The review of "Ionospheric Anomalies Associated with Mw7.3 Iran-Iraq Border Earthquake and a Moderate Magnetic Storm" by E. Şentürk et al.

Data processing techniques are utilized to investigate total electron content (TEC) abnormal variability prior to the Mw7.3 Iran-Iraq Border earthquake. TEC data were analyzed along with geomagnetic condition coefficient fluctuations. Based on the results, authors propose that anomalies 8-9 days prior to the earthquake relate to the precursory activity.

Generally, ionospheric precursory methodologies still remain a debatable topic, nevertheless increasing interest from scientific society and rocketing of publications in recent time. In my opinion, the main problem with the used here methodology lays in a lack of the analysis of anomaly appearances on long-time intervals (Major comment 4,5). It is hard to assess the validity of the methodology having observations only 2 weeks prior to the earthquake. Also, authors do not provide error analysis, what makes it difficult to understand how anomalies of 1-4 TECu are far from TEC accuracy threshold (Major comment 1-3). Thus, although the results deserve the publication, I would suggest some major revisions that in my opinion are required for better understanding of the material and clarification of subtle moments.

SPECIFIC COMMENTS (MAJOR COMMENTS)

1. As of my awareness, CODE provides interpolated (spherical harmonic fitted) TEC maps (please provide a citation at L105). This may result in biases generated by data interpolation. What accuracy is expected for the derived vTEC values over the epicenter based on CODE TEC maps? Why authors found it is necessary to analyze interpolated CODE maps, instead of just considering 5 available stations (Table 1) and calculating TEC over epicentral position with them? Also, does CODE use the same IGS stations in the considered region to produce vTEC maps? If so, authors analyze the same data twice (e.g., Figure 4 and 5). Please, clarify which stations in the considered region are used by CODE. Again, how good "anomaly maps" are for the estimation of absolute deviations (as they also based on CODE GIM interpolated data)? Were they cross-checked with vTEC over the epicenter calculated based on 5 stations? Do values agree?

2. Authors introduce satellite and receiver biases (eq. 2), but do not indicate if these biases were corrected. It is not clear what methodology is used for the correction of these biases and what errors are expected for the determination of vTEC. The analysis and incorporation of these biases is an important factor while discussing the variations of absolute vTEC and I believe this should be clarified in the text.

3. Authors provide the equation for the calculation of TEC averaged from all satellites (eq. 7). However, it is not clear if all Ionospheric Pierce Points (IPP)

for used observations were over the earthquake preparation area (determined as 1380 km). If authors carry out the selection of TEC observations outside this area, it seems possible that found anomalies results from the area outside of it. For eq. 6, it is not clear what ionospheric shell height is used for the calculation of vTEC. Also, it is not clear what elevation angle cut-off is used for vTEC observations based on eq. 7.

4. In my opinion, authors use very narrow range of days and only prior to the earthquake (from 10/29 to 11/13). It is crucial to understand whether positive anomalies appear only before the earthquake or on a constant basis during quite times. Such analysis requires additional processing of data before and after the earthquake. I would consider range between -3/+3 months, along with the analysis of geomagnetic indexes and the use of the same stations over the same region.

5. Authors reference publications by Forbes et al., 2020 and Mendillo et al. 2002, but they do not explicitly mention that TEC observation variability cannot exceed 30%, incorporating possible satellite and instrumentation biases as well as integrated nature of TEC, IPP locations, recalculation of vTEC from sTEC etc. Also, Forbes et al., 2000 discuss "high frequency" variability of 25-35% under quite Kp index < 1, whereas (according to Figure 3), Kp index on 3rd and 4th of November seem to be higher than 1 (especially on 3rd of November, where Kp index approaches 4). Do authors expect the same ~25-35% variability for Kp index of 4?

TECHNICAL COMMENTS (MINOR COMMENTS)

1. Please, clarify the choice of the window for Gaussian function as 0.005. What period it corresponds?
2. Consider using the same x-axis on all plots (e.g., on Figure 7, there are days prior to the earthquake, although on Figure 5 there are Month/Day). Also, authors may want to indicate periods instead of frequencies, as it is difficult to assess the period from ~10^-5 Hz).
3. From Figure 5, I didn't find anomalies up to 4 TECu, nor from Figure 4 (as stated in the Conclusion). Please, clarify what is a maximum absolute deviation/anomaly value found and if it is higher than expected threshold for the calculation of vTEC.
4. Authors may consider moving Figure 10 and appropriate discussion to Section 3, instead of discussing data analysis results in the Conclusion.
5. Please, consider introducing all abbreviations in the text (not only in the abstract), e.g., LMTF, CMONOC, IGS, GIM etc., along with indexes in paragraph 80 (IMF, Ey, Vsw).
6. Please revise paragraphs 25-60, as they discuss studies that are related to both post-seismic (acoustic-gravity driven disturbances in the ionosphere) and pre-seismic activity. These are 2 completely different fields of studies

and this should be clarified for readers not familiar with the topic (instead, the discussion of post-seismic studies may be fully excluded from the text).

7. Paragraph 205-210 – Should it be November instead of October?
8. Figure 1 – Should it be the indication of the northern hemisphere latitudes as N (not K)?
9. Why abnormal TEC variations are seen 8-9 days after the earthquake and not in closer dates? What is a physical explanation authors may suggest for this?