

Anonymous Referee #1 : angeo-2018-31

“Dynamics of the electrojet during intense magnetic disturbances” by Liudmila I. Gromova et al.

Interactive comment on Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2018-31>, 2018

We would like to thank the reviewer for the thorough evaluation of our paper. The authors appreciate the constructive comments and criticism. We have answered all comments by inserting our response behind the comment in italic and blue, and hope that our replies will clarify the critic's items. For convenience, we include the revised manuscript with corrections indicated in red.

General comments

The manuscript analyze high latitude ionospheric Hall currents derived from scalar magnetometer measurements from LEO orbit from the CHAMP satellite during 6 different geomagnetic storm periods. On the basis of these data, the estimated Hall electrojets are characterized in different MLT sectors, and interpreted as either the Eastward (EE), Westward (WE), or Poleward electrojet (PE), the latter occurring only on the dayside in vicinity of the cusp, also in an east/west direction.

The manuscript serve to characterize how the EE, WE, and PE vary in location (latitude) and strength in the different MLT sectors during the different storm phases. Possible relationships to geomagnetic and IMF/SW indices are explored through a regression and correlation analysis.

The results appear reasonable, however, the manuscript presently fails to place the results into the present state of knowledge in the field. One example is that much attention is focused on the latitudinal variability of the EE and WE, where the electrojets are mainly found to move equatorward in the storm main phase, compared to the initial and recovery phase. However, this result is not discussed together with the expanding/contracting nature of the open magnetosphere (e.g. Cowley and Lockwood 1992, Milan 2015), largely explaining why. This needs to be mentioned, and I think the manuscript would benefit from more rather focusing on the specific latitudes that the expansion reach (that they also mention), with relation to e.g. the SYM/H index and the Newell coupling function.

Again, for the discussion section, I would very much appreciate to see how the present results add to the earlier works cited in the introduction. Presently the discussion is largely void of references, and it is not at all clear what the present manuscript has specifically done that is different from the earlier works, other than the method.

Cowley, S. W. H., Lockwood, M. (1992). Excitation and decay of solar wind-driven flows in the magnetosphere-ionosphere system. *Annales Geophysicae*, 10(1–2), 103–115.

Milan, S. E. (2015). Sun et Lumière: Solar Wind-Magnetosphere Coupling as Deduced from Ionospheric Flows and Polar Auroras. In S. Cowley FRS, Stanley W. H. and Southwood, David and Mitton (Ed.), Magnetospheric Plasma Physics: The Impact of Jim Dungey's Research (Vol. 41, pp. 33-64). Springer International Publishing. <https://doi.org/10.1007/978-3-319-18359-6>

Specific comments

Title: I find the word “Characteristics” more suitable than “Dynamics” for the title. As the manuscript focus on the correlation of the location and strength of the electrojets deduced from a LEO satellite, with geomagnetic and SW/IMF variables, the study is more characterizing in nature, than describing dynamics, which is usually referred to as changes on shorter time scales than 90 min.

Here, we completely agree with the Referee and changed the Title accordingly.

Abstract: The extensive listing is unusual. The abstract might be too long for the journal. I would prefer a shorter summary of the method and findings.

The Abstract is now entirely redrafted, shortened, and we eliminated the extensive listing. We think that it now expresses essentially the main findings, next to a short description of the method.

Introduction: The introduction mention quite a few historic works on the characterization of ionospheric horizontal currents at high latitudes. Although this is highly relevant for the present manuscript, the authors some times fail to convey the implication of the work they describe. The introduction often reads more like a brief summary of the previous works, mainly mentioning what they have been measuring, and not putting it into broader context. In particular this is evident in the paragraph starting on line 98. I would suggest to remove this paragraph unless the authors can more directly state how this is relevant for the present study.

This paragraph (previously starting on line 98) has been removed.

I also have a similar concern with the paragraph starting at line 160. This is mostly a listing of what the referenced literature observe, no explanation of their result is provided. For the reader it would be very satisfactory to briefly explain their findings. Example: Why does the electrojets expand to lower latitudes during increased activity (the oval expands...)? Also, how the electrojets disrupt into space could be elaborated more on, if not removed.

This paragraph has been largely modified. Some sentences from other places in the former manuscript have been incorporated here, and we tried to extend the number of relevant references.

The same arguments goes for the paragraph starting at line 176. Please provide how their findings are relevant for the present manuscript. At the moment it only states that Wang et al investigated electrojet currents from space, which is not very informing.

This paragraph (formerly starting at line 176) provides the reader with an outline of the manuscript. The subsequent sections are briefly described and put into a logical order. We think that the paragraph is justified at this place at the end of the Introduction and therefore we kept it as it was.

Possibly you meant the preceding paragraph (formerly starting at line 170), which mention the paper of Wang et al. (2008). This work is quite close to our subject and the authors made use of the same CHAMP data set. They investigated Hall currents for two intense storm periods, but didn't consider the polar electrojet at all. We think that this paper is therefore of relevance for our study and kept this paragraph, though in a slightly modified version.

Lines 96-97: "Potential differences at cusp latitudes". In the present manuscript cusp latitudes is referred to as a point. To get a potential difference, a distance need to be involved. It is not obvious what is the distance in this case. I think you refer to the distance where dayside merging occur. To get around this, maybe an electric field, or velocity value in the east/west direction could be used?

In the present manuscript the cusp is not considered as a point, but as a distance. This distance is determined as the latitudinal interval of the existing polar electrojet (PE).

Lines 134-159: Suggest to merge this into one paragraph as both paragraphs are dealing with the same Ritter et al 2004 paper.

These two paragraphs have been merged and largely shortened. It is also partly shifted to the Method section, where it belongs to.

Data: Line 215: Need to define Φ (or Φ_{PC} as Newell calls it): Magnetic flux inside polar cap. The coupling function describe the change of this related to reconnection at the magnetopause, $d\Phi_{MP}/dt$. Together with the nightside reconnection rate ($d\Phi_N/dt$), this control the expansion/contraction of the polar cap ($d\Phi_{PC}/dt$). This is relevant for the present study as expansion/contractions of the polar cap lead to similar displacements of the electrojets. [Cowlet and Lockwood, 1992].

We reformulated the statements concerning formula (1), aiming at a more precise description as suggested by the Referee, and introduced the term Φ (or Φ_{PC} as Newell et al, 2007, call it) of open magnetic flux inside the polar cap, as well as the Index N (IndN) as a proxy of $d\Phi_{MP}/dt$.

Also relevant for this study is the time averaging of the solar wind data used to estimate IndN. Using a long interval (20-60 min) would likely produce a better correlation to e.g. AL, than using the instant 1 min values. How is this done in the present manuscript?

For the determination of all indices throughout this study we used time averaging intervals of one minute.

Method: A short introduction to the ionospheric currents at high latitudes would be appreciated, along the lines that we can decompose the currents into Hall, Pedersen, and Birkeland currents, all contributing to the field measured at LEO altitude, but mainly the Hall currents are detected on the ground as explained by Fukushima (1976). As no information regarding the electric field is provided in the manuscript (needed to define the directions of Hall and Pedersen currents), it should state something like "we assume Hall currents to be divergence free, i.e close entirely within the ionosphere,

while the Pedersen currents are curl free.” Laundal et al. 2018 showed (see their Figure 14) that during summer conditions, which is the case in for the present manuscript, the divergence free and curl free ionospheric currents mainly represented the Hall and Pedersen currents, respectively.

We find this a very good idea and give now at the beginning of this section a short introduction into high latitude currents and the possibilities of their decomposition, as proposed by the Reviewer.

Lines 241-242: Suggest that you write that only one of the events in Table 1 is presented in the paper, and the rest are included in the Appendix. (a rewrite of the present sentence)

Done.

Line 248: which is the symmetric part of the ring current. As you write on line 252, the ring current is in general asymmetric.

Corrected.

Lines 255-260: I think these sentences belong to the introduction. Is this connection of the EE to the PRC the same as described in lines 166-168? If so, it could be moved into that location to give better background information.

We shifted this paragraph to a suitable place in the Introduction and reformulated it slightly.

Fukushima, N.: Generalized theorem for no ground magnetic effect of vertical currents connected with Pedersen currents in the uniform-conductivity ionosphere, Rept. Ionos. Space Res. Japan, 30, 35–40, 1976

Laundal, K. M., Finlay, C. C., Olsen, N., Reistad, J. P. (2018). Solar wind and seasonal influence on ionospheric currents from Swarm and CHAMP measurements. Journal of Geophysical Research: Space Physics, 123. <https://doi.org/10.1029/2018JA025387>

Events: Line 265: Is this number referring to the SYM/H index? That should be explained before presenting the values. Also a reference to the figure showing SYM/H and the orbits should appear here. Further, from looking at the SYM/H data at the WDC Kyoto webpage, it is provided as integer numbers. How does the decimal number arise?

The decimal numbers result from averaging the 1-min integer SYM/H values over the interval of observations during the polar cap crossing, as indicated by closely related dashed vertical lines with the abbreviated orbital number in between.

Figure 1 is never introduced. This need to be improved. It would also be of interest to show the AU index in this figure as the paper discusses measurements of the EE during this interval.

Fig. 1 is now introduced in the text and, yes, the numbers are referring to the SYM/H index, as it is now explained properly. Further we have added the AU index into the plot as recommended.

Figure 2: No label or units on the y-axes. Accordingly → respectively, and put to the end of the sentence.

The Figure's labeling has been added, and the caption text corrected. This has been done likewise for all the corresponding figures in the Appendix.

Lines 282-286: It is not obvious if the method allows for the determination of the direction of the Hall currents. Is an east/west direction assumed? Sign information is provided in Figure 2, however, the text does not explain what direction positive currents represent (only the figure caption does).

The Hall current intensities, shown in Fig. 2, are now completed with an information about their direction (as given already in the caption of Fig. 2). And, yes, the single-spacecraft method has to make assumptions about the East-West orientation of (infinite) current sheets, which is now expressed accordingly in the text.

Section 4.1: Although this subsection is devoted to show variations of the observed Hall currents with relation to the SYM/H index, the EE and WE is much discussed. I find it therefore appropriate to mention the expected closer relationship of the observations to the AU and AL index, which is regarded as a measure of the auroral electrojet from the ground.

A corresponding sentence about the AU and AL indices as measures of the electrojet intensities from the ground is now formulated here.

Lines 378-380: Despite showing the AL index, the authors have not discussed the trend of the measurements with respect to this index.

We added this statement: "The correlations with the ground-based AL index are similar to the ASYM/H behavior."

Lines 380-381: For the reader to judge this statement, a measure of the dependency should be provided, e.g. a correlation coefficient. Although mentioned in the text, this dependency is really hard to see from looking at Figures 1 and 2. This become much more evindent in the later figures where the correlation analysis is performed. This conclusion should therefore not be made at this point.

This conclusion is kept here, but we added a reference to the Tables with the correlation coefficients later in the manuscript.

Section 4.2: From line 363 the manuscript summarizes the observed trends. This should therefore not be a part of subsection 4.2, but rater ha separate subsection (if the summary is really needed), or all the subsections could be combined into one section.

A new subsection 4.3 Summary of the observations has been created.

Discussion: The present discussion does a poor job in relating the observational findings (which is the majority of what is presented in this section) to the state of the art knowledge of the characteristics of the high latitude electrojets. Are the present findings mostly in agreement with the present knowledge, and where does it represent new findings/analysis that could lead to new knowledge? To my knowledge, a possible link between the asymmetric ring current and IMF By has not been established earlier.

That is one piece of information that I think the present manuscript is hinting towards.

Thank you very much for this assessment. We will emphasize this result more clearly.

Figure 3: The caption should indicate what the red and blue points refer to.

Added to the caption of Fig. 3.

Lines 437-439: If the PE variations during quiet intervals are not due to IMF By, what could it be then?
I don't understand this sentence.

We reformulated this sentence.

Lines 452-489: In my opinion, the term “solar wind electric field penetration” is a misleading term/concept. My take on this is that reconnection opens field-lines on the dayside. During By conditions, these newly opened field lines has a strong bend leading to tension force in the east/west direction. Field-aligned currents facilitate the energy needed to reconfigure these field lines (there will be a drag in the ionosphere due to collisions), and associated east/west plasma flows are seen in the ionosphere, together with the field aligned currents and their associated horizontal currents in the ionosphere, whose magnitude is determined by the ionospheric conductivity. On the ground one observe mainly (at least during summer) the divergence free Hall currents related to this, your PE. In this description it becomes meaningless (in my opinion) to say that the solar wind electric field (originating from the Lorentz transformation) penetrate into the high latitude ionosphere to drive currents. Hence, in order to foster understanding, to distinguish cause and effect, being one of the most fundamental tasks for physicists, I think the “electric field penetration” is a bad term for this application. That being said, I don't see how these paragraphs really fits into the context of characterizing the PE. I therefore suggest to remove these paragraphs.

Yes, you are right, the term “solar wind electric field penetration” and the physical concept behind it is misleading. We omitted these paragraphs completely in the revised manuscript.

Lines 495-499: This result has the strongest correlation. Could the authors try to elaborate on possible explanations for this result? The ASYM/H describe to some extent an asymmetry in the inner magnetosphere. The PE arises, as the manuscript shows, due to IMF By, which is maybe the most important parameter affecting the asymmetry in the magnetosphere. From simple linear correlation of |IMF By| and ASYM/H one get $r=0.29$, while with SYM/H one get $r=-0.16$ (using 5 min OMNI data from 1981-2016). This supports the results in the manuscript, and suggests that IMF By could be a source of asymmetry in the ring current.

We reformulated and added some sentences.

Line 540: What is meant by “changing SYM/H” index? Is there a criterion in the data selection that the SYM/H should drop by e.g. a fixed amount? If so, how what is the reason for that?

This sentence is corrected to “...show cases of AE appearance with a change of the SYM/H index”. No, there is no such criterion; the former sentence was capable for being misunderstood. Hopefully it is more clear now.

Line 551: Strictly speaking, IndN does not characterize a current function, but is designed to be proportional to the amount of opened magnetic flux per unit time, which in turn affect the current systems.

The sentence in question is now reformulated to address cause and effect more clearly.

Line 563: Need to introduce Figure 5.

Introduced now.

Line 282-583: The mentioned trend of Mlat vs IndN is not obvious from Figure 6d.

This is reformulated now with more cautiousness.

Lines 595-596: I disagree. As they should in principle be the same, the large observed spread (Figure 7c) give insight to the difference in technique used to probe the WE (CHAMP and AL index). I was surprised the relationship was not clearer. Could some of this variation be due to difference in sampling/averaging window?

The Discussion section, now entitled “Correlation analyses and discussion”, has largely been redesigned and reformulated.

Conclusion: Similar to my comment regarding the abstract, I find the conclusion section very long. It is a very comprehensive listing of the detailed discussion provided earlier. I would have appreciated if the authors would have rather summarized what they believe is the most important and significant among their findings.

The Conclusions have been radically shorten and focused on the main findings.

Lines 606-712: This has not been mentioned before and should therefore be moved to the discussion

This long part of the former manuscript has been shifted from the Conclusions to the Discussion section, while the Conclusions section has been completely redrafted and considerably shortened. The former subsections about Hall current behavior form now a subsection within the Discussion.

Minor comments

We made all the minor corrections/changes according to the Referee’s recommendations. Thank you for pinpointing all these details.

Line 10: IndN needs to be defines. Suggest that you only refer to it as the Newell et al coupling function in the abstract.

Line 24: Should this be 80 degrees MLAT, to be consistent with the equatorward motion mentioned?

Line 28: Need to define acronyms in abstract. Or maybe just refer to it as ring current in the abstract.

Lines 74-77: What level of confidence does this refer to? Could a reference or a name of the formula be provided?

Line 111: compilation → comparison

Line 112: demonstrates

Line 131-132: From looking at Figure 26 in Sandholt et al 2004, these fast convection channels are located on open field lines, hence poleward of the OCB and not at the OCB as the manuscript states.

Lines 134, 139, and 153, and throughout the text: The use of “density” referring to the strength of the electrojets is sometimes unfortunate in my opinion. It can easily be mixed with how dense the current lines in the model is placed. Consider to change it to “strength”.

We changed this term “density” throughout the manuscript to either “strength” or “intensity” in accordance with the respective context.

Line 151: Delete “currents”

Line 160: EISCAT is an incoherent scatter radar network, not a magnetometer network

“EISCAT” was till 1993 the name of a magnetometer chain; the new name “IMAGE” was received only in 1997.

Line 180: electromagnetic

Line 207: 12 observatories

Line 219: BT is actually only constricted from the y and z components of IMF

Line 220: One should rather use $\theta = \arctan2(B_y, B_z)$, which is a common algorithm that handles the different signs of B_y and B_z properly, compared to the mathematical function $\arctan()$.

Line 233: delete “there”

Line 236: density → level

Line 245: Suggest to delete “mean”

Line 282: density → intensity

Line 331: delete “there”

Line 395: Transferred is not a good word here.

Lines 405-406: Suggest to replace “occurrence and dynamics” with “strength” and “domains” with “processes”

Line 409: Delete “from geomagnetic activity researches”

Lines 411-412: This sentence is not related to any of the following text.

Line 415: add “all the” before 6

Line 414: Add something like “versus IMF and geomagnetic indices”

Lines 419-420: I don't understand this sentence.

Line 511: Suggest to rewrite: “. . . correlation of IndN with PE Mlat ($r=0.52$).”

Lines 517-518: Suggest to delete this sentence.

Line 528: Suggest to add something like “from the individual orbits”. As I read these figures, each dot correspond to a specific orbit within the MLT range, among the 6 events studied.