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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Energetic particle precipitation in ECHAM5/MESSy – Part 2: Solar Proton Events" *by* A. J. G. Baumgaertner et al.

Anonymous Referee #1

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General comments: —

The authors present modelling results for the October 2003 solar proton event (SPE) and compare those with observations by the MIPAS/Envisat instrument. The paper needs a major revision.

A new and interesting aspect in the work is the use of the drastically modified parameterization to model N/NO production during the SPE. Although the authors show that it leads to improvement of model N2O when compared to MIPAS, a clear scientific justification is missing (see the detailed comments below). The authors should carefully address this matter before the article is published.

Detailed comments: —

Page 4507, lines 3-8. There are differences up to 100% between the two ionization rate profiles even after the adjustment of the parameters. Is this similar for other times that those shown? When looking at NOx production on a longer time scale, it might be useful to check if the ionization rates integrated over the whole event are similar to not.

Page 4508, lines 6-7. What does "almost identical" mean? The authors should state the differences.

Page 4508, lines 16-17. The different baseline versions might introduce significant differences (or improvements) for certain gas products. Does this affect the results? A line or two about this matter would be appropriate.

Figure 4 and those similar. What is shown in the figure? An average of 60-90N observations/model results, OK. But are they day or night data, or both? Any solar zenith angle limits? If both day and night data are used, how does this affect the results (the response to SPE can vary diurnally). The caption should be much more informative.

Page 4509, lines 8-9. The authors state "excellent agreement", but this is not based on Fig. 1 surely. It would be better to give some numbers, e.g. the difference is on average XX% or something similar would be more useful than the qualitative statements alone.

Page 4509, line 14-15. Again here, please define "small".

Page 4510, lines 6-7. There are clear differences in NO2 between MIPAS and EMAC. Between Oct 30 - Nov 5, the model clearly overestimates NO2 production, especially at 60 - 70 km by 100%. Also, the change extends maybe 5 km lower in the modelling. The situation is reversed in Nov 5 - Nov 10. What are the reasons for this behavior? After that, the "contamination" due to transport from the upper mesosphere makes the comparison all but straight forward. The text should be revised.

The authors should tell the reader more about MIPAS observations. Especially, the precision and accuracy of the observations should be given for each of the gases presented. This is very important because it will put into a proper context the differences that exist between the model and the observations. Also, I assume that the data used are not the ESA's official MIPAS products, the origins should be clearly stated.

Page 4510, lines 14-15. Again, "by far". The authors should be more quantitative. The red color in Fig. 5 is saturated so that it is not clear what the model result actually is.

Figs 5 and 6. There seems to be a very clear discontinuity between the uppermost and the one-below levels of the model. How sensitive is the top level result to the boundary conditions used? If it is very sensitive, as I would guess, the top level results should not be presented. In any case, the authors should discuss this matter in the text.

Section 4.1. N/NO production. This is an interesting discussion but the authors should go more into the details. Does the total atomic nitrogen production change from the assumed 1.25Q wrt. altitude or is the branching between N(2D) and N(4S) altitude-dependent? For the EMAC model it does not matter but for the reader this is an important question. The authors' approach is a bit worrying because it does not start from the known uncertainties of the involved processes but simply modifies the parameterization for best results. In an complex atmospheric model there are surely other parameter combinations that could be just fitted to get a good agreement with the observations, but the question is if this is physically sound. The authors should discuss the production/branching uncertainties and see if the modified parameterization is within those limits or not. Above 55 km the total NO (or N2D) + N production is very low, i.e. < 0.35Q. Where does the proton energy go if not to production of atomic nitrogen by dissociation of N2? Further, should the same N/NO production ratios be used when modelling electron precipitation, or are they just for protons? The authors should discuss also this matter.

Section 4.2. NO2. There is a better agreement between MIPAS and EMAC in Oct 30 and Nov 5 but in Nov 5 - Nov 10 the situation gets worse. The authors should explain this in the text. Also, SPE-related NO2 changes are typically well captured by most atmospheric models, so no correction for the parameterization is required in that

sense. I wonder if these models now become worse in NO2 if the new parameterization is used? By the way, the N/NO ratio is reduced at all altitudes quite drastically (Fig.9). One would expect that this would lead to increase of NOx because there is less N to react with NO (NOx loss). However, at least NO2 is decreased. The question is: why?

Section 4.2 N2O The model improvement is clear but I would not call a 35% overestimation "slight". Also, the time-altitude extent of the changes is now smaller in the model than in the observations. Some discussion on this would be good to have.

Page 4515, ozone. The results from EMAC and MIPAS are actually quite similar, although there is generally some more ozone loss in the model as discussed by the authors. However, when comparison the SPE/NO-SPE runs, there is long-term decrease above 60 km after the SPE. This must be a chemistry-related effect since the dynamics in the two model runs are the same. Based on Fig. 14, there is no related HOx increase, so it cannot explain the ozone decrease. The authors should discuss this matter in the text and provide some answers.

Page 4516, lines 25-29. N2O5 conversion to HNO3 by ionic reactions. It is likely that the missing conversions to HNO3 can explain at least part of the N2O5 overestimation in the model. It should be noted that the paper by Verronen et al. (2008) discusses the conditions during the SPE while the authors now study what happens after the proton forcing has ended. For the after-event explanations, it might be useful to read the paper by Stiller, et al. (2005), J. Geophys. Res., 110, D20303.

Page 4517, lines 1-7. HNO3 production. Now, this part seems to be about the during-the-event changes in HNO3. The authors should clearly separate the two situations, during and after the SPE (see the previous comment).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4501, 2010.