitudes. Auroral precipitation would occur above about 100 km. I recommend that the authors check to see if photochemical lifetimes and

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descent rates at this time are compatible with descent from ˜100 km to ˜60 km. Another possibility is that it is due to higher energy particle precipitation, which would affect the upper mesosphere directly, if such particles were available before 26 October.

Page 4675, line 25 (same as above): In addition to the above comment, but related more to the overall conclusions of the paper, it would be interesting to see a plot similar to Figure 4 for NO2, especially for 20031026. The 20031026 panel in Figure 2 suggests that there were no NO2 enhancements on this date at 58 km, calling into question the proposed mechanism for forming N2O, unless the 26 October N2O "enhancements" are actually due to a tropospheric source. Perhaps this is just an issue of color scales, or chemistry that might lead to a poorer correlation between N2O and NO2 at lower altitudes, or retrieval errors. In any event, I think it is important that the authors comment quantitatively about this apparent contradiction.

Page 4678, line 25. Here the authors state that below 70 km, nighttime NO2 is a good proxy for NOx. This statement contradicts the model results in Figure 7, which show that at 60 km (for example), NO mixing ratios that are more than half as large as NO2 mixing ratios.

Page 4678, line 28. The authors should quantify "rather good agreement". Even just considering the first few days after the SPE, it looks like the level of agreement varies with altitude and time, and in some cases differences are as much as 40-50%.

Page 4679, line 3. The authors state that the model overestimate of NO2 from 55-65 km is consistent with the "slightly larger values" of the predicted N2O enhancements in this altitude region. Again, this should be quantified, as should the overestimate in the N2O predictions (which are hard to infer quantitatively from Figure 5). Is there a one-to-one correspondence between errors in simulated NO2 and N2O? If so, this would suggest an over-prediction in N2O of around a factor of 2 near 60 km (based on this difference between the model and observed NO2 in Figure 7), which is not "slight".

Figures: In a printed version of the paper, the text labels on the figures are small and

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quite difficult to read. I would appreciate it if the authors made them larger. There is some superfluous information that can probably be removed from the figures themselves and simply incorporated into the caption (e.g., "N2O (NO2) night 58.0 km" in Figures 1 and 2, etc.).

Figure 4 caption: I suggest adding "in panels b-d" after "The enhancement of N2O at high latitudes above 40 km is evident".

Figure 6: The caption should state whether or not the model results include the MIPAS averaging kernel (I assume they do).

Figures 5-7: There is some information at the top of the panels that can probably be removed in order to make the labels bigger (e.g., IMK...spe..."jtab").

Technical Corrections: Page 4672, line 23: Change "being" to "to be". Page 4675, line 11: A comma needs to be added: "...and NO2, which indeed is very...." Page 4675, line 23: Change "fews" to "few". Figure 7 caption. Remove "the" before "November 2003".

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4669, 2008.