We thank the reviewer for their thorough evaluation of and constructive feedback on our manuscript. We propose 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 propose to cite 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 propose adding 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 will revise this to read as follows in the revised manuscript:

> *To our knowledge, all published studies in which conductance distributions have been estimated experimentally via Equations 5–6 have assumed $\boldsymbol{U} = 0$ (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 will 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 will rewrite 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 will replace 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 σ_P 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 will add the following paragraph on lines 204–212 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 ($\sim 10^{-4}$ *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).*