

Review comments on paper “**Characteristics of ionospheric irregularities near the northern equatorial anomaly crest**” by Li et al., 2019

The paper attempts to discuss the occurrence of ionospheric irregularities using total electron content data derived from GPS observations over one location Taoyuan (24.95°N, 121.16°E) during the years 2003, 2008 and 2014 based on the ROTI parameter. [On this note, the title should have specified the location of the study, otherwise in its present form, one may be led to believe that it is a global study near the northern equatorial anomaly crest].

Using one location's GPS data, the authors used TEC values at the ionospheric pierce points and categorised their analysis into three latitude bands of 3 degrees difference (20-23, 23-26 and 26-29).

Results are discussed in terms of local occurrence rate (LOR) and monthly occurrence rate (MOR) for both seasonal and daily analyses.

It is possible that the results may not be statistically significant when one station's data is used, the only difference being the separation of this data into three latitude regions. What stopped the authors from carrying out a detailed statistical analysis using data from various locations from mid-latitudes extending all the way to low/equatorial latitude region? In any case, during their discussions, they attributed some of their observations to non-equatorial processes possibly originating from mid-latitude regions.

The statement in the abstract that says “The results suggest that irregularities near the crest in May to August are mainly originated from nonequatorial process, which is more frequently happened but weaker than plasma bubble in both spatiotemporal scale and strength” is not supported by analyses/results and therefore is speculative.

Page 3, line 60; where they mention that systematic research of the ionospheric irregularity with ROTI in a specific... : The authors should see papers by Mungufeni et al., (2016); Modeling of ionospheric irregularities during geomagnetically disturbed conditions over African low-latitude region, *Space Weather*, 14, doi:10.1002/2016SW001446 and Mungufeni et al., (2016): Trends of ionospheric irregularities over African low latitude region during quiet geomagnetic conditions, *JASTP*, 261–267.

Pages 3-4: Details on how TEC (from where ROTI was derived) is calculated are missing. Please provide some statements about this and include the references where details of the algorithm/software used can be accessed.

Subsection 2.3:

Line 105, is the word “medium” supposed to be “median”? Under this subsection, the method of threshold detection is not clear and should be detailed. This should include a graphical demonstration to enable the reader understand the extent of data-length (in terms of time) which would typically fall within the time period chosen and what fraction fits the threshold definition.

On this, the text which mentions “ROTI is calculated on a 5-min time window with 11 successive data” is very difficult to understand. What is the meaning of 11 successive data?

On page 4, the authors considered ROTI values between 6:00-18:00 LT during irregularities' detection. However under subsection 2.4, the time has changed to 17:00-7:00 LT. Isn't this inconsistency?

Page 5, the statement “Moreover, the irregularities observed in the same traverse event are not necessarily from the same source”. How do the authors come to this conclusion given that they are using data over one location?

Page 6, line 140, the authors say “There is no irregularity observed in March and November for all the area”. This is a strong statement. Is this typically the case? How much data was available for the analysis during these months? Is there any literature available to support the authors' statement? I suggest that the authors perform similar analysis over a different location within the same region to confirm their statement.

Subsections 3.3 and 3.4: As I have mentioned in the previous comment, the division of the analysis into three latitude bands of 3 degrees separation based on data over one location could have its considerable limitations. Discussions in these subsections referring to maxima values of ROTI may therefore be very subjective. Based on this, the statistical results may not be statistically significant. It is suggested that the authors rather consider this location and perform the analysis without separation of different latitude regions, and have a look at a different location within the same region. Comparison of results and subsequent analysis based on two or more GPS locations is likely to provide reliable and realistic picture of irregularity occurrence. If the concern is about the satellites providing TEC data over a wider coverage area, the authors could limit their analysis to data with elevation threshold of 40-50 degrees.

Pages 8-9, lines 200-225: The authors are stating existing literature without tying it to their results/interpretation. This text therefore appears redundant in the paper.

Page 10, line 250 states “As shown in Fig. 2, the LOR in solar maximum year of 2014 generally decreases with latitude, ...”. Firstly, there should be clarification whether LOR decreases with decreasing or increasing latitude. I notice that this clarification is required in the subsequent text as well. Secondly and perhaps most important is that the latitude range considered in this paper/analysis may be too small to make this conclusion.

How is Figure 6 generated?

Lines 260-270: The discussions here attributed irregularities to plasma bubbles and non-equatorial processes. However there is no evidence of each of these processes/mechanisms. The reader would expect authors to present occurrence of plasma bubbles and relate them to the irregularities discussed. There are a number of processes that take place in low latitudes including occurrence of plasma bubbles, scintillation, etc.

Lines 290-295, text talking about mid-latitude and suggestion that a study from mid-latitude to low magnetic equator is required.. I don't see why this wasn't done as GNSS receivers for this purpose are available.

There are a number of language usage errors that should be corrected.

In summary, what I see as a major shortcoming of this study is categorisation of data into three latitude blocks of 3 degrees separation and yet data is over one location. The subsequent statistical analysis is based on this which may be subjective. A similar analysis over two different locations may confirm the authors' results. A comprehensive study would be to look at generation of 2-D ROTI maps from where analysis can be performed. This would require consideration of a number of GNSS locations.