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Atmospheric Chemistry and Physics Discussions

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 #1 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.

1. General comments:

Impression of the paper quality and overall comment

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, Novel research should be more highlighted in the recommended revision.

Reply: We have modified the paper (see section 1. Introduction; paragraphs 5 -7) to focus on the novel aspects of the research.

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 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.

Reply: We have added more information about WACCM3 (see section 4.1, paragraphs 1-3) so that readers will understand the modeling tool more completely. We are now comparing with the MIPAS version V3O_9 for HNO3, N2O5, and ClONO2. The discrepancies between WACCM3 and MIPAS for HNO3, N2O5, ClONO2, HOCl, and ClO are described more completely (see section 5.4 - paragraphs 1-6).

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.

Reply: The old MIPAS data used in the original version of our paper represented the higher atmosphere at lower altitude resolution. Besides masking some structures, limited altitude resolution can shift the VMR maximum in altitude if the averaging kernels are asymmetric. The revised MIPAS data are represented at higher altitude resolution, in particular at higher altitudes, which reduces the altitude difference between model and observations. The reason for the previous differences therefore was not

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mis-pointing. As noted above, we are now comparing with the most recent MIPAS data version V3O_9, which is in better agreement with the model. Some of the reactions important in HNO3, N2O5, ClONO2, HOCl, and ClO chemistry are noted in section 5.4 - paragraphs 1, 3, 5, and 6.

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.

Reply: We do not totally understand this comment. It is not clear that there is an overestimation of the HOx effect as we do not have any measurements of HOx during the very large SPEs discussed in the paper. Regarding the NOx effect: The model atmosphere and actual atmosphere have very large resources for production of NOx. NOx is produced by dissociation of N2, which makes up 78% of the atmosphere. Thus potentially, NOx enhancements in the thousands of ppmv or more would be possible. Both model and measurement show NOx enhancements of tens of ppbv, which is easily possible. We are not aware of any NOx production limits that have been reached in our model computations.

2. Specific comments

10547:25 Are Alpha particles included in the WACCM3 model?

Reply: Alpha particles (as discussed in section 2. Proton measurement/ionization rates) are included only from the IMP 8 satellite measurements, thus only over the period 1974-1993. In that section we note that - Alpha particles were found to add about 10% to the total ion pair production during SPEs.

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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?

Reply: IMP and GOES have very different orbits. For example, IMP-8 is in a very circular orbit at a distance of a little more than half way to the moon. It takes 12+ days to orbit the Earth. The GOES spacecraft are in geosynchronous orbits, which take 1 day to orbit the Earth. We are not aware of any systematic differences between the proton flux detectors aboard IMP and GOES. However, there are uncertainties associated with the proton flux data. We estimate the proton flux uncertainties to be up to 50%, given some straightforward comparisons of proton flux instruments aboard two different GOES spacecraft. The SPE production of HOx and NOx production would be nearly linearly affected by an increase or decrease in protons. For the period 1994-2005, we have primarily used the proton flux measurements from the GOES-7, GOES-8, and GOES-11 spacecraft, which are most favorably positioned in their orbit to measure precipitating solar protons (Terry Onsager, private communication, NOAA Space Weather Prediction Center). It is beyond the scope of the present study to undertake a more rigorous evaluation of proton flux differences, however, we do recommend that such a study be accomplished by experts in the field of solar particle observations. We have now added some text (see section 2 - paragraph 5) about this proton flux uncertainty.

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.

Reply: We have added more information about WACCM3 (see section 4.1 - paragraphs

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1-3) so that readers will understand the modeling tool more completely. WACCM3 has fully coupled dynamics, radiation, and chemistry and thus simulates the entire diurnal cycle of constituents at all levels in the model domain.

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.

Reply: The reviewer is correct that strong proton storms can deform the polar cap region, exposing more of the atmosphere to SPE perturbation. However, the magnitude of that deformation is at the core of the issue. We do have observational evidence for the Aug. 1972 SPEs discussed in McPeters et al. [1981] and shown for the Jul. 2000 SPE in Figure 1 of Jackman et al. [2001], for the Oct./Nov. 2003 SPEs in Figure 4 of Jackman et al. [2005a], and for the Jul. 2000 SPE in Figure 7 of the present paper that the protons primarily impact the polar caps ($>$60 degrees geomagnetic latitude). The ozone reduction slightly outside the polar cap near 90 degrees E longitude (see Figure 7 of the present paper) was probably caused by the Earths magnetic field being perturbed somewhat during this very large solar disturbance. On the other hand, the ozone near 0 degrees longitude just inside the polar cap boundary was not reduced as much as predicted. Again, the Earths magnetic field was probably perturbed in such a way as to reduce the proton flux in a small part of the polar cap. It appears that for the purposes of most very large SPEs: An assumption of uniform proton fluxes over the entire polar cap is generally reasonable. M. Sinnhuber et al. [A model study of the impact of magnetic field structure on atmospheric composition during solar proton events, Geophys. Res. Lett., 30, 1818, doi:10.1029/2003GL017265, 2003] were focused on the impact of SPEs on the Earths atmosphere during a polarity transition of the Earths magnetic field, which is a different condition than what exists today.

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.

Reply: The reviewer is correct that there are other hemispheric differences as well. We were just pointing out the differences in the SPE perturbed areas for the NH and SH. We have now added two sentences about other differences in the two hemispheres, primarily driven by the different seasons (see section 4.2 - paragraph 1). High-latitude ground-based neutron monitors have detected higher relativistic solar proton flux enhancements in sunward (Bieber, J. W., et al., Energetic particle observations during the 2000 July 14 solar event, The Astrophysical Journal, 567, 622-634, 2002) and anti-sunward (Bieber, J. W., et al., Relativistic solar neutrons and protons on 28 October 2003, Geophys. Res. Lett., 32, L03S02, doi:10.1029/2004GL021592, 2005) directions for very large SPEs. Solar protons with energies greater than ˜2 GeV reach the ground. It is unclear if such asymmetries detected in relativistic protons apply to lower energy protons ($<$300 MeV) for which we have satellite instrument measurements (see section 2). Also, there is evidence from the BUV instrument observations aboard the Nimbus 4 satellite (R. D. McPeters et al., Observations of ozone depletion associated with solar proton events, J. Geophys. Res., 86, 12071-12081, 1981) that only about a 15% hemispheric difference exists in fluxes for protons with energies $>$72 MeV. We lack measurements for protons with energies $<$300 MeV in the different hemispheres for the SPEs considered in this study. Given the McPeters et al. (1981) results, we conclude that our assumption of no interhemispheric differences in protons with energies $<$300 MeV is reasonable.

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 seventy km. Does the model consider this?

Reply: It is true that the satellite instruments measure constituents at different local times and with different methods. We have tried to use output from the WACCM3, which is most comparable to the measurements. The NOx family, defined as NO+NO2, consists of two constituents that have very large diurnal cycles in the mesosphere and upper stratosphere. Fortunately, NO and NO2 cycle mainly from one to the other over a 24-hour period, so that their sum (NOx) has a very small diurnal cycle. Since we compare NOx, rather than the individual NO and NO2 constituents, to MIPAS and HALOE measurements in Figures 4, 5, and 14, the comparisons are valid for any time of day. Figure 7 presents the percentage changes in ozone and uses a similar sampling of both MIPAS and WACCM3 with day and night values. The ozone destruction from SPEs occurs mainly during the day and the percentage change, relative to the base value, is essentially frozen to the ending day values during the night. Likewise, Figures 8 and 15 present percentage change in ozone from SBUV/2 and BUV and WACCM3, which should be comparable quantities. Figures 6, 9, 10, 11, and 12 show nighttime changes from October 25 in NO2, HNO3, N2O5, ClONO2, and HOCl from both MIPAS and WACCM3, again comparable quantities. Figure 17 shows SAGE II sunset and sunrise measurements and WACCM3 predictions of NO2 and ozone percentage changes (31 March 1990 relative to 31 March 1987). Since we compare percentage change, rather than absolute change, in Figure 17 these measurement-model comparisons should be valid. We now have added some text about the tightly coupled nature of NO and NO2, which allows comparison of WACCM3 NOx taken at various times to be compared with HALOE sunrise or sunset NOx (see section 5.2 - paragraph 3).

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

Reply: The reviewer makes a good point that there are other measurements, like SCIAMACHY (See G. Rohen et al., Ozone depletion during the solar proton events

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of October/November 2003 as seen by SCIAMACY, J. Geophys. Res., 110, A09S39, coi:10.1029/2004JA010984, 2005), as well as others (e.g., Weeks et al. 1972; Thomas et al. 1983; Heath et al. 1977; Zadorozhny et al. 1992; Degenstein et al. 2005) that show short-term effects of SPEs. It is beyond the scope of this paper to compare with all such measurements. We have tried to compare with a representative group of the measurements that show different aspects of the SPE-caused perturbation.

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

Reply: We are not sure, but believe the reviewer is referring to the sentence on lines 26-27 (rather than line 6) of p. 10552 that reads The atmospheric effects from these SPEs are probably the best documented for any solar events. For clarification, we will add the subordinate clause For the short-term effects of SPEs, to the beginning of the sentence.

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

Reply: The reviewer is correct that the figures are noted incorrectly in the text and the caption for Figure 3. We have changed left to top and right to bottom.

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).

Reply: Since there are no measurements of HOx increases during the Oct./Nov. 2003 SPEs, it is difficult to say whether or not the computed increases of 700% are realistic. The water availability will limit the HOx increases, although there is plenty of water in the mid to lower mesosphere (0.05 to 1 hPa) for HOx enhancement. Water amounts in this region are 0.3-6 ppmv compared with SPE-induced HOx amounts of only 0.7-8 ppbv.

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.

Reply: The model (WACCM3) is now better described (see section 4.1 - paragraphs 1-3). The model is a free-running general circulation model with self-consistently computed dynamics. We are constrained to the color table picked for MIPAS NOx (Figure 4, top), which was originally presented in Lopez-Puertas et al. (2005a). We have now used the very same color table in construction of Figure 4 (bottom).

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.

Reply: The resolution of HALOE is about 2 km. We used HALOE NOx sunset measurements from 30 Oct. to 7 Nov. differenced with HALOE NOx sunrise measurements from 12-15 Oct. to derive the HALOE values in Figure 5. Since NO and NO2 are tightly coupled and the quantity NO + NO2 is highly conserved during a 24-hour period in the upper stratosphere and mesosphere, it is possible to compare sunrise NOx measurements with sunset NOx measurements and derive the perturbed atmospheric NOx values for a short period (approximately a week). This is explained in Jackman et al. [2005a], where we first presented the HALOE measurement plot, and we also now discuss this (see section 5.3 - paragraph 3). The pattern that the reviewer ascribes to as a diurnal pattern in the measurements on 1 Nov. is a result of the sampling by HALOE, which makes about 15 sunset measurements a day. We now have redone Figure 5 (top) and computed diurnal average values, which now does not show this diurnal pattern. We have also redone Figure 5 (bottom), which shows WACCM results, with a similar latitude sampling as was done for HALOE.

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

Reply: The review paper of C. H. Jackman and R. D. McPeters (2004) summarized much of the agreement and disagreement between observations and models regarding SPE influences. Since models change over time, it is unclear how much such a discussion would add to the paper. We focus on the ability of a current general circulation model with chemistry (WACCM3) to simulate the large polar influences from four very large SPE. We discuss the results of a comparison of observations to WACCM3 simulations in this paper for current Figures 4-12, 14, 15, and 17 (see sections 5.2, 5.3, 5.4, 6.1, 6.2, and 6.3).

10556:12 This must be explained in more detail in the section about WACCM3. 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.

Reply: We do not quite understand this comment. Our discussion on p. 10556 (lines 8-12) reads - The SH modeled ozone below ˜45 km indicates a larger ozone depletion after 2 November, than indicated in the measurements. The reason(s) behind these NH and SH model-measurement differences are still unclear, but are probably caused in part by the fact that transport in WACCM3 is not meant to simulate any specific year. In other words, the WACCM3 does show a large ozone depletion on 3-4 November in the SH, but the MIPAS measurements do not. These WACCM3 model predictions are similar to at least those model results shown in Figure 8 of Rohen et al. [2005]. Therefore, we are puzzled by the comment.

10557:10 Is this reaction included in WACCM?

Reply: Yes, the reaction NO2 + OH + M to HNO3 + M is included in WACCM3. We have now made that clear in the text (see section 5.4 - paragraph 1).

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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 is 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.

Reply: It is beyond the scope of the paper to include the four ion chemistry reactions noted on p. 10557 in the WACCM3 simulations. We do have some ion chemistry reactions in WACCM3, but they only entail constituents N2+, O2+, N+, NO+, O+, and electrons. The old MIPAS data used in the original version of our paper represented the higher atmosphere at lower altitude resolution. Besides masking some structures, limited altitude resolution can shift the VMR maximum in altitude if the averaging kernels are asymmetric. The revised MIPAS data are represented at higher altitude resolution, in particular at higher altitudes, which reduces the altitude difference between model and observations. The reason for the previous differences therefore was not mispointing. We now present the MIPAS data using the new retrieval methodology. The peak altitude of the HNO3 enhancement is fairly similar between MIPAS and WACCM3, however, the magnitude of the HNO3 increase is larger in MIPAS by almost 2 ppbv in the upper stratosphere and lower mesosphere for the 29 Oct. to 1 Nov. period. The enhanced HNO3 in MIPAS and WACCM3 by 15 Nov. is fairly similar in this altitude region. Relatively fast chemistry appears to be creating HNO3 during the SPE, which is not included in WACCM3. The fact that net HNO3 by the end of the plotting period is fairly well simulated indicates that the enhanced SPE-produced HNO3 is not extended much beyond a week or so. The peak altitude of N2O5 enhancement in MIPAS (near 40 km) is about 5 km below the peak from WACCM3 (near 45 km). Also, the magnitude of the enhanced N2O5 is larger from WACCM3 (near 6 ppbv) compared with MIPAS (near 1 ppbv). This may point towards some issues in creating N2O5 in the first place that are discussed in section 5.4 - paragraph 3).

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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.

Reply: We are puzzled by the comment. The WACCM3 simulation leading to the Fig. 9 (bottom) did not have any SPEs in it. Thus the predicted changes in N2O5 are likely caused by seasonal changes. The predicted N2O5 enhancements between 30 and 40 km on 15 November make physical sense as the solar zenith angle is increasing leading to less predicted photodissociation of N2O5.

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

Reply: We assume the reviewer means p. 10558 (line 17). This is a big puzzle. The formation rate of N2O5 may be smaller than modeled. Conversely, N2O5 photodissociation and/or the N2O5 decomposition may be larger than modeled. WACCM3 uses reaction rates from Sander et al. (2003). This is now discussed in the paper (see section 5.4 - paragraph 3).

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

Reply: We do not know the answer to this question. If we did know the answer, then we might be able to correct the chemistry of WACCM3 or retrieval algorithm of MIPAS.

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

Reply: We assume the reviewer means p. 10558 (line 28). We agree that the text needed to be changed. New text is now in the paper (see section 5.4 - paragraph 5).

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 reprocessing 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

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interesting in the paper and may be revised.

Reply: We assume the reviewer means p. 10559 (line 25). We now show the new version of MIPAS data, known as V3O_9, for HNO3, N2O5, and also added NO2 and ClONO2 (see Figures 6, 9, 10, and 11). By the way, the old MIPAS data used in the original version of our paper were not incorrect but represented the higher atmosphere at different altitude resolution, thus masking some structures. The more recent MIPAS data are better resolved in altitude, i.e. the averaging kernels are narrower. As a consequence, the more recent data are better suited for direct comparison with model results.

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

Reply: We have compared WACCM3 to MIPAS temperatures. There are similarities as well as differences and it probably requires a separate study to go into this in any detail. 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. Also, Garcia et al. (2007) evaluated the WACCM3 temperatures in comparison with TIMED SABER temperatures. 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.

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?

Reply: The reviewer is correct. The absolute NOx concentrations are also generally larger. We are not sure about the reason behind the differences, but suspect that either a) the NOx created by auroral electrons in the lower thermosphere is too large; b) the downward transport of NOx from the lower thermosphere is too large; c) a combination of a) and b); or d) other differences between WACCM3 and the atmosphere. We now discuss this in the paper (see section 6.1 - paragraph 4).

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?

Reply: The reviewer is correct that the figures are noted incorrectly in the text and the caption for Figure 16 (previous Figure 14). We will change top to left and bottom to right. The reviewer comments on the correlation between NOx enhancements and ozone depletion are useful. Ozone is being affected by NOx as well as other constituents and dynamics. Thus there is not necessarily a one to one correspondence between NOy increase and ozone decrease. A medium-sized SPE occurred in March 1990 and created NOy in the upper stratosphere and lower mesosphere. The longer-lived NOy from this event was not enough to deplete ozone in this region, however, this event would also have created short-lived HOx that would have depleted ozone for a day or two. There is much output from WACCM3 and for the simulations for the 1 January 1989 through 31 December 1991 period, we had output every five days (e.g., 16 March and 21 March 1990) which, unfortunately, missed the days of the SPE (19-20 March 1990). We now discuss this in section 6.3 - paragraph 2.

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

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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?

Reply: We do point out the differences between the SAGE II observations and WACCM3 predictions. There is fairly substantial interannual variability in NO2 and ozone, much of which is driven by dynamical changes. Thus differences from one year to another (such as 31 March 1990 compared with 31 March 1987) can be significant just from year-to-year dynamical differences. WACCM3 generates its own dynamics, which do not necessarily correspond with the real atmospheric dynamics for a particular year. The reviewer has a good point that looking for a perturbation to the atmosphere (via SPEs) by comparing observations and model results five months after the event is fraught with issues. We have tried to use published observations of SPE perturbations for most of our comparisons and some of these show difficulties in the evaluation of level of agreement.

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

Reply: Thank you. This has been corrected.

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

Reply: Thank you. This has been corrected.

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