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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Short- and medium-term atmospheric effects of very large solar proton events" *by* C. H. Jackman et al.

C. H. Jackman et al.

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We thank Reviewer #2 for helpful comments and suggestions. The Reviewer Comments are noted first and then we give our Reply: to the comment. We are submitting a revised manuscript that has an additional two figures.

General comments

I would recommend to mention in the text which questions remained unresolved in the previous publication and to emphasize the novelty of the presented results

Reply: We have now added some text to the paper with an emphasis on motivating the current study as well as emphasizing the novelty of the results (see section 1. Introduction: paragraphs 5 - 7).

Major issues

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1. As was stated in the manuscript the influence of the SPEs on the chemical composition of the atmosphere has been already studied in details with a range of different models, however the authors did not explain why the new study was necessary. I suggest to add a paragraph to the introduction with a brief summary of the previous results and a list of questions which remained open. It will provide the motivation and help to emphasize the novelty of the presented study. I think it will help readers to better understand the problem and the main goals of the study. I think it will not be difficult because the first author participated in the most of the previous attempts to study SPEs.

Reply: We have now added some text to the paper with an emphasis on motivating the current study as well as emphasizing the novelty of the results. There are some recent measurements of SPE-caused enhancements in HOCl, ClO, ClONO2, HNO3, and N2O5 (von Clarmann et al. 2005; Lopez-Puertas et al. 2005), which (as far as we know) have not been compared with model simulations in a paper. Also, some HALOE-measured NOx enhancements in September 2000 (Randall et al. 2001), attributed to the July 2000 SPE, have been simulated. We are not aware of any other published results on using a model to account for the Randall et al. (2001) HALOE observations. This study is also a test of the ability of a general circulation model with chemistry to simulate the influence of SPEs in several different time periods. See new text in section 1. Introduction - paragraphs 5 - 7.

2. In the section 3 it is important to mention that the applied parameterization of the HOx and NOx production based on the 1-D model is rather simple and does not include the complete description of ionospheric processes in the layer D. Does this hamper the ability of the model to simulate atmospheric response?

Reply: As the reviewer notes, the applied parameterization of HOx and NOx production is relatively simple and straight-forward. WACCM3 presently does not have a comprehensive description of the ionospheric processes in the layer D, thus a relatively easy way of including HOx and NOx was needed. We have had reasonable success with the methodology in the past. For example, Jackman et al. [2005a] employed these

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HOx and NOx parameterizations in a study on the Oct./Nov. 2003 SPEs using a two-dimensional model. Jackman et al. [2005a] had reasonable success in predicting the ozone depletion in the southern hemisphere at 0.5 and 1 hPa (measured by SBUV/2), which is primarily caused by HOx enhancements. Jackman et al. [2005a] also had reasonable success predicting NOx enhancements in the southern hemisphere between 2 and 0.006 hPa. The recent paper of Verronen et al. [2006] showed comparisons with Aura MLS OH measurements of predicted HOx using the sophisticated Sodankyla Ion and Neutral Chemistry model, also known as SIC. This study showed that models with a parameterization of the production of HOx as a function of ion pair production (Q) could underestimate the HOx production and the resulting ozone depletion during certain periods. Verronen et al. [2006] suggested that there may be a way of improving the parameterization of the HOx production per Q. We would certainly welcome such a new parameterized improvement in SPE-caused HOx production.

3. The comment in the last paragraph of the introduction to the section 5 is very important for the understanding of the model results, but the authors did not pay any attention to this process in the subsequent analysis. It is very important to distinguish in the further analysis the influence of the SPE and other related processes. Even if this mechanism is absent in the model, it does present in the observation data. The authors should find the way to estimate the potential magnitude of the NOx and HOx changes due to this process. Otherwise, it is dangerous to compare the model results (with only SPEs) with the observations (SPEs and electrons).

Reply: We made the comment in the last paragraph of the introduction to section 5 to be clear to the reader that we would not be addressing the issue of the lower energy electron precipitation. Although we cannot be totally sure, we think that the charged particle precipitation impacts that we present in the paper due to the Halloween Storms of 2003 were primarily driven by SPEs. Our reasoning is that the measured impacts are for altitudes less than about 70 km and for a time period during the SPEs, 26 Oct. to 5 Nov., up through 13 Nov. It is unlikely that the electron-produced NOx in the lower

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thermosphere above 100 km could be transported 30 km or more so quickly. Such descent rates would have to be about 1.8 km/day (2.1 cm/s), which are possible but unlikely to be sustained over the entire 30 km altitude range. Also, there seems to be no evidence in the MIPAS data (see current Figure 7, Ozone change in 70-90N band) of an ozone depletion coming down from above 70 km. Conversely, the MIPAS ozone change above 60 km from 8-13 Nov. shows an ozone increase. The ozone depletion caused directly by the SPEs 27 Oct. to 6 Nov. above 60 km goes away by 8 Nov.

4. It is rather surprising that having ensemble simulation the authors did not estimate the statistical significance of the results. The authors mentioned many times that the WACCM circulation cannot exactly coincides with the real circulation for any particular year. But the ensemble run gives some opportunity to estimate the dependence of the atmospheric response on the atmospheric state. This opportunity was not exploit by the authors. For example, the analysis of the ensemble run could help to understand the difference in the shape of NOx changes mentioned in Section 5.2 or ozone response (section 5.3). The latter is very important to understand, because in the southern hemisphere the model completely (different sign of the effect) disagree with the observations below 50 km while in the northern hemisphere the agreement is more reasonable.

Reply: The reviewer has a good point. We have now derived standard deviation values from our model simulations, which can be used to create 2-sigma levels of changes caused by the Oct./Nov. 2003 SPEs. We have done Figures 5-7, 9-12, and 15 with this information.

5. The asymmetry between hemispheres could be an interesting issue. For example, in the figure 3 there is a huge difference between the hemispheres however the authors do not try to address it at all. The same is true for the analysis of the Figure 6 and in Section 6.3.

Reply: The reviewer is certainly correct that the asymmetry between hemispheres is

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an interesting issue. Our original paper made only brief mention of this asymmetry. We now describe in some detail the reason behind these hemispheric differences, especially for Figures 3 and 6 (see section 4.2 - paragraph 1; section 5.1 - paragraph 1; section 6.3 - paragraph 3).

6. Section 5.4 is very interesting, but is not acceptable in the present form. The reader will not be able to make any meaningful conclusions from this section, because the both observations and model results are questionable. I think that if it not possible to use updated/reprocessed MIPAS data this chapter should be omitted.

Reply: The reviewer has a good point. We now compare WACCM3 results with the reprocessed MIPAS data (see updated section 5.4 and Figures 6, 7, and 9-11).

7. It is also surprising that the authors did not show the obtained temperature and circulation changes. The application of the sophisticated CCM provides such an opportunity and the temperature effects could be also compared with observations helping to evaluate the model performance.

Reply: We had addressed some of the short-term SPE-caused mesospheric temperature and circulation changes in our Jackman et al. [Mesospheric dynamical changes induced by the solar proton events in October-November 2003, Geophys. Res., Lett., 34, L04812, doi:10.1029/2006GL028328, 2007] paper and were planning on addressing the longer-term SPE-caused dynamical effects in a follow-on paper to this manuscript. The Jackman et al. (2007) results showed simulated temperature changes up to +/-2.6K, which would be difficult to observe. Garcia et al. (2007) evaluated the WACCM3 temperatures in comparison with TIMED SABER temperatures. Garcia et al. (2007) also discussed a comparison of WACCM3 and URAP (UARS Reference Atmosphere Project) zonal mean winds. Since it is beyond the scope of this paper to include discussion of temperature, we modify the title of the paper slightly to focus on constituents. The paper title is now: Short- and medium-term atmospheric constituent effects of very large solar proton events

8. The analysis of the NOx and ozone response shown in Figure 15 (section 6.3) should be extended. In the present form it is not what the authors would like to convey to the reader. If they really would like to say that it is almost impossible to predict ozone response after 5 month, then it should be included in the conclusion section and probably in the abstract. In this case the study of the long-term SPE influence does not seem very promising.

Reply: We have now added some more discussion to the text regarding the results of the current Figure 17 (previous Figure 15). The major point here is that NO2 is elevated months after the very large SPE, which should affect ozone either directly (NO2 + O to NO + O2 followed by NO + O3 to NO2 + O2) or indirectly through its influence on other constituents important to ozone (e.g., NO2 + ClO + M to ClONO2 + M; NO2 + BrO + M to BrONO2 +M). This is now pointed out in the text more clearly (see section 6.3 - paragraph 7). The NO2 enhancements in Figure 17 (previous Figure 15) are statistically significant at the two sigma level, however, the ozone changes are not. It is not clear if there are any statistically significant long-term SPE influences ($>$ 5 months) at this point.

9. The conclusions are rather short and not instructive. I would prefer to see more critical analysis of the result and the author suggestion how the further progress can be reached.

Reply: We have tried to modify the text to address this issue (see section 7 - Conclusions).

Minor issues

1. 10544, 10: lasted years after the events. This is not confirmed by the presented results.

Reply: Agreed. The Abstract has been modified.

2. 10548, 22: I think also does not fit there.

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Reply: Agreed. also has been removed.

3. 10549,4 : with each other. I think they are reactive not only with each other but also with other components (ozone, for example).

Reply: We certainly agree that HOx constituents react quickly with other constituents. The point we were trying to make (obviously, not all that well) was that the HOx constituents had a short lifetime because of their reactions with each other. We have now modified the text to be clearer.

4. 10551, 20-21: Does every day output mean daily averaged or just a snap-shot for some particular local time?

Reply: Every day output means a snap-shot at 0:00 GMT. We have now made this clear in the text (see section 4.2 - paragraph 2).

5. 10553, 18: left and right are wrong

Reply: Agreed. We have now corrected the text to top and bottom.

6. 10554,10-12: Nash criterion (Nash et al., 1996) is based mostly on the analysis of the potential vorticity. They used only some threshold zonal mean zonal wind velocity as an indicator of vortex existence. Therefore it should be explained how the proposed modification works. I guess, the authors did not use PV at all and applied some threshold CH4 and CO value to define the boundary of the polar vortex. If it is true, than the Nash criterion is not directly relevant.

Reply: The PV values were used in the analysis of Lopez-Puertas et al. [2005b]. The vortex edge is normally located at the equivalent latitude where PV shows a very large gradient. For the days presented in Lopez-Puertas et al. [2005b], the PV analyzed showed several pronounced gradient (not only one) at different equivalent latitudes. In order to discern which one was determining the vortex edge, the CO (and CH4 fields) were compared with the results. It was assumed that the vortex edge was located at the equivalent latitude where there is simultaneously a large gradient in PV and a strong gradient in CO (and CH4). The use of CH4 was somewhat redundant with that of CO as the gradients in both coincides in most cases, but CH4 is a better tracer at lower altitudes while CO is better at the uppermost regions. We have now added a sentence explaining this (see section 5.2 - paragraph 1).

7. 10555, 5: It is rather 50 ppbv than 100 ppbv.

Reply: The text was correct as written. Obviously, it was somewhat unclear thus we have added (red color) to denote the region, which shows values greater than 100 ppbv.

8. It is not clear why the upper level for the WACCM results in the Figures is always different. For some figures it can be due to the satellite data, but why it is only 68 km in Figure 3, and 65 km for Figure 11, 14? I think, it is interesting what are the results in the upper mesosphere and maybe even higher up.

Reply: In this paper we have focused on the lower mesosphere and stratosphere. As the reviewer notes, the domain of the satellite data drives the upper altitude level for WACCM3 results for many of our comparisons. We have focused on a smaller region than the whole model domain to be able to concentrate on the traits important to SPE influence in this region of the atmosphere. Our manuscript is very heavily driven by the observations and explaining the lower mesospheric and stratospheric behavior caused by SPEs. The manuscript is quite long now and would require much more text if WACCM3 results through the lower thermosphere were shown, thus we have decided to keep the original domains for the figures.

9. 10561, 2: This sentence looks too emotional. It is better to provide scientific arguments.

Reply: We have now used the statistical significance information from four additional simulations with SPEs to derive the likelihood for the large enhancement in September 2000 to be caused by the July 2000 SPE rather than interannual behavior. NOx

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changes greater than 5 ppbv are statistically significant to the two sigma (95% confidence) level. The WACCM3 and HALOE NOx enhancements in September 2000 are greater than 10 ppbv beyond the baseline amounts, thus are clearly statistically significant. We have now modified the text (see section 6.1 - paragraphs 1 and 3).

10. Figure 15, Error bars should be added.

Reply: Figure 17 (previous Figure 15) shows percentage difference values for SAGE II NO2 and ozone in comparing 31 March 1990 with 31 March 1987. It is unclear what error bars should be shown for the SAGE II measurements, especially since the systematic errors in the measurements would cancel out through the differencing. For this plot we have decided to leave off any error estimates of the SAGE II percentage differences.

Technical corrections

1. Figure 3, caption: left and right should be changed to top and bottom.

Reply: Agreed. We have now corrected the text to top and bottom.

2. Figure 4, quality should be improved. The left 2 figures can be eliminated and the resting 4 figures can be enlarged.

Reply: We have tried to improve the quality of the WACCM3 plots shown on the bottom. We prefer to leave the left two figures because they show the non-perturbed (before SPEs) NOx levels on 27 October, which can then be contrasted to the perturbed (during SPEs) NOx levels on 29 and 30 October.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10543, 2007.