

Response to review comment 1 on “Spatial characteristics of the dayside auroral ionosphere observed by Incoherent Scatter Radar”

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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 T_e across the OCB because it is defined as a T_e 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).”

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”.

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 T_e 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 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 50: need to add a sentence to connect this to the present work. “Observations of T_e , T_i , and N_e 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 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 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:

Samsonov, A. A., Sibeck, D. G., Dmitrieva, N. P., Semenov, V. S., Slivka, K. Y., Šafránková, 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 T_e , 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 T_e and N_e . This relationship is used to adjust the T_e threshold to account for the possible variation in T_e due to N_e 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 T_e , N_e , and T_i 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 N_e can introduce T_e variations of up to 1000 K given a constant heating rate. In addition, T_e falls off linearly with exponentially increasing N_e given a constant electron heating rate. This relation between T_e and N_e is then used to adjust an initial N_e enhancement threshold of 2000 K to include the N_e effect. Observations where this threshold is reached are classified as being within the dayside aurora or on open field lines. “

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 T_e , T_i , N_e , and v_i . For clarification we add “The relative error is calculated as $|dp/p|$ where $|p|$ is the ionospheric parameter and $|dp|$ is the error from the GUISDAP analysis.”

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.”

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 T_e gradient and not the fact that there is an increase in T_e across the boundary. Because a threshold on T_e 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 T_e across the boundary is expected due to the T_e -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, T_e shown in panel d 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 T_e 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: From looking at the figure, I would describe the T_i 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 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 see 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.

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.

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

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

“The enhancements in Te 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 (PMAF). These are seen as defined structures of enhanced Te that move poleward (Northward).”

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 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.

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 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 strong T_e gradient across the OCB.”

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. This quantity can provide information on the ratio of high to low energy precipitation. “

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.

All figures: very readable and well formatted figures

Thank you!

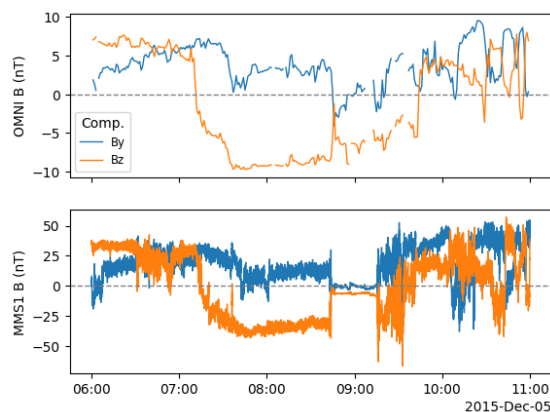
Line 302: if the ion velocity is kept in, it's worth noting that the timing of the velocity reversal is not aligned with the OCB latitude change

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.



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 T_e gradient across the OCB is.

Yes. This paragraph will be updated according to this comment.

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"