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Comment on angeo-2021-25

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Community comment on "Simulated seasonal impact on middle atmospheric ozone from high-energy electron precipitation related to pulsating aurorae" by Pekka T. Verronen et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2021-25-CC1, 2021

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 O_3. This perspective is especially important in understanding the 'indirect effect' of mesospheric NO_x enhancements and descent on reductions of O_3 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.

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?

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 O_3 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?

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?

4. Recommend adding a more thorough discussion of why these seasonal and spatially

limited O_3 reductions are important in atmospheric processes at various altitudes (dynamics, radiative transfer, chemistry). For example, why is a ~5% decrease in O_3 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)?

Minor:
Line 38 – "40%-60% ozone depletion" at what altitudes?
Line 79 – 80% at what altitude?
Line 144 - what electron energies do "below 85 km" correspond to?
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)
Line 272-273 – Quantify "severely depleted"
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).

Technical corrections:
Line 139 – "patterns" (typo)