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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.

Anonymous Referee #2

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General comments

The manuscript describes the effects of large solar proton events (SPEs) on the chemical composition of the middle atmosphere simulated with the chemistry-climate model (CCM) WACCM and its comparison with the results obtained from several satellite observations. This subject is definitely relevant to the scope of ACP. The manuscript does not present really novel concepts, ideas or data, however the application of a comprehensive CCM WACCM to the analysis of the atmospheric response to the SPEs is original. In general, the authors confirmed previously published estimates concerning the short-term HOx, NOx and ozone response to SPEs. The analysis of HNO3, HOCl and N2O5 response to the SPE is rather new and potentially interesting, however the authors did not reach affirmative conclusions about their behavior. The applied scientific methods and assumptions are mostly valid and clearly outlined. The obtained results are sufficient to support the conclusions, however the statistical analysis of the results is completely missing. The numerical experiments with the model are clearly described and can be readily repeated by other scientists. The previous publications about the subject of the manuscript are properly credited, 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. The manuscript is well written and structured. Therefore, I suggest publication of the manuscript with major/moderate revisions which are described bellow.

Major issues

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.

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?

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

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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).

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.

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.

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.

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.

8. The analysis of the NOx and ozone response shown in Figure 15 (section 6.3)

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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.

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

Minor issues

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

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

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

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

5. 10553, 18: left and right are wrong

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.

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

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

10. Figure 15, Error bars should be added.

Technical corrections

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

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