Atmos. Chem. Phys. Discuss., 10, C13049–C13055, 2011
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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

K. Semeniuk et al.

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Received and published: 31 January 2011

Reply to Referee # 1

General Comments

1) We believe that a comparison of our results with observations is worthwhile. This is true even in the case of large observational uncertainties, in which case the comparison can be qualitative instead of quantitative. We did not claim to capture all of the observed behaviour as our work is a process study.

There exists no publication that we are aware of and none is cited in the CCMVal

(2010) report, which explains the processes giving the vertical structure of the ozone variation with solar activity. Our results demonstrate that EPP can produce a tropical ozone response minimum without SST variability. This is an interesting result worthy of publication.

The WACCM simulations presented in Austin et al. (2008) used variable SSTs, yet they were the only model to show a tropical lower stratosphere water vapour variation. This shows that EPP effects on the tropics require study and that is one of the goals of this paper.

In addition, most models in the CCMVal intercomparisons do poorly when it comes to reproducing the observed tropical temperature variation with the solar cycle, specifically around and below 10 hPa. Our results are in better agreement with observations. One of the factors is that models without GCR are missing an in situ chemical modulation of the ozone by the solar cycle in the tropical stratosphere.

Some wording changes have been made in the abstract and introduction to highlight that this is a process study.

2) We have added text at the beginning of section 4.1 to clarify what we treat as statistically significant. The 90% contour is essential in our figures to identify which regions are statistically significant. Plotting marginal significance does not detract from the content but rather adds to it.

Specific Comments

p.24854, l.10: We have changed the text to add the caveat that this is a process study. But we do not agree that lack of interannual variation in the SSTs negates comparison with observations. In any process study comparison with observations is valuable since it tells you whether you are on the right track. EPP is producing an ozone minimum at 10 hPa without SST variation and this is significant in itself.

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p.24855, l.18: We have corrected the text in various locations (see highlighted manuscript attached as a supplement).

p.24855, l.23: The reference has been added.

p.24855, l.29: The text has been changed in the introduction to highlight the point made by the referee. However, we disagree with the cost/benefit analysis aspect. CMAM PSCs are not deficient in terms of their impact on ozone and CMAM is not an outlier in this regard in recent inter-model comparisons. The implicit point made by the referee is that NAT PSCs produce larger effects than EPP. But warm PSCs do not typically affect the ozone above 23 km and they do this only in early spring. EPP depletes ozone in the polar and sub-polar regions in the upper and middle stratosphere in the fall and winter. In particular, it produces a negative ozone anomaly in middle to high latitudes between 20 and 30 km, which affects the polar vortex evolution.

p.24858, l.19: We used the NAM index method (McLandress and Shepherd, 2009) to analyze major SSW frequency in our simulations and find them to occur about 50% of the time if final warmings are included. There is a high degree of variability in the grouping of major SSWs and they often occur in successive years instead of every other year. But there has been variation in the grouping of major SSWs in the real atmosphere in the last 30 years as well.

There is also large variation in the transport characteristics of major SSW events in different years. These variations constrain the impact of auroral NO_x on the NH stratosphere. We are not aware of any detailed comparison of model SSW climatology with observations. This discussion has been added in section 2.

The excessive SSW frequency cited by the referee refers to the coupled ocean-atmosphere version of CMAM, which had problems with convective adjustment tuning.

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The CCMVal-1 version of the model with prescribed SSTs did not have this problem. Our version of the model is based on the CCMVal-1 version with fixed SSTs.

p.24858, l.26: We have added text in section 2 to clarify that we used one of the SST ensemble members used in the CCMVal-1 CMAM runs.

SST variation is a distinct process and does not exclude EPP effects. We could equally argue that without EPP model comparison with observations is not worthwhile.

p.24862, l.25: Our reading of the reference format instructions gave us the impression that URLs were to be confined to the Reference section and not embedded in the body of the article. The explicit URLs are included in the reference section in our paper. This may need to be clarified by the editor.

p.24863, l.12: We have changed the text in section 2.2.1. Plumes of descending air do not conserve mixing ratio. Specifically, their volume does not become exponentially smaller with depth, which would be required to conserve mixing ratio as the ambient number density increases exponentially. Some small scale mixing and diffusion is present at all altitudes that produces the effect we point out. The Odin HNO_3 observations in the stratosphere polar regions show this effect clearly. Large scale mixing is not necessary, since this effect is present in the SH polar vortex interior, which is isolated from large scale Rossby wave breaking. But in the MLT, transport by large amplitude planetary wave disturbances is significant and we now discuss this.

p.24864, l.8: The reference has been corrected.

p.24865, l.11: See reply to comment above regarding p.24862, l.25.

p.24866, l.25: The text has been modified to point out the Student-t test is being used.

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p.24869, l.2- end R11: We feel a detailed statement of the chemical reactions is worthwhile. The meaning of “indirect” in this case is that HO_x attacks atomic oxygen thereby reducing ozone formation.

p.24869, R12-R14: We think this is necessary for clarity and does not encumber the paper.

p.24869, R15-R18: CMAM does indeed have these reactions in its chemistry solver and has had them for a long time.

p.24870, l.7: The grammatical error has been corrected here and elsewhere.

p.24870, l.11-21: The text has been changed for clarity as suggested by the referee.

p.24872, l.1: The grammar has been corrected.

p.24872, l.27: The text has been changed to point out that the SPEs NO_x is deposited at lower altitudes and does not experience the losses during transport in the mesosphere like in the auroral case.

p.24873, l.7: Ozone self healing is a non-issue below 25 km. The chemical processes responsible for this mechanism are part of our chemistry solver.

p.24873, l.28: The text has been changed to clarify that the ozone production in the troposphere is excessive given lack of wet removal of NO_x. Our concern is more with the dynamical impact of this excess ozone on the stratosphere via changes in wave forcing and propagation. Given the small level of ozone in the troposphere, a 15%

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increase does not have a significant impact on the heating. For example, tropopause height in the combined EPP without solar variability ensemble does not change over most latitudes. Except in the SH polar region, where it is driven by ozone accumulation from above. The tropospheric ozone increase is well mixed and does not selectively enhance temperature gradients which further limits its impact on the dynamics. These points have been added to the text as well.

p.24876, l.5-6: The point is that combining all EPP types does not produce zonal wind changes significantly larger compared to the impact of each type by itself. As noted by the referee there is non-additivity in the high latitude ozone field. So the atmospheric response to all three EPP sources combined is such that it reduces their effect.

p.24882, l.1-2: We have added more discussion to compare with other model results from CCMVal-1, which is most relevant for our analysis since we use the Fioletov ground based data.

p.24882, l.5-13: We do not agree that our results are a coincidence, especially not in an ensemble run. They highlight a response mode of the middle atmosphere to polar vortex perturbation as well as changes in the wave driving in the TTL in the presence of the solar cycle. There is a change in tropical upwelling which has two distinct regimes above and below the 10 hPa level. We do not present any detailed analysis of this since it is beyond the scope of our paper.

Clearly, EPP has a significant effect in the lower tropical stratosphere in our simulations. Also, GCR induces a direct chemical solar cycle in ozone between 10 hPa and the tropopause.

p.24882-3: We mostly agree with the referee and have added text in the introduction.

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p.24885: This is addressed by other text changes. Our results can hardly be classified as a model pathology, especially given the performance of CMAM in model intercomparisons. As noted before WACCM develops a water vapour solar cycle signal in the TTL region and it is the only model in the CCMVal intercomparisons with EPP.

Figures

The figure has been corrected to identify the coordinate as log-pressure height.

The reason for the cusps in for the aurora in Figure 3 is the piece-wise exponential fitting of the three energy channels.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C13049/2011/acpd-10-C13049-2011-supplement.pdf

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

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