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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Composition changes after the "Halloween" solar proton event: the High-Energy Particle Precipitation in the Atmosphere (HEPPA) model versus MIPAS data intercomparison study" *by* B. Funke et al.

C. Randall (Referee)

cora.randall@lasp.colorado.edu

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Summary:

This paper compares atmospheric simulations from a number of different models to MIPAS measurements during and after the Halloween storms of Oct-Nov 2003. The comparisons focus on the short-term (∼ one month) effects in the stratosphere and mesosphere, and include numerous trace constituents in the northern hemisphere

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(NH) high-latitude region in which significant perturbations were observed by MIPAS. All models used the same proton and electron ionization rates, derived from the AIMOS model, in order to control for this particular source of variability among the comparisons.

The results show that there are many areas of both agreement and disagreement among the various models and between the models and MIPAS. In come cases the causes of the discrepancies are understood, but there remain many areas of uncertainty. Overall, this is an outstanding baseline study of our capabilities to simulate the effects of particle precipitation on atmospheric composition. The paper is very well written and organized. It is both comprehensive and concise, and will likely be highly referenced and considered a standard for this type of model evaluation experiment.

In my opinion this paper should be published in ACP after the following comments, most of which are quite minor and may or may not require any changes to the paper, are considered. The authors will note that several of my comments relate to a single issue, that of how to interpret the differences between WACCM, WACCMp, and MIPAS. The electron effects are relatively small, but the paper emphasizes in several places the "better agreement" that would be obtained if electrons were not included. Given the large uncertainties in other quantities, I do not feel that this emphasis is justified (see below for details). After the comments on content I have also included a list of grammatical suggestions.

Specific comments:

Title (and abstract and throughout): There are inconsistencies in the way the "Halloween" storm(s) is(are) described. If referring only to the SEP that occurred on 28 October, the singular form should be used (as in the title). But if referring to more than one storm, the plural form should be used. Gopalswamy et al. (GRL 2005) could be cited for a description of the events, but it would also be very helpful if, near the beginning of the paper (at least by the time Figures 4-5 are described), a timeline of the ionization events were presented. This should include, for instance, a listing of the main proton and electron precipitation events. Although this is available elsewhere, it is necessary for interpreting the results presented in this paper, and would make it much more convenient for the reader if the information were all in one place in this paper.

p. 9409, lines 20-22. The abstract states that the simulated NOy enhancements near 1 hPa are typically 30% higher than the observations, and that this can be partly attributed to an overestimation of simulated electron-induced ionization. The paper describes the uncertainties in specifying the electron ionization source, but in my opinion does not provide compelling evidence that these uncertainties are the main explanation for the overestimate. As noted below, the argument that agreement between MIPAS and WACCMp (no EEP) supports EEP being in error is very weak – this would only be the case if EEP was expected to have a negligible effect. On the other hand, there is a long discussion, starting on page 9438, of other possible factors that contribute to the model/measurement (and model-to-model) differences. If anything deserves mention in the abstract, I believe it would be these other arguments.

p. 9418, line 2: Consider citing Orsolini et al. (2005) as independent confirmation (but with better vertical resolution) of the MIPAS results.

p. 9423, ~line 5: The atmospheric parameters upon which ionization rates depend were taken from HAMMONIA and MSIS. How representative are these for the actual conditions during the Halloween storms, and are they a significant source of error?

p. 9430. Is SOCOL free-running, or nudged to ECMWF?

p. 9434, top: I would argue that EMAC does a fine job below 1 hPa.

p. 9434, lines 6-7. The text states that the stratopause temperatures are too high in several models. Since Figure 7 does not show the individual model results (as opposed to differences), this cannot be inferred from the figure. Is it clear that the stratopause temperature is too high, or is it possible that the stratopause height in the models is

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just shifted vertically from that observed?

p. 9434, lines 10-11. The text states that no significant trend can be observed in the models or observations. For the most part, this is true. Yet around 1 hPa or slightly above WACCM starts out near or slightly higher than MIPAS, and ends after 27 Nov with an $\sim$10-15 K low bias. This is interesting in that WACCM is nudged up to 40 km, which suggests that its temperatures near 1 hPa should be reasonable. It would be interesting to read a comment on this.

p. 9434. Please define the CH4 meridional anomalies (i.e., to what are the anomalies referenced?).

p. 9435, lines 18-19. To say that the general behavior of CH4 is qualitatively reproduced by the models seems to be an overstatement, given that several of the models predict very different (opposite) behavior in the mesosphere.

p. 9435, last paragraph. This explanation seems to leave out the possibility that NOx might be produced via particle ionization in air that has already descended. Thus air with high CO and high NOx does not *necessarily* indicate descent of a NOx enhancement. Is it possible that by neglecting these air masses, you are underestimating the actual particle effects?

p. 9438, lines 16-18. Referring to the NOy overestimate near 1 hPa, the text states, "A possible overestimation of electron ionization is also supported by the better agreement of the WACCM simulation without electrons (WACCMp) with the observations." This would only be the case if you really do not expect a significant effect from electron ionization. Although the quantification of the electron effect is uncertain, as described well in the text, is it really expected to be negligible? If this can be supported, it would be an important conclusion. But given the large spread in the model results at 1 hPa, which (as written) suggests that something else is causing large errors, there seems to be little, if any, justification for inferring such a conclusion. I therefore suggest deleting this statement.

p. 9442, lines 10-15. Related to the last paragraph on p. 9435: doesn't this (excluding model and observed NOy profiles where MIPAS CO was higher than 1 ppmv) ignore air parcels that had descended and subsequently experienced particle ionization? Also, given the very poor agreement shown in Figure 14, I would question whether the geographic locations of the CO-enhanced air parcels observed by MIPAS are the same as in the simulations. If they are not, is the exclusion of profiles based on MIPAS CO values really effective? I think that this could (should) be easily checked (not necessarily presented) by making a figure similar to Figure 14, but for CO.

p. 9442, line 18. The text states that Figure 15 shows the NOy enhancements "related to the SPE, only". Perhaps I have a misunderstanding (which would also be reflected in my last comment) – to what does "the SPE" refer? Is it only proton ionization? Is it proton + electron ionization, but only during the SPEs in late October and early November? This could perhaps be clarified by referring directly to Figure 4.

p. 9442-9443. The last paragraph on 9442, which continues onto page 9443, discusses the overestimate of NOy by WACCM, attributing the overestimate largely to the "excess production during the second event" from electron ionization. WACCMp matches MIPAS quite well for most of the time period, below about 0.2-0.3 hPa, which leads the authors to conclude that the electron ionization must be in error. But rather than matching, shouldn't there be a persistent underestimate in WACCMp, since it neglects the electron source? I agree that electron ionization errors are likely contributing to the overestimate. However, in keeping with my previous comments, it is not clear to me that the electron errors are necessarily any worse than other contributing errors.

p. 9451, last sentence. The text notes that the model simulations overestimate the seasonal buildup of ozone in the mesosphere. What conclusion should be drawn from this? Related information is given on the bottom of page 9453, but the implications of the results are still unclear.

p. 9452, lines 15-16. The text once again points out the better agreement with MIPAS of WACCMp than WACCM. Although the implication is unstated, the reader is left to infer that the conclusion to be drawn from the results is that the contribution to ozone loss by electron precipitation is negligible. As noted above, it is not clear that the results support this conclusion.

p. 9453, lines 13-15. Similar to my previous comment: The text states that the differences between WACCM and MIPAS "could be significantly reduced when excluding the electron contribution to atmospheric ionization." That is a true statement, but is excluding the electron contribution justified?

p. 9454, lines 8-11. This repeats the statement that "agreement between models and observations could be [even] improved when excluding the electron contribution to atmospheric ionization." While the agreement might be improved, is it improved for the wrong reason?

p. 9462, middle paragraph. Same issue as above. In order for this paragraph to warrant so much space in the conclusions, it is necessary to show that WACCMp agrees better with MIPAS for the right reasons; that is, that electron ionization really should not be a significant contribution. Otherwise, turning off the electron precipitation is just a proxy that compensates for some other source of error.

Figures:

The text in Figures 1-3, 15, and 25-26 is so small that it is very, very difficult to read when printed. Also, the white dashed lines often disappear, so they should be made thicker.

Figure 3 labels: Chlorine should have a lower case l, not upper case.

Figure 6. Is the middle panel WACCM with application of averaging kernels? This would seem to contradict the sentence on the top of page 9433 that states, "...as modeled by WACCM with and without application of averaging kernels. In the latter case, the vertical distribution is broader and slightly shifted towards lower altitudes...."

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Perhaps this should say "In the former case. . .."?

Figure 12 caption: Omit "model" before "multi-model mean". Also, it appears as though the single model, SOCOLi, is skewing the model results. Would a median be more representative of the models?

Figure 14 caption: "averaged over the period. . ." instead of "averaged of the period. . .".

Figure 14 description (page 9441). This figure shows exceptionally large discrepancies between the models and MIPAS. Yet it gets relatively little discussion in the paper. Most of the discussion centers on the SOCOLi results, which appear to be anomalous even among the very disparate results from the other models. This figure seems to be pointing to a systematic flaw in the models, and in my opinion deserves more discussion. Perhaps the description of SOCOLi was supposed to be illustrative of what is wrong with all of the models; if so, this should be made more explicit.

Figure 21 caption: The caption states that differences between modeled and observed averages are shown, but I think that only the changes in N2O5 since 26 October are shown.

Figure 22 caption: Same issue as with Figure 21.

Figure 33 caption: "in" not "n".

Minor editorial suggestions and corrections:

Title (and p. 9411, line 13): In keeping with the HEPPA convention "High Energy" should not be hyphenated.

p. 9409, line 16 (and throughout): implications "for" (not "on").

p. 9410, line 9: sporadic, not sporadically.

p. 9411, line 26: Use either "from 25-75 km" or "between 25 and 75 km".

p. 9417, line 7: "by the end of November" instead of "until the end. . ."

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p. 9417, lines 13-17: This sentence currently lacks a verb – it can be corrected by writing, "This version has already been used...". Also, add a comma before "which allows for...".

p. 9420, line 22: "Vertical resolution "is" 8-12 km below 2 hPa and coarser "than" 15 km above."

p. 9421, line 25: "A detailed description of ... " (not "on").

p. 9422, line 9: "high energy" instead of "high energetic".

p. 9423, line 1: "sea level" instead of "sea-level".

p. 9423, line 11. Remove "the" before "28 October".

p. 9424, line 26: Remove "started".

p. 9425, line 7: Is it really 3402 K, not 3400 K?

p. 9425, line 11: Remove the hyphen (wind fields, not wind-fields).

p. 9425, line 16: ClOx, not CLOx.

p. 9427, line 4: "been" used (not "be" used).

p. 9429, lines 1-4. Redundant sentences.

p. 9430, line 12: "was" taken into account (not "were").

p. 9430, lines 23-24: "describes", not "describe".

p. 9431, line 24: "extended", not "extend".

p. 9434, line 4: Remove the comma after "troposphere"

p. 9434, line 10: Consider, "No significant trend in either, observations or model data...."

p. 9434, line 11: "interest", not "interested".

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p. 9435, line 27: Remove the comma after "event" and add a comma after "models".

p. 9436, line 5: "averaged" instead of "average".

p. 9436, lines 10-11: "decreased" not "decreases".

p. 9438, line 11. "energy spectra were" or "energy spectrum was" – not "energy spectra was".

p. 9438, line 12: "brings into question" instead of "questions".

p. 9440, lines 7-8. Write out "reactions".

p. 9443, line 17. "high energy" not "high-energetic".

p. 9443, line 19-20. "associated with" not "associated to".

p. 9443, line 25. "giving rise to" not "giving rise for".

p. 9447, lines 13-14. Consider: "The second enhancement, occurring around 15 November at 1-2 hPa, is only reproduced by KASIMA. In this case, however, the observed increases are overestimated by a factor of 3."

p. 9447, line 23. "quantitative" instead of "quantitatively".

p. 9449, lines 26-28. Consider: "The HNO4 enhancements are also simulated by the models in the first days of the SPE, but are generally overstimated."

p. 9450, line 14. "Additionally" not "Aditionally".

p. 9452, line 23. "gives rise to" instead of "give rise for".

p. 9453, line 21. "higher polar" instead of "polar higher".

p. 9453, line 22. "despite the" instead of "despite of the".

p. 9460, line 14. "increased" not "raised".

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9407, 2011.