Authors' response to reviewers' comments on "Simulated seasonal impact on middle atmospheric ozone from high-energy electron precipitation related to pulsating aurorae" by Verronen et al.

Please find below our answers (in blue) to the comments (in black).

Response to the comments of Referee #2 (Katharine Duderstadt)

General Comments: This study uses observations of pulsating aurora location, occurrence rates, and spectra to estimate potential impacts to atmospheric HO_x, NO_x, and O3. This perspective is especially important in understanding the 'indirect effect' of mesospheric NO_x enhancements and descent on reductions of O3 in the upper stratosphere. The authors provide a useful comparison between WACCM studies using PsA-EEP and CMIP6 MEE on atmospheric ionization and composition. The paper is well-written and figures are clear.

Response to the general comments: We thank the reviewer for her comments. We also appreciate the time devoted to the evaluation of our paper.

Specific Comments:

1. It would be valuable to place PsA-EEP estimates in context of what is known (and not known) about radiation belt electron precipitation. Since the PsA-EEP driven WACCM results are so close to the CMIP6 MEE simulations, does this imply that most of the electron precipitation from the outer radiation belt should produce pulsating aurora? Are there other mechanisms for precipitation that do not result in PsA but are observed by polar orbiting satellites? How are PsA-EEP related to substorm and microburst precipitation? How might the results of this study inform our understanding of electron precipitation processes from the radiation belts?

Response: PsA-EEP is related to other types of electron precipitation and cannot be fully separated in satellite-based electron flux observations. According to the current understanding, the primary cause of PsA is electron precipitation from the plasma sheet and the inner magnetosphere (outer radiation belt) (Nishimura et al., Space Science Reviews, 216, 4, 2020). PsA-EEP is often observed during substorm activity but also extends beyond substorm disturbance and includes higher-energy electrons than typical substorm precipitation. A relation between PsA and microburst precipitation is expected theoretically (Miyoshi et al. Geophys. Res. Lett., 47, e90360, doi:10.1029/2020GL090360, 2020), but has not been observed and is not understood in detail (Miyoshi et al., J. Geophys. Res., 120, 7728–7736, doi:10.1002/2015JA021562, 2015b).

In the revised manuscript, we discuss this in Section 1.

Considering the question about radiation belt or magnetospheric PsA processes leading to precipitation, they are not addressed in our study but we are focusing on atmospheric impact from PsA-EEP. They have, however, been addressed in recent studies, e.g., by Miyoshi et al. (Geophys. Res. Lett., 47, e90360, doi:10.1029/2020GL090360, 2020) and references therein).

2. Recommend authors provide a deeper discussion about the uncertainties associated with the "MCMC median" ionization profile. The authors appear to use an energy spectrum from a single event (17 November 2012) to drive the entire simulation. Turunen et al. suggest large difference in O3 reductions… 10s of percent… depending on energy spectra. Tesema et al. (figure 4) show a large range of possible energy spectra. What observations are needed in order to better constrain this estimate and associated variability? How might the spectra vary with magnetospheric activity, given changing pitch angle distributions and anisotropies in precipitation? What are uncertainties associated with assuming the same PsA-EEP forcing throughout the year given that previous studies such as Bland et al. identify seasonal differences in occurrence rates?

Response: Turunen et al. spectrum is indeed based on a single event, but it is in agreement with the statistical study by Tesema et al. (2020) making it a well-validated median spectrum and is thus well-suited for our long-term impact study. To constrain the spectral variability with magnetic activity, in the future we would need more detailed studies of precipitation spectra during different types of PsA, including the effect of patchiness. Because the PsA-EEP simulation uses a 50% wintertime occurrence frequency that is higher than in summer (Bland et al., 2019), by applying median PsA forcing throughout the year we are overestimating the summertime forcing by a factor of about 2.5 in our simulations. For the assessment PsA impact, however, this has a small overall impact: our results show that the long-term atmospheric response is clearly driven by the wintertime forcing.

In the revised manuscript, we have revised the text on spectrum selection and uncertainties.

3. The authors emphasize the importance of using the full WACCM-D chemistry. It would be helpful to quantify the difference on NO_x production using this chemistry as a function of altitude and electron precipitation energy spectra. That is, at what altitudes and electron energies is using the full WACCM-D chemistry most critical?

Response: As shown by Andersson et al. (2016) in the case of the January 2005 solar proton event, detailed D-region chemistry resulted in 30–130% more NO_x at 70–85 km compared to the standard parameterization. We expect a similar factor-of-two increase in PsA-EEP NO_x response from the detailed ion chemistry. Note that this is direct enhancement in the mesosphere at electron energies of about 40–200 keV and, depending on dynamical conditions, the extra NO_x can then descend and affect lower altitudes.

In the revised manuscript, we have added this information (Section 2).

4. Recommend adding a more thorough discussion of why these seasonal and spatially limited O3 reductions are important in atmospheric processes at various altitudes (dynamics, radiative transfer, chemistry). For example, why is a 5% decrease in O3 within the winter polar vortex at 40 km important (e.g., Figure 7)? And how significant is this decrease with respect to other energetic particle precipitation impacts (i.e., solar protons, GCRs, and other sources of electrons)?

Response: The simulated 5% ozone depletion from PsA-EEP alone is a substantial contribution to the total EPP impact because it is comparable to that seen in satellite observations (up to 15% in the SH upper stratosphere, Damiani et al. 2016) and in simulations (e.g. 7% in the SH upper stratosphere, Andersson et al. 2016). Capturing the magnitude of the stratospheric ozone response is important for realistic simulations of the proposed ground-level climate connection because middle atmospheric ozone controls the dynamical response through absorption of solar ultraviolet radiation. The ozone response to EPP is typically seen in the polar cap areas (as shown e.g. in Figure 7), but these ozone changes affect the temperature balance between the mid and polar latitudes, and subsequently the zonal winds, and connects to the ground-level climate variability (e.g. Baumgaertner et al. 2011). Currently, the EPP-related ground-level regional temperature variability from observations ($\pm$5 K, Seppälä et al. 2009) exceeds the simulated variability ($\pm$1 K, Rozanov et al. 2012), and improvements in the EPP forcing could help to reduce the difference.

In the revised manuscript, we have improved the text in Section 4.

Minor:

Line 38 – “40%-60% ozone depletion” at what altitudes?
Response: In the revised manuscript, “in the polar upper stratosphere” has been added.

Line 79 – 80% at what altitude?

Response: In the revised manuscript, “at 75–80 km” has been added.

Line 144 - what electron energies do “below 85 km” correspond to?
Response: In the revised manuscript, “(electron energies larger than about 40 keV)” has been added.

Lines 223-225 – It would be useful to mention this model “spin-up” earlier in the paper. (Or showing results just for the second winter)
Response: In the revised manuscript, we mention this in Section 2. Note that the discussion on the NH results is already focusing on the second winter.

Line 272-273 – Quantify “severely depleted”
Response: In the revised manuscript, this is replaced “by more than 70%”.

Lines 305-307 or below – When referencing the limitations of the CMIP MEE electron precipitation estimates, recommend mentioning that APEEP uses the MEPED 0 degree telescope and does not fully take into account pitch angle anisotropies. (Authors reference the work of Nesse Tyssøy et al., but it would be useful to explain more thoroughly in the text).
Response: The underestimation of the CMIP6 MEE which is based on the measurements from the POES/MEPED 0° telescope and makes no use of the 90° telescope could indeed be related to the incomplete pitch angle coverage (e.g. Nesse-Tyssøy et al., 2019). Note, however, that even the use of both telescopes requires assumptions about the pitch angle distribution. In the context of the current paper, we feel that addition of these details would require an extensive discussion of MEPED observations which would be a distraction for the reader. Thus we decided not to add these details, and trust that the reviewer can agree with our view.

Technical corrections:

Line 139 – “patterns” (typo)
Response: In the revised manuscript this is corrected.