Atmos. Chem. Phys. Discuss., 10, C11333–C11337, 2010
www.atmos-chem-phys-discuss.net/10/C11333/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Middle atmosphere response to the solar cycle in irradiance and ionizing particle precipitation" *by* K. Semeniuk et al.

Anonymous Referee #4

Received and published: 20 December 2010

This paper looks at a combination of solar and energetic particle forcing on the middle and lower atmosphere using the CMAM chemistry-climate model. The authors have performed extensive simulations and analyzed them with respect to ensemble mean differences and multiple linear regression. While CMAM is clearly a state-of-the-art model, in my opinion the choices of the set up of the simulations limits their applicability to understanding particle and irradiance effects in the present day atmosphere. In particular, ignoring EPP production above the model top, performing simulations prior to the formation of the ozone hole, and having no surface NOx emissions. These limitations need to be clearly stated. That said, I think the results presented here are certainly worthy of publication.

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Specific comments by line number:

24856/29 In reference the Jackman et al "Based on their work it is apparent that CCMs are not able to reproduce all the features found by satellite measurements of atmospheric composition. Indeed, the work of Callis et al. (2001) demonstrates that SPEs are not the only type of EPP that can have a significant impact on ozone in the stratosphere and that auroral electron precipitation also needs to be taken into account." Missing sources of ionization are just one possible way there could be a discrepancy between a CCM and observations. The most likely is that in a chemistry climate model, the winds are not the same as observed at any specific time. The Jackman et al. (2008) paper did not conclude that there was a missing source of ionization but that water ion chemistry was not modeled and also uncertainty in the NO2 + OH + M reaction. Finally, the Jackman et al. study included auroral electron precipitation but not medium energy electrons that might produce NOx and HOx directly in the mesosphere and stratosphere.

24857/12 "However, to the best of our knowledge there is no published analysis focused on the global role of EPP, including galactic cosmic rays, coupled with the solar cycle evolution of the atmosphere in a chemistry climate model." As stated in the introduction, the Schmidt et al. (2006) study include the solar cycle irradiance change, GCR and auroral NOx.

24858/25 The simulations were conducted under 1979 halogen conditions. Since that time equivalent effective stratospheric chlorine (EESC) has doubled - see e.g. Figure 10 of Newman et al. (2007). The ozone hole area in 1979 was essentially zero, while in 2000 it was almost 25 million square kilometers. Since the effect on ozone of EPP-NOx could actually be to increase ozone by tying up active chlorine in ClONO2, some discussion is needed as to the impact of this choice of chlorine loading for assessment of the present-day atmospheric response to EPP.

24857/21 "Three types of EPP were included in the model: auroral electrons, solar

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proton events (SPEs) and galactic cosmic rays (GCR)." I think the use of 'auroral electrons' is misleading, and should be changed to medium energy electrons throughout the paper. The aurora is outside the model domain (model lid at 95 km with the top two scale heights, or ∼14 km, acting as a sponge layer) so cannot be included. It appears the lowest energy electrons considered are in the 30-100 keV range, which have peak ionization rates between 70 and 90 km. The aurora is produced mostly by electrons in the 1-10 keV range. 1 keV electrons have a peak ionization rate at ∼120 k. Note, SPEs were already defined on page 24856.

24861/19 "PHOx = aI, where a(z) is a height dependent function that varies from a value of 2 at 40 km to zero above 90 km and is taken from Solomon and Crutzen (1981)." The production is also a dependent on I, being less effective at high ionization rates (e.g. at 80 km dropping from 1.4 to 0.95 for an increase in ion pair production from 100 to 10000 cm-3s-1) from - was this considered? If not, what error is introduced by this omission.

24863/1 "Firstly, during descent in the lower thermosphere and upper mesosphere region air parcels experience large meridional excursions through tidal and gravity wave action." This statement needs to be proved. The spatial extent of an individual gravity wave is not sufficient to move a parcel very far in the meridional direction, and unlikely to be a significant source of chemical-eddy loss. Similarly, tides are thought to have small altitudes at the poles in general, and not likely to contribute to significant losses. Perhaps large scale planetary waves can contribute to the losses (as suggested by observations of the 'd-region anomaly'), but this is not discussed.

24863/8-12 This argument ignores the very rapid increase in NO density as observed by HALOE and SNOE. NO increases by several orders of magnitude, and can outpace the atmospheric density drop off. I think this is a fundamental problem with this study. Sources of auroral NOx (aurora here referring to NOx produced in the aurora between 100 and 130 km) and their variability over the solar cycle have been modeled by HAMMONIA and WACCM and shown to affect NO concentrations into the meso-

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sphere. This study should have included this variability as a varying upper boundary condition. Since it did not this important omission needs to be clearly stated in the text and abstract.

24864/20 What happens between 45 and 60 degrees? Also, is there scientific support for SPE ionization all the way down to 25 degrees. I seems most other studies strictly limit ionization to poleward of 60 degrees.

24866/18 I am struck by the high degree of similarity in the SH polar regions above 20 km. Why should GCR produce the same signal as SPEs when the chemical changes they produce occur in very different locations? Can the authors please comment on this?

24867/2 Is this speculation? Have EP-flux divergences and their differences been calculated?

24867/20 Suggest you reverse the order of figures here, since you start with JJA.

24869/14 Since no temporal evolution of NOx is shown how does the reader know this NOx is from the previous winter?

24870/1 The authors state that the surface and lightning emissions of NOx are neglected. Therefore the response to GCR below 20 km is certainly inflated.

24875/3-15 Is this speculation, or has the wave-driving been calculated?

24876/25 It should be made clear that the identified pattern was attributed to the development of the ozone hole.

24879/11 Is Figure 16 necessary - it seems that it can be stated the response is very similar to Figure 10.

24881/22 'yearly variation' here is confusing, since it could also mean annual cycle. Better to say global mean, annual mean total column ozone. Unfortunately there is a problem comparing SH high-latitude variations since the Fioletov data includes the

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development of the ozone hole.

24885/13 No changes in drag or flux divergence have been shown in this paper, so this is speculation.

References:

Newman, P. A., Daniel, J. S., Waugh, D. W., and Nash, E. R.: A new formulation of equivalent effective stratospheric chlorine (EESC), Atmos. Chem. Phys., 7, 4537-4552, doi:10.5194/acp-7-4537-2007, 2007.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24853, 2010.