Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-56-AC2, 2020

Interactive comment on "Electron precipitation characteristics during isolated, compound and multi-night substorm events" *by* Noora Partamies et al.

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Received and published: 13 November 2020

We thank the referee for careful reading and thoughtful comments on the manuscript. Below are our responses to each comment, with comments in *italic*.

Point-by-point responses:

- *The DMSP and POES electron measurements are only presented one figure (Figure 7) and the discussion in section 3.2 focuses on contrasting the DMSP energy ranges to the pulsating aurora fluxes. What about the POES observa-*

tions? Why does there appear to be a large discrepancy between the DMSP and POES fluxes between 1–10 keV energies? What about higher energies? Note that the value of the top energy range is cut off from the left panel in Figure 7.

The figure discussion focusses on the comparison of the substorm energy fluxes with the pulsating aurora fluxes because that is our "known" reference at the moment. The discrepancy between the DMSP and POES fluxes at energies of 1–10 keV is most likely due to the fact that the two spacecraft are mainly not measuring the same events. We have simply just collected all data from overpasses of the two spacecraft during any of the analysed substorm expansion and recovery phases, so very few of them are time-wise close to each other. Thus, discontinuities are expected from one spacecraft measurement to another, both at low and high energies. This will be commented in the revised version for clarity. The top energy range of the left panel will be made visible.

- *The authors relate the substorms under investigation to the potential atmospheric impacts throughout the paper. I note that the atmospheric impact in the paper is used to discuss both the ionisation impact and referring to NO_x and HO_x production/ozone loss and often it is initially not clear to the reader which one is meant. I recommend reading the text through carefully and clarifying where needed.*

The atmospheric impact we are interested in is the production of NO_x and HO_x, which then leads to depletion of mesospheric ozone. The ionisation impact, which is also talked about, is just what we can measure as an enhanced CNA. So, strong ionisation is considered equal to the production of the catalysts, which is why the "impact" may refer to either process. We will emphasise this logic in the revised version of the introduction: "In this paper, we use the cosmic noise absorption enhancement as a measure for the medium-energy electron precipitation, which have the potential to produce odd hydrogen and odd nitrogen and thus lead to depletion of mesospheric ozone."

- *Substorms are indeed one of the main unknowns in the existing proxies for electron precipitation particularly when considering the eV to tens of keV vs. hundreds of keV energy precipitation. Global atmospheric models that extend to the thermosphere usually include the lower energy range via parameterisation that is driven for example by the Kp-index, while the higher energies are implemented via a POES/MEPED based precipitation model that is driven by the Ap-index (for both, see Matthes, K. et al. (2017), Solar forcing for CMIP6 (v3.2), Geoscientific Model Development, 10(6), 2247–2302). The authors touch on this in the discussion section (lines 360–362) where they write in the context of their results that: "These findings emphasise the atmospheric influence of the compound and multi-night substorm events, which may explain why the global geomagnetic activity indices serve us so well as energetic particle precipitation proxies, despite their poor temporal and spatial resolution." and again the in conclusions: "This would explain why the geomagnetic indices have been good energetic particle precipitation proxies despite their poor temporal resolution and spatial coverage." Unfortunately I found no explanation or background of the use of global indices in proxies — for anyone unfamiliar with the details of electron precipitation proxies, this important result will likely be lost and thus I recommend revising the text to make the context clear. Can you comment on the relation of indices like Ap and Kp to the ones used in this study — this again would be useful context for the users of those global indices.*

Some more background on magnetic index proxies is certainly needed. The revised discussion text will say:

"The atmospheric and climate models currently use Kp and Ap index based proxies to describe the ionisation rates due to energetic electron precipitation

(Matthes et al., 2017). As the temporal resolution of these global indices is three hours, a short-lived (less than 3 hours) isolated substorm may only result in a minor magnetic variation with respect to its maximum magnetic deflection in an electrojet index data (used in this study), which is available in 1-min resolution. Thus, the current energetic electron precipitation proxies are likely to only describe well the longer-lasting compound and multi-night type substorm events. Our findings emphasise the atmospheric influence of the compound and multi-night substorm events, which may explain why the global geomagnetic activity indices serve us so well as energetic particle precipitation proxies, despite their poor temporal and spatial resolution"

"This would explain why the geomagnetic Kp and Ap indices have been good energetic particle precipitation proxies in climate models despite their poor temporal resolution and spatial coverage."

- *On several occasions the impacts on NO_x and HO_x production are referred to as a direct atmospheric consequence of electron precipitation. For example in the discussion: "Compound and multi-night substorms will, however, be significant contributors to the direct production and HO_x and NO_x radicals in the atmosphere." I agree that this might be the case, but a statement like this should either be backed up by suitable references or toned-down (e.g. . . . "likely be significant". . .) as the present work does not involve any direct analysis of the production. Please check these aspects in the text and clarify where needed.*

Since no chemical modelling or observations are done in this study this is a good point. This particular statement in the discussion will be backed up by referring to Seppälä et al. (2015) who presented ion chemistry model results for a series of substorms, which corresponds to our multi-night substorm events. Those results showed significant mesospheric ozone loss due to production of HO_x and NO_x radicals.

- *Lines 151–152: Are the locations of the Abisko and Sodankylä riometers the same as the magnetometers in section 2.1? No location is given for the two, only Ivalo.*
  The locations of the Abisko and Sodankylä riometers are indeed the same as the magnetometers, which is why no new locations were given. But the fact that they are co-located will be mentioned in the revised version for clarity.

- *Line 294: Wording check: Does this mean events with IL$<$-300 nT are excluded? or ones with IL$>$-300 nT?*
  This means that events with IL$>$-300 nT are excluded. The new version will say: "deflections smaller than 300 nT", which should leave less room for misunderstanding.

- *Line 306: First mention of HSS?*
  Yes, the revised version will read: "high-speed stream"

- *Lines 370–373: In the context of the atmospheric impact, do you mean when contrasted to the likely impact from the expansion and recovery phases?*
  This is indeed a statement comparing to the likely impact from the expansion and recovery phases, and will be re-written to: "They are, therefore, unlikely to sum up to a large global atmospheric impact in contrast to the expansion and recovery phases."

- *Line 380: What is M–I?*
  The revised version will read as: "magnetosphere–ionosphere"

- *Line 396: "deplete the mesospheric ozone" indicated the process is destroying all mesospheric ozone. Suggest "cause depletion of mesospheric ozone" instead.*
  The phrase will be changed accordingly.

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