Manuscript egusphere-2024-483: "How does auroral electron precipitation near the open–closed field line boundary compare to that within the auroral oval during substorm onset?"
by M. Grandin et al.

Response to Reviewer #1

We thank the Reviewer for their careful scrutiny of our manuscript and the valuable comments. Below we respond to the queries and explain how we have implemented changes in the revised manuscript.

Please note that, whenever we indicate line numbers, we refer to the **revised manuscript with highlighted changes**.

The paper presents a comparative study of precipitating electron characteristics during auroral breakups from two different latitudes, one close to the open-closed magnetic field line boundary and the other site approximately residing inside the auroral oval. 57 events from the auroral oval and 25 events closer to the open-closed field line boundary were selected based on a mix of machine learning-aided labelling and visual inspection approaches. By using an electron density inversion method called ELSPEC, the paper characterizes the peak energy, the number and energy flux of the precipitating electron profiles. Further applying a superposed epoch analysis, the paper demonstrates and compares the before and after of the poleward expansion into the radar field of view. The paper conducts a *well-defined, coherent, and attainable* study to analyze properties of precipitation associated with auroral breakup phenomena, however, the low number of events identified at the open-closed magnetic field line boundary and simplifying assumptions about the vertical profile of precipitation raises concerns that authors are advised to further discuss. It is evident that the instrument-based limitations pose difficulties for the authors to address, therefore strengthening these points with results from prior literature is suggested.

Thank you for these preliminary remarks; we have added references to earlier studies as suggested.

Specific Comments

- The paper discusses a lack of events to decisively conclude the differences between the two cameras due to the limited number of events detected by the Svalbard all-sky camera. However, it is not clear why the Svalbard all-sky camera data set is not further extended to further differentiate the properties in a statistically significant manner. Furthermore, the paper doesn't adequately discuss the lack of significant energy variations before and after the break-up events at the Svalbard location. I suggest authors either explain in more detail why only 25 events were available or extend the Svalbard data set to be comparable to Tromsø. In addition, I suggest the authors further discuss why energy levels were higher at a higher latitude during pre-breakup events.

  There are indeed only 25 events available from the Svalbard camera. This low number results from (i) the short optical season at such high latitudes (Nov–Feb, i.e. 4 months only); (ii) cloud cover (statistically ~50% of the nights), which severely reduces the number of clear nights when auroral activity was present; (iii) the lower number of substorms occurring at Svalbard latitudes, compared to Tromsø latitudes; (iv) the date when the Sony camera started operating (4 November 2015); and (v) the need for field-aligned EISCAT observation to apply the ELSPEC method. Because of (iv), we unfortunately cannot further extend the Svalbard dataset.

  In a revision of our manuscript, we phrased those constraints more clearly, in a similar way as in the above paragraph (see l. 132–139). We have also added a brief statement about the pre-breakup properties of the precipitation (or lack thereof) above Svalbard. Since there were extremely few (< 3) data points contributing to the pre-breakup median values shown in the initial version of Fig. 5, we masked those points in the revision, and made a note that no statistically-significant properties of the spectra could be obtained (l. 274–276).

- The methodology for detecting break-up aurora events from discrete labels is not well explained. As the auroral arc brightens and expands northward, wouldn't it make more sense to find events where initial arc labels are followed by consistent discrete labels? I suggest the authors further justify why arc labels were not used in the event selection.
  The reason why we did not consider the sequence of arc labels followed by discrete labels, and rather directly looked at discrete labels only, was that this step was only a way to obtain an initial list of auroral events compatible with the breakup phase (so discrete aurora, but more complex than just arcs). Since that step was then followed by a visual inspection of the ASC images to accept/reject candidate breakups, our approach (compared to that you suggest) only meant that there were more candidate events to go through visually; it neither added nor left aside any event. That being said, your suggestion would certainly have saved us a bit of time, since the initial step would likely have led to a smaller number of events to look at. We added a brief justification why we considered the "discrete" label as our initial step when searching for auroral breakups (l. 103–104).

- The paper doesn't adequately justify the reasoning behind obtaining the median of a wide altitude range which can significantly impact the energy obtained from the inversion method. In the literature, precipitation due to breakup aurora could be observed at altitudes as low as 65 km. Using the median between 85 to 125 km could significantly mischaracterize the precipitation characteristics. I suggest the authors provide a range for these characteristics, especially in Figures 3 and 5.
  The step at which we consider a median within 85–125 km altitude is only about discarding data points associated with no E-region density enhancement above the radar. It is a quick sanity check based on the assumption that, in presence of auroral precipitation within the radar beam, the electron density within this altitude range should be enhanced – even when the precipitating spectrum does not include high energies leading to ionisation down to 65 km altitude. The ELSPEC analysis considers radar measurements from all the available altitudes, and will therefore retrieve the precipitating spectra including at energies affecting altitudes below 85 km. Therefore, the used approach does not mischaracterise the precipitation characteristics.
  We have rephrased and tried to clarify the corresponding paragraphs in the revised manuscript, to avoid misleading the reader (l. 214–236).

Technical Corrections

- The title can be improved as currently, it implies the precipitation profile from two different cameras for the same event are being used to compare and contrast the precipitation characteristics, however, events are not related.
  Thank you for raising this point. To better reflect our methodology, we have updated the title as follows: "Statistical comparison of electron precipitation during auroral breakups occurring either near the open–closed field line boundary or in the central part of the auroral oval".

- Paragraphs in the introduction section, especially after paragraph four, are disconnected, hence they do not adequately motivate the study. Authors can add a transitionary sentence at the end of paragraphs highlighting how the study differs.

Thank you for pointing this out; we have reorganised the introduction, added better logical links between the paragraphs and clarified the motivation of the study.

- In the paragraph indicated with 220, the claim that “at energies greater than 20 keV, for which those two curves are almost one order of magnitude higher than the subsequent ones.” seems to be misleading as at 20 keV the difference seems to be twice to that of later times.
  Good catch! We have rephrased this into "at energies within 20–50 keV, for which those two curves have values 2–5 times greater than the subsequent ones".

- Further comparisons with literature where auroral breakup characteristics were provided (using satellite data) can improve the discussion of the paper. (Kataoka et al., 2019 and referencing literature)
  Thank you for providing this reference. We have expanded the discussion in the revised manuscript and included it, as well as some of the literature it refers to, where relevant (l. 353–354; l. 393–399).

Response to Reviewer #2

We thank the Reviewer for their careful scrutiny of our manuscript and the valuable comments. Below we respond to the queries and explain how we have implemented changes in the revised manuscript.

Please note that, whenever we indicate line numbers, we refer to the **revised manuscript with highlighted changes**.

In their submission the authors discuss the electron precipitation during magnetic substorms at two different locations: central auroral oval (Tromsoe) and further North (Svalbard). At first different drivers have been investigated, which did not [s]how significant differences between onsets observed in the central oval region and those near the open-closed field line boundary. In a second step the precipitation is investigated using all-sky cameras and incoherent scatter radars. Electron flux spectra are derived based on the radar signal. Further processing of the spectra lead to statements on the peak differential flux, peak energies, and the integrated precipitating energy flux and their temporal evolution during the substorms. Here differences between the two locations have been identified.

In general the paper is well written and understandable. It addresses a topic that is of interest and the used methods seem to be reasonable with some caveats concerning the limited differentiation between temporal and spatial evolution at Svalbard. However I have one major point of criticism that may be of interest for the whole study.

Thank you for this introductory statement.

Major issue:

– The complete second part of the study deals with details of the electron spectrum that is derived from the radar signal using the ELSPEC method.

However the reader has no idea how exact such a derived spectrum might be. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JA025636 for example states that "When the distributions have two maxima (D, F, and G), the maximum at higher energy is well reproduced, but the secondary maximum at lower energy is not as well modeled." Fig. 1b shows that double peak structures (e.g. at approx. 17:48, 17:52) and thus known periods of enhanced uncertainty appear. I don't want to enlarge on double peak structures here, but the reader should get an impression of the uncertainty of the derived spectra and how reliable the statements on the results (e.g. peak flux) are.

Thank you for taking the time to refer to the original ELSPEC publication and raising this point.

The paper in question shows that:

1. The integral energy flux is always very accurate, even if the spectrum shape is not reproduced exactly.
2. The integral number fluxes may be noisy in presence of double-peak structures, but there is no systematic bias.
3. The peak energies, as defined as the peak of the differential energy flux, reproduce the stronger peak accurately, if the spectrum contains two peaks and one of them is clearly stronger than the other.
4. If the peaks have about equal amplitudes in differential energy flux, both of them are reproduced reasonably well.

From 1., we can tell that, in our study, the integrated energy fluxes (shown in Figs 3c and 5c) should be fine, even in presence of double-peaked spectra. There might however, as you point out, be some level of uncertainty when it comes to peak energies and peak fluxes (Figs 3a-b and 5a-b), for occasionally we do see double-peaked spectra in our results. We further should point out that, in the current study, we consider differential number fluxes in the analysis, whereas Virtanen et al. (2018) discusses differential energy fluxes, which might lead to small differences.

What comes to the overalls shape of the spectra (Figs 4 and 6), the main characteristics with largest fluxes should be reliable, because they must come from the stronger peak even if there are double peaks. Some small ripple with low fluxes may of course be unreliable, but those are not discussed in our paper.

In the revision, we provide more details and discussion on the applicability and uncertainties associated with ELSPEC, in particular for the cases when there are two peaks in the retrieved spectra (see l. 400–412).

Minor issues:

– ELSPEC is an important tool in this study thus please add enough information to Section 2.3 to allow a reader to follow you. For example why do the resulting spectra all (or mostly) look Maxwellian? Is that an assumption in the model or does it appear naturally? What about the fine structure of the curves, does that depend in the internal energy resolution of ELSPEC (or EISCAT)? Also ELSPEC is an acronym that is not introduced.
Likewise, we now give more details on ELSPEC (and introduce the acronym, which stands for ELectron SPECtrum) in the revision (see l. 148; l. 152–156). The fact that most spectra look Maxwellian is not per-se an assumption of the model, although Maxwellian distributions are one of the solutions which are considered (see eq. (9) in Virtanen et al., 2018: Maxwellians are one type of solutions when L = 1). Distributions with finer structure as well as kappa distributions are also possible solutions with higher-order terms.

– Fig. 1: please check units: 10^12 m^-3 appears in many publications as typical peak electron density at 100km, you show 10^12 cm^-3.
Good spotting, thank you! This was of course a typo and has been corrected.

– For better understanding of the "valid data points" described in l. 188 the according periods when data from in Fig. 1 contributes should be marked there. Also it is not ideal to have a full paragraph on "data points" but the information that a data point last for 30s (and not as initially described in the EISCAT section 5-6s) follows in the next paragraph.
Thank you, this is a good idea! We now indicate in Fig. 1 the times with suitably enhanced electron density profiles to yield "valid data points" in our analysis (light-grey dots forming a line at the top of panel b). We also moved the sentences introducing the 30 s time resolution for data points before this paragraph to avoid confusing the reader (l. 210–213).

– l. 190: which median? of all altitudes?
The median electron density value considered at this step is for lower E-region altitudes only (85–125 km altitude). We have adjusted this part of the sentence to make it clearer (l. 216).

– l. 195: do I understand this right that this method is similar to a selection of the periods when the all-sky camera shows strong illumination above the radar? So why not simply using a threshold of the magenta graph in 1c?
In essence, you are right: the approach you propose should lead to similar results. The brightness curve gives an additional independent component to the timing and at the same time tells how fast the expansion of the aurora is, if the brightening of the image coincides with the electron density measured by the ESR. The reasons why we considered EISCAT data for this selection step are twofold. (i) This way, we ensure that we apply the selection criteria to the data set which will directly serve as an input from ELSPEC. This removes potential uncertainties coming from the exact mapping of the radar beam to the optical data, the optical data temporal resolution, etc. (ii) Processing a large amount of raw

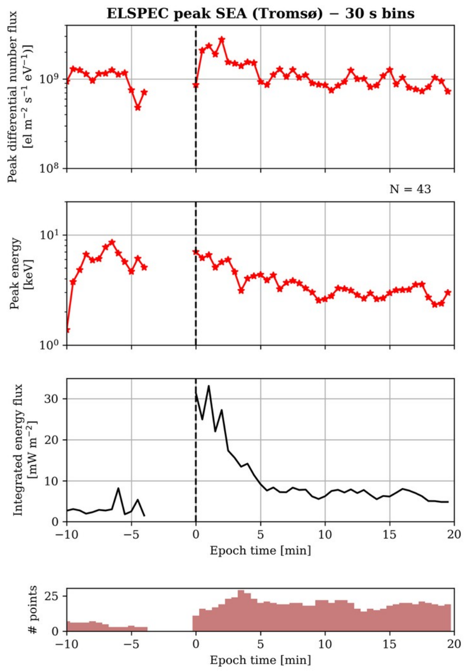
optical data is less straightforward than looking at the EISCAT data, due to memory requirements when loading all the individual images, making the data processing a lot slower and more cumbersome.

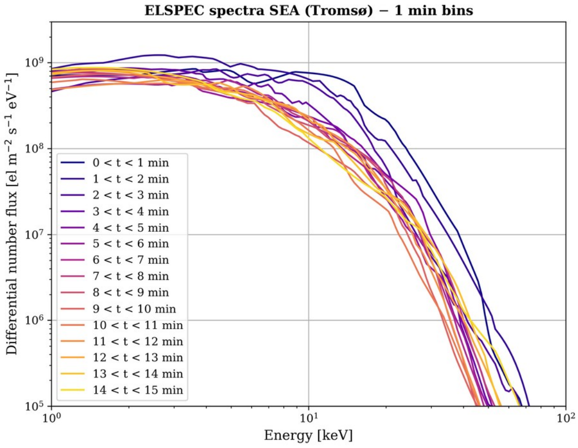
– l. 197: A spectrum is accepted when it exceeds background level by factor 3, OK, that should select periods with increased particle precipitation. But doesn't this introduce a major offset, especially during the period before the onset? I mean the background "reference" value is eliminated, so this should not be very representative.
Thank you for raising this question. As you point out, the selection applied in this paragraph leads to discarding the data points which are not associated with enhanced particle precipitation within the radar beam, including prior to the auroral breakup time.
Consequently, the precipitating spectra properties prior to the zero epoch shown in Figs 3 and 5 come from a fairly low number of data points (as can be seen in Figs 3d and 5d): the occasional ones where there was some auroral precipitation within the radar's field of view before the breakup happened.
What can be seen, however, is that precipitation prior to the breakup (when it exists within the EISCAT beam in Tromsø) has lower integrated energy flux, lower peak differential number flux, and lower peak energy, compared to what comes after the breakup. For Svalbard, there are so few valid points before the zero epoch (0–2 per time bin) that the obtained results are not significant, and we do not discuss them. In fact, we decided to mask all points obtained from less than 3 events, and updated Fig. 5 accordingly (see l. 241–244; l. 273–276).
Hopefully, the illustration of the selection of enhanced-precipitation data points on Fig. 1b, following your earlier suggestion, as well as its description on l. 227–231 helps in the understanding of the reasoning.

– l. 234: "zero epoch" should be clarified here. Due to the different locations of onset and radar at Svalbard (and thus the time delay of the observation) it helps to remember the reader that the onset time is meant here.
Agreed, we have clarified this (l. 273–274).

– l. 295: It is probably easier to label the two options with a) and b) and refer to that in the following paragraph(s).
This is a good suggestion, thank you. We have implemented it in the revision (see l. 335–348).

– l. 317: Shouldn't be a big deal to exclude those 14 events and check if impacts 3 and 4, isn't it?
Indeed, this is something we checked when preparing the revision. We provide below the corresponding figures, when excluding the 14 events in question for the Tromsø data set. We can see that, at post-zero-epoch time, those figures do not differ significantly from Figs 3 and 4 in the paper (in fact, the peak integrated energy flux just around the breakup time is even a bit higher than when considering all 57 Tromsø events). This therefore suggests that the equinox effect is not responsible for the differences between Svalbard and Tromsø breakup precipitation.
We have added a brief note about this in the discussion (l. 363–365).





Figures R1 & R2 *– Reproduction of Figs 3 and 4 from the manuscript excluding equinox breakups (Tromsø events).*

Typos and similar:

Thank you for noting these; we have fixed all of them.

– l. 80: compare substorm characteristics

– Fig. 1 l. 2: "the the"

– Fig. 1 l. 4: the arbitrary unit is linearly scaled? Show be clarified because its drawn on a log graph.

– l. 207: in the order

– l. 226: can be seen in the first 4~min, as

– l. 339: include

Note:

Currently temporal evolution and distance to the onset impacts the measurements at Svalbard in a probably similar and indistinguishable manner.

I don't know if that could be successful, but you may try to plot a version of Fig. 5b with all individual events and a color coded distance to the onset. In that way the currently cloaked distance to the onset may give a more comprehensive picture. Well, maybe.

This is indeed an valid idea! We did consider this when preparing the study; however, the problem is that, in the vast majority of cases, the breakup seen in the Svalbard camera data occurs very close to the edge of the field of view (sometimes partly outside of it), leading to relatively large uncertainties in the mapping to derive a distance.

Therefore, we cannot implement this idea in a satisfactory way and leave this type of analysis to future work.