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Discussions

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Interactive comment on "Emergence of a localized total electron content enhancement during the G4 geomagnetic storm of September 8, 2017" *by* Carlos Sotomayor-Beltran

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Response to Referee #1

Firstly I would like to sincerely thank the referee for his/her valuable comments in an effort to improve my article.

In the revised version I have already addressed all the concerns of referee #1:

1) Total electron content (TEC) enhancements during ionospheric storms of 2017 and 2015 are analyzed in the paper. The author uses his own method of determining deviations in TEC during a storm from quiet conditions. In the majority of ionospheric storm studies, the deviations in foF2 or TEC are studied comparing observed values of the studied parameter with its values during the preceding quiet days, or with a median. The author presents a brief discussion of the method used in the paper (Section 2), however the description is not clear. As far as I understand, for each spatial sell of the data, the 8-day running window is used to calculate the median (X). However, the median is not mentioned later in the text. The formulae (1) and (2) for the upper and lower bounds (UB and LB, respectively) relate UB and LB to μ and σ (UB = $\mu+\sigma$ and LB = $\mu-\sigma$), "... were μ and σ are the mean and standard deviation, respectively". One could understand form this determination that μ is a mean deviation. However then formulae (1) and (2) became senseless, because UB and LB would have a dimension of errors, but not of absolute values of TEC. If the author means that μ is a mean value, then it is not clear how it has been obtained. Probably, X should stand in formulae (1) and (2) instead of μ. Then at least, the formulae would be understandable.

The brief description of the statistical method indeed is not that understandable as it appears in the paper. But the method (equations) I am following and which I implemented in my software are the ones used and shown in detail in the paper of Zhu et al., 2010. In view of this, I will change lines 19-21 in page 2 with the following text to keep equations (1) and (2) as they appear in the manuscript: "... Li et al.,2015). Assuming that for each cell or line-of-sight the VTEC follows a Gaussian distribution, the mean (μ) of the 8-day VTEC and its associated standard deviation (σ) are calculated in order to define the upper and lower bounds:". If desired I can add the exact calculation of the mean and the standard deviation. However, this will be basically the same as the ones that appear in the paper of Zhu et al., 2010.

2) The description of the results begins from an error. In the first paragraph of Section 3, Figure 1 is considered. In this paragraph, March 7 and March 8 are mentioned while considering this figure. However, it follows from the caption to Figure 1 that the figure contains data for September, 2017. Obviously, March 7 and March 8 in the text should be September 7 and September 8, respectively.

This is correct. I have already changed in lines 17 and 19 of page 3 the month of March with the month of September (the correct one).

3) Figure 1 does not have dates at the abscissa (only numerals 2), so it is impossible to relate the behavior of geomagnetic and interplanetary indices to UT and dates and to compare this behavior with the TEC data shown in Fig. 2.

This is correct. In the manuscript version ready to download from the ANGEO website these dates are missing. However, I noticed this a couple of days after my manuscript went online. Reason why, I posted the corrected figures as a comment on July 13, 2018 (which is online in the interactive discussion area). I believe this was a problem with the font types. Now, I am using ones that do not disappear. All figures are now complete.

4) Besides the comments made above, I think that a figure similar to Figure 1 should be included for the March 2015 storm in order to make it possible to compare the data in Fig. 4 with the behavior of geomagnetic and interplanetary indices.

A new figure has been produced (attached to this reply Fig. 1) and its description added to the paper.

5) The language of the paper is poor and needs a serious improvement.

It is correct and my sincere apologies because my mother tongue is not English. I have once again thoroughly checked for typos or gramatic mistakes, and all that needed to be changed has been corrected in the revised version.

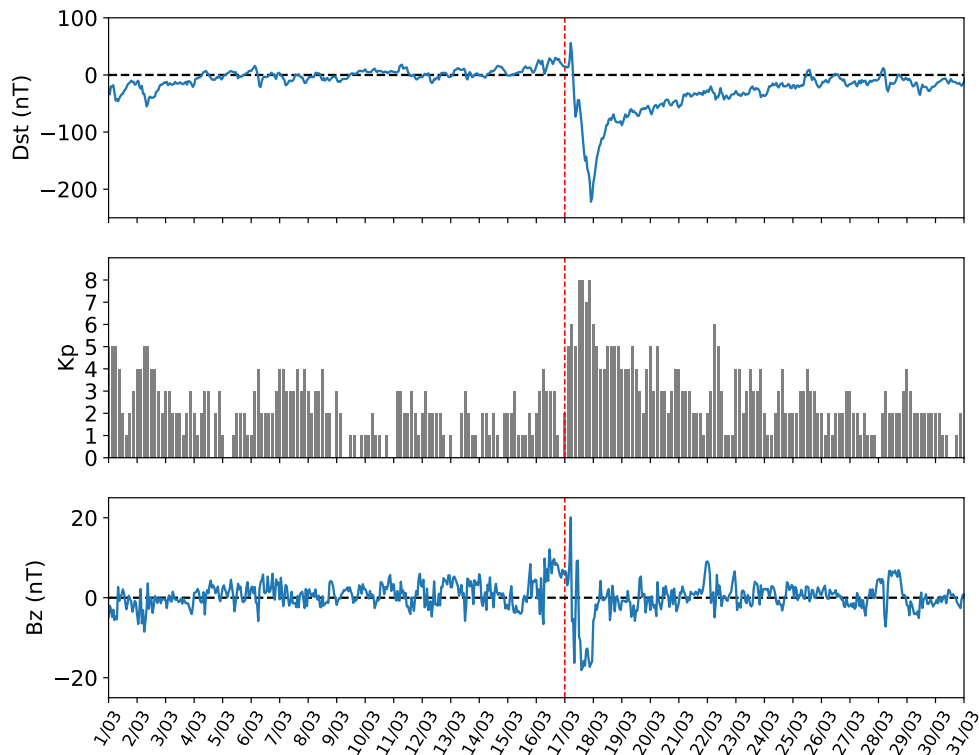Interactive comment on Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2018-83, 2018.

Fig. 1. The Dst and Kp geomagnetic indices and the southward interplanetary magnetic field (Bz) for the month of March 2015.