

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

Anonymous Referee #2

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1 Summary

The paper evaluates the combined effects of solar uv-vis variability and solar modulated energetic particle precipitation effects in the middle atmosphere using the chemistry climate model CMAM. There are a lot of papers addressing solar cycle effects by UV-vis variability. Especially during the recent past also many papers focusing on particle effects have been published, but the combined effects hasn't been studied in detail. Recent observations and model studies showed pronounced effects of particle precipitation in the polar middle atmosphere. So, this study which probes the sensitivity of the EPP impact compared to solar uv is very relevant and much in time. The concept of

C11238

the paper is straightforward and logical, and the methods used are state of art. The authors convincingly show that it is the particle precipitation which can bring observations and model simulations of solar cycle effects in significant better agreement compared to simulation neglecting this effect, at least when using their model.

My main objection is about the robustness of the results as the model lacks a realistic ocean, a qbo or a comprehensive troposphere. At many places the discussion tends to overinterpret results without being quantitative. Nevertheless, the paper should be published after some minor revisions as it brings the impact of particle precipitation in the context of current studies of chemistry climate interaction.

2 Specific comments

p24858_19: The description of CMAM leaves somewhat open what tropospheric processes are included in the model and to what detail. Meehl et al., 2009 demonstrate the influence of coupling of ocean surface temperatures, precipitation and tropospheric circulation together with stratospheric processes for the solar signal. Could you give additional specifications of the model in this respect?

p24858_111: The model does not contain processes on NAT particles in the stratosphere as I understand from the references given. As the production of NO_y is the most important effect of the EPP, some coupling via NAT is expected. Can you comment on that?

p24858_111: The model has no HNO₃ production by water cluster ions? This should be mentioned as it was shown that this reaction can explain the secondary HNO₃ maximum observed during polar winter with strong NO_x intrusions (Reddmann et al.)

p24858_119: Here you write that CMAM models have a realistic SSW rate. On page

C11239

24882_13 you write that the model NH polar vortex is too disturbed compared to the real atmosphere. Isn't that a contradiction?

p24863_11: The arguments are weak. First, there is polar night also in 100 km altitude. Then NO has a regular maximum in absolute concentration above about 60 at about 105 km, transport retains mixing ratio, and from Arctic recent winters (2003/4, 2005/6, 2008/9) we know that fast and efficient transport from the lower thermosphere can occur. It may be sufficient to note that the model does not transport big amounts of NO from altitudes above 90 km downwards and so for this study this will not change the conclusions.

p24863_113: Using the peak fluxes mean that ionization calculated is an upper limit?

p24864_13: What does realism mean?

p24866_section 4: The following subsections describe in detail the effects of the individual particle types. Whereas the presentation of the individual results is necessary in the structure of the paper, I'm not quite sure if the detailed description of individual features is really justified. Sentences like "there is modest decrease (but not statistically significant) which may be associated with ..." can be found very often and leave the reader a bit helpless. I would strongly suggest to condense this part and to concentrate on the most important features.

p24866_122: The interpretation of the difference plots of the wind and the stream function is difficult without knowing the position of the jets. For me many zonal wind plots (also for SPE etc.) show a dipole structure which could be explained by a shift in the position of strong wind fields.

p24867_19: What causes the strong change of the mass stream function in the troposphere and what could that mean for STE?

C11240

p24867_114: At least we know from observations that NH winter events can be large and may bring more NO_y into the atmosphere compared to SH events. Is the argument then specific to the CMAM model and related with the fact that you have a more disturbed NH winter stratosphere?

p24867_127: Information on reactions can be found in textbooks and could be restricted when you extend or change standard chemistry. Also applies for page 24869.

p24868_13: This refers to the SH only.

p24868_18: This is a feature of the model and not the real atmosphere.

p24870_125: Here you state that the increase of O₃ is of low statistical significance. Indeed, near the tropopause this is not very clear, and so is the explanation by increased STE. Can you be quantitative?

p24871_11: One important finding of the paper is increased H₂O at the tropopause. Would it not make sense to include H₂O plots too?

p24871_125: But how is it possible that vmrs at the top level NH is higher in summer than in the maximum values in winter?

p24876_17-8: Ozone reduction between 20 - 40 km.

p24876_115: Do you mean that for example doubling SPE would also show no dynamical change? I find this sentence misleading. Is there a other regime with higher dynamical sensitivity?

p24876_125: Polvani and Kushner applied changes in their model doubling the maximum polar jet speed, and observe a change in the troposphere of a few m/sec as a result. The change in your model results must have a different origin.

p24877_14: You change the season within the paragraph? Fig 9 lower panel is DJF.

C11241

p24877_I16: How strong do the two realisations differ? Without being specific one can get the impression the results are very sensitive to the basic state of the runs. In addition, what is the meaning of the significance contours when from one to the next realization even the sign of maxima changes which have in one realization a significance level of 95% (Fig.11, streamfunction, tropical lower stratosphere)?

p24878_I17: But there is no significant cooling in JJA between 17-25km in the tropics where a signal is expected, but a significant cooling there in DJF when BD weakens.

p24881_I15: Is the increase of H₂O only seen in the combined runs? What radiative forcing is connected with the increase?

p24896_Fig.2: What is meant with pole (geomagnetic?, geographic?). Is the pole appropriate for the auroras? The caption should give the source or refer to the text.

p24898_figures_contourplots: The figure panels have to be re-processed to include a caption what is shown, to label also the rows, and to add units to the color bars.

3 Minor comments

p24854_I19 *variability* of solar radiation

p24860_I24: What are the parameters of the look-up table? Over-head O₃ column, T?

p24862_I11: Stiller et al. describes observations.

p24870_I11: SH winter polar region *above 50 km*.

p24870_I17: From the foregoing sentence, it is the winter SH. Photolysis then?
C11242

p24882_I10: "... thought to be responsible for this feature". Please give a reference.

4 Typos etc.

p24863_I19: parameterized

p24885_I25: 2010),

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