We thank the reviewer for their thorough evaluation of and constructive feedback on our manuscript. We have made several changes below that we believe address the reviewer's commentary and improve the manuscript. These changes are summarized in this letter, along with specific responses to the reviewer's comments. Below the reviewer's comments our response is shown in blue. Modifications and/or additions to the manuscript are shown in *italics*.

Reviewer #1 Evaluation

The paper presents a comprehensive discussion of how the neutral wind is and could be considered in height-integrated ionospheric electrodynamics. Rocket and radar measurements are applied to study the actual "effective neutral wind" and evaluate common proxies. The paper is a very interesting contribution to the field of ionospheric electrodynamics and addresses a relevant issue. It can be published with minor revisions. Please see some suggestions below:

- Thank you for the positive feedback!

1. Equation 1 and following:

I assume b is the unit vector in magnetic field direction, i.e. b=B/|B|? Please clarify.

- We have added the following statement just below Equation 1 along with the description of the other variables in Equation 1:

  *"The unit vector **b** points in the direction of **B**."*

2. Section 3.3 and Figure 3

The statistics applied here are somewhat unclear to me: What is the statistical meaning of Q3+1.5IQR? Also, just roughly estimating for the left box in Figure 3a, IQR seems to be ~200m/s (with Q3~225m/s and Q1~25m/s), but the upper horizontal line (Q3+1.5IQR) is at about 300m/s. This might be a misunderstanding on my part, but in general, I don't see the need for too much statistical analysis on only 15 wind profiles, so just showing the median values and Q3 and Q1 should suffice.

- To the first question, a good way to understand the statistical meaning of Q3+1.5 IQR is via comparison with a Gaussian distribution. For a Gaussian, values outside of the range defined by Q1-1.5IQR and Q3+1.5IQR represent less than 1% of the data. Our understanding is that this rule-of-thumb test for identifying outliers originates with the statistician John Tukey.

  To the second question, here the reviewer has identified a mistake in our manuscript (thank you). For all but one box plot, the horizontal lines above and below indicate the maximum and minimum value within each distribution. The exception is in panel b of Figure 3 for $\mathbf{u}_{proxy} = \mathbf{u}_{\sigma H,peak}$, where the point above the upper horizontal line is an outlier. We have modified the main text as follows in the revised manuscript:

  > *Each distribution is presented as a vertical box plot, where each box indicates (from top to bottom) the upper quartile Q3, the median, and lower quartile Q1. The horizontal lines above and below are respectively given by the largest value not exceeding Q3+1.5 IQR and the smallest value not below Q1-1.5IQR, where IQR = Q3-Q1 is the interquartile range. (The range Q1-1.5IQR to Q3+1.5IQR is a common rule-of-thumb test for identifying statistical outliers; see, e.g., Dekking et al, 2005.) In Figure 3a these lines simply indicate the minimum and maximum of*

each distribution, since no values are outside these thresholds.

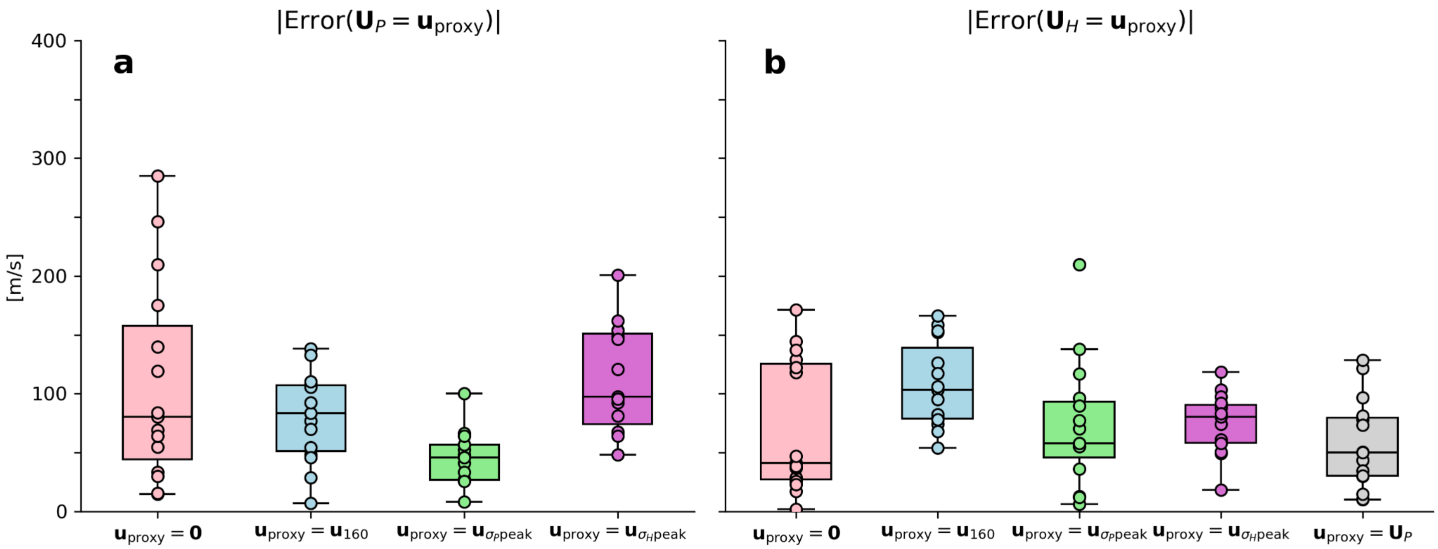
In analogy with Figure 3a, Figure 3b shows the error associated with assuming $\mathbf{U}_H = \mathbf{u}_{proxy}$*. One value in the distribution of errors for* $\mathbf{u}_{proxy} = \mathbf{u}_{\sigma Ppeak}$ *(shown in green) is identified as an outlier by the rule-of-thumb test mentioned above.*

We have also modified the caption for Figure 3 in the revised manuscript as follows:

The horizontal lines above and below each box-and-whisker plot are the minimum and maximum except in panel b for $\mathbf{u}_{proxy} = \mathbf{u}_{\sigma Ppeak}$*, shown in green, where one value is an outlier (see main text).*

With these revisions, we believe the text and figures in the new manuscript essentially does what the reviewer suggests by showing the median, Q1, Q3, and the minimum and maximum of each distribution except for the one described in the revision. We hope the reviewer will agree that this is a sensible strategy.

- Incidentally, regarding Figure 3 we found a bug in our code that incorrectly calculated the size of the errors associated with assuming the Pedersen-weighted neutral wind $\mathbf{U}_P$ and the Hall-weighted neutral wind $\mathbf{U}_H$ are either equal to zero (pink box-and-whisker plot at left in each panel of Figure 3) or $\mathbf{u}_{160}$ (light blue box-and-whisker plot, second from left). We have replaced the old Figure 3 with the following figure:



In the updated figure the correctly calculated errors are smaller for both of these, and $\mathbf{u}_{160}$ is no longer the obviously worst choice. (In panel b we also added a box-and-whisker plot to assess the performance of $\mathbf{U}_P$ as a proxy for $\mathbf{U}_H$.) We have therefore modified the abstract and several portions of the text in the revised manuscript. We revised the abstract as follows:

We also find that measurements of the thermospheric wind at altitudes of ~100–120 km are a more accurate estimate of the thermospheric wind terms in expressions of height-integrated, high-latitude electrodynamics than simply assuming the neutral winds are zero in Earth's corotating frame of reference. This points to the possible utility of, for example, Fabry-Perot interferometers that measure 557.7-nm (green-line) emissions around this altitude range.

Also, it seems in Figure 3b that u=0 has a slightly lower median error than the wind at the Pedersen peak. The difference is minor, but contradicts the statement in lines 16 and 17, which is very general in claiming to have found the best proxy (one might come up with other proxies than the four investigated here). Figure 3 suggests that the Pedersen peak wind could serve as an improved proxy for effective neutral wind compared to the commonly applied u=0 and u=u160.

- We agree with the reviewer, and acknowledge that there may be other, even better proxies than those we've investigated. We have revised the manuscript so that our claim about the wind at the Pedersen peak is not so absolute and general. We have specifically replaced the wording "is the best proxy" in the originally submitted abstract with the revision stated above, which leaves open the possibility of other, even better proxies.

3. Lines 223-225

This statement is somewhat confusing. From Lines 111-112, I understood that the Pedersen-weighted neutral wind is the most natural definition of the effective neutral wind, given Equation 11. I do not see the connection to Figure 3, where the Pedersen-weighted neutral wind is used as a baseline to compare different proxies.

- We agree that this statement was confusing. In light of the results contained in the corrected Figure 3 (shown above), we have modified the paragraphs starting on line 244 in the revised manuscript as follows:

> *Section 3.3 addresses the commonly employed assumption in experimental studies that the winds are zero in Earth's corotating frame of reference, as well as the concept of a two-dimensional "effective neutral wind" pattern introduced by Lu et al. (1995) and employed by both Baker et al. (2004) and Billett et al. (2018). These studies take the effective neutral wind pattern to be the neutral winds at 160-km altitude, $\mathbf{u}_{160}$, although analysis in Section 2 indicates that a more suitable definition for the effective neutral wind is the Pedersen-weighted neutral wind $\mathbf{U}_P$, which appears in both the height-integrated Ohm's law (Equation 7) and the lower-bound estimate of Joule heating (Inequality 11).*
>
> *Given the scarcity of simultaneous measurements of the quantities needed to calculate $\mathbf{U}_P$ and $\mathbf{U}_H$ (altitude profiles of conductivities and neutral wind), another motivation for this study is to investigate potential proxies for $\mathbf{U}_P$ and $\mathbf{U}_H$. Figure 3a explores the accuracy of four proxies for $\mathbf{U}_P$. Statistically speaking, the best proxy for $\mathbf{U}_P$ is $\mathbf{u}_{P,peak}$, the neutral wind at the altitude of the peak Pedersen conductivity (located between 115-km and 128-km altitude for the 15 rocket flights presented in Table 1). In contrast, the proxy for $\mathbf{U}_H$ with the lowest median error is the zero vector (pink).*
>
> *Regardless, all proxies perform similarly in the sense that all are associated with errors of order at least several tens of m/s. While this result is subject to the same caveat given above regarding limited sampling, it does suggest that one should be skeptical of "quick fixes" for the neutral wind problem, including ignoring the winds or using wind estimates from higher altitudes such as those estimated via Fabry-Perot interferometers attuned to the 630-nm line (e.g., Shiokawa et al, 2014, and references therein), which tend to cover the F-region ionosphere. On the other hand Fabry-Perot interferometers attuned to 557.7-nm (green-line) emissions, which tend to peak between 100-km and 130-km altitude, could provide neutral wind estimates relevant for analysis of high-latitude IT electrodynamics. The primary caveat with these measurements is that the peak height of green-line emissions can range over ~80- to 180-km altitude via the influence of auroral precipitation (Whiter et al, 2023).*

4. Section 3.4 and associated discussion

It would be interesting to give an estimate of how large the third term of Equation 10 RHS is in comparison to the other two terms, e.g., for a constant electric field or shown as a plot over varying electric field strength. This would allow to assess the inequality in Equation 11 and how good the application of Pedersen-weighted neutral wind as the effective neutral wind is. Also, if I understand Equation 10 correctly, the mix term (second term RHS) also carries some of the difference caused by assuming U_P as the effective neutral wind? Therefore, why do you focus solely on the exclusively wind-dependent term?

- We chose to focus exclusively on the wind-dependent term since getting an accurate estimate of the electric field for each flight involves significant additional analysis, and would be experimentally demanding, as we have stated at the close of the manuscript. We agree that it would nevertheless be worthwhile and valuable to understand how the third term in Equation 10 compares to the other terms.

  To examine the total Joule heating and the role of the neutral wind we have added another figure (Figure 5 in revised manuscript, shown below).

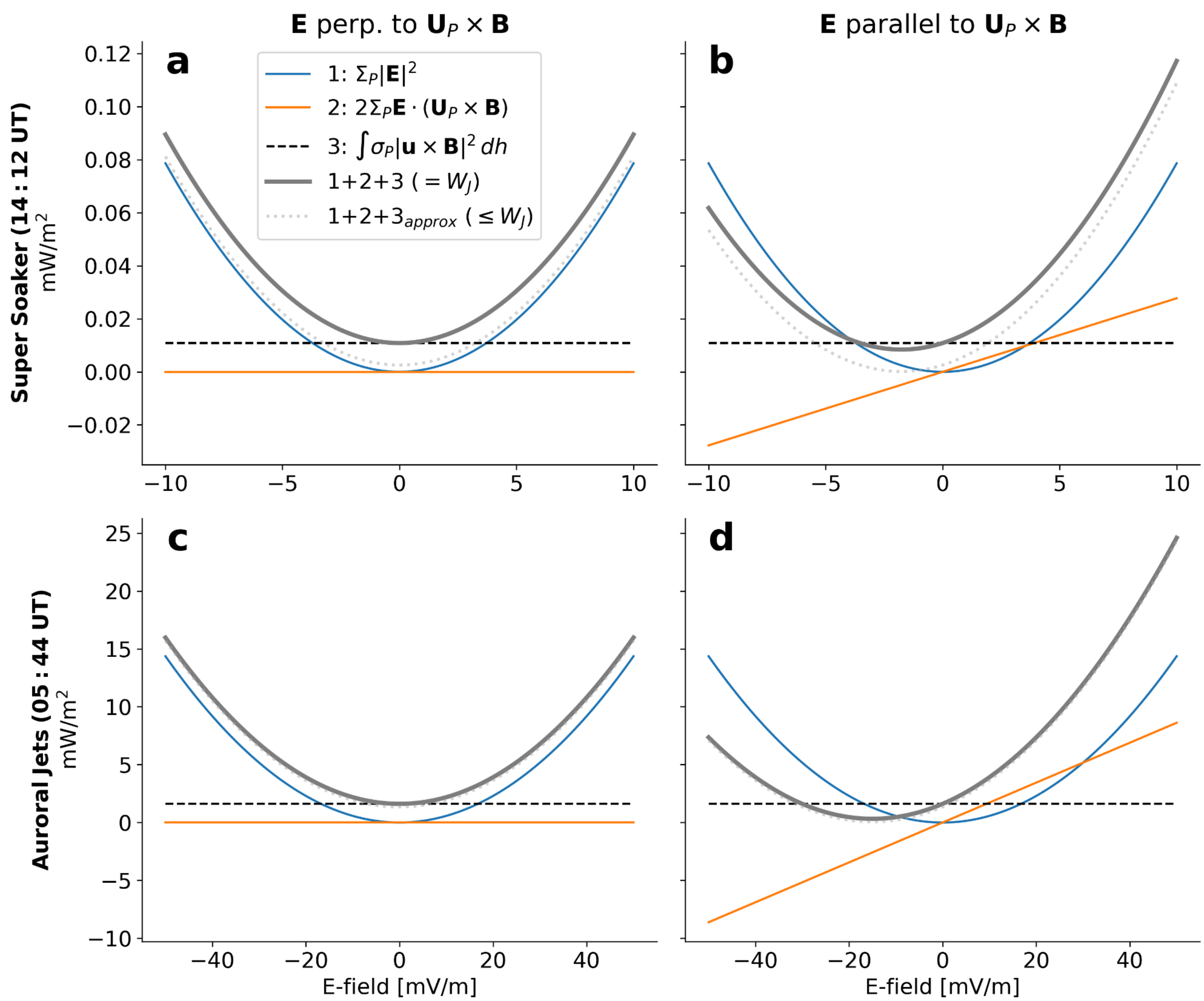


Figure 5. *Comparison of terms in Equation 10 during quiet (top row, Super Soaker campaign) and active (bottom row, Auroral Jets campaign) geomagnetic conditions for a hypothetical electric field that is either perpendicular or parallel to UP ×B (left and right columns, respectively). The blue, orange, and dashed black lines respectively correspond to the first, second, and third terms in Equation 10. The thick, gray line shows the sum of all three terms, and the dotted gray line shows the sum when the third term ($\int \sigma_P |\boldsymbol{u}_n \times \boldsymbol{B}|^2\, dh$) is replaced with the approximation $\Sigma_P\, |\boldsymbol{U}_P \times \boldsymbol{B}|^2$. Note that the range of both Joule heating values and electric field values in the bottom row is larger than in the top row.*

We have also added the following paragraphs in the discussion section of the revised manuscript:

> *To further illustrate how the neutral wind affects height-integrated Joule heating, each panel in Figure 5 plots the three terms in Equation 10 as a function of a hypothetical electric field, assuming the electric field is either perpendicular or parallel to* $\mathbf{U}_P \times \mathbf{B}$ *(left and right columns, respectively). The values for the neutral wind and Pedersen conductance are taken from the Super Soaker and Auroral Jets campaigns, corresponding respectively to quiet (top row) and active (bottom row) geomagnetic conditions. The total Joule heating (thick gray line) is also*

plotted in each panel, along with the approximate total Joule heating (dotted gray line) given by replacing the third term $\int \sigma_P |\mathbf{u}_n \times \mathbf{B}|^2\, dh$ with the approximation $\Sigma_P\, |\boldsymbol{U}_P \times \boldsymbol{B}|^2$, as in Inequality 11. Electric field values in the top row vary from -10 to 10 mV/m, and from -50 to 50 mV/m in the lower row, corresponding to typical ranges during geomagnetically quiet and active periods (e.g., Walach et al, 2021).

When the electric field is perpendicular to $\mathbf{U}_P \times \mathbf{B}$ (Figures 5a and 5c), the second term (orange line) is zero by definition. In this case, the total Joule heating $W_J = \Sigma_P\, |\mathbf{E}|^2 + \int \sigma_P |\mathbf{u}_n \times \mathbf{B}|^2\, dh$. The contribution of the neutral winds is to shift the parabola representing Joule heating upward. That is, the neutral winds make a constant and non-negative contribution to the overall Joule heating. The difference between the true total Joule heating and the approximate total Joule heating, dictated by Inequality 11, can be seen by comparing the thick gray line and dotted gray line that respectively represent them.

When the electric field is parallel to $\mathbf{U}_P \times \mathbf{B}$ (Figures 5b and 5d) the situation is more complicated, since the second term offsets total Joule heating via a linear dependence on the electric field. The two cases shown in these panels illustrate that the vertex, or minimum, is shifted horizontally such that it is located at $\mathbf{E} = -\mathbf{U}_P \times \mathbf{B}$. The minimum Joule heating value would be zero if the third term $\int \sigma_P |\mathbf{u}_n \times \mathbf{B}|^2\, dh$ were equal to the approximation $\Sigma_P\, |\boldsymbol{U}_P \times \boldsymbol{B}|^2$. It is precisely the difference between the true term and its approximation that causes true Joule heating (thick gray line) to be everywhere greater than zero, in agreement with Inequality 11. When the third term is replaced by the approximation $\Sigma_P\, |\boldsymbol{U}_P \times \boldsymbol{B}|^2$, the minimum of the approximate total Joule heating (dotted gray lines) is zero as expected.

Two additional general conclusions can be drawn from Figure 5: (1) During quiet geomagnetic conditions (top row), the neutral wind influences total Joule heating via both its orientation with respect to the convection electric field (second term, horizontal shifting of the parabola) and its magnitude (third term, vertical shifting). (2) During active conditions (bottom row), the magnitude of the neutral wind is likely less important, with its primary influence being to reduce or enhance the the total Joule heating. This can most easily be seen in Figure 5 as the difference between the parabola that defines the total Joule heating (thick gray line) and the parabola that defines the first term (blue line).

Some estimate of how this affects the Joule heating estimate quantitatively would be appreciated, and I'd suggest stating this in the abstract instead of the sentence in lines 14-17, which seems a bit vague.

- We agree that the lines the referee has pointed to were somewhat vague. In the revised manuscript starting on line 14 we have replaced them with the following lines, which refer to the new Figure 5 and text describing it in the revised discussion:

  We demonstrate how during geomagnetically quiet periods both the magnitude and direction of the neutral wind may influence total Joule heating, while during active periods the neutral wind influences total Joule heating primarily via its orientation relative to the plasma convection.

We thank the reviewer for their thorough evaluation of and constructive feedback on our manuscript. We have made several changes below that we believe address the reviewer's commentary and improve the manuscript. These changes are summarized in this letter, along with specific responses to the reviewer's comments. Below the reviewer's comments our response is shown in blue. Modifications and/or additions to the manuscript are shown in *italics*.

Reviewer #2 Evaluation

This paper presents an investigation of how the neutral wind is approximated in height-integrated ionospheric electrodynamics in commonly used expressions for Joule heating. It is found experimentally from rocket flights combined with ISR measurements that the thermospheric winds at the altitude at which the Pedersen conductivity peaks is the best proxy for the thermospheric wind term in height-integrated, high-latitude electrodynamics, and that assuming zero winds is a better approximation than using winds at other altitudes as proxies. It is also found that the relative error associated with the term that depends exclusively on the winds decreases with increasing geomagnetic activity.

The paper is well written and at the forefront of research, and we recommend publication after the following minor comments are considered:

The title seems a bit general; would the authors consider something that is more closely associated with the findings and scope of this paper? e.g., "Representation of weighted neutral winds in high-latitude, height-integrated ionospheric electrodynamics", or something similar (this is just a recommendation; the authors are welcome to keep the existing title if they feel it is more appropriate).

- We appreciate the reviewer's taking the time to consider whether another title might be more suitable. We agree that the title is somewhat general. The first author feels that it is appropriate, since it reflects the question that led to this study being carried out. For this reason, we prefer to retain the original title.

L34: The more recent study by Baloukidis et al. (2023) could be referenced here, as an assessment of the contribution of neutral winds on Joule heating statistically in EISCAT and TIE-GCM.

- We have cited this study as follows in the revised manuscript:

> *Some notable experimental investigations of Equations 1 and 2 include that of Thayer (1998) and those based on the Joule II suborbital sounding rocket campaign (Sangalli et al., 2009; Burchill et al., 2012), while others have focused on experiment-model comparisons, modeling, or theoretical aspects (e.g., Baloukidis et al., 2023; Song et al., 2005; Strangeway, 2012).*

L49: "which is typically left undefined and most often simply taken to be zero" —> could the authors provide examples of studies that assume either of the two cases?

- Excellent point, thank you for this suggestion. We have added a number of citations on the relevant line of the revised manuscript, as follows:

> *Most often the wind is "neglected", which is generally equivalent to assuming it is zero in Earth's rotating frame of reference (e.g., Amm, 1995; Amm et al., 2008; Vanhamäki and Juusola, 2018; Matsuo, 2020; Weimer and Edwards, 2021).*

L64: "To our knowledge, all published studies in which conductance distributions have been estimated experimentally via Equations 5–6 have assumed U = 0" —> same as above, could the authors give some examples?

- Here we failed to specify these studies, thank you for catching this. We have revised this to read as follows starting on line 68 in the revised manuscript:

> *To our knowledge, all published studies in which conductance distributions have been estimated experimentally via Equations 5–6 have assumed **U** = 0 (Amm, 1995; Amm et al., 2015; Weimer and Edwards, 2021; Hatch et al., 2024).*

L55: Since Jper and Eperp are defined in relation to the magnetic field, isn't it more general and correct representation to use b^ instead of r^? It is understood that in the context of the discussion above these are the equivalent, since B is assumed to be radial.

- We agree with the reviewer. We now refer to b^ in the revised manuscript.

L96: "or when the neutral wind u does not vary with altitude. As noted in the Introduction, these idealized conditions are virtually never manifest in measured altitude profiles of the conductivities and neutral winds..."

Attached in the figure in the supplement pdf there is an example of neutral winds that are fairly constant with altitude (these are from the JOULE-1 rocket, PhD thesis of Laureline Sangalli, 2009). Could the authors comment? If these measurements are considered accurate and valid, perhaps the above statement ("virtually never manifest") could be revised.

- We have rewritten the latter sentence so that it reads as follows in the revised manuscript:

> *As noted in the Introduction, these idealized conditions are rarely manifest in measured altitude profiles of the conductivities and neutral winds (e.g., Larsen, 2002).*

L10: what is the meaning of the double vertical line in || u x B || in equation (10), compared to the single vertical line in |Eperp +uxB| ?

- Thank you for catching this. There is no distinction; but was simply inconsistent use of notation on our part. :) We have replaced the double vertical lines with single vertical lines in the revised manuscript.

Going one step back in this analysis: a main assumption of this work is the condition of steady-state stress balance between Lorentz and collisional drag forces. How accurate do the authors think that the assumption is, and how confident are we that this applies in particular in the regions where σP maximizes, or in the 110-130 km altitude range? What are the implications to this work if this assumption does not hold true, in particular during active times? Could the authors add a comment in the discussions on this topic?

- This is a great question, and we agree it would be sensible to add commentary on this point to the discussion. We have added the following paragraphs starting on line 202 of the revised manuscript:

> *This study aims to clear up misconceptions about how to incorporate thermospheric winds, which are sparsely sampled, and highly variable and structured in altitude, into IT data assimilation techniques that rely on 2D uniform slabs to describe IT electrodynamics. The assumption of a steady-state force balance between Lorentz and collisional drag forces is*

central to the expressions for the ionospheric Ohm's law and Joule heating we have used. The accuracy of assumed steady-state stress balance can be assessed by comparing the ion inertial term to the collisional drag term in the ion momentum equation, i.e., by the ratio τ_{relax}/τ_{var}, where $\tau_{relax} \sim \nu_{in}^{-1}$ is the ion–neutral relaxation time and τ_{var} is the characteristic timescale of variability. In the 110–130 km altitude range ν_{in} is large, making τ_{relax} of order ~0.01–0.1 s; here inertial effects become significant only for variability on comparable sub-second timescales. Even under highly active conditions, typical electrodynamic variability occurs on timescales of seconds to minutes, implying that the inertial term is smaller than the drag term by at least an order of magnitude.

Even for fluctuations at several Hz (representative of extreme Alfvénic forcing), the inertial contribution would remain ≲10% of the drag term, and substantially smaller for more typical variability. Other neglected terms, such as pressure gradients and gravity, are smaller still (~10^{-4} of the drag term). Therefore, in the altitude range where the conductivities maximize, the steady-state balance between Lorentz and collisional drag forces is expected to be accurate to within a few tens of percent under extreme conditions, and considerably more accurate under typical conditions. Thus while transient departures from steady state may occur during highly dynamic events or at very small spatial scales, the quasi-steady approximation we have used is generally robust for describing high-latitude electrodynamics in this altitude range (e.g., Vasyliūnas, 2012).