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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #3

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Major Comments

A comprehensive multi-model intercomparison with MIPAS observations of the 2003 SPEs is presented in this study. The chemical diagnostics are extensive and well presented. However, some points need to be clarified before this paper can be accepted for publication.

1. Discussion on p9438 of electron ionization overestimation and Fig. 12. This does not appear to be consistent since the model mean significantly underestimates NO_y between 0.2 and 0.03 hPa. The WACCMp simulation underestimates NO_y even more 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 NO_y compared to observations. The fact that the model mean exhibits higher values in the stratopause region is consistent with too rapid vertical transport.

   Overestimation of NO_y 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 NO_y production just hides this problem. In the short run too much vertical descent will underestimate NO_y in the mesosphere, especially from the short lived SPEs, but in the long run there will be too much NO_y produced from electron ionization descending from above 75 km. The EPP forcing and transport in these simulations and in the observations are transient.

   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 NO_y 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 this MIPAS filtered data and the models would be such as done for WACCMp: have the models only simulate proton ionization.

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

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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 NO_2 become excessive in the lower mesosphere and as a result N_2O production is too high, which is directly relevant for the discussion on p9443 and p9444. The NO_2 bias will likely affect other chemical species distributions as well.

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 50% larger than the actual resolution to prevent aliasing. T31 is basically a 5.8 x 5.8 degree horizontal resolution and T42 is 4.3 x 4.3 degree. This confusion is found in many publications and is unfortunate.

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