**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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Response to Referee # 3

General Comments

The notion of lengthy and difficult to follow is subjective. We feel our paper covers the important aspects of our model simulations with sufficient detail. The referee applies the label "speculation" extensively when in fact it is application of textbook dynamics and chemistry. We have included a number of the references that the referee points to.

Specific Comments

p24855, l1-11: We have added the suggested reference (see highlighted manuscript in the included supplement).

p24855, l16: The conclusions by Gray et al. (2010) are rather sweeping and by no means close off the field on this subject. The problem with analyzing UARS data is that the period is simply too short. There is also the problem of spurious aliasing on the 11 year signal from inter-decadal variation in the chaotic ocean-atmosphere system. The text has been modified in the introduction to highlight this issue.

p24856, l13/14: We have added a reference to the Langematz et al. (2005) study.

p24857, l5: We have added references to Chapter 8 of the CCMVal-2 SPARC report. WACCM is still the only participating model that includes comprehensive EPP.

p24857, l11 ff: None of these studies include the combined effect of the three types of EPP that we consider. So they do not fit in this paragraph. We have added them elsewhere in the introduction.

p24858: We have added a table detailing the experiments and where they are analyzed. Our analysis compares at most two ensembles in a section so it is not difficult to keep track of the model experiments.

p24862, l10 ff: It is not clear what the referee wants. The only controversy we are aware of pertains to GCR effects on clouds and thereby climate. We are not considering cloud or aerosol effects of GCR so this climate impact discussion would be out of place in

our paper. The text has been changed in section 2 to make this point more clearly including a reference to Pallé et al. (2004).

p24862, l17 ff: The MEPED electron flux data is a NOAA data product. The energy deposition calculations are based on first principles physics.

p24865, l116 ff: The referee needs to be more clear exactly what they are complaining about. There are a total of three acronyms in this portion of the text. The first has a reference so just like with WACCM or HAMMONIA there is no point expanding it, it is a tag in itself. The other is AR(1), which was explicitly defined.

p24865, l26 ff: The text has been revised to explicitly refer to orthogonal QBO wind components.

p24866, l2: In the leading order sense, we do not see where there would be a difference. The functional form of ENSO indexes is similar. For example, using MgII instead of F10.7 shows no significant differences in the regression analysis (not shown in the paper).

p24866, l7 ff: Yes. But including these results does not fit in our analysis framework as we already note in the final paragraph of section 3.

p24866, l25: We have rewritten the text. The temperature response between 15 and 50 km reflects the change in the vertical circulation in this altitude region, which has the correct sign. The statistical significance extends to 40 km.

p24867, l4: There are too many figures already as noted by some other referees. The CMAM climatology is reasonable and has been presented in the references we cite.

p24867, l9: We have changed the order of the text and figures.

p24867, l26: The referee should consult http://en.wikipedia.org/wiki/Viz. for a definition. There is nothing wrong with the term "cannibalistic".

p24868, l3: We have changed the text to give more details. In particular, that the photochemical lifetime of NO_y at low altitudes is long and that there is "fossilization" of vortex interior air during summer (Orsolini, 2001).

p24869, l12 ff: A couple of references are Orsolini (2001) and Orsolini et al. (2003). Since there is poor removal of NO_y by transport and its lifetime is long in the lowermost stratosphere there is no reason why none would survive after six months.

p24870, l1: Some of these features are due to transport differences compared to the reference run.

p24870, l3: The word "chemical" has been added.

p24870, l6: We do not understand what the referee is referring to in the text. Also there have been changes to the text in response to this and other reviewers so this portion of the text no longer exists.

p24870, l8 ff: There is more NO_x transported to these altitudes in the stratosphere in the SH compared to the NH due to significant differences in the polar vortex containment. As a result there is more NO_x surviving into the following summer after the final vortex break-up in spring.

p24870, l13 ff: The text has been modified to include specific figure references.

p24870, l20 ff: The text has been modified to include specific figure references.

p24870, l25 ff: We have changed the text to remove the fudge words "appears" and "could". The chemical and dynamical processes that we describe are the only plausible candidates to explain the model response. The fact that background ozone values are low in the troposphere and the plots are percentage changes has been highlighted.

p24871, l8: The Student-t test is not the be all and end all of statistical confidence assessment for the atmosphere since the atmosphere is not a Gaussian statistical system (e.g. Yoden et al.). The dynamically and chemically consistent mean response pattern contains information as well. We have added a reference to the Yoden et al. paper and some more discussion to the text.

p24871, l11 ff: The text has been modified to explain this feature. The pattern is the classical response to a localized wave drag change (e.g., Haynes et al, 1991) with a quadrupole temperature anomaly accompanied by a vertical dipolar mass streamfunction anomaly. There is a streamfunction decrease above 50 km and a general increase below 50 km in the 50S to 90S latitude belt. The streamfunction in the upper dipole lobe is weaker than in the lower one. Of course in these CCM simulations there are structured changes in the wave drag that introduce additional features in the streamfunction since the wave drag change is not simply an analytic localized feature.

Here we also see the limitation of the Student-t test. The response is dynamically consistent but only parts of it are being identified as statistically significant.

p24871, l23 ff: Figure 3 shows a single rather extreme SPEs case from 2003. We have replaced Figure 3 with one showing what the temporal average of the EPP is like.

p24872, l4/5: Rossby waves penetrate to 25 km in summertime in both hemispheres
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(e.g., Orsolini, 2001). There is evanescent penetration of Rossby waves into the easterlies above the summertime zero wind line. In addition, there is significant orographic gravity wave drag especially at the edges of Antarctica and it penetrates deep into the stratosphere.

p24872, l7/8: HO_x is short lived in the case of aurora where it forms above 60 km, so it cannot be transported to the 50 km and below. SPEs form HO_x at these altitudes.

p24872, l8: The text has been modified. The features are all in the NH polar region and the HO_x reduction is around 30 km.

p24872, l10: The text has been modified to identify the sign of the correlation.

p24872, l13/14: The text has been modified as suggested.

p24872, l17/18: The text has been changed. This is summertime NO_x generation by SPEs in addition to any memory from the previous winter. Effects at these altitudes from aurora are completely transport dependent.

p24873, l2: No. The NO_x is subject to the same inter-hemispheric difference in vortex interior survival regardless of its generation mechanism. The results presented here demonstrate this fact.

p24873, l12: The text has been modified to highlight the statistical significance.

p24873, l23: We speculate in the text that the ozone loss between 20 and 30 km is important for the vortex response. GCR produces middle latitude ozone which aurora and SPEs do not at these heights.
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p24873, l27: The phrasing does not refer to height but to amplitude.

p24874, l6: There are plenty of sources of dynamical variability in the SH summertime polar region. Rossby waves, orographic gravity waves, etc. The zonal flow in the SH is by no means axially symmetric in summer.

p24875, l6: Yes and no. The Student-t test significance is low for SPEs, but there is a distinct ozone loss around 60S between 20 and 30 km in winter.

p24875, l23: We are highlighting the similarity.

p24875, l28: The text has been changed to state that the ozone impact from SPEs can last over a year.

p24876, l6/7: The text has been changed to refer to top and bottom panels.

p24876, l14/15: The main issue is the zonal wind impact of the EPP types separately and in concert. They all act to reduce the polar vortex in a weak manner. The structure of the response is different but not profoundly and they all act in the same way qualitatively. The purpose of this section is to highlight that the combination of the EPP types has a weak effect on the dynamics and is not all that different from the effect of each particle type. The text has been modified for clarity.

p24876, l18: The text has been modified to make explicit that low amplitude means that the strength of the polar vortex change is small regardless if the EPP is acting individually or combined.

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p24877, l1: The text has been changed to identify Polvani and Kushner feature earlier in section 4.1.

p24877, l15: We do not see a problem.

p24877, l18: The text is quite clear that we are referring to two of the three ensemble members of the combined EPP ensemble run.

p24877, l28/29: The text has been corrected. The dynamical response is very weak and there are small differences in the ozone field between the two cases. We looked at different months to see if we could establish a pattern of evolution but nothing clear could be extracted from zonal mean diagnostics. The dynamical evolution is very sensitive to even small differences in ozone, which underscores the need for ensemble runs in these sorts of studies.

p24878, l3: The text has been changed to state that this is sensitivity to initial conditions in nonlinear dynamical systems. Here, small differences in the ozone field are enough to produce a bifurcation in the polar vortex response to EPP.

p24878, l15: The text has been changed.

p24878, l25: Figure 15 is necessary since it shows the cumulative impact of changes in the Brewer-Dobson circulation. While the BD circulation may be noisy from year to year, the age of air accumulates the net change, much as would chemical tracers.

p24879, l13/14: The text highlights the fact that the composition and dynamical difference between solar maximum and solar minimum states of the middle atmosphere are too small to change the basic chemical response to EPP.

p24879, l19: It is not clear what the referee wants. The total column ozone figure is sufficient to make the point.

p24880, l1: The text has been modified as suggested.

p24880, l10: The region where the age of air is reduced is now identified in the text.

p24880, l13: The text has been changed to refer to solar activity.

p24881, l16: The text has been altered to highlight the fact that GCR ozone formation below 20 km is lowest during solar maximum years. GCR activity is in the minimum of its cycle during solar maximum years so the positive ozone anomaly (at these altitudes) associated with it is negative. Yet the temperature anomaly is positive. This underscores the fact that dynamical variation is the origin of this TTL temperature cycle.

p24881, l23: The regression fit of the data from Fioletov has been added to this figure. We also found a mistake in the software used to produce the original figure which resulted in the model ozone variation being 2/3 its true value.

The CCMVal 2010 report (Chapter 8) lacks many diagnostics found in Austin et al. (2008) that are relevant to our paper. In particular, it lacks any figure like the one which is being targeted for revision.

p24881, l24: We have changed the text to note that the integrated SPEs and auroral ionization during the solar maximum period around 1990 was higher than the one around 2000. The observed total ozone column variation also shows that this solar maximum had lower peak values compared to the other two maxima in the timeframe of the model.

p24882, l4: A reference to CCMVal (2010) has been added in addition to text changes for clarification.

p24882, l8 ff: We have added some text to this section to discuss pertinent issues. The CCMVAl (2010) report refers to Austin et al. (2007,2008) and Matthes et al. (2010) to speculate on the possible role of SSTs and QBO in affecting the vertical structure of the ozone solar cycle in the tropics. Austin et al. (2008) attributes the improved structure of the ozone response below 10 hPa in models to two possible factors. One possibility is use of time varying solar irradiances compared to constant maximum and minimum cases simulated previously. Our results suggest this is not likely. The other possibility is that observed SSTs are affecting the response. Both Austin et al. (2008) and the fixed forcing study by Matthes et al. (2010) do not explain the origin of the ozone minimum at 10 hPa. The reference to Matthes et al. (2010) has been added to the text.

p24883, l2: The text has been revised for clarity.

p24883, l4 ff: This paragraph has been removed as part of the revision in response the referee comment for p24882, l8 ff.

p24883, l24 ff: GCR does not act like aurora and SPEs by depleting ozone near the poles. The main similarity with the effects of the other two EPP types is the ozone depletion in middle and high latitudes between 20 and 30 km, which is mostly direct and not a transport feature for GCR. We infer that this leads to the similar dynamical response. This aspect requires more extensive analysis beyond the scope of this paper.

p24884, l18: The annual mean regression coefficient captures the wintertime vortex behaviour. We obtained similar results comparing solar maximum and minimum peri-

ods by averaging winter periods instead of annual means but we do not show these figures in the paper.

p24884, l19: There is no need to use another term since it is a fact that their picture is a qualitative cartoon and not a law of science like $F = m\,a$. As presented in our paper, it is clear that small changes in ozone do not always give a systematic dynamical response. The baseline state of the SH in our model configuration gives a sensitivity opposite to that suggested by Kodera and Kuroda without EPP. Their idealized picture did not take EPP effects into account and is not applicable to the SH in their absence.

p24885, l6 ff: We think that this level of description is sufficient for this paper. Detailed Analysis of this mechanism will be presented in an separate paper.

p24885, l10 ff: Clearly we are referring to one of the main results of our paper: the solar cycle variation of H_2O in the TTL induced by EPP alone. It should be noted that this variation is absent in most of the models which include variable SSTs, except for WACCAM which includes EPP (and possibly HAMMONIA but they did not participate in CCMVal-1 or 2).

p24885, l13/14: The wording has been changed to highlight that this has been noted before and suggested reference has been added.

p24885, l22 ff: Our paper is too long already and detailed analysis of these mechanisms is not its focus. This feature would be best analyzed using mechanistic model simulations and presented in another paper.

Technical Comments

Figure 4: The temperature, zonal wind and mass stream function all have different amplitude ranges so we do not see what the complaint is about. Frankly, we do not see any benefit from increased verbosity in the captions. The term "run mean" is self-explanatory and cannot be mistaken for some subset of the total number of years in the run.

Figure 5: The outlying contour intervals (1900 and -100) simply do not affect the detail level in the GCR plot. We use the same set of contours for all EPP types for consistency.

Figure 7: The use of mixing ratio for this figure adds additional content to the paper and also does a better job of highlighting the additivity of the chemical effects. Specifically the ozone response in the SH stratosphere is easier to plot in mixing ratio than with percent since the latter tends to highlight the troposphere and mesosphere more.

Figure 17: The reference to Figure 13 in the figure caption is sufficient.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C13072/2011/acpd-10-C13072-2011-supplement.pdf