Review comments on manuscript *A case study of the large scale travelling ionospheric disturbances in the Eastern Asian sector during the 2015 St. Patrick's Day geomagnetic storm* by Liu et al (2019).

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The current manuscript presents a detailed analysis of large scale travelling ionospheric disturbances which occurred during the March 17, 2015 geomagnetic storm. The authors combined data from multiple GNSS receiver networks to create 2-dimensional maps of total electron content perturbations from where azimuth, velocity and periods were determined. In addition, they made use of ionosondes and HF doppler radars, and in my view, have presented a very complete and comprehensive study especially focusing on the northern hemisphere mid-latitude region over the Asian region. They intentionally avoided discussing low latitude changes or TID related variations in this particular region (which I think they shouldn't have) as I will point out later in the comments. Nevertheless, this is an important contribution to the TIDs studies. In my view, the strength of this paper is two-fold

1. The utilisation of dense networks of diverse instrumentation (GNSS receivers, doppler radars and ionosondes) to bring out finer details of large scale TIDs during the March 17, 2015 geomagnetic storm.

2. The agreement of TID velocity estimated from 2-D TEC perturbation maps and HF doppler radar data. In fact the authors missed an opportunity to discuss this issue in detail and should be revised in the paper as it is an interesting one. Let me elaborate in more details here. From their Figure 3, they estimated the TID velocity to be 553 m/s (between 10:24-10:45 UT). Later in Figure 8, the estimated velocity from VTECP' maps (I assume it to be around the same time because the time of Vt and Cv was not actually clearly stated) was 578 ± 16m/s. Page 7, lines 36-37, they did comment that that '...Vt is in good agreement with the resultof 553 m/s derived from the Doppler observation'. Usually velocity values estimated from spaced instruments such as ionosondes and HF Doppler radars tend to be higher that the actual TIDs' velocities because it assumes "perfect equatorward propagation". If the spaced instruments (e.g., ionosondes or doppler radars are in 'perfect alignment' with the propagation of TIDs, then the velocities from the two methods can have a high degree of agreement. Taking a look at Figure 9 of the authors, their azimuth values point to this direction that the position of the HF dopper radars actually was in the direction of the TID's propagation. This may be the main reason why the velocity values from these two methods agree. I encourage the authors to add some discussion in this regard. Below are the references that talk directly to this second point

    - Afraimovich et al., (1998), GPS radio interferometry of travelling ionospheric disturbances, J. Atmos. Solar. Terr. Phys., 60, 1205-1223
    - Habarulema et al., (2013), Estimating the propagation characteristics of large-scale traveling ionospheric disturbances using ground-based and satellite data, J. Geophys. Res. Space Physics, 118, 7768-7782, doi:10.1002/2013JA018997

Below are detailed comments which may help the authors to improve their paper.

Abstract: Page 1, lines 13-15 which talk about first observation of LSTIDs in East Asian sector for the first time may not necessarily be entirely correct. I would like to point the authors to Habarulema et al., (2018)

which analysed LSTIDs during this storm period in the African, Asian and American regions. These authors reported LSTIDs in the Asian region between 0900-1200 UT reaching velocity values of over 800 m/s. Perhaps in the current paper, the authors have used more data and so their propagation parameters may be 'more accurate', but certainly this is not the first study over the Asian sector for this particular storm. Please re-word this accordingly.
Reference: **Habarulema et al., (2018)**: Storm time global observations of large-scale TIDs from ground-based and in situ satellite measurements. Journal of Geophysical Research: Space Physics, 123, 711-724, https://doi.org/10.1002/2017JA024510

Page 1, lines 17-19, the concept of negative and positive LSTID is not understandable. Usually TIDs are seen as periodic changes in VTECP' or electron density appearing like wave structures. As such, these structures would have 'troughs' and 'crests'. What are the authors calling negative LSTIDs?Is it where VTECP is negative? Shouldn't this be the 'troughs' of the wave or TID? Please check this and revise if necessary

Introduction: Page 2, lines 4-6, perhaps, the authors can talk about AGWs in general as they can also lead to MSTIDs? I think in the introduction, the authors missed critical papers which have done similar analysis for the March 17, 2015 storm. They include: **Borries et al., (2016)**: Multiple ionospheric perturbations during the Saint Patrick's Day storm 2015 in the European-African sector. Journal of Geophysical Research: Space Physics, 121, 11333-11345, https://doi.org/10.1002/2016JA023178; and **Ramsingh et al., (2016)**: Low-latitude ionosphere response to super geomagnetic storm of 17/18 March 2015: Results from a chain of ground-based observations over Indian sector. Journal of Geophysical Research: Space Physics, 120, 10864-10882, https://doi.org/10.1002/2015JA021509

Data and Methods: Page 4, lines 36-39 which discuss the ionosonde data that have been used in the study. Has this data been manually cross-checked to ensure that errornous ionograms were not used in the analysis and interpretation? This may be one of the errors associated with Figure 6 where the downward phase propagation did not manifest in a number of stations? I will comment on this later. For some reference about errors that could be in data due to wrongly scaling of ionograms, please see **Habarulema and Carelse (2016), I think their Figure 1?**: Long-term analysis between radio occultation and ionosonde peak electron density and height during geomagnetic storms, Geophys. Res. Lett., 43, 4106-4111, doi:10.1002/2016GL068944; and **Krankowski et al., (2011), Figures 4-5** : Ionospheric electron density observed by FORMOSAT-3/COSMIC over the European region and validated by ionosonde data, J. Geod., 85, 949-964

Observations: Page 5, lines 20-21, the text sounds misleading. The reader may go to Figure 1 looking for the 'disturbances which are observed successively' only to find the location of HF doppler radars.

Page 5, Line 23, the times indicated here are different from the times shown by arrows in Figure 3 of 10:24 UT and 10:45 UT?

Page 5, Lines 30-32, the authors should mention the limitation of this assumption to be valid when the AGW is in a perfectly equatorward direction and add references I mentioned in the opening statements

Page 5, Lines 33-34 are not clear. If the authors refer to the equation in section 2, then perhaps number it and refer to it here. Otherwise 'above' doesn't give appropriate guidance to the reader.

The motivation of transforming VTECP to VTECP' is not very clear to me. If the method used to estimate the background TEC values is consistently used, why would this be required? May be the VTECP values will be significantly small, but positive and negative perturbations should still come out of the 2-D maps?

Page 5, Line 39 and every where, Lime lines appear green? Not sure of the color. The issue which

requires more details is where the authors talk about 'values close to zero'. How close to zero? Is it possible to provide a range may be between -0.05 and 0.05?

Page 6, Lines 10-17, I suggest that the author have a look at the paper by **Pradipita et al., (2016)**: Interhemispheric propagation and interactions of auroral traveling ionospheric disturbances near the equator. Journal of Geophysical Research: Space Physics, 121, 2462-2474, https://doi.org/10.1002/2015JA022043

Page 6, Lines 18-27, considering Figure 5 where there is a temporal shift seen at 38 degrees North followed by 29 degrees North, have the authors considered investigating such LSTID to be originating from northern hemisphere and propagation towards the equator with possibility of crossing the equator towards the southern hemisphere?

Page 6, lines 28-44, In my view, ionosonde data and its interpretation should be given more attention than is done in the current version. If the uplift of virtual height (h') is due to the AGW which results into the TID that reaches ionospheric heights, then we would have seen the dominant trend in downward phase velocity. An important consideration with ionosonde data is to check that scaling was done correctly as I have already mentioned. In the current manuscript, the authors paid too much attention to the higher iso-line and connected it to the one at the lower h'. In my opinion, this should be re-looked at, because the ionosonde is 'more accurate' at measuring the bottomside ionosphere. Although I don't know the actual height corresponding to virtual height of 600 km, it may be possible that this could fall within the extrapolated topside? Therefore, in actual sense, we should be able to see the downward phase velocities from the lower h' values for the analysis to be reliable. For a recent analysis of ionosonde data during this storm period, please see `https://doi.org/10.1002/2017JA024510`.

Estimating Propagation Parameters: I would like to first commend the authors for presenting a straight forward and easy to understand method of doing this.

Page 7, lines 11-12, the text which talks about morphology changes of the TID changing as it propagates from high to lower latitudes: Is this backed by any references? Because the analysis of the authors is limited to northern hemisphere mid-latitudes

Page 7, lines 15-20: The authors can consider labelling the 'rectangles' as A, B, C, etc or something along this line for the reader to easily identify them in the Figure.

Page 7, lines 37-38: This is where a discussion/description of the agreement between the two techniques (VTECP' and doppler radar) should have discussed. Please refer to my earlier comment in the opening statements.

Page 8, line 6, after Chimonas , 1970; add a reference `https://doi.org/10.1002/2016GL069740` as these authors directly reported related results based on GNSS TEC observations and other measurements. Very recently, **Jonah et al., (2018)**, available on `https://doi.org/10.1029/2018JA025367`, reported related results during storm conditions. Consult this reference as well (I think their Figure 4). On this point, `https://doi.org/10.1002/2017JA024510` reported equatorward LSTID propagating from the southern hemisphere crossing to the northern hemisphere in the Asian region during this storm period. In fact, their analysis showed that these TIDs may not have exceeded 30 degrees North, which may be in agreement with your analyses and is more clearer in Figure 8(c) at around 1200 UT. Please have a look at their Figure 3(e) and possibly add some discussion to this effect.

Summary: Page 10, lines 1-3: The authors may want to rephrase this statement given that an earlier study by Habarulema et al., (2018)– https://doi.org/10.1002/2017JA024510– provided some analysis for this particular storm in the Asian sector. May be the analyses was not as detailed as provided in this paper, but definately this is not the first analysis for this storm in the Asian region. The strength of this paper over what was presented in Habarulema et al., (2018) and other attempted studies is the use of multiplicity of data sources to provide more details and clarity during this storm period.