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Interactive comment on "Dynamics of the electrojet during intense magnetic disturbances" *by* Liudmila I. Gromova et al.

Anonymous Referee #1

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General comments

The manuscript analyze high latitude ionospheric Hall currents derived from scalar magnetometer meaurements 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 strom phases. Possible relationships to geomagnetic and IMF/SW indices are explored through a regression and correlation analysis.

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Discussion paper

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 in 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-ionophere 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

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Discussion paper

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.

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.

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. 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. 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.

Lines 96-97: “Potential differences at cusp latitudes”. In the present manuscript cusp latitudes is refereed 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?

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

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].

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?

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.

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)

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

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.

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?

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.

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

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).

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.

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

Lines380-381: 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.

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.

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.

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

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.

Lines 452-489: In my opinion, the term "solar wind electric field penetration" is a mis-

leading term/concept. My take on this is that reconnection opens field-lines on the day-side. 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 ionsphere due to collissions), 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, who's 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.

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 paramater 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.

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?

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.

Line 563: Need to introduce Figure 5.

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

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 is in sampling/averaging window?

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.

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

Minor comments

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, theese 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" refering to the strength of the electrojets is sometimes unfortunate in my opinion. It can easily me mixed with how dense the current lines in the model is placed. Consider to change it to "strength".

Line 151: Delete "currents"

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

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(By,Bz), which is a common algorithm that handle the different signs of By and Bz 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 sentece 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 dont 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.

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