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).

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.

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?

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?

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.

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 below 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.

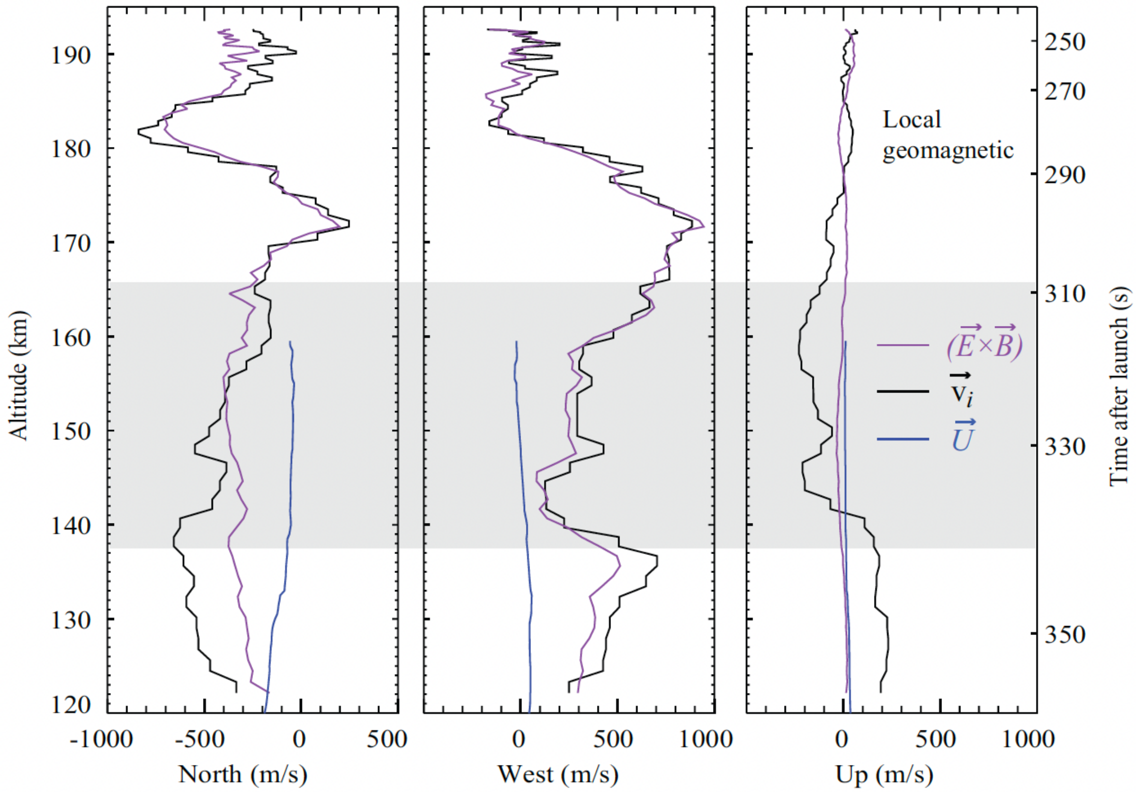

Figure 4.18: Comparison between ion drift velocity components $\vec{v}_i$ (solid black), $\vec{E} \times \vec{B}$ drift velocity components (purple line) and the neutral wind velocity components $\vec{U}$ (blue line) in the local geomagnetic frame for the flight downleg. The ion velocity $\vec{v}_i$ is calibrated against $\vec{E} \times \vec{B}$ during the descent over the altitude ranges 186-191 km. The neutral wind velocity $\vec{U}$ is obtained from TMA trails.

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

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?