The paper *Does high-latitude ionospheric electrodynamics exhibit hemispheric mirror symmetry?* provides a comprehensive perspective over high-latitude ionospheric electrodynamics based on almost 9 years of Swarm data. The paper brings a significant contribution to the field and is certainly suitable for publication. Before that, however, perhaps there is room to optimize the transmission of the paper's message to the reader.

The (very) comprehensive character of the paper is both a merit and also an issue. Its substance could easily fill three papers, I would say, focused as follows and, piecewise, easier to absorb:

- One paper on the Hi-C convection model, perhaps including more details on the math (Section 3) in a less dense presentation;
- Another paper on combining Hi-C with AMPS to produce Swipe, and the resulting maps on electromagnetic work and conductance, W, Σ_P, Σ_H. This paper could also benefit from more discussion of the results.
- The (a)symmetry between northern and southern hemisphere could be the object of yet another paper, once again assisted possibly by more discussion.

As of now, the (a)symmetry 'paper' also gives the title of the full manuscript, while the visibility of the other two 'papers' is somewhat obscured. This might be regarded as a weakness, even if a rather uncommon one.

In the following I make a few comments on each of these three 'papers', then list a few more issues that may require the authors' attention. More comments on Hi-C, which feels also the more demanding.

1. The Hi-C 'paper'

a) The derivation of 2D maps from the 1D cross-track TII measurements may deserve some discussion beyond the math. To some extent, this has been done also before, by Lomidze et al. (2019), and was validated by comparison with Weimer (2005). However, Lomidze et al. (2019) concentrated on the cross-track component, whereas here both components of the convection are derived. My understanding is that the 'jump' from 1D individual measurements to 2D statistical results is related essentially to the potential nature of the electric field, that prevails most of the time, and then convection is dominated by electric drift. But I wonder if the 1D character of the measurement does not still have some impact, in particular on the accuracy / error margin of the results (see also 4e). While the error margin is beyond the scope of the paper (see also point 3), it may still be worth to comment on this matter. For example, when the convection map shows plasma velocity mainly in E-W, cross-track direction, I expect this result is more accurate than plasma velocity shown by convection map mainly in N-S, along-track direction. To some extent, this reminds me the SuperDARN maps, where the model is based on solar wind parameters, though one expects better accuracy around the radar measurement points.

b) The Modified Apex coordinates, MA-110, appear to play an important role in the formalism, taking care, e.g., of the mapping from Swarm altitude to ionosphere (L180–181, L221–227) or the distortions of the magnetic field (L193–194, L575–576). For details regarding MA-110 the reader is referred to the paper by Richmond (1995). While this is in principle fine, it would be good to provide more clarifications, starting with the definition of the apex altitude (L202). Other features that could be detailed a bit are the way MA-110 takes care of i) mapping and ii) magnetic field distortions (mentioned above), iii) the possible bias of the SH model (L190–194), iv) how strongly non-orthogonal are ($\mathbf{d}_1$, $\mathbf{d}_2$ – L197) and ($\mathbf{e}_1$, $\mathbf{e}_2$ – L207) (both pointing roughly in the same directions), v) what is roughly the difference between the two and the standard spherical system (L223–224) (the energy argument at L237 suggests perhaps less than 1^o?), vi) some brief explanation of the difference between 44^o QD latitude (L166) and 47^o MA-110 (L250, L256) (is 47^o MA-110 just the average of 44^o and 50^o QD, L251? how significant is actually the difference between QD and MA-110?).

c) The decimation at L168-169 is not explained (together with the related limitation to scales larger than some 40 km). Is this because of further matching with AMPS?

d) The two paras at the end of Section 6, L556-569, are quite helpful to understand the math, but they might fit better to Section 3, where the math is done (perhaps in Section 3.2?). Those paras are quite specific, they do not seem to belong to the Discussion section.

2. The Swipe 'paper'

a) I think an important issue here is the error margin. While the authors eliminate problematic regions by asking the Hall and Pedersen conductance to be positive (with more constraint on Pedersen, via the threshold of 0.5 mW/m^2 in Eq. 37), the error margin is deferred to another study (L383–386). Nonetheless, a rough estimate of the uncertainty of W_N and W_S is provided at L436-437. Could this estimate be briefly explained? And perhaps similar estimates could be provided and tentatively discussed for other quantities, like the conductances? Based on 'common sense' knowledge, conductances around a couple of mho and less are more and more uncertain, the lower the conductance is. On the other hand, low conductance areas are actually quite broad and can make a significant contribution to Joule heating, which is a major driver of ionospheric electrodynamics. More discussion on the error margin seems appropriate, even if the actual (quantitative) solution is beyond the scope.

b) Related to this matter, the threshold of 0.5 mW/m^2 for W is explained at L379–382 based on typical values of the constituents – neutral velocity, magnetic field, and sheet current – together with a qualitative remark on apparition of *very sharp gradients* in the conductance below this threshold. Given the importance of this threshold for the low conductance areas and for the validation of the results (L138–139), it would be nice to elaborate a bit and quantify roughly the *very sharp gradients*.

3. The (a)symmetry 'paper'

a) In the literature one can find two different perspectives on the (a)symmetry between northern and southern hemispheres: Papers of the sort here, that look at the northern and southern hemisphere under similar conditions of tilt angle and IMF By, as well as papers that concentrate on the instantaneous asymmetry – driven, to a large extent, by the different conductance between the summer and winter hemisphere (and also by the tilt angle and By, whose values are not mirrored for such studies). Judging just by title, one could question what is the perspective here, before the matter becomes clear in the text. It may still be worth to comment a bit on these two complementary facets of the (a)symmetry.

4. Other issues

a) L153–154: This sentence is correct, but the association between the neutral wind and the definition of the Poynting flux (Eq. 14), rooted in the energy conservation Poynting theorem, may drive some confusion.

b) L155: Perhaps *Methodology and data for Hi-C*? This is, indeed, the core, but methodology includes also combination with AMPS, and then exploring (a)symmetry in all quantities (the three 'papers'…).

c) L156 – 158: Perhaps say simply that \hat{y} = \hat{x} \times \hat{r} / |\hat{x} \times \hat{r}| ?

d) Eq. 22: The tilt angle is not under sin or cos, like the clock angle. Is this because the tilt angle is (rather) small?

e) L304–305: Please explain briefly the origin of the ill-condition. Can this be related, at least to some extent, to the 1D measurements (point 2a above)?

f) L312–313: Not sure I understand: sectorial resolution, associated with M, is not the same with zonal (or longitudinal)? And N is not associated with latitudinal resolution, rather than zonal?

g) L372: Could you describe / illustrate briefly the differences between Hi-C and AMPS?

h) Eqs. 37 indicate a difference in the treatment of Σ_P and Σ_H, in that for Σ_P some margin is considered above zero (via the 0.5 mW/m^2 threshold). Please comment a bit on this difference, how comes that no margin is needed for Σ_H?

i) L392–394: This appears to hold in particular for local winter.

j) L414–415: *neutral wind field corotating with the Earth* is a bit confusing. Strictly, this means no neutral wind, whereas what is likely meant is that the neutral wind has the same direction as the Earth rotation.

k) L423–424: Any comment on the dominant direction of the neutral wind? (considering also the previous para)
l) L451 and L480: The Supporting Information is missing (Figures S1–S6).
m) L484–486: This suggests that hot spots might be related to IMF By (?).

5. Typos and alike

L101 and L129: Define the LHS and RHS acronyms; L181 and Fig. 1: The black lines are hardly visible, change black to some color (?). Add scales to the distributions (?); L208: Laundal et al. (2018) useD; L224, L225: EquationS 16, 18; L312: sectori**a**l, lower than than; L341: If the average CPCP is 51 kV for both NH and SH, it cannot be 3% greater in the SH, as stated in the caption of Fig. 2; L355: similar -> more or less similar (?); L362: contours -> colored contours (?); Figs. 6–8: The black line and color contours can be compared easily in terms of shape, less easily in terms of value. The integral values at the top right corners help; L409–410: Delete from ‘as mentioned…’ to the end of the sentence. L439: Regardless of B_T (?); L460: By -> IMF By; L461: IMF By -> IMF Bz; L474: distribution OF; L516: examines (?), mirror asymmetry -> mirror symmetry; L522: These studies are of relevance to this study; L537-538: Weimer and Edwards (2021) -> WE21; L584: field field; L618: HH -> HT (?).