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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Composition changes after the "Halloween" solar proton event: the High-Energy Particle Precipitation in the Atmosphere (HEPPA) model versus MIPAS data intercomparison study" *by* B. Funke et al.

Anonymous Referee #2

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This paper describes an inter-comparison study aimed at evaluating the performance of different models in simulating the observed atmospheric composition changes generated by Solar Proton Events (SPE). The SPE chosen for this study are those that occurred during October/November 2003. These were some of the largest events to be observed in the past forty years. The MIPAS instrument aboard the Envisat was operational during these events and it has measured the changes in the abundances of chemical constituents in the high latitude upper atmosphere following the "Halloween"

SPE. These data are compared with the results from 10 different models. All models use the same input information for ionization rates, thereby assuring the same perturbing influence for all the models. Observed and calculated changes in the composition immediately following the SPE as well as those occurring after a few weeks are shown and discussed. The topic of this study is well suited for ACP. This paper is well written and it describes various aspects of the study adequately. However, a few items, in my opinion, need further clarification. I recommend publication of this paper after the following comments are addressed.

1. The descriptions of the models do not specify exactly when the model runs begin. The time evolution is shown to begin on Oct 26. How long was the spin-up period? The pre-SPE distribution could be important for some of the species, as mentioned in the section dealing with Chlorine species.

2. As mentioned in the conclusion section (page 9463), the use of family approach in the chemistry formulation leads to some limitations for the models. It would be better to include in Table 3 information about whether a model treats the species separately or whether it uses the family approach. Species such as H_2O_2 in polar night conditions and N(^{4}S) under cold temperatures have longer lifetimes and may have to be treated as individual transported species.

3. It is mentioned that low values of the Averaging Kernel (AK) do not hint at a large a priori content of the MIPAS IMK/IAA retrieval (Page 9414, line 28). Equation (1) (Page 9432, line 19), which explains the adjustments to the model results, seems to suggest otherwise. It would be clearer if a sentence were added to say, "This adjustment procedure yields species profiles that MIPAS would see if it were to sound the model atmosphere".

4. NO_y enhancement profiles shown in Figure 12 are area weighted over the latitude range 40N – 90N. At 1hPA the models overestimate the NO_ycompared to MIPAS

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data. It would be interesting to look at a similar plot but averaged over 70N-90N. Most of the plots for other species show averages over 70N-90N. Significant proton ionization rate at 1 hPA occurs at latitude north of 55N on October 28 (Figure 5). If the NO_y enhancement is area weighted over the latitude range 70N-90N, the model overestimation at 1 hPa maybe even larger (based on the distribution shown in Figure 14). This may add more weight to the suggestion (Page 9462, line 14) that the discrepancy in NO_y enhancement as well as in some other species could be related to uncertainties in the simulated ionization rate profile used as input.

5. The upper boundary in some of the models is located in the mesosphere, very much in the altitude range of interest to this study. At least in one case, it is mentioned that the mixing ratios of the chemical families at the upper boundary are fixed. This does not appear to be a valid boundary condition for NO_y or HO_x when strong ionization due to SPE and electrons occur.

6. While discussing the stronger decrease of ClO towards the polar night (70N-90N) in the models during the pre-SPE conditions, it is suggested that the fast conversion of ClO in the models could be related to the reaction path ClO + OH ♢ HCl + O_2 (Page 9458, Line 28). In the pre-SPE conditions, there won't be any OH in the polar night latitudes and the OH levels will be very low near the terminator outside the polar night. Conversion of ClO to HCl through this reaction path is doubtful. At 2 hPa, conversion of ClO to $ClONO_2$ is possible given the longer nighttime (or polar night conditions). There should be enough NO_2 available even in the pre-SPE conditions and this could partly explain the decrease in the simulated ClO.

Minor (Technical) Comments:

Page 9410, Line 4: 'cause' instead of ' causes'

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Page 9413 Line 19: delete 14.3 times a day (either 14 or 15 ; only an integer makes sense for specifying the number of equatorial crossings per day)

Page 9416 Line 11: Corrections for Line of sight variations of NO_x partitioning – Are these corrections sensitive to large enhancements in NO_y caused by STE?

Page 9418 Line 13: Is the vertical resolution same below and above 2 hPa?

Page 9431 Line 24: 'extended' instead of 'extend'

Page 9433 Line 2: The latter case is without Averaging Kernel. The former case is with AK and this is the case with broader peak at lower altitude.

Page 9434 Line 4: 'troposphere only' instead of 'troposphere, only '

Page 9434 Line 11: 'period of interest' instead of 'period of interested'

Page 9450 Line 14: 'Additionally' instead of 'Aditionally'

Page 9453 Line 22: 'despite the relatively high NO_y ' instead of 'despite of the relatively high NO_y'

Page 9459 Line 18: 2ppbv increase in active chlorine? Maybe it is 0.2 ppbv. Please check.

Page 9461 Line 7: $ClONO_2$ decrease occurs on November 13 not 16^{th}(See Page 9460, Line 14, also Figure 36)

Page 9463 Line 25: .... in the simulations 'cause' (instead of causes)

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9407, 2011.