Reply to reviewer 1

Thank you for your valuable and useful comments. According to your suggestions, we made major modification in this manuscript. Data from another GPS receiver located at (31.10 ℃N, 121.20 ℃E) was added. Unfortunately, no data can be obtained at lower latitudes than TWTF along the longitude of 121 ℃E in the three years. The data from the two stations can provide the reliable and realistic results. In addition, we improve the English writing. In the following, we show the responses to the major comments one by one, and present the correction in Table 1.

Table 1 the response to reviewer 1

No.	Comments	Modification/explanation
1	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 ℃N) 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].	The title has been changed to "Characteristics of ionospheric irregularities near the north EIA at 121 ℃E"
2	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 irregularitiesduring 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 eomagnetic conditions, JASTP, 261–267.	We have read the papers and added the two references in the manuscript.
3	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.	ROTI was derived from the relative slant TEC. The details on how to get it are stated in the sub section 2.2 of the manuscript.
4	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.	The word "medium" has been corrected to "median". The method to get the threshold was added in subsection 2.3, equation (6). Figure A-1 shows an example of the traverse irregularity event detected by ROTI.

5	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?	This means the ROTI is calculated using 11 successive relative slant TEC. With the 30 seconds sampling interval, 11 successive data are in 5 minutes. We improved the description of ROTI in subsection 2.2.
6	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?	The ROTI values between 6:00~18:00 LT are used to calculate the threshold. The detection of the irregularities is based on the ROTIs during 17:00-7:00 LT and the threshold. We have improved the description.
7	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?	The statement is a speculation based on the large spatial range of IPPs. It may be inaccurate. We have deleted this in this manuscript.
8	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.	This is only description to Figure 3 in 2008. No data outage is in March of 2008 and the number of the observation days is 31.

9	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.	Data from another GPS station named SHAO (31.10 ℃N, 121.20 ℃E) have been added to this paper to provide reliable and realistic picture of irregularity occurrence.
10	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.	We rewrite section 4 and tie the literature to our results and interpretation.
11	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.	Figure 2 is written by a mistake. It has been modified to Figure 3. The latitude dependence is more clear and reliable after SHAO station is used. The discussion has major modifications.
12	How is Figure 6 generated?	This figure is not very useful to the explanation after SHAO station is added. And it is removed from the manuscript.

13	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.	Here the word “plasma bubbles” means the equatorial plasma bubbles (EPBs). The EPBs-induced irregularities can reach different latitudes from the dip equator in different events; therefore, the occurrence of these irregularities must decrease with latitudes in statistics. Otherwise, the irregularities are not from the EPBs, which are referred as non-equatorial process. By adding SHAO station (31.10 ℃N, 121.20 ℃E), obvious latitude dependence of MOR and LOR can be observed.
14	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.	SHAO station has been added according to your suggestion.
15	There are a number of language usage errors that should be corrected.	We tried to improve the English writing in the new manuscript.

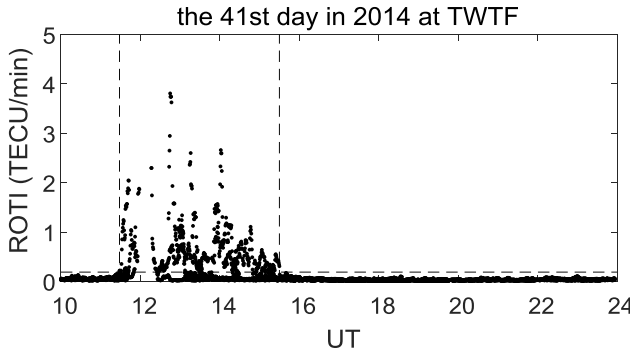

Figure A-1