Notes on the revised manuscript of "Spatial characteristics of the dayside auroral ionosphere observed by Incoherent Scatter Radar"

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The answers here are the same as those that were posted in the discussion, but the changes in the manuscript are described in more detail and any new text, as well as the page numbers of changes in the revised text are added. In addition to the changes here, we have also made a few minor technical corrections to the text. These are also highlighted in the track changes document.

Review comment 1

We thank the reviewer for reviewing our manuscript and for providing comments and suggestions. The full comment is posted below. Our responses are written in bold blue text in-between the reviewer's comments.

This paper studies the ionospheric structure, including density and temperature in the vicinity of the cusp region, using the EISCAT Svalbard radar. They show that the ionosphere is structured by its position relative to the open-closed field line boundary, being markedly different equatorwards of it in the closed field line region and polewards of it in the polar cap. The paper is well written, the results are interesting, and the data is presented clearly. Hence, I recommend it for publication. I have only a few minor comments, listed below.

Lines 94-95: "Each experiment … visual inspection." It is not clear what is meant by this sentence.

The data from each experiment was inspected visually, and the events with a clear auroral oval at 300 km were included in the data set used for this study. We will add this sentence in the revised text for clarity. (Line 96 in the revised text).

Line 115: Perhaps state that it is IMF B_T that is being referred to.

Agreed. The sentence will be rewritten as such: "Finally, for the statistical analysis, we remove data points for which the IMF $B_T < 9$ nT". (Line 121 in the revised text)

Lines 204-208: The authors are looking for a relationship between latitudes of regions and PCN or AE. Perhaps it should be stated that it is expected that the OCB should be in a state of flux when AE or PCN are elevated.

We will add the AE expectation to a more detailed introduction to the latitude description, in the first paragraph of the section: "The location and movement of the OCB is linked to the opening of magnetic flux at the dayside magnetopause and to the closing of flux in the magnetotail, and its latitude is therefore expected to relate to both the IMF and geomagnetic indices such as AE (Lockwood et al., 2005, references therein). This is previously shown in studies based on ASI (Johnsen and Lorentzen, 2012b) and satellite (e.g. Newell et al., 1989; Newell et al., 2006)." (Line 208 in the revised text.)

In the revised text, we make corrections according to the following four comments:

Line 155: Mis-spelling of "first".
Line 341: Mis-spelling of "assess".
Line 382: Is "increases" meant here?
Line 412: Should read "beneficial to include".

Review comment 2

We thank the reviewer for carefully reviewing our manuscript and for providing detailed comments and suggestions. The full comment is posted below. Our responses are written in bold blue text in-between the reviewer's comments.

This paper uses many observations from the EISCAT Svalbard Radar (ESR) to quantify the ionospheric properties of the dayside auroral region, and present both case study and statistical results for these ionospheric properties separated by magnetic topology. The authors use a recent technique to identify the Open Closed Boundary (OCB) using the electron temperature and solving the 1D electron energy equation to identify the region where there is an energy source. They then define the Polar Cap (PC) as the region +3 degrees latitude poleward of the OCB. Using these definitions they look at two case studies, one with primarily northward IMF, and another with a southward turning IMF. They find that there is more structure to the ionospheric properties and evidence of poleward moving auroral forms in the southward turning case. They also conduct a statistical survey of the ionospheric properties with respect to the defined boundaries. They find that there are clear and persistent gradients in electron temperature across the OCB, and less strong signatures in ion temperature or electron density. In terms of local time variations, they also find less clear variations in ionospheric properties, but some evidence of change in the ratio of electron density in the E region to F region. The results are overall consistent with other studies, and this work demonstrates the effectiveness of ISR and the OCB boundary identification technique, while providing clear benchmarks of ionospheric properties for others to compare against.

Thank you!

Overall comments:

The authors do a great job describing the treatment of the data and the boundary identification techniques, and they make consistent and detailed comparisons of this work to previous studies.

Thank you!

One of the apparent main conclusions appears circular, the increase in electron temperature across the OCB cannot be both the technique used to define the OCB itself, and also the conclusion. By definition, there MUST be increased Te across the OCB because it is defined as a Te threshold. There are still plenty of interesting points that are made, including discussion on the magnitude and width of the gradient, and the fact that it is so strong and persistent to me is one of the most important takeaways.

Enhanced T_e is used as a proxy for low energy precipitation and to identify the location of the OCB. Therefore it is also expected that the statistical analysis yields higher T_e on open field lines than on closed field lines. We will clarify that we do not consider this fact as a main conclusion. As the reviewer states, there should be a clear emphasis on the behavior of the difference/gradient across the OCB, such as the magnitude, steepness and width. In the revised version we will carefully review and adapt the text so that this is clear. See also our replies to later specific comments concerning this.

It appears to me that some of the trends in the results and subsequent discussion are a bit overstated. When the uncertainties are so much larger than the supposed trend and the trend is made of up only a handful of bins on the X axis, it is okay to soften the claims. Indeed in some places the authors do a good job of this, but it is inconsistent.

We agree, the claims both could and should be softer such that the text is more consistent. In the revised manuscript, we will carefully go through all such statements.

In the discussion and conclusion there is explanation about the shadow height and ion velocity and how these can help explain some of the results, but I believe this can be sufficiently covered just in the text. This would clear up space from panels c and h for the case study figure, as these panels are otherwise not the focus of the results.

Yes, we agree that the shadow height plots are not necessary. In the revised manuscript we will remove both panels.

We will add the following sentence about the 2014 case: "The shadow height at the ESR latitude is approximately 330 km at the beginning of the experiment and 105 km at the end of the experiment. The altitude is calculated using solar elevation angles from the PyEphem Python package (Rhodes, 2011)." (Line 143 in the revised text)

We will add the following sentence about the 2015 case: "The shadow height at the ESR latitude is approximately 340 km at the beginning of the experiment and 115 km at the end of the experiment". (Line 179 in the revised text)

For completeness, we prefer to keep the ion velocity measurements. Although there is a data gap in the middle due to observational constraints, the ion velocity data to the North and to the South are useful to display variability of the plasma convection.

Overall, I think this a well conducted, detailed study of ISR observations of the dayside auroral region and is a worthy contribution to the field. The comments about the existence of the Te gradient across the OCB are the only major comments, the rest can be considered minor.

Thank you!

Detailed Comments:

Line 13: Introduce EISCAT Svalbard Radar before ESR the acronym appears.

We have added the acronym to the first mention of the EISCAT Svalbard in the abstract. (Line 4 in the revised text)

Line 36: "… the following." Change to "section 4.1."

Here, "the following" refers to the following sentences and paragraphs of the introduction. For clarification the sentence is rewritten as "Examples of previous ISR observations of the dayside auroral ionosphere or cusp region are introduced in the following paragraphs". (Line 36 in the revised text).

Line 50: need to add a sentence to connect this to the present work. "Observations of Te, Ti, and Ne can be used to identify FTEs and PMAFs" or something

We add the following sentence: "PMAFs have been observed in T_e, N_e, and T_i (e.g. Lockwood et al., 1993, 2000)." (Line 52 in the revised text).

Line 60: Can you give explanation of why the open field region is split into OCB and PC here?

This is done to extract the observations close to the OCB and more likely covering the cusp and newly opened field lines from the remaining open field lines. We add a sentence to explain this in

the text: "The open field lines are split in the two open regions to extract the observations closest to the OCB and more likely covering the cusp from the remaining open field lines." (Line 62 in the revised text).

Line 86: Why is 15 minutes chosen as the time delay. Is there a reference that could be pointed to?

Previous studies have shown that typical delay times range from 10-15 minutes. We actually chose 12 minutes to be in the middle of the range. We applied delay times in testing, and no significant changes appear in the statistical results. We will update the text and add this reference:

"As the OMNI measurements are propagated to the bow shock, a delay is added to the IMF data to account for the delay time between the bow shock and the ionosphere. This added delay is 12 minutes (Samsonov et al., 2018)." (Line 88 in the revised text).

Samsonov, A. A., Sibeck, D. G., Dmitrieva, N. P., Semenov, V. S., Slivka, K. Y., Šafránkova, J., & Němeček, Z. (2018). Magnetosheath propagation time of solar wind directional discontinuities. Journal of Geophysical Research: Space Physics, 123, 3727–3741. https://doi.org/10.1029/2017JA025174

Line 101: This paragraph was a bit difficult to understand. My interpretation of the method by reading Frøystein et al. 2024 is that by including a neutral atmosphere model and making some simplifying assumptions one can solve the electron energy equation for the heating rate Q at all locations when ESR gives data. This Q is effectively what tells us where the auroral region is because where Q is large is where energy precipitation is happening. Instead of setting a threshold on Q itself, however, the results are used to adjust a threshold on Te, given the solution to the energy equation. Is this the correct interpretation?

Yes. The electron energy equation is used to calculate heat sources within and outside the dayside aurora. The heat source and the background electron density are scaled to obtain a relationship between Te and Ne. This relationship is used to adjust the Te threshold to account for the possible variation in Te due to Ne variation. This is significantly quicker and simpler than adding a threshold to Q, which would mean that Q would have to be calculated every time the method is applied.

We include more details in the paragraph to explain the steps more clearly: "For identification of the dayside aurora in the ESR elevation scans, we use the method described by Frøystein et al. (2024.). A short description of the method is given here. By running the ESR in a scanning mode, observations of the aurora and the quiet ionosphere can be made within a short time lapse. Using observations Te, Ne, and Ti within and outside the dayside aurora, the electron heating rate due to precipitation is estimated from the electron energy equation, and it is found that changes in Ne can introduce Te variations of up to 1000 K given a constant heating rate. In addition, Te falls off linearly with exponentially increasing Ne given a constant electron heating rate. This relation between Te and Ne is then used to adjust an initial Ne enhancement threshold of 2000 K to include the Ne effect. Observations where this threshold is reached are classified as being within the dayside aurora or on open field lines. " (Line 99 in the revised text)

Line 110: how is the averaging done to obtain the expected value required to calculate relative error here?

The GUISDAP analysis results include estimates of errors in the ionospheric parameters. These errors and the parameters are used to calculate the relative errors for each of the ionospheric parameters Te, Ti, Ne, and vi. For clarification we add "The relative error is calculated as $|dp/p|$

where $|p|$ is the ionospheric parameter and $|\mathrm{dp}|$ is the error from the GUISDAP analysis." (Line 114 in the revised text)

Line 123: remove the sentence starting with "Then, statistical …"

We will rewrite this sentence to combine it with the following sentence: "Next, statistical results of OCB latitude, as well as the altitude and latitude variation of the ionospheric parameters are presented." (Line 128 in the revised text)

Figure 2: Great job with the error bars and quality flags, along with the boundary panel (i), this was very easy to understand and a good way to present these results.

Thank you!

Figure 2: Consider removing panels (c) and (h) or moving them to supplementary

We remove the panel c and rewrite the text slightly, ref. the earlier comment and reply about these panels.

Line 150: see general comments, need to focus only on the descriptions of the Te gradient and not the fact that there is an increase in Te across the boundary. Because a threshold on Te is used to define the boundary, there will always be a gradient (except when there's not and that triggers the quality flag so those points are removed…). It may be worth noting this for the reader here.

It is true that the increase in Te across the boundary is expected due to the Te -based method. However, the main purpose of this description is to describe the spatial and temporal characteristics of the enhanced region. To clarify, we will update the text like follows:

"As expected, Te shown in panel c is enhanced along the identified boundary and the gradient from closed to open field lines is generally well defined. During intervals where the gradient is not well defined, the boundary latitude is flagged as uncertain, e.g. at approximately 09:00 UT. In addition, the enhancements in Te are clear during the entire experiment and span several degrees in latitude. In particular, no differences between the OCB region and the PC are visible here. " (Line 157 in the revised text)

Line 157: From looking at the figure, I would describe the Ti enhancements as "patchy", "intermittent" or something like that, rather than "enhanced in several regions"

Yes, intermittent is more descriptive here. We will rewrite to: "T_i shown in panel f, exhibits intermittent enhancements.". (Line 166 in the revised text).

Line 161: I don't think the inclusion of the ion velocity measurements are helpful. In the discussion it's noted why the property would be useful to have, but that the measurement is difficult and that is sufficient. Could either remove this paragraph or move these to supporting information.

We thank the reviewer for the suggestion, please se earlier comment and reply about this.

Line 172: Is this 20 minute shift on top of the 15 minute shift that has already been done to the data?

We thank the reviewer for pointing this out. This 20 minute delay is not an added shift, but the observed delay between the southward IMF turning and the start of the equatorward movement of the OCB. In the text, we add a more detailed description of the observations following the southward turning of the IMF:

“Following the southward turning of the IMF at approximately 07:15 UT, several features are observed in the ESR measure ments. For Te, the strength of the S-N gradient across the OCB increases. The enhancements in Ti increase. vi is strongly northward from the southward turning, and until the IMF northward turning. These three features are observed just after the southward turning at 07:15 UT. The OCB moves equatorward following the southward turning, but the movement only starts ~20 minutes after the turning and the features are observed in the ESR parameters “ (Line 199 in the revised text).

Line 174: the AE signal has a sharp peak which is not reflected in the OCB latitude, overall the enhanced AE lines up well with OCB latitude change. Why not plot AE vs OCB latitude and do a quick correlation calculation?

The correlation coefficient between AE and the OCB latitude for the 2015 case in Fig. 3 is -0.91. For the 2014 case in Fig. 2 it is -0.65. We will add these numbers to the text:

“The correlation coefficient between the OCB latitude and the AE index is -0.65.” (Line 153 in the revised text).

“In fact, the correlation coefficient between the OCB latitude and the AE index is -0.91.” (Line 186 in the revised text).

Line 175: see above, Te must be enhanced by definition¨

Yes, see reply above. For clarity, we reformulate:

“The enhancements in Te (shown in panel c) on open field lines have a wide peak centered at approximately 08:20 UT. After 08:20 UT and until approximately 09:30 UT, there is a strong N-S Te gradient from the region on CF to the OCB region while there is a softer gradient northward of the Te enhancements into the polar cap. During this interval, there are also several Polar Moving Auroral Forms (PMAFs). These are seen as defined structures of enhanced Te that move poleward (Northward).” (Line 187 in the revised text.)

Line 185: The increase in ion velocity occurs before the OCB latitude changes which is noteworthy

Yes, this is an interesting point. In the text we will add more details about the southward IMF turning.

Line 209: change “the trend is similar” to “the trend is slightly weaker”

We change this sentence to: “For B_z (panel c) the trend is similar, but weaker.” (Line 227 in the revised text).

Line 223: see above, there is plenty of great content in this paper without the ion velocity results

Vi is not included in the statistical analysis, but it felt natural to explain why. To remove some of the emphasis on vi, we will condense the text concerning the vi difficulties in both the results and discussion. The paragraph here is removed.

The new paragraph in the discussion reads: “Next, the velocity vi was omitted from the statistical analyses. Due to the nature of elevation scans, the observed line of sight vi is a mixture of vertical and horizontal velocities depending on the current elevation angle. When calculating the horizontal N-S vi, data is lost overhead the radar and emphasis is therefore put on the edges on the FOV. This is especially critical for the E-region, where the FOV is narrow. In addition, depending on the ESR position relative to the convection pattern, the magnitude of N-S and E-W velocities will

vary (Moen et al., 2001). Since the data set includes experiments along both the geographic and geomagnetic meridians, the interpretation of what parallel velocity that is observed is slightly different (magnetic or geographic N-S)." (Line 356 in the revised text).

Line 234: see above, what's important here is how the properties of the Te gradient at the OCB, not the fact that there is one

Yes. It is still of interest to investigate where along the altitudinal column the enhancement peak. The magnitude of the difference from closed field lines is also interesting to note as it facilitates comparison with other studies. To move the emphasis from the CFL/OCB separation we rewrite the first sentence in the paragraph to: "From Fig. 5a, it appears that the Te difference between CFL and along the OCB increase with altitude, especially above 200 km." (Line 247 in the revised text).

Line 236: same as above

Here, we do not compare closed and open field lines, but rather the observations closest to what has been defined as the OCB to the observations further into the polar cap. The difference between these observations is not directly affected by the Te method, and the difference is noteworthy.

Figure 6: very nice visualization of the data

Thank you!

Line 255: see above, the Te increase poleward of OCB is a given

We rewrite this sentence to move the emphasis to the gradient: "Most striking is the strength of the T_e gradient across the OCB." (Line 268 in the revised text).

Line 256: For future studies, perhaps the 3degrees definition for the PC region should be bumped up to 4 degrees based on these results

Yes, that could be done. For this study, we saw little difference in the behavior of the altitude profiles (Fig. 5) when tweaking this 3-degree definition (mentioned in section 5.2 in the discussion).

Line 274: Rather than just provide the reference, could you give a one sentence overview of what this ratio might be able to tell us?

We rewrite the sentence to: "In this section NeE to NeF ratio is quantified for the scans used in this study, which may provide information on the ratio of high to low energy precipitation. " (Line 290 in the revised text).

Line 284: To me, the trend in the ratio for all three regions is too small relative to the uncertainties. The most important feature that stands out to me is that the CFL ratio is higher than both the OCB and PC. If someone wanted to find the OCB and only had access to Ne, maybe this ratio could be useful for that.

This is a pertinent comment. The difference in the ratio between open versus closed field lines is the primary result from this analysis. The text will be modified accordingly to improve clarity. Yes, using the ratio only could be useful to provide some first order estimate on whether the field lines are open or not. The updated text is (from line 297 in the revised text):

“Even though the FOV is limited in the E region and the spread in the data is naturally relatively large, some trends with respect to MLT can be perceived. The behavior of the Ne ratio is different between the three regions.

On CFL, the ratio peaks pre-noon at values up to 0.6. After 11 MLT the ratio varies little around approximately 0.5. On open field lines (OCB and PC) the ratio is almost constant with MLT, and any MLT variation is far within the error bars. The ratios averages at almost 0.3.

Larger ratios are explained by either larger E region densities or lower F region densities. In general, larger ratios are more likely on CFL than on open field lines regardless of MLT range.”

All figures: very readable and well formatted figures

Thank you!

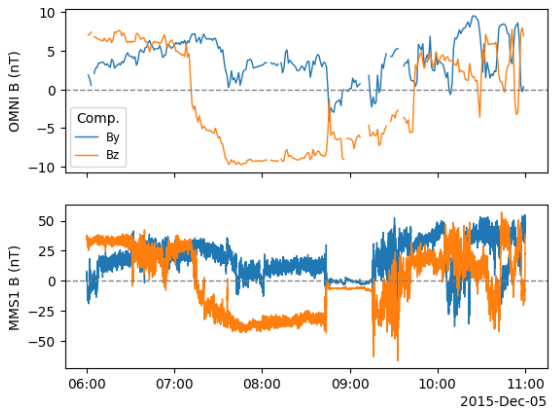
Line 302: if the ion velocity is kept in, it’s worth noting that the timing of the velocity reversal is not aligned with the OCB latitude change

Yes, see earlier comments and reply about the southward turning and the delayed response of the OCB. The description of the ion velocity will be added.

Line 316: did you look to see if there are any spacecraft upstream in the solar wind that are closer to Earth for these two case studies?

This is a good suggestion. For the 2015 case, shown in Fig. 3, MMS is positioned well upstream. The IMF Bz and By in the OMNI dataset (propagated from L1 to the bow shock) and observed by MMS is shown below. The MMS data is obtained through the pyspedas python package (Grimes E. W., et al. (2022) The Space Physics Environment Data Analysis System in Python. Front. Astron. Space Sci. 9:1020815. doi: 10.3389/fspas.2022.1020815).

The timing of the Southward IMF turning is the same for both. There are some differences between the observed By after 10 UT. Overall, the agreement is good, but the observed differences also point to the fact that it can be difficult to directly relate the ESR observations to IMF measurements. For this study, we only keep the OMNI IMF, but a more detailed analysis also based on satellite measurements closer to the Earth, such as from MMS, would absolutely be of interest in the future.



These sentences are added to the revised text: “For instance, for the 2015 case (Fig. 3), the NASA Magnetospheric Multiscale Mission (MMS) satellites crossed the S-E line during the experiment

(seen in MMS orbit plot from https://lasp.colorado.edu/mms/sdc/public/plots/#/historical-orbit. Last accessed: 08.04.2026). The MMS observed IMF exhibits overall similar characteristics to that of the OMNI dataset, including the timing of the Southward turning; however, some small variations can be perceived (seen in MMS quick look plots at https://lasp.colorado.edu/mms/sdc/public/plots/#/quicklook. Last accessed: 08.04.2026). A more detailed analysis of the effect of such variations between IMF observations at different positions along the S-E line and ESR measurements could be interesting, but it is outside the scope of this study. " (Line 333)

Line 324: I'm confused about the phrase "a bias is possibly introduced." It is perfectly acceptable to make the boundary definition based on one of the measured quantities, but when you do so it's important to note that. It is not a bias, but simply how the boundary is defined. For the magnetopause boundary under southward IMF we sometimes look for rotations in the magnetic field in spacecraft data as the indicator of a magnetopause crossing. It's not a bias to say that the field rotates across the magnetopause, but we also don't call it the most striking feature. We would describe how fast it rotates, just how you can describe how sharp and wide the Te gradient across the OCB is.

Yes. This paragraph will be updated according to this comment. The updated text is: "First, Te is the main parameter used in the method for obtaining the OCB latitude and for separating open from closed field lines. Therefore, a separation between open and closed field lines and higher Te on open field lines is expected. However, Te is the parameter exhibiting the strongest latitudinal differences between open/closed field lines when viewing the experiment observations separately (e.g. Figures 2 and 3). In the superposed epoch analysis, we also note that the OCB is typically placed where the Te gradient is strongest. Further, every timestep where the latitude of the OCB is uncertain is not included in the statistics." (Line 343 in the revised text.)

Line 345: Proving out that this OCB boundary identification holds across this large set of conditions, and that it has advantages over other methods, is one of the most important findings of this study, in my opinion.

Thank you!

In the revised manuscript we make corrections according to all the following comments:

Line 44: change "of the observations covering the cusp region" to "of observations located in the cusp region"
Line 259: change "def" to "d-f"
Line 373: change "def" to "d-f"
Line 378: consider "slight trends" or revising this sentence
Line 382: remove "The precipitation rather increase the density in the F region."
Line 398: superposed
Line 400: change "if" to "of"
Line 247: I believe it's "superposed" not "superimposed"
Line 261: change to "slightly enhanced"
Line 59: list out ESR, this is the first time it appears outside the abstract
Line 107: change "band is discarded" to "buffer is discarded"
Line 68: Maybe a colon after "Section 4 presents the results"
Line 153: change to "region of slightly lower density"