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

Anonymous Referee #1

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Reviewer's report on the manuscript titled "D-region impact area of energetic particle precipitation during pulsating aurora" by Emma Bland et al. (manuscript #: https://doi.org/10.5194/angeo-2020-58)

In this manuscript, the authors conduct event-based and statistical analysis of estimating the areas of energetic particle precipitation during pulsating auroras using the SuperDARN HF radars data. As demonstrated in the manuscript, the SuperDARN data can provide information on the energetic particle precipitation area from the attenuation of the backscatter echoes and background radio noise. The topic is very interesting, the paper is straightforward and well written. On the other hand, I read the manuscript and found that there were several points such as the interpretation of the results which need detailed clarification.

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Discussion paper

I consider that the following points should be addressed and revised before the manuscript is ready for publication in Annales Geophysicae.

Major points: 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.

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

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-HAL-ASC 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).

Minor points:

1. 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.

2. 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.

3. 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.

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

5. 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.

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

7. 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.

8. 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.

Reference:

Oguti, T., S. Kokubun, K. Hayashi, K. Tsuruda, S. Machida, T. Kitamura, O. Saka, and T. Watanabe, Statistics of pulsating auroras on the basis of all-sky TV data from 5 stations, 1. Occurrence frequency, Can. J. Phys., 59, 1150-1157, 1981.

Please also note the supplement to this comment:
https://angeo.copernicus.org/preprints/angeo-2020-58/angeo-2020-58-RC1-supplement.pdf

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