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Interactive comment on "Characteristics of ionospheric irregularities near the northern equatorial anomaly crest" *by* Jinghua Li et al.

Anonymous Referee #3

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This manuscript presents statistical analysis of rate of TEC as observed from low latitude station (24.95o N) from Chinese sector. The statistics is obtained from a single GPS station using data from 3 different years of 2003, 2008 and 2014.

The manuscript has several lacunae with regard to analysis of data, result presentation and interpretation. Even considering this as a report, I could not find anything that adds to the existing knowledge on scintillation.

I provide my comments below, that boil down to rejection of the manuscript.

1. The geomagnetic latitude of the station is 18.20o north, which can not always be called the crest location under varying levels of solar activity. The crest of EIA has been used as a misnomer in several studies before, however, in reality this crest is

a dynamic latitudinal peak in TEC that varies even day-to-day, season-to-season and moves grossly towards dip equator during low solar activity periods. The peak in NmF2 may again differ from what one observes from TEC. Hence, for year 2008, the location cannot be granted for the crest of EIA. Authors shall mention this and carry necessary corrections in the manuscript.

2. 5-minute ROTI index has been calculated using estimated TEC. However, it has not been shown how TEC is estimated? If the GPS carrier phase data is used then how cycle slips are corrected which is an oft occurring event due to equatorial plasma bubbles passing over the site. Thus, ROTI itself can be ill-defined index to present the statistics. Result then become doubtful. Authors must clarify this issue by detailing.

3. Coming to the criterion used to declare traverse (occurrence)of EPB is not established by any means. Authors must provide 3-4 examples of estimation of TEC from RINEX data, then estimation of ROTI in panel below and then the criterion plotted along with the threshold. Thus, they may establish the validity for using it for all the data sets.

4. What are the physical rationales behind choosing 1-hr gape to reset the counter of EPB event? This seems gross qualitative measure. Now I cannot understand the statistics what it really represents?

5. MOR and LOR are ill-defined. There must be a plot to showcase how many days of observations were made in each month for all 3 years. Then MOR shall statistically significant and this must be quantified. At this level, nothing is known. In case of LOR, the number of irregularity counters are already proven wrong because of ill-defined criteria as mention in point 3 above. So how LOR is significantly true ?

6. I have studied several years of GPS observations using scintillation S4 index as well as ROTI index. The start time of irregularities can never be uniquely defined using a gross averaging index like ROTI? How much accurate will be this and this must be clarified?

7. Coming to the seasonal changes in variation of LOR and MOR, what is new that authors provide to a reader. All such variations are known. Amplitudes may vary that also is known. What is contribution of authors to add to existing knowledge is nowhere established.

8. How an average index of daytime solar radio F10.7 cm flux is related with ROTI amplitude?

9. Discussion section is highly flimsy. With help of some previous reports from very different durations than the present study covers, the discussion claims to the effect of solar activity of production of EPBs. This cannot be allowed in any sane scientific report. Production of EPBs depend upon two major physical processes that occur in post sunset duration over dip equator. One is triggering of EPB with seed perturbation and then non-linear growth of EPB. Then only it will be traversing over the low latitudes. Again, the fate of EPB depends upon background zonal drift, space weather events and electric field within the bubbles along with some secondary processes that produce a break the irregularity turbulence spectrum.

Hence just using a highly qualitative criteria based upon half-believable ROTI index cannot represent the truth that has occurred over the skies of this GPS station. Further, the latitudinal segregation of results is notional. Authors must use a greater number of sites to establishes any latitudinal behavior before commenting on fundamentals processes behind latitudinal changes in ROTI based criterion. Data is globally available and I am not finding any hindrance in using all the data to firmly establish what they wish to do.

Based on all above comments, I cannot suggest the manuscript to be even worth publication in discussion section of ANGEO. Editor may decide how much authors are willing to revise their paper and how much they will be able to really do with one station data?

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