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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.

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We thank Referee #2 for helpful comments and suggestions. The "Referee's Comments" are noted first and then we give our "Reply:" to the comment.

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.

Reply: We agree that the models' background states are largely determined by their respective initializations and spin-up periods. For this reason, the corresponding information for all models involved in this study will be included in Section 4 of the revised version.

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 H2O2 in polar night conditions and N(4S) under cold temperatures have longer lifetimes and may have to be treated as individual transported species.

Reply: A family approach is used in the following models: B2dM (below 50 km), B3dCTM, FinROSE, and CAO. This information has been included in Table 3. Additionally, a short description on which species are calculated using the family approach has been added to the descriptions of the models mentioned above (Section 4). We agree that the use of a family approach might introduce significant errors for longer-lived species. Such species (i.e., H2O2, ClONO2, HNO3, N2O5, etc.) are treated as families only in CAO. Larger differences of these species in CAO compared to the observations and other models might be related to the use of a family approach for longer-lived species. When appropriate, this reason has been added in the revised version as a possible explanation for larger mismatches of CAO results and observations. For instance, regarding the N2O5 enhancements simulated by CAO until 4 November caused by seasonal variations (discussion of Fig. 21), we state in the revised version: "The overestimated seasonal N2O5 buildup in this model is most likely related to the use of a family approach for NOy". In the discussion of Fig. 33 (top of page 9458), we state: "The reason for the higher background ClO concentrations in this particular model is most probably related to the use of a family approach for ClOy"; and regarding the temporal evolution of modeled ClONO2 enhancements (discussion of Figure

36, page 9460) we have added: “As an exception, CAO yields a quasi-instantaneous ClONO2 increase with the onset of the proton forcing which can be explained by the use of a family approach for NOy and ClOy in this model”.

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”.

Reply: We state on page 9414 that low values of the AK diagonal elements (not the whole AK matrix) do not hint at a large a priori content. A measure of the a priori content at a given profile point would be the sum over the corresponding AK row which should be close to unity if the a priori content is small. In this case, which is typical for IMK/IAA MIPAS retrievals, only a small a priori contribution is added to the model results by application of Equation (1). Nevertheless, we fully agree on the suggested addition “This adjustment procedure yields species profiles that MIPAS would see if it were to sound the model atmosphere”, which is very illustrative. This sentence has been added after the introduction of Equation (1) in the revised manuscript.

4. NOy enhancement profiles shown in Figure 12 are area weighted over the latitude range 40N – 90N. At 1hPA the models overestimate the NOy compared to MIPAS 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 NOy 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 NOy enhancement as well as in some other species could be related to uncertainties in the simulated ionization rate profile used as input.

Reply: The reviewer is right. NOy enhancement profiles area-weighted over the latitude range 70-90N show an even more pronounced model overestimation around 1 hPa (around 50%). An additional Figure similar to Fig. 12 but for 70-90N will be added and discussed in Section 6.2 of the revised manuscript.

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 NOy or HOx when strong ionization due to SPE and electrons occur.

Reply: Fixed upper boundary conditions for NOy and HOx are used only in the case of the CAO model. This model, however, covers the vertical range up to 90 km. Since SPE-induced ionization occurs mainly at altitudes below 80 km, upper boundary effects of this particular model are unlikely to perturb the results presented here.

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 + O2 (Page 9458, Line 28). In the pre-SPE conditions, there wonÊijt 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 ClONO2 is possible given the longer nighttime (or polar night conditions). There should be enough NO2 available even in the pre-SPE conditions and this could partly explain the decrease in the simulated ClO.

Reply: We agree that the proposed reaction path ClO+OH -> HCL+O2 is highly speculative and for this reason we do not exclude other possible causes. A stronger conversion of ClO to ClONO2 in the models, however, can be excluded as possible reason for the lower ClO availability in the models before the SPE, since modeled pre-SPE ClONO2 abundances are generally smaller than those observed in the region of interest. This is already stated on page 9458 (line 25).

Minor (Technical) Comments:

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

Reply: The correction scheme for line of sight variations of the NOx partitioning close to the terminator takes into account the fast photochemical conversion of the species NO and NO2 at high solar zenith angles, particularly at lower altitudes (i.e., below 40 km) where this conversion results in large horizontal gradients. This correction, however, is not sensitive to horizontal variations of total NOx (or NOy), which, in turn, are corrected for in the retrieval by means of a joint fit of latitudinal and longitudinal vmr gradients.

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

Reply: The vertical resolution of N2O5 above 2 hPa is 9-10 km. This has been corrected in the revised version.

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.

Reply: The reviewer is right. The text has been changed accordingly.

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

Reply: The reviewer is right. In the revised version, we state “resulting in a net increase of active chlorine by approximately 0.2 ppbv. . .”

All other typos encountered by the Reviewer have been corrected.

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