Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2020-58-AC1, 2020

Annales Geophysicae Discussions

Interactive comment on "D-region impact area of energetic particle precipitation during pulsating aurora" *by* Emma Bland et al.

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Received and published: 23 November 2020

We thank the referee for raising important items for further discussion and clarification. Point-by-point responses to each comment are given in italics below.

1. Effects of polar cap absorption. In the introduction, the authors mentioned past studies of the effects of polar cap absorption including their work (Bland et al., 2018). I wonder why this effect is not discussed in the present manuscript.

   In Lines 318-320 the authors said "...75-77 latitude (35% and 24% respectively). Although Grono and Donovan (2020) reported zero occurrence of optical PPA

   and PA at these latitudes, there are several PPA events..." I am concerned that the HF radar wave absorption at these latitudes is the effect of polar cap absorption.

   The absence of pulsating auroras at 74-77 latitudes was reported not only by Grono and Donovan (2020) but also by old literature by Oguti et al. (1981) (in the Supplement, see their Fig. 4). The authors should discuss the possibility of the effect of polar cap absorption.

   The event list used in this study is the same as from Bland et al., (2019), where solar proton events (i.e. PCA) had been excluded. This was not stated clearly in the original manuscript so we will clarify this in the revision. Any contribution from PCA in our results would be caused by events with proton fluxes below the threshold used by NOAA (10cm^{-2}sr^{-1}s^{-1} for proton energies $>$ 10MeV). A small number of events in our list occurred a day or two before a solar proton event, so it is entirely possible that some weak proton precipitation is present for these events. However, the timing of the absorption detected by these polar latitude radars matches well with the optical PsA observed at Syowa Station. It therefore seems more likely that the polar latitude absorption in this case is related to the PsA activity occurring equatorward. We will add this information to the discussion section. We will also add references to earlier literature on the latitude extent of PsA, including Oguti et al (1981), and an earlier paper they cited, Kvifte and Pettersen (1969), who reported PsA observations at 75 degrees magnetic latitude.

   Kvifte, G. J., & Pettersen, H. (1969). Morphology of the pulsating aurora. Planetary and Space Science, 17(9), 1599-1607.

2. Discussion of longitude span. In Lines 326-330, the authors showed that the probability of simultaneously observing HF radar echo attenuation at longitudi-

nally separated stations is higher at lower latitudes (KER-HAL) than higher latitudes (SPS-ZHO), and concluded that the longitudinal extent of the energetic particle precipitation is wider at lower latitudes. I am concerned that it is just the effect of latitudinal distribution. At lower latitudes, the probabilities of KER-ASC, HAL-ASC, and KER-HAL-ASC sets are 38%, 75%, and 44% respectively. At higher latitudes, the probabilities of SPS-ASC, ZHO-ASC, and SPS-ZHO-ASC sets are 35%, 24%, and 17% respectively. From these values, I consider that the difference of 44% and 17% is mostly due to the latitudinal difference (38%, 75% and 35%, 24%). The author should discuss this latitudinal effect.

We agree that the latitudinal distribution of PsA should be discussed more carefully in this context, since PsA are known to have higher occurrence rates near the equatorward edge of the auroral oval. There will also be a MLT distribution effect that we have not accounted for or discussed in this study. We will modify this paragraph to clarify how the probabilities should be interpreted, and how this relates to the latitudinal and MLT distribution of PsA (as described by e.g. Grono and Donovan (2020)).

3. The validity of statistical analysis with a limited number of samples. When I checked the major point 2 as shown above, I also noticed that the probability of KER-HALASC set (44%) is higher than KER-ASC pair (38%). I understand that it is due to the different datasets with simultaneous operation of a camera and radar(s), but then I wonder how accurate and reliable the value 44% is. I consider that this issue should be carefully discussed because it affects the interpretation of the latitude dependence of longitudinal extent (major point 2)

   The small sample size used in this study was mentioned on line 303 (discussion section), but we agree that greater emphasis should be placed on this when discussing the results. Our event list is small due to the requirement for optical observations at Syowa Station, and by radar data availability. The requirement for optical data also limits our event list to the winter months, which may bias

the results since PsA occurrence rates may have a seasonal dependence (Bland et al., 2019). Our somewhat qualitative event selection procedure would also play a role in the accuracy of the occurrence rates. We attempted to capture some of these uncertainties in Figure 3, but we agree that this should have been discussed more carefully in the context of the statistical results, especially the longitude span described above. We will provide this information in the discussion section of the revised manuscript.

The HF absorption results from the radars provide new information about the EEP impact area, since they can provide continuous, multipoint observations without the requirement of dark, clear skies that limits the use of optical data. However, the radar method introduces several additional sources of error and the sample size was limited. Therefore, if one was to design an EEP impact area for an atmospheric model run, we would recommend that our spatial coverage results be combined with results from other studies (e.g. Grono and Donovan, 2020), rather than using our results in isolation. We will clarify this in the discussion.

4. Lines 63-64: “determining whether an atmospheric chemical response will occur” should be rewritten as “determining whether a noticeable atmospheric chemical response will occur”. Atmospheric chemical responses always occur.

   Agreed. This will be corrected in the revised manuscript.

5. Lines 95-96: “(180-600 km range), which is the approximate area where the transmitted radiowaves pass through the D-region ionosphere”—In order to say this, the authors should describe the typical elevation angle range of the SuperDARN radio waves.

   The low-elevation range for the SuperDARN antenna radiation patterns is about 5-10 degrees, which sets the 600km upper limit. The 180km lower limit comes from the distance to the first range gate. This will be added to the revised manuscript.

6. Line 138 and Figure 2 caption: “beam 5” – beam 5 direction for each radar is not shown. Probably Figure 1 is an appropriate point of showing beam 5

   The beam 5 orientation for each radar will be added to Figure 1.

7. Line 196: “backscatter noise measurements” should be rewritten as “background noise level”.

   This will be corrected in the revised manuscript.

8. Line 197: “classified as clearly observed/not observed or probably observed/not observed”—the authors should describe the dynamic range and indicate the number of colours in one colour table, to make the manual inspection as objective as possible.

   The number of colours is 256 for all plots of this type. The colour scales are deliberately saturated at the top end in to emphasise the low-power features. Also, the dynamic range of the raw data is different for each radar and the units are arbitrary. We will add this information to the manuscript where Figure 2 (the sample event) is described. We will also describe in more detail how we made the distinction between “clearly/probably observed/not observed” near line 197.

9. Line 244 and Figure 4 caption: please describe how the dark grey shading areas were drawn.

   The manuscript text will be updated as follows:

   “We define this area as the region bounded by all radars that detected attenuation during this event. To determine the perimeter of each region, we performed a linear interpolation between the magnetic latitude and MLT coordinates of the adjacent stations, which produces the curved lines in the polar plots”.

   The caption of Figure 4 will be updated as follows:

   “The dark grey shading shows the EEP impact area, defined as the latitude/MLT region bounded by all radars that detected attenuation during that event (see text for details). The light grey shading shows...”

10. Lines 245-247: “Since the event onset times are slightly different for each radar, we use the dark grey shading to represent the total area over which attenuation was observed during the event.”—It is better to say “The event onset times are slightly different for each radar. To focus on the presence/absence of the events, we use the dark grey shading to represent the total area over which attenuation was observed during the event. ”

    Your suggested wording is much clearer. We will update this in the manuscript.

11. Figures 3 and 7 captions: the style for showing geomagnetic coordinates are different, i.e., (-58°,124°) and (58°S, 124°E). Use the same style.

    Geomagnetic coordinates will be updated to have a consistent style throughout the manuscript and figures.

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