

## ***Interactive comment on “A case study of the large-scale traveling ionospheric disturbances in the East Asian sector during the 2015 St. Patrick’s Day geomagnetic storm” by Jing Liu et al.***

**Anonymous Referee #2**

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The manuscript addresses the interesting scientific problem of understanding the properties of large scale travelling ionospheric disturbances (LSTIDs), which are frequently observed during geomagnetic storms. This manuscript discusses the properties of LSTIDs during 17th March 2015 with focus on the Chinese and Japanese sector. Although, descriptions of LSTID occurrence during this event have been published before, this manuscript adds new aspects on the longitudinal dependence of the LSTID properties in the Chinese/ Japanese sector based on GNSS, HF and ionosonde data. The manuscript is well structured, well written and presents analysis of high quality in a well understandable way. Thus, my overall evaluation is publishing after solving minor remarks. The manuscript in its current form has three weak points.

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- I. First, already in the abstract the authors are referring to negative and positive LSTIDs and seem to treat them in the course of the manuscript like separate phenomenon. Since these LSTIDs are the signature of atmospheric gravity waves, both signatures belong to the same wave. Therefore, I would recommend to avoid discussing positive and negative amplitudes separately.
- II. Second, the key point of the manuscript is the discussion of longitudinal dependence of LSTID properties. But, this is impacted by the data coverage. The data coverage is lower in the east and west boundaries of the investigated region. I argue that this impacts the accuracy of the estimation of the LSTID properties. The discussion of the LSTID properties (wavelength, period and speed) should be treated with more care concerning reliability of the results.
- III. Third, the authors present also the result of the LSTID occurrence in Europe. This has been extensively discussed in Borries et al. (2016, <https://doi.org/10.1002/2016JA023178> ). Specifically the LSTID occurring between 11 and 12 UT has been discussed to be “special” because it is impacted by winds and prompt penetration electric fields at the same time. This fact should be included in the discussion of this manuscript. It supports the finding of the authors that the LSTID properties in Europe differ from the LSTID properties in the Chinese/ Japanese sector.

Next, I like to add some technical recommendations/corrections:

1. Jakowski et al. (2008) did not discuss LSTIDs but large scale gradients (no discussion of wave properties)
2. URLs of the data sources are usually provided in the acknowledgements, not in the text.

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3. Presenting the LSTID results with VTECP' has the advantage of better illustrating most wave properties, but it does not represent the true wave amplitude anymore. On a quick view, the figures might be misinterpreted. Therefore, I recommend to make it very clear that this is an "artificial" amplitude.
4. Figures 3 and 5 do not have much content. But they are supposed to be compared with each other. Therefore, I recommend to join the content of both figures into one figure. This will increase the information density and allow better comparability.
5. In figure 8, the impact of EIA is addressed. I assume, the dashed black lines indicate the boundary of EIA. This should be made clear in the text and figure description.
6. In the discussion section, the authors exclude the impact of electric fields on the LSTID propagation and favour the impact of winds, driving the LSTID propagation because of field-aligned propagation. For a better understanding, the authors should explain, what would be different in case of electric field impact. In fact, since Borries et al. (2016) describe prompt penetration electric field impact in Europe at that time, more emphasis should be given to discuss electric field impact in the Chinese/Japanese sector at the same time.
7. I detected a few spelling errors and grammar issues (indicated in the supplementary material). I expect, there are more than I found and recommend professional editing.

Please also note the supplement to this comment:

<https://www.ann-geophys-discuss.net/angeo-2019-63/angeo-2019-63-RC2-supplement.pdf>

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Interactive comment on Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2019-63>, 2019.

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