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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 #1

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The manuscript uses simulations of the well-established model CMAM to investigate the impacts of the solar cycle and particle precipitation on the atmosphere. The main conclusion is that for accurate simulations of ozone and dynamics, these processes need to be properly taken into consideration. The paper is well suited to publication in ACP and in my opinion needs only minor revisions. My main concerns were in the overgeneralisations of the results obtained from the one model in the comparisons with measurements, and the need for more comments on statistical uncertainty in some places.

General Comments

1. I am uncomfortable about some of the comparisons of the model results with measurements due to the large observed uncertainties. In addition, the authors accept the work of others which indicate that SSTs play a role in possibly aliasing on to the solar ozone signal. Yet their simulations do not take this into account, except via linear regression. This needs to be resolved, either by reducing the claims for agreement with observations, or by completing more simulations forcing with the observed SSTs.

2. In many places, model results are indicated as "statistically significant" but the significance level is only explained in the caption to Figure 4. It would be helpful if the text indicated also how significance was determined. Also, I do not see the advantage of including significance at the 90

Specific Comments

p.24854, l.10. ".....closer to observed patterns." This is not justified by the large uncertainties in the model and observations and should have a suitable caveat. Also, the absence of observed SSTs in the model forcing invalidates the comparison in many respects.

p.24855, l.18. Typo: "an" –> "a". Please review the use of a and an throughout the text.

p.24855, l.23. Solomon et al. [1983] is not given in the references.

p.24855, l.29. The effects have not been ignored just because of their complexity, although this is indeed a contributing factor. Rather, the effects have been ignored because of their overall small impact on climate timescales which has been of most interest to users of CCMs. While I take the point that systematic errors (missing processes) in CCMs should be fixed, the cost also needs to be taken into consideration. The authors argue very strongly in favour of including these processes in view of their undoubted expertise. On a cost/benefit analysis putting resources into including a more complete representation of PSCs in CMAM might prove more beneficial than solar effects.

p.24858, l.19. Actually, the CMAM stratospheric warming frequency in CCMVal-2 was somewhat higher than observed [Butchart et al., 2010], with 0.9 or 1.0 +/- 0.1 SSWs per year depending on the experiment, compared with the observed 0.6. CMAM is something of an outlyer on the high side. Would you like to modify the accuracy of the results in the light of this information?

p.24858, l.26. The source of the forcing files should be properly specified. The full references are given in the Eyring et al. paper as well as other CMAM papers. A repeating lower boundary condition eliminates an important degree of variability which corrupts later comparisons with observation. See General point 1, above.

p.24862, l.25. Presumably the website information needs to be spelt out in accordance with journal policy.

p.24863, l.12. This is poorly phrased. While relevant for fixed fluxes, once those fluxes are converted to NOx mixing ratio changes, the mixing ratio is kept constant under conservative transport (with no mixing). So descent of a given parcel of air from 100 km to 80 km would maintain the mixing ratio. Presumably, though, you are thinking of

an episodic process in which the flux at 100 km ceases after a period of time, short compared with the descent time.

p.24864, l.8. Should this be Starkov (1994)?

p.24865, l.11. Presumably the website information needs to be spelt out in accordance with journal policy.

p.24866, l.25. Please indicate in the text how significance is indicated in the figure.

p.24869, l.2- end R11. For conciseness this could all be replaced by "reduction of ozone by the HOx catalytic cycles". Why does the original text refer to the "indirect" reduction of ozone?

p.24869, R12-R14. Again, do we need all this basic chemistry?

p.24869, R15-R18. CMAM is designed for the middle atmosphere of course. Are you sure that these reactions are in this version of the model?

p.24870, l.7. Typo: a –> an. Please review the use of a and an throughout the text.

p.24870, l.11-21. Some of this is quite confusing. Highlights from 6 panels are being described. Please specify which panel refers to which comment, and the altitude being considered in all cases.

p.24872, l.1. Again I would tend to speak this aS it's written which would require a –> an.

p.24872, l.27. Do you mean "......shielded from ozone destruction."?

p.24873, l.7. This mechanism really needs proper verification. There is also the ozone self healing effect, whereby ozone production occurs lower down from O2 photolysis due to increased penetration of UV.

p.24873, l.28. To what extent are these results realistic in view of the simplified chemistry of the troposphere in the model?

p.24876, l.5-6. I don't understand this argument. Obviously the dynamics is driven by the ozone field and you could argue from Figure 7 that the ozone field is distinctly non-additive in places. Without further quantitative analysis, including an assessment of the uncertainties, it is not clear what the impact of the nonlinearity is.

p.24882, l.1-2. Compare this result with the results from other models e.g. Austin et al. [2008].

p.24882, l.5-13. The comparison with measurements is not at all convincing and needs to be completely rewritten. The inclusion of EEP has not had a significant effect on the results anywhere in the tropics. The slightly better agreement could be quite fortuitous, bearing in mind, as the authors indicate, one of the main processes driving the solar signal during this period are the SSTs.

p.24882-3. The rest of this section has problems. In comparing model results with

observations the authors of the current paper have just a single model which has its own strengths and weaknesses, which for all the advantages of the linear regression method do not necessarily allow the solar effects to be separated accurately. Decadal length timescales occur by chaotic processes in climate models, and these can easily be aliased into the solar signal. It is therefore extremely challenging to separate these affects from genuine atmospheric variability, not just in models but in observations as well. With a model you can of course run an ensemble and at least in principle reduce the random error to an arbitrary small size. Of course whether three ensemble members is enough the authors rightly question and practical decisions need to be made regarding resources available and likely benefits. However you look at it, the atmosphere has completed just one "simulation" which puts severe constraints on the size of the signal. Just looking at Figure 21, the solar signal in observations could be anything from -1 to 2.25

Regarding the manuscript, caveats need to be included on the performance of just the one model in comparison with observations. The long time scale of the processes needs to be acknowledged as influencing the analysed "observed" signal. Ideally we would want 100 years of observations to eliminate the SST issues and other decadal timescale variability in atmospheric processes, but of course we will have a long time to wait for that!

p.24885. There is of course no harm in indicating that there is improved agreement between CMAM and observations, providing the comment is made specific to CMAM and may not reflect the performance of other models.

Figures

The figures are specified in km, but presumably this is not a constant height surface. If this is correct, please specify the correct coordinate.

Figure 3: Which is the correct, and which is the approximate coordinate? Are these the results for the geomagnetic pole? What is the reason for the cusps in the aurora ion production rate?

Reference

Butchart et al., Multi-model climate and variability of the stratosphere, JGR, in press, 2010.

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