Response to Reviewer #3 (Octav Marghitu)

RC3: 'Comment on egusphere-2023-2920', Octav Marghitu, 17 Jan 2024

We thank the reviewer for their careful reading of our manuscript and their comments and suggestions. Below the reviewer's comments are shown in **black**, and our responses are shown in **blue**.

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.

- We agree with the reviewer that this study is outside the norm in terms of length (also indirectly hinted at by the second reviewer), and we agree that our choosing to squeeze all three "papers" into a single study is not unproblematic. For us, this choice came down to a simple question of available bandwidth; we found it most expedient to write a single and perhaps unfortunately lengthy paper.

  We have attempted to partially make up for this weakness by making the model visible (announced so far on three different mailing lists), open source, and easily accessible (available on PyPI, GitHub, Swarm VirES, and Zenodo).

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.

- We confirm the reviewer’s understanding that the jump from 1D cross-track measurements to a derived 2D convection pattern relies heavily on the (assumed) potential nature of the electric field. We completely agree with the reviewer that this is an issue; it is the reason for the statement made on Line 255 of the original manuscript, where we state that “model convection in the north-south direction is … essentially unconstrained by measurements.”

  In the revised manuscript we will insert a new paragraph between the first and second paragraphs of the discussion section. Here we will cite the previous validation work done by Lomidze et al (2019) via comparison with Weimer (2005), and make the reviewer’s point that that validation effort concentrated on the cross-track component. We will then point out that we have not explicitly validated the model’s predicted along-track convection, that the Swarm orbits are such that the direction of the cross-track component does not vary much from orbit to orbit, and that the uncertainty in the along-track direction must of necessity be larger since it is not constrained by measurements. We will also refer to Lomidze et al (2021), Section 4.2.2, where they elaborate on the data quality of the along-track ion drift "proxy", and provide a clear rationale for excluding along-track ion drift.

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 (d 1 , d 2 – L197) and (e 1 , 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°?), vi) some brief explanation of the difference between 44° QD latitude (L166) and 47° MA-110 (L250, L256) (is 47° MA-110 just the average of 44° and 50° QD, L251? how significant is actually the difference between QD and MA-110?).

- We thank the reviewer for pointing out that many of these points simply were not explained in enough detail. In the revised manuscript we will explain that the representation of the electric field and convection coefficients in Equations 16 and 18, respectively, explicitly indicate that these coefficients are constant along field lines, regardless of whether the field lines themselves depart from dipolarity (we know that they do). We will also include some general comments about how strongly nonorthogonal the various basis vectors are (poleward of ±60° MA-110 latitude, the angles between $\mathbf{d}_1$ and $\mathbf{d}_2$ and $\mathbf{e}_1$ and $\mathbf{e}_2$ do not deviate from orthogonality by more than 15° in either hemisphere), and refer the reader to the review paper by Laundal and Richmond (2016), which explores many of these and related questions.

Regarding the difference between QD and MA-110 coordinates, QD coordinates are not strictly constant along field lines, but MA coordinates (including MA-110 coordinates) are. So QD coordinates are good for things that vary with height, such as the B-field of divergence-free currents, while MA coordinates are good for things that map along field lines, like E-fields, convection velocities, and FACs.

Laundal and Richmond (2016) also describe that in practice, the difference is essentially the reference height to which the dipole mapping is done. In QD coordinates, the reference height is the height of the point in question. In MA-110 coordinates, the reference height is 110 km. This means that if the point in question is at h=110 km, QD coordinates and MA-110 coordinates are identical. For points above this height, the QD latitude is equatorward of the MA-110 latitude, and vice versa for points below this height. (See Equation 43 in Laundal and Richmond, 2016). At Swarm altitudes of ~400–500 km, an MA-110 latitude of 47° corresponds to QD latitudes of ~45.4°–45.8°.

In the revised manuscript we will add a succinct summary of these additional points to the existing discussion, and make a clearer reference to the other documents where detailed information can be found, including the previously cited Richmond (1995) paper as well as Laundal and Richmond (2016, doi: 10.1007/s11214-016-0275-y), Emmert et al (2010, doi: 10.1029/2010JA015326), and Laundal et al (2016, doi: 10.1186/s40623-016-0518-x).

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?

- In the revised manuscript we will point out that this choice was made because we found that increasing the effective measurement cadence (i.e., including more measurements) did not visibly affect the shape of the potential patterns.

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.

- This is a fair point, in the revised manuscript we will move these paragraphs to Section 3.2.

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.

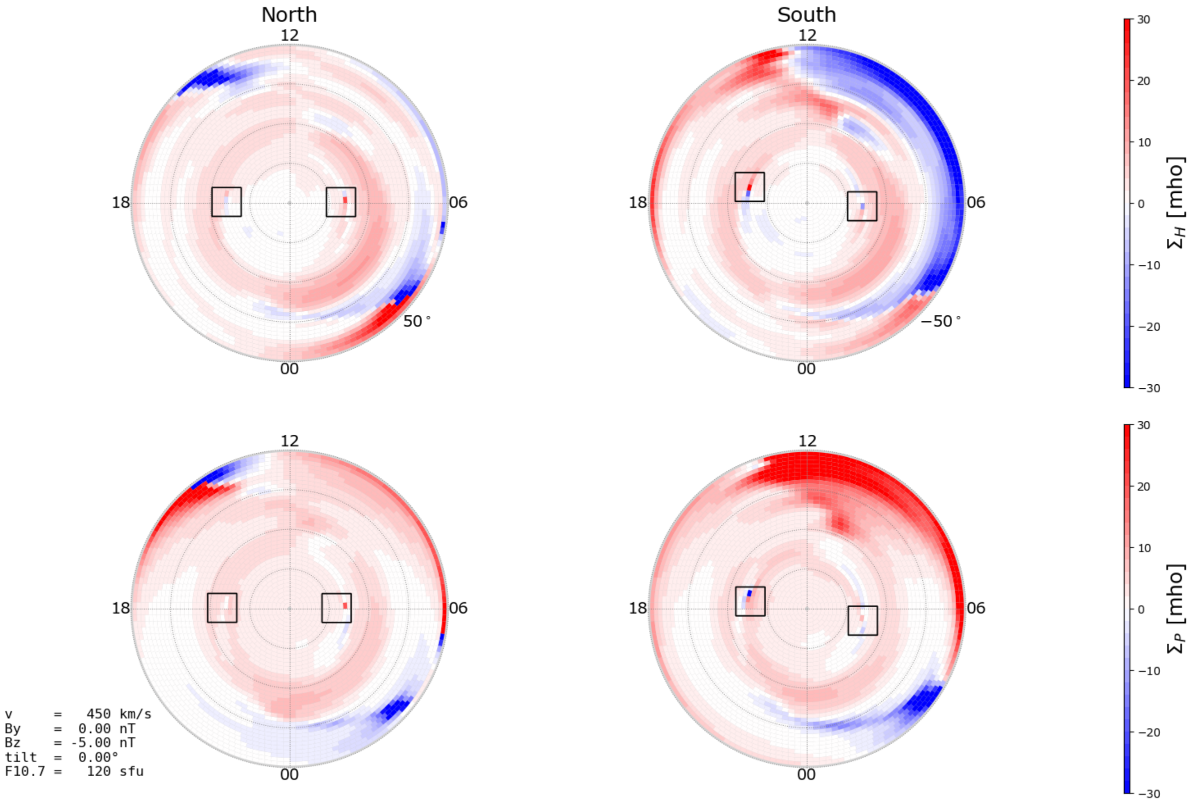
- We agree that the question of uncertainty is one that deserves serious attention, and as the reviewer notes, we think this is most properly addressed as part of a dedicated study. To our knowledge, no existing study involving development and demonstration of an empirical model of quantities such as ionospheric potential has yet taken up the question of uncertainty estimation.

  We agree that the statement that we made in the original manuscript was too vague to be of much use. In the revised manuscript, we will refer the reader to a second, very brief appendix where we state how one may use standard error propagation techniques together order-of-magnitude estimates of the uncertainties of the magnitudes of J, E, and the angle between them to arrive at a very rough estimate uncertainty of the hemispheric integrated EM work that turns out to be of order 0.1–1 GW.

  Regarding the uncertainty of the conductance: Since the conductance depends critically on having a proper estimate of the neutral winds and is generally much more poorly understood than the electromagnetic work and/or Joule heating, we feel it is best to defer discussion of the uncertainty of conductance estimates to a dedicated study. We hope the reviewer will understand that our desire is to do the subject justice, and we do not feel this is possible within the present manuscript given the (already large) scope of the study.

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.

- Rather than adding more text, we propose to add a note in the vicinity of the lines indicated by the reviewer where we state that we include a Supporting Information figure that shows typical distributions of the conductances when no screening criteria are applied, such as the following:



Here one can see, for example, locations indicated by black boxes where the conductances change by as much as 50 mho from one grid cell to the next. There are also many cells within which the conductance is negative.

Of course, the locations of the sharp gradients and negative conductances are not static, but vary with IMF orientation, solar wind speed, and such. We have not spent any significant amount of time trying to identify where the negative conductances or sharp gradients occur, since we know that our model is missing key information about the neutral winds.

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.

- We appreciate the reviewer's making this distinction between the types of studies that one encounters in the literature. In the revised manuscript we propose to make the following addition to the discussion, which relies heavily on the language that the reviewer has used (we thank the reviewer and will acknowledge their contribution in the acknowledgements):

  > In the literature one encounters different approaches to the topic of symmetry between the two hemispheres that may be roughly separated into two categories: those that examine asymmetries in the NH and SH under complementary conditions of tilt angle and IMF By,

and those that concentrate on instantaneous asymmetries that are driven, to a large extent, by differences in conductance between the summer and winter hemispheres (but also by tilt angle ψ and IMF B_y, whose values are not mirrored). This study belongs to the former category.

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.

- Good point, in the revised manuscript we will make the following change:

  The neutral wind $\mathbf{v}_n$ notably does not appear in Equation 14, as the Poynting flux is frame dependent and arises in connection with the well-known energy conservation (Poynting) theorem.

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'…).

- In the revised manuscript we will change the name of this section to "Methodology and data for Swarm Hi-C model" as suggested

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

- Thank you for the suggestion, we will revise the manuscript accordingly.

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?

- Precisely, this is because the tilt angle remains within a (relatively) narrow range of ~±30°.

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)?

- As acknowledged above, the fact that the measurements are 1D must have some influence on the derivation of the model. However, it is unlikely that the need for regularization is connected with the fact that the measurements are 1D. Without regularization the cost function would be entirely dependent on measurement-model misfit (first term on RHS of Equation 35). With such a simple cost function the norm of the model parameter vector **k** generally ends up being far too large, corresponding to overfitting or in the worst case numeric overflow.

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?

- We apologize for the possibly confusing choice of words here. In the revised manuscript we will write "That M is much less than N indicates that the longitudinal resolution of the model is much lower than than the latitudinal resolution."

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

- The difference between the models that we had in mind, which is implicitly referred to here but not explicitly stated, is that the perpendicular current vector **J** from AMPS and the electric field vector **E** from Swarm Hi-C are not in any way "co"-constrained to ensure that $\mathbf{J}\cdot\mathbf{E} > 0$ and $(\mathbf{J}\times\mathbf{E})\cdot\mathbf{b} > 0$, as required by the physics (NOTE: assuming the neutral wind is zero in the Earth's rotating frame of reference!). In the absence of information about the neutral wind, it seems impossible to make a meaningful comment about how the models might be different if they had been co-constrained.

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 ?

- The reason for imposing a different threshold is that we found the two thresholds correspond to different types of issues with the conductance distributions, as discussed on Lines 376–378: the 0.5 mW/m² criterion primarily addresses the issue of negative or unphysically large conductances or large conductance gradients within the polar cap and equatorward of ±60° MLat, while the $\Sigma_H >$ 0 mho requirement mostly addresses locations poleward of 70° where the Hall conductances may be negative (between -6 and -1 mho for the conditions we examined).

  In other words, these values were chosen on a heuristic basis, and we by no means intend to imply that they are somehow the "right" values (please see our comment to the second reviewer on this point). The simple fact of the matter is that for the conditions we examined, when we reduced the first threshold to 0 mW/m² or even 0.3 mW/m², we found that the Pedersen and Hall conductance distributions still evinced sharp gradients and/or negative or extremely large values. We chose the (approximate) minimum value necessary to mask regions with these problems, and found that this minimum value corresponds to what might be considered the typical contribution from a height-averaged neutral wind with a magnitude of 100 m/s and a current sheet density of 100 mA/m, as described on lines 379–382.

  We will add some explanatory remarks along these lines below Equation 37 in the revised manuscript.

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

- Thank you for your careful examination of these figures, we will add this comment on the relevant lines in the revised manuscript.

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.

- This is of course a question of frame of reference, such that if one is standing on Earth we do indeed mean $\mathbf{v}_n = 0$ (no neutral wind), whereas if one is looking down at the Earth in, say, a GSM frame of reference the neutral wind field rotates together with Earth.

  We thank the referee for pointing out that this may be a source of confusion, and in the revised manuscript we will change the phrase "reducing the electric field in the neutral wind frame" on Line 416 so that it reads "reducing the electric field in Earth's rotating frame of reference".

k) L423–424: Any comment on the dominant direction of the neutral wind? (considering also the previous para)

- We agree that this would be valuable to comment upon. Unfortunately we do not feel comfortable commenting on the dominant direction of the neutral wind, as the presence of neutral wind shears at the relevant altitudes greatly complicate determination of a dominant direction. As mentioned on Line 125, this is a topic that we are actively researching.

l) L451 and L480: The Supporting Information is missing (Figures S1–S6).

- Thank you (and the other reviewers) for catching this oversight on our part. We will be sure to include them during resubmission.

m) L484–486: This suggests that hot spots might be related to IMF By (?).

- This is a good point that we will include on this line in the revised manuscript.

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: sectorial, 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 (?).

- Thank you for catching all of these typos and small mistakes. We will correct all of them according to the referee's suggestion in the revised manuscript.