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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 #2

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Review of

Energetic particle precipitation in ECHAM5/MESSy – Part 2: Solar Proton Events

by Baumgaertner, Jöckel, Riede, Stiller and Funke

General comments:

This manuscript deals with ECHAM5/MESSy (EMAC) model simulations of the effects of solar proton events on the chemical composition of the polar middle atmosphere as well as dynamical and thermal effects. To a large extent, the paper is a demonstration that EMAC – including a new SPE submodel – is capable of realistically modelling the effect of SPEs, i.e. older results with other models are reproduced. The performance of the model is investigated using comparisons with MIPAS observations of several minor atmospheric constituents. In general, good agreement is found. Most aspects are only discussed briefly, and sometimes it is not really clear what the focus of this work is. Still, the manuscript contains enough and interesting material to warrant publication. One aspect that is new – to my knowledge – is the attempt to establish the N and NO production per ion pair from a combination of 1-D model simulations and MIPAS NO2 and N2O observations. The resulting values differ significantly from the traditionally used values, which will stimulate research in this direction.

Overall, the paper is well written and informative, and I recommend publication after the following main and specific comments have been considered by the authors. I have two main comments that I believe should be addressed:

a) I suggest adding a plot (and discussion) showing the MIPAS temperature time series at high southern (and perhaps also northern) latitudes and at several altitude levels. For all of the species comparisons with MIPAS are shown, but not for temperatures. To my knowledge, there is no convincing experimental study showing the temperature decrease in the middle polar mesosphere that has to happen because of the massive catalytic ozone loss leading to reduced solar heating. This paper could make an important contribution here.

b) The modelled SPE effect on the total ozone column is not convincing because of the apparent differences between the SPE and NOSPE cases already before the SPE (see detailed comments below).

Specific comments:

Page 4503, line 22: 'Northern hemisphere polar ozone depletion greater than 0.5% was predicted to last for 8 months' . It's not entirely clear what altitude range this statement applies to? Lower mesosphere? Or perhaps TOC? Please clarify.

Page 4505, line 11: 'SPE-induced temperature anomalies up to 2.6 K ..' This statement is not very precise. Jackman shows a cooling of 2.6 K in the middle polar mesosphere

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AND a dynamically induced heating of 2.3 K in the polar upper mesosphere/lower thermosphere region. The temperature change values are for Oct. 30, i.e. one day, in the Jackman 2007 paper. Therefore, if massive proton precipitation continues, higher temperature effects may be expected.

Page 4505, line 15: 'are attempted to reconcile' -> 'are attempted to be reconciled'

Page 4505, line 19: I think 'NMHC' has not been spelled out before. Most readers will know what it means, but it's good to spell it out.

Page 4506, line 13: It's not clear to me, what the 'range in meters' actually means. Equation 1 probably holds for a homogeneous medium? The atmosphere is not homogeneous, but neutral density drops exponentially with altitude. Perhaps I'm missing a point here, but a few more details may help the reader understand how the equation is applied to this specific problem. Along the same lines: What exactly is meant by 'column mode' in line 15?

Page 4507, equation (2): I suggest mentioning that f(h) cannot become negative in your study, because the model atmosphere does not extend above 83 km. The possibility of f(h) >0 initially irritated me.

Page 4508, section 3: I suggest mentioning the altitude coverage of the MIPAS retrievals used here, in particular the upper limit.

Page 4510, first paragraph, NO2 comparison: Some of the MIPAS observations were probably made under sunlit conditions? Then the diurnal variation of NO2 becomes important. Where the model data sampled at the exact locations and times of the MIPAS observations? This should be mentioned for such a species with pronounced diurnal variability.

Page 4510, line 15: 'Figure 15 depicts changes' -> 'Figure 15 depicts N$_2$O changes'

Page 4511, line 2: 'Middle Atmospheric' -> 'Middle Atmosphere'

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Page 4511, equations (R2) and (R3): Are these branches introduced based on certain evidence, or just ad-hoc assumed to better reproduce the observations? A brief comment may help the reader to understand the implications of these additional reactions.

Page 4513, line 22/23: ', with an almost negligible production of N atoms below and above 65 km'. The second part of this statement is not backed up by the analysis and untenable. The top filled symbol in the left panel of Fig. 9 is at about 65 km altitude, so no statements can be made about higher altitudes. The MIPAS observations only extend up to 68 km, if I remember correctly.

Page 4514, lines 15 – 18: 'The agreement with MIPAS is better compared .. ' Still, the model shows a much faster NO2 reduction after the SPEs at altitudes above 50 km. This could be commented upon and put into the context of uncertainties in the NOy partitioning mentioned further below.

Page 4514, lines 23 – 25: 'Therefore, the sensitivity simulations for the N/NO production efficiency appear to have provided a significant improvement for the 3-D EMAC simulation'. This is not really surprising, because the N/NO production efficiency was 'tuned' to improve the agreement of NO2 and N2O between MIPAS and the model (using the 1-D model, admittedly).

Page 4515, lines 19 – 20: 'From 28 October onwards, a reduction of approximately 5 DU is evident, growing to 10 DU at the end of November'. Looking at Fig. 13, I'm not sure these statements can be fully backed-up. Even before the SPE there are differences of 2 – 3 DU between the SPE and the NOSPE case, most pronounced near the maximum around October 22/23. Is this difference expected? Given this initial difference it's hard to believe that the SPE causes a 5 DU difference on October 28 and the following days. Also, how does one know that the about 10 DU difference at the end of November is not also to a substantial degree caused by the model runs drifting apart – independent of the SPE – if there is a 2-3 DU difference in mid-October?

Page 4517, lines 26-28: 'An average cooling of up to 2.5 K in the lower mesosphere

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is evident during the first week after the SPE, similar to the results by Jackman et al. (2007)'. If one checks the details of Jackman et al., then the results of this study are not fully consistent with Jackman et al. Jackman et al. report a cooling of the middle polar mesosphere of 2.6 K for October 30, i.e., 2.6 K / d for this particular day. Looking at the change from October 29 to October 30, or 30 to 31 in Fig. 18, the difference is significantly smaller than 2.6 K, which may of course have different reasons. Jackman reported changes for 90S, this study an average between 70S and 90S. I recommend including a brief discussion on these differences and their possible origins, rather than simply suggesting that the two model studies agree.

Page 4517, same sentence: I suggest showing the temporal evolution of modelled and observed temperatures at different altitudes throughout the SPE period and thereafter. This aspect should be expanded and discussed in more detail, because the effect of SPEs on the thermal structure of the atmosphere is not well backed up experimentally. The model response is shown in Fig. 18, but it would be more interesting to compare EMAC and MIPAS temperatures. The atmospheric temperature has to decrease if ozone is reduced, but to my knowledge this has not been demonstrated convincingly with observations. This paper could make an important contribution here.

Page 4517, section 4.4: There are several recent studies also dealing with thermal and dynamical effects of SPEs that perhaps should be included:

- Krivolutsky, A. A., A. V. Klyuchnikova, G. R. Zakharov, T. Yu. Vyushkova, and A. A. Kuminov (2006), Dynamical response of the middle atmosphere to solar proton event of July 2000: Three-dimensional model simulations, Adv. Space Res., 37, 1602 – 1613.

- Becker, E., and C. v. Savigny (2010), Dynamical heating of the polar summer mesopause induced by solar proton events, J. Geophys. Res., doi:10.1029/2009JD012561, in press.

Both of them show that the altered temperature fields change dynamics through the thermal wind balance. The Becker paper also shows how the reduced solar heating in the middle mesosphere leads to a dynamically induced heating in the upper mesosphere.

Page 4519, lines 15: ‘approximately 5 DU’. Again, this value may be somewhat too large given the differences of the SPE and NOSPE model runs even before the SPE. Also the ’10 DU’ two lines below are questionable.

Page 4502, line 19: along the same lines: The total column loss of about 10 DU appears like a very rough estimate and my be too large.

Page 4524: I don’t quite understand what this table means. Why are 2 baseline versions listed for most species? Which one was taken? Please add some clarifying comments.

Page 4529: Fig. 5: I suggest adding ‘MIPAS’ and ‘EMAC’ after ‘a)’ and ‘b)’ (perhaps also for all of the other Figures). If Fig. 4 and 5 are on different pages, the figure will then be easier to understand.

Page 4533: Fig. 9, left panel: I suggest limiting the altitude range shown to 42 km to 66 km. Above 66 km, your method does not yield any information, if I understood correctly, but the figure suggests this.

Page 4533, caption of Fig. 9, line 2: ‘the average deviation from’. I suggest making clear that the (weighted) averaged deviation between model and MIPAS NO2 and N2O is shown, as this is not clear from the caption only.

Page 4537, Fig. 13: Please add a label showing which line is which.

Page 4540: Caption of Fig. 16:’here, the MIPAS averaging kernel was not applied’. Why not? Was there a good reason? If not, it should be applied.

General comment on Figures: The labelling of the panels, e.g. ‘a)’, ‘b)’ is too large. Please adjust the font size

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