Dear Referee#2,

Thanks for your positive opinion on our manuscript. Below we reply to our minor comments.

1) *The receiver IDs (e.g., Unpg) are capitalized in the manuscript, but are customarily given in all caps. I think the authors should adjust the manuscript to follow the standard convention.*

   In our manuscript, we have identified the GPS receivers with the same acronyms used by Nenovski et al (2015). However, we agree with you on the use of the GPS ionosphere community standard scheme. Therefore, in the revised manuscript we will change our capitalization scheme to "all caps" (e.g., UNTR instead of Untr) and add a sentence such us the following:

   > "Note that we will be following the standard station naming scheme of all capitals in this paper rather than the scheme used in Nenovski *et al.* (2015). For example, we will use UNTR rather than Untr."

2) *Page 2, "receiver are unreliable due to calibration problems." I'm not sure if this conclusion is justified. Are the data unavailable or of poor quality (or miscalibrated)?*

   In our manuscript, we have replicated the DTEC analysis of Nenovski et al. (2015) by using the same GPS receivers (M0SE and UNTR) they used. We have not included AQUI data analysis. As stated by Nenovski et al. (2015), the AQUI data record shows a gap starting after the earthquake. Nenovski et al. (2015) have not used the AQUI data set in the DTEC analysis because the presence of gaps severely affects data calibration. To be more precise, we can change the sentence at page 2 as follow:

   > "As stated in Nenovski *et al.* (2015, pg. 245), collection of data at AQUI, the closest station to the earthquake epicentre, stopped for some hours starting at the time of the earthquake. They also state that, due to this gap in AQUI data, they were unable to use these data for calculating DTEC because of calibration problems."

3) *Many physical mechanisms are identified as not driving the feature of interest. Later, it is stated that "As this variation is not germane to this discussion, we will not speculate on the source." I think the weakest element of the ionospheric precursory research is that the physics connecting the two remains open. However, the authors commit a similar "crime" here: there is a systematic daily effect, it is (supposedly) unrelated to the earthquake, but what it is remains open. I think the authors need to address this in some way.*

   In the revised manuscript, we will briefly address this point adding a sentence such us the following:

   > "We believe that the diurnal variation evidence in our Figure 4, as well as that we can see in Nenovski et al. (2015, Figure 10a), is not an ionospheric signal but rather an artifact due to an assumption made in the calibration processes, that the biases being solved for are constant over the time of the calibration analysis (24 hours). While this assumption is good for the time-delay biases at the satellite, it is not as good for the bias imposed at the receive end (from the antenna to the correlator processing within the GPS receiver). As described in Ciraolo *et al.* (2007), the time-delay on the ground segment can be effected by the ambient diurnal temperature variation, which will be different at different locations and for different

equipment set-ups. Thus, the diurnal variation in Figure 4, which can be seen does not change much across the time of the earthquake, is due to a different diurnal variation in the receiver-end time-delay at the two stations being differenced (UNTR and M0SE in our case)."

Ciraolo, J., F. Azpilicueta, C. Brunini, A. Meza, S. M. Radicella (2007), Calibration errors on experimental slant total electron content (TEC) determined with GPS, *J. Geod. 81,* 111, doi:10.1007/s00190-006-0093-1.

4) *The SCORE method is used to determine satellite and receiver biases. However, I am confused. If the analysis involves DTEC only, then are the biases then not of concern?*

Calibrations do not cancel out in the DTEC calculation, and they do not because there are uncorrelated errors in the calibrations at the two stations. The biases are an integral part of the problem if absolute TEC from two different stations are being compared, as they are in the calculation of a DTEC as defined by Nenovski et al. (2015). These calibrations are really only good to within +/- one or two TEC units due to many factors such as the temperature variation of the biases just described. Calibration difficulties are the primary reason that you really cannot compare GPS-derived absolute TEC from two different stations looking for natural variations as small as the one identified by Nenovski et al. (2015).

5) *The ultimate conclusion is that the authors "find no evidence for anomalous signals prior to, during, or after the earthquake occurrence." To better assess that no evidence is found after the earthquake, it would be helpful if Figure 2, 4, and 5 included data for more than 1 day after the earthquake.*

We have replicated the DTEC analysis reported by Nenovski et al. (2015) investigating the same period they used (11 days, from 28 March to 7 April 2009). However, we will change the sentence above such us the following:

"we find no evidence for anomalous signals prior to the time of the earthquake occurrence"

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