Atmos. Chem. Phys. Discuss., 11, C5498–C5503, 2011
www.atmos-chem-phys-discuss.net/11/C5498/2011/


Atmospheric Chemistry and Physics Discussions

ACPD
11, C5498–C5503, 2011

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.

**B. Funke et al.**

bernd@iaa.es

Received and published: 27 June 2011

We thank Referee 3 for helpful comments and suggestions. The "Referee's Comments" are noted first and then we give our "Reply:" to the comment.

*1. Discussion on p9438 of electron ionization overestimation and Fig. 12. This does not appear to be consistent since the model mean significantly underestimates NOy between 0.2 and 0.03 hPa. The WACCMp simulation underestimates NOy even more*



Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

*above 0.1 hPa so cannot be regarded as solving the problem. At these altitudes vertical transport is large on the timescale of a week. So a comparison of a three day mean will show non-negligible differences due to transport. If the models overestimate downward transport in the middle and upper mesosphere they will have lower values of SPEs induced NOy compared to observations. The fact that the model mean exhibits higher values in the stratopause region is consistent with too rapid vertical transport.*

Reply: Regarding the model underestimation of NOy enhancements between 0.2 and 0.03 hPa, we speculate that this could be related to an overestimation of NO photolysis rates in the models caused by an underestimated thermospheric NO column and hence reduced selfabsorption of thermospheric NO. This hypothesis is supported by the fact that a model underestimation of NOy is not seen when averaging only over the polar region (70-90N) where photochemical losses of NO are small. Regarding the model overestimation of NOy enhancements around 1 hPa, we agree with the Reviewer that this cannot be explained by overestimated electron-induced ionization rates alone (in fact, electron-induced ionization contributes by only 15the models, and transport schemes could contribute. However, in order to explain the encountered model overestimation exclusively by differences in vertical transport, as suggested by the Reviewer, modeled vertical descent rates must be too fast around 1 hPa by 2-3 km per day. Such fast stratospheric vertical velocities have never been reported. Further, a systematic model overestimation of descent rates by such large values are not consistent with the observed and modeled CO distribution shown in Fig. 11.

In order to account for other possible reasons than overestimated electron ionization for the differences in modeled and observed NOy enhancements around 1 hPa, we have re-written the discussion on page 9438. In the revised version, this paragraph reads: “The systematic behavior of the NOy overestimation around 1 hPa suggests that these differences are related – at least partly – to the simulated ionization rate profile. In this pressure range, uncertainties in the modeling of electron precipitation at 300 keV to 5 MeV, contributing to the total ionization by approximately 15highest electron



ACPD
11, C5498–C5503, 2011

Interactive Comment

channel on POES does not provide data up to 5 MeV, the energy spectrum was extended according to Klassen et al. (2005). In addition, the energy range of the highest electron channel mep0e3 is not known for sure (private communication, Janet Green, NOAA) and it might be smaller than the published 300 keV–2.5 MeV (Evans and Greer, 2000). A smaller energy range would also result in increased NOy production within 40–90N at 0.1 hPa in agreement with the observations. A possible overestimation of electron ionization alone, however, cannot explain the mismatch between modeled and observed NOy increases of up to 50enhanced NOy than the nominal simulation, including protons and electrons. Even when assuming that electrons do not contribute to the SPE-induced ionization at stratospheric altitudes, only about half of the differences between modeled and observed enhancements could be explained. Additional ionization by alpha particles, included in CAO, FinROSE, SOCOL, and SOCOLi contributes only by approximately 590N, hence increasing the SPE-related NOy enhancements only marginally. Other possible error sources in the ionization rate calculation are related to uncertainties of the GOES proton flux observations and to the spatial interpolation scheme for particle fluxes. Also, uncertainties of atmospheric parameters (density, altitude, composition, and temperature) used in the AIMOS calculations could produce errors in the ionization rates. These parameters, taken from HAMMONIA and MSIS calculations, might differ from the actual atmospheric conditions during the Halloween SPE. Apart from possible deficiencies in the ionization rate calculation, also differences of the true and modeled atmospheric background state and/or dynamical conditions could contribute to the encountered model overestimation of NOy enhancements. However, such differences are likely to produce a spread in the modeled NOy increases rather than a systematic bias compared to the observations.“

*Overestimation of NOy by the models after November 1st is again a reflection of vertical transport differences compared to observations. Mesospheric polar descent in WACCM is too strong. Removing electron induced NOy production just hides this problem. In the short run too much vertical descent will underestimate NOy in the mesosphere, especially from the short lived SPEs, but in the long run there will be too*



ACPD
11, C5498–C5503, 2011

Interactive Comment

*much NOy produced from electron ionization descending from above 75 km. The EPP forcing and transport in these simulations and in the observations are transient.*

Reply: We do not agree that mesospheric polar descent in WACCM is too strong. The temporal evolution of observed and modeled CO abundances averaged over 70-90N (Fig. 11) clearly shows a very good agreement between MIPAS and WACCM. On the other hand, we agree that different descent velocities of NOy produced above 75 km would cause differences in modeled and observed NOy at lower altitudes on a mid-term scale. For this reason (and because this aspect is beyond the scope of our study) we have eliminated air parcels descended from the MLT (showing CO abundances higher than 1 ppmv) from the comparison.

*The concept behind the MIPAS filtering for Fig. 15 is rather strange. The models are being forced with both electron and proton EPP. So there will be low and medium energy electron Noy descending from the upper mesosphere into November. The model lids are much higher than 0.1 hPa as is the vertical distribution of EPP. Why were the models not filtered for the electron source? A proper comparison of the MIPAS filtered data and the models would be such as done for WACCMp: have the models only simulate proton ionization.*

Reply: The concept behind the filtering of MIPAS observations with CO abundances higher than 1 ppmv is not to exclude the electron source but to exclude EEP indirect effects (i.e. the descent of NOy produced in the lower thermosphere) that partly mask the observed SPE effects on a midterm scale. This has been made clearer in the revised version. The rational behind the selected approach is that EEP indirect effects are not well represented in atmospheric models in general. For models not including the lower thermospheric source region this shortcome is evident. But also models with higher lids tend to underestimate the indirect EEP contribution for reasons that are currently not fully understood. Since the analysis of EEP indirect effects is beyond the scope of this manuscript (though it is the topic of a new intercomparison exercise which has been launched recently), and since these effects interfere with the observed SPE-related direct effects, we decided to exclude air masses affected by transport from the MLT region.

*The attribution of NOy biases in models to the electron ionization source is too narrow. The role of model dynamics in producing these biases should be directly stated.*

Reply: We have revised the discussion of possible reasons for the NOy biases on p9438, including also other error sources than deficiencies in the modeling of electron ionization (see reply above).

*2. In addition to the transport issues in the models it is not clear from their descriptions in section 4 how the photochemical J-values were treated at high solar zenith angles. Based on my experience with low lid CCMs they do not consider photolysis for SZA > 95. This approximation breaks down in the mesosphere and SZA values as high as 100 should be included. Without this correction values of NO2 become excessive in the lower mesosphere and as a result N2O production is too high, which is directly relevant for the discussion on p9443 and p9444. The NO2 bias will likely affect other chemical species distributions as well.*

Reply: We agree that the proper treatment of photolysis rate calculations under twilight conditions is crucial for the mesospheric NO2 availability and hence the production of N2O. Indeed, differences in the modeled NO2/NOx ratio have been encountered close to the terminator (see Fig. 20 and discussion in Section 6.3.1) that can be related to different cut-off SZAs in the photolysis rate calculations. In particular, CAO, EMAC, FinROSE, and HAMMONIA, overstimating the NO2 fraction close to the terminator, use cut-off SZAs lower than 95 degrees. Other models use values equal to or higher than 98 degrees. In the revised version, we have extended the discussion in Section 6.3.1 to make this point clearer. Regarding the possible overestimation of N2O enhancements related to underestimated NO2 photolysis in the terminator region, however, a clear relationship of modeled N2O and cut-off SZA, used in the photolysis rate calculation, could not be established. For this reason, we do not explicitely mention this issue in the discussion of observed and modeled N2O changes (Figure 18 and Section 6.2).

*Minor Comments*

*Section 4: The specification of horizontal resolution for several models is incorrect. For example, T31 is said to correspond to 96 x 48 and T42 is claimed to be equivalent to 128 x 64 in terms of the Gaussian grid. In both cases these are the nonlinear transform grid dimensions which are 50degree horizontal resolution and T42 is 4.3 x 4.3 degree. This confusion is found in many publications and is unfortunate.*

Reply: In principal we agree with the ReviewerÊijs comment. In the case of EMAC, however, the wording "T42 (corresponding to a quadratic Gaussian grid of approximately 2.8 by 2.8 in latitude and longitude)" is correct because "quadratic" does not mean that the grid is quadratic but that second order terms, i.e. quadratic terms, can be transformed "aliasing-free" between the spectral and grid point representation. The effective resolution, i.e. the "linear Gaussian grid" is only used for the solution of the dynamical primitive equations, and the finer "quadratic Gaussian grid" for physics and chemistry. The HAMMONIA model description has been reworded. We now state: “Linear terms of dynamics are calculated using triangular truncation at wavenumber 31 (T31), while nonlinear terms of dynamics and spatially dependent physical and chemical quantities are computed on a Gaussian grid of approximately 3.75 $\times$ 3.75 degrees.

---

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

ACPD

11, C5498–C5503, 2011

Interactive Comment