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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 #1

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Referee Comments

On

MS-NR: acpd-2007-0271 Version: 1 Received: 26 June 2007, 15:17 CET Title: Short- and Medium-term Atmospheric Effects of Very Large Solar Proton Events Author(s): C. Jackman, D. Marsh, F. Vitt, R. Garcia, E. Fleming, G. Labow, C. Randall, M. Lopez-Puertas, and B. Funke

1. General comments:

Contents of the paper

The authors describe atmospheric effects as a response to very large solar proton

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events (SPE) in the past forty-five years, mainly depletion and enhancements of the constituents ozone, hydrogen, nitrogen and chlorine compounds. After an overall introduction of the main effects of SPEs in the atmosphere, a compilation of all relevant previous works on atmospheric effects of SPEs is given. The first chapter deals with a brief description of the ionization rates provision for the specific Whole Atmospheric Community Climate Model 3 (WACCM3) runs. The well known HOx and NOy production mechanisms are explained which finishes with an overview of the largest SPE events in the past forty-five years. The WACCM3 model is described and the done WACCM3 simulations are outlined briefly. They consist of mainly seven simulations of the effects during and after three major events in the years 1989, 2000, and 2003, as well as a long-term simulation from 1963 to 2005. An overview of the model runs is given in a table as well as a figure which shows the correlation of Sun activity and the corresponding NOy production through the solar cycles twenty to twenty-three. Corresponding satellite measurements are described in a following section which are mainly (S)BUV, SAGE II, HALOE and MIPAS. The well-known short-term effects in atmospheric composition driven by HOx and NOx produced at the Halloween Storm nd measured by MIPAS, have been simulated. The ozone depletion during the Halloween storm as measured by MIPAS as well as during the Bastille storm in year 2000 observed by SBUV/2 and corresponding model runs are shown. A more interesting subject is treated in the next section which deals with the “other constituents” HNO3, HOCl, ClONO2, and N2O5, where large discrepancies of the model results to the MIPAS measurements of HNO3, N2O5, HOCl during the Halloween storm in 2003 are obvious. We will come later to these differences what in my view is one of the major critic points of the paper. At last, comparisons of simulations to the total amount of NOx concentration in 2000 with HALOE measurements displays only partly the capability of the WACCAM3 model for medium-term effects on atmospheric constituents, also shown by a comparison of thirty-five years old SBUV ozone measurements. In the end simulated NOx and ozone changes as well as comparisons of SAGE II ozone profiles on 31 March 1990 caused by this largest SPE ever observed are shown. The authors conclude with a brief assessment of the model's quality and a summary of the most important atmospheric effects caused by the largest SPEs.

Form of the paper

In general, the paper is well written and very well structured. The paper is understandable and no information is missing. The figures are mostly well represented and illustrate the subjected issues adequately, the captions are precise and informative.

Impression of the paper's quality and overall comment

At the first moment I had some problems to fix the aim of the paper. The paper may function as a review about the effects of SPEs on atmospheric constituents as well as as an introduction to the WACCM3 model regarding large SPEs. Well, the paper may satisfy both: First, it can be used as a compilation of the research in the context of large SPE effects - although most of the results regarding ozone and NOx have already been shown in several publications, e.g., especially regarding MIPAS. Even the authors themselves published similar reviews about the mechanism and effects of SPEs in terms of ozone, HOx, and NOx (see publications cited in the introduction). Those also consist of model runs and showed similar results of ozone depletion and changes of other atmospheric constituents. In terms of this, the paper presents known mechanisms and measurements. It may be that there are some model effects which have never been modelled so far, but then this is not be noted or highlighted in particular. This kind of review can function as a for someone too large introduction and background information about the effects of (large) SPEs on atmospheric constituents and the state of the art of research. Novel research should be more highlighted in the recommended revision. The second aim of the paper - and this contains much more scientific issues - is the behaviour of the WACCM3 model during really large SPEs. But then, two following issues are missing here:

First, a more detailed description of the "relatively new" model (what does that mean?), since its features are not clear in the end: is it a long-term climate model more than a

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short-term three dimensional chemistry and transport model? The model description makes it a little bit difficult to estimate the quality of the results, e.g., in terms of the other constituents except those of ozone, HOx, and NOx. Although there are citations given for the WACCM3 model, there should be more information and an assessment of the model which may let us trust or at least estimate the results of the model. It may be advisable to compare the model and its benefits and/or disadvantages in contrast to other models briefly. I am sure that more scientific findings in particular regarding some later presented discrepancies in Sec. 5.4 can be concluded from that: for instance the vertical shifts in Fig.8. The authors are recommended to revise at least section 5.4 which presents results of NOy and chlorine species from the WACCAM model in comparison to MIPAS measurements which are supposed to be imprecise (too low) in the altitude pointing. Additionally, announced but not shown improvements of the model as well as of the measurements should be presented in a revision version, like the reactions which are most likely responsible for the large differences of HNO3, N2O5, and HOCl.

Secondly, it may be advisable to highlight the characteristic effects of large SPE events in contrast to normal conditions in order to estimate up to which extent of the SPE the model is capable to reproduce the atmospheric changes. For instance, one possible reason for the overestimation of the HOx (or NOx) effects - which were also seen in the comparisons of the first MIPAS and SCIAMACHY with models - may be due to the fact that the model does have inexhaustible resources but the atmosphere does not. However, for me, it would be very interesting to know about the behaviour of the model and if there are limits like the above described.

All in all the paper consist of the potential of interesting research but should be revised in terms of description of the model, reprocessing the comparisons with the MIPAS HNO3, N2O5, and HOCl measurements and clear the minor discrepancies as described in the Specific Comments.

2. Specific comments

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10547:25 Are Alpha particles included in the WACCM model?

10548:15 It would be interesting for the reader to know about potential differences of the proton flux sources, are there any between IMP and GOES and which consequences do they have on the model?

10550:1-23 The expression relatively new may be not adequate. As mentioned in the general comments, the model should be described in more detail even if some references are given: which chemical reactions are employed, which dynamics and meteorology and how do the mentioned parts of the model interact with each other? Since the model is a climate model it certainly has characteristic features which may impact the results. It would be also necessary to describe how the model simulates diurnal changes since both daytime and nighttime satellite measurements are used for comparison in the following sections.

10551:6-9 The statement that protons precipitate only at the polar caps should be noted as an assumption since there are many hints that the magnetosphere is massively deformed by particle pressure during strong proton storms like the Bastille event. I know that this is still an uncertain research but it hides a large potential error source, see e.g., the studies of M. Sinnhuber in that research field.

10551:9-11 The main reason for the inter-hemispheric differences of the effects itself has two other causes: First, the different chemical compounds (like water vapor) at different seasons in the northern and southern hemisphere, see e.g., Rohen, 2005. And secondly, there are indications that the particle flux on the winter hemisphere (the opposite side of the Earth towards the Sun) is larger than on the summer hemisphere.

10551:25 This is a very brief compilation of the used measurements. Since they use fairly different techniques there should be a short comment about their potential (or not potential) effects if they were used for comparison with models. For instance, HALOE measures ozone during sunrise and sunset whereas MIPAS can also measure during the night, and ozone is known to have a considerably diurnal change by a factor five at

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seventy km. Does the model consider this?

10552:1-4 Observations of SCIAMACHY have also been done to investigate the short-term effects.

10552:6 This sentence may irritate. Add the subordinate clause "For the short-term effects of SPEs, Ě", although later also effects of the year 2000 event are presented. This constraint should be also mentioned explicitly in the abstract.

10553:17 Exchange the position notes "left" with "bottom" and "right" with "top" in Fig.3.

10553:25 Is an increase of 700% realistic? Are there any limitations regarding the water availability in the model? At least, this should be mentioned since the HOx effect is overestimated in many simulations (see the listed publications).

10554:19-23 At this place, it is useful to know about the characteristic features of the model, e.g., how the dynamics is implemented and how this explains the different positions of the NOx enhancements. By the way, the alignment of the enhancements in the model results cannot be identified definitely at last. Perhaps the colors of the map features can be selected properly.

10554:25 Fig.5: The resolution of the measurement and the model results are obviously different. Both must be mentioned. Which HALOE measurements have been used (sunset or sunrise). This may reason the diurnal pattern in the measurements (e.g. on 1 November at higher altitudes) which cannot be seen in the model results.

10556:13 Perhaps it may be useful to cite other observations and simulations and draw a brief summary about observed differences and similarities.

10556:12 This must be explained in more detail in the section about WACCM. The missing ozone depletion through HOx in the upper atmosphere by the model on 3rd and 4th November is a feature which should be detected and was seen by other models fairly easily.

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10557:10 Is this reaction included in WACCM?

10558:3 It would be advisable to implement the reactions above and run the model with them. Perhaps it is a little bit work, but the results will provide a clear statement about the reasons for the different features. This would be a very exciting finding. I think it's worth it. Furthermore it should be checked if MIPAS does have a proper altitude pointing of the observed features: is MIPAS too low by about 5 km as also indicated in Fig. 9? This could also be a reason for the discrepancy.

10558:13 The features in Fig. 9 (bottom) cannot be identified to be caused by seasonal effects since the changes of N2O5 seem to be correlated to the SPE events. I hardly can recognize seasonal changes.

10588:17 Big puzzle Ě any suggestions for possible reasons or an attempt to improve the model run?

10558:28 Again: Is MIPAS too low or is the model too high?

10588:28 The effect can be seen but the quantitative disagreement is obvious and fairly large.

10599:25 It may be advisable to show these figures instead of those with incorrect MIPAS data. The paper would benefit from the revision of this section 5.4. by re-processing several model and measurement runs. Although this section gives several reasons for the reader not to trust the model treating NOy, this section is certainly very interesting in the paper and may be revised.

10560:10 MIPAS does also provide temperatures, this would be a good opportunity to compare at least sample temperatures used in the model.

10561:9 Not only the variability is larger, but the absolute NOx concentrations are also larger except for 1991 what is in contrast to the fact that one year ago there was a large SPE. What can be the reason for the general overestimation?

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10562:25 Exchange the position of Figs. 14 from “top” to “left” and “bottom” to “right”. The ozone depletion seemed to be more than two weeks delayed than the NOx enhancements. For instance, the maximum in NOx enhancements at 27 km happens beginning of February whereas the adequate ozone depletion occurs mid of February. Secondly the large NOx enhancements at mid of March above 55 km seem not to be followed by a adequate ozone depletion? What may be the reasons for these features?

10563:29 The model shows again a substantial overestimation of NOx and the estimation of NOx between 20 and 25 km seemed to be fairly bad. At least in a non-disturbed lower stratosphere the model should predict the NOx more precise, similar the ozone in the lower stratosphere. Additionally, there seemed to be an altitude shift in the modeled ozone relative to SAGE ozone. Is this due to incorrect transport modeling of WACCM beside the given reason that it is difficult to model such a large time period of five months?

10569:15 The name of the coauthor is Schröter, not Schroter.

10570:20 There seems to be a V missing in the name of the author.

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

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