

Interactive comment on “Ionospheric total electron content anomaly possibly associated with the April 4, 2010 Mw7.2 Mexico earthquake” by Jing Liu et al.

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This paper studied TEC anomalies potentially related to the 2010 M7.2 Mexico Earthquake. The article is reasonably well written, with a few exceptions (please see below). The content appears interesting, although some points are not clear or not well clarified or developed. I agree with the other referee about the fact that normally the application of a technique to a single study only could not be sufficient. However, the procedure to analyse TEC data is clear and partly original: this is, in my opinion, the most interesting contribution of the work, differing from most of the literature on this topic. I also liked the use of an atmospheric model to see if the found TEC anomaly on 25 March

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2010 could have been produced by atmospheric forcing, not related to the impending earthquake.

In the following, I list some points that are fundamental before any publication. My final advice is a very major revision.

Major points

1. In general. In this article, specific information about the Mexico EQ is missing (tectonics of the region, fault style, effects of EQs in terms of deaths, economic losses, references, etc.). Also some literature on possible precursors of this EQ is missing.
2. Lines 64-65. You missed our recent publication on Scientific Reports (De Santis et al. 2019; <https://doi.org/10.1038/s41598-019-56599-1>), that proposes a unified and possible standard method.
3. Line 93. Is it the spatial mean at each time within the considered area? It is not clear. Could you please clarify?
4. Figure 2 (line 129). There is a clear spike in TEC data on 03/26. Why? Did you remove it before the analysis? It seems not.
5. Line 137. The reason to use $M \pm 1.5$ sigma is not convincing. I would prefer at least 2 sigma. By the way, why do not you use median and IQR (e.g. 1.5 IQR), because ionosphere is very irregular and it does not have a Gaussian distribution around a mean?
6. Line 181. Why do not you show an analogous figure as Figure 2 also for the other period analysed as confutation period, i.e. December 12 2009 to January 4 2010? By the way, this confutation period is very short. Why do not add at least another period, too, with same quiet magnetic conditions?
7. Since you analyse the data considering 15 days before and 15 days after the day of concern, for estimating mean and sigma of the anomalous day of 25 March, you also

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include the day of the earthquake, where a possible co-seismic effect in TEC could have been produced. Have you considered this? Do you think it did not affect your results? By the way, have you looked at it to see if some effect is visible?

8. Finally a remark. You find a single anomaly occurring around 10 days before the Mw7.2 earthquake. Why excluding the possibility of some other anomaly even well before, for instance in February, i.e. a month not analysed in this work? According to Rikitake law (the precursor time scales with earthquake magnitude) we would expect several months before for a such an earthquake.

Minor points.

9. Line 42. Please insert "2003" before "Colima Mexico earthquake".

10. Line 57. Please change "Statistics" with "Statistical".

11. Line 60. Please insert ""an original" before "software".

12. Line 91. This is the portal. Which is the precise site? At which date did you download the data? Please indicate better punctual information. Thanks.

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