Atmos. Chem. Phys. Discuss., 10, C11345–C11346, 2010
www.atmos-chem-phys-discuss.net/10/C11345/2010/

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.

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Received and published: 20 December 2010

In this article, the authors investigate the coupled chemical and dynamical response of the middle atmosphere to particle precipitation in a chemistry-climate model (CMAM). In particular, the interplay of the particle precipitation and the solar cycle is examined.

The authors have carried out an extensive set of simulations, and the issue of great importance to understand the solar impact on climate. I however have doubts about the methodology used to interpret the dynamical results. While the chemical response induced by the different types of particle precipitation (SPEs, auroral electrons or galactic cosmic rays) is quite clear, for example, in terms of perturbed NOx or O3 amounts, the attribution of the dynamical response is difficult to understand, very qualitative and at

times speculative. For example, two ensemble members give winter-mean dynamical responses in the SH that are quite different: the high-latitude jet shows weakening in one member and a strengthening in the other member (Fig. 11). Hence, I wonder how robust and reproducible is the dynamical response? Can the wind patterns in Figures 4,8,9 be attributed to response in ozone changes due to particle precipitation, or are they the results of different trajectories followed by model integrations? For example, does the jet weakening in the austral winter when auroral electrons are taken into account appear every year, is it particularly strong when auroral ionization is large, or does the run-mean weakening results from a particular year only, when model integrations were strongly diverging. I think the authors should demonstrate that, before the paper is acceptable for publication.

The authors also attribute the changes in Brewer-Dobson circulation to wave propagation and forcing, but the sources terms of the mean meridional circulation should be evaluated (e.g. diabatic heating rate gradient and EP flux divergence).

I do not understand why the SPEs only have an impact in SH winter (Figure 4), while ozone changes seem as large in DJF (Figure 5; there is no figure corresponding to figure 4, for DJF). In fact, several important SPEs took place in the boreal cold season (Halloween 2003, January 2005), and most recent modeling studies focused on those events.

The paper is lengthy and wordy, and the number of figures is large (23 figures). A table with recapitulative characteristics of the various runs would be desirable. There should be more a coherent presentation of the figures: NOy, HOx and O3 differences are first presented as rows (Figures 5, 6), then later as columns.

The abstract does not bring up the main points in an orderly fashion. The first result mentioned concerns ozone changes which are shown on Figure 17.

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