

Comments on, “**Latitudinal variations of ionospheric-thermospheric responses to Geomagnetic Storms from Multi- Instruments**” by Shahzad et al

General comments:

Using ground and space-based multi-instruments the authors investigated the longitudinal and latitudinal variations of ionospheric total electron content and the plausible reasons during the geomagnetic storm conditions. The strength of this study is the multi-instrument data however the presentation is not clear. For example, the authors used GNSS TEC, SWARM-TEC, and TIMED-O/N2 but they did not talk about the co-incident time of these three different measurements and how these observations agree or disagree with each other. Instead, they only mention the overall changes. There are more figures but the scientific content them are not explored. Moreover, it is very hard to follow the result section because the GNSS data is for a particular location and the SWARM and TIMED-O/N2 are on the global map. First, the authors should compare these parameters where the GNSS data is available which is more important for this study then they can give additional information about other locations. In my opinion, this manuscript needs a major revision. Therefore, I recommend to the editor for a major revision. The detailed comment and suggestions are as follows:

In the results section:

There is no clear information about how the authors differentiate the day-to-day variation and storm-induced variations in the TEC. Moreover, the TEC variation at different locations may due to local time variations. This point should be clarified.

What do the authors mean by anomalies? How does it differ from the daily variation? How the anomalies are defined?

The authors' wish to convey a message from the GIMs model is not clear. How much does the model agree or disagree with the observation? Clarify it.

Discussion:

Lines 369-374: These are vague arguments! First of all the exact time of these three different observations should be mentioned before the comparison! At least, there should be a table that should show the VTEC of these three measurements with coinciding time.

Lines 408-413: It is hard to see these results from the figures presented in this study.

Line-by-line comments:

Line 57, The Ionospheric irregularities... unknown. These references cited here are not relevant to the above statement.

Line 187, equation (5), in the original equation from Ley Huy and Amory-Mazaudier, did not include the H_0 , and H is ΔH in their equation. Since the authors use the equations from other paper better to use the equation and the symbols as it is.

Lines 203-210, Provide more detail about which are the stations used inside EEJ and which stations are considered as outside EEJ in this study.

From Figures 2 & 3, How does the SSC are identified? I feel from the Dst of figure 2 the SSC1 should be around 18 UT but the authors marked it at 05:46 UT why?

Line 228, Does the solar wind speed have any role in the IMF B_z variations? If not then no need the emphasis the solar wind speed in that sentence.

Line 251, similar intensity in which parameter?

Line 254 remove the space between 18 and <

Line 280, Do the authors think 2TECu variation is an enhancement? Remember that the error is the TEC estimation itself few TECu. Comment on it.

Line 281, depletion: How much reduction in the TEC is considered depletion?

Lines 305-309, Does the time of $vTEC$ and O/N2 observations are similar? If not then the argument is not valid.

From figures 10 and 11 it is very hard to interpret the TEC variations, the TEC range from the color bar looks like a daily and latitudinal variation rather than the storm-induced variation. Comment on it. Better to make the color bar clear with more tick labels.

In figure 11, $VTEC$ is negative! How is it possible?

Lines 416-417: The ionosphere...satellite data. It is well known. What is the new message here, emphasis on it?