

Dear Editor,

We welcome comments and feedback on our manuscript. However, we believe that Referee#1's comments are biased and sometimes very offensive towards us. Comments to be positive should be unbiased, fruitful within the scientific debate in the topic of earthquake precursors, and directed to the results reported in the submitted manuscript, and should not be directed toward the qualifications and presupposed motivations of the authors.

The main criticism of Referee#1 is that no one can replicate the results presented in Nenovski et al. (2015) without using the special (and not clearly documented) method of analysis of Luigi Ciralo. Referee#1 needs to give substantive citations to support his statement. Without a substantiated study that provides a comparison between different ways to handle TEC data, there is no justification to support Referee#1's statement that the method used by Ciralo is better than any other for ionospheric precursory studies. By using our software, indeed, we have shown that the TEC enhancement that Nenovski et al. (2015) claim to be earthquake-related, is not anomalous. This because similar enhancements occur throughout the period they investigated.

With regards to our motivations, we have no prejudices either on precursors or on the possibility of furthering knowledge that will enable us to forecast earthquakes. However, science can make progress only through testable hypotheses, and the key point for a sound strategy for forecasting earthquakes is the identification of actual precursors. In our papers, we want to shed light on the real origin of reported earthquake-related effects. Our purpose is not to attack or demolish someone.

We feel that it is inappropriate for Referee#1, or any other reviewer, to speculate on the expertise of the authors as he does. The definition of "*encyclopedic character*" for our expertise is very offensive and unacceptable for us. However, we agree with Referee#1 when he affirms that we are trying to clean science from unworthy papers through replication and reanalysis studies. In some of our recent papers, we have shown evidence that many reported pre-earthquake changes in ionospheric data and in ULF magnetic records are likely not related to earthquakes.

Moving on to content of our manuscript, here we reply to Referee#1's criticism on the method we used for calculating the vertical TEC.

- 1) In section 3 of our manuscript, we have briefly discussed our method for calculating the vertical TEC. We also cite many references where you can find details.
- 2) We would like point out that the description in the original paper by Nenovski et al. (2015) of the method they use for calculating the vertical TEC is not at all clear, nor did they cite references in order to provide more details. Furthermore, Nenovski et al. (2015) have never stated to use the software of Luigi Ciralo, as claimed by Referee#1.
- 3) We have not found any studies (nor we see cited by Referee#1) showing that the method used by Luigi Ciralo to process TEC data is better than any other. If this is true, this method should be adopted by the scientific community as "international protocol" to calculate vertical TEC. At the same time we have not found any studies (nor we see cited by Referee#1) showing that the software developed by Luigi Ciralo gives adequate results for the ionospheric precursors studies. This is just the Referee#1's opinion.
- 4) We would like to point out that our method uses the data of all satellites with elevation angle higher than set value. More precisely, data down to elevation angles of 30 degrees from all satellites are used to determine satellite biases, and down to 10 degrees elevation angle in fitting the phase-

derived relative TEC to the psuedo-range-rederived absolute TEC for each individual pass. Finally, we may display data only above 67 degrees to match what had been plotted in Nenovski et al. (2015).

What it is important, is that we have reproduced the hump-like shape in Delta TEC that according to Nenovski et al. (2015) is anomalous and may be explained as related to the earthquake. However, our Figure 4 clearly shows that the hump-like shape is not anomalous, but, instead, it is part of the daily modulation in Delta TEC time series. The modulation is evident throughout the investigated period with maxima, similarly to 5-6 April 2009, occurring in the same night hours. Still, even if it is less evident (maybe this is due to the software they used), the modulation in Delta TEC data can be seen also in the original figure of Nenovski et al. (2015) (see Figure 2 of our submission). Why does Referee#1 avoid to comment on our results?

Referee#1 shows some questionable results and unsupported arguments. More precisely, he claims that:

- 1) *“The L’Aquila case was studied by many scholars and published in several papers and monographs. And all of them demonstrate the same result as Nenovsky: the positive anomaly on 5 of April over L’Aquila”*

We have not found papers that show a clear positive TEC anomaly over L’Aquila on 5 April, nor has Referee#1’s cited these papers in his Interactive Comment.

- 2) *The main identifier of the precursor is not the sign of deviation, not the magnitude of deviation, but its locality, connected with the impending epicenter position.*

Just speculation. The real issue is to demonstrate that earthquake is actually preceded by precursors, not speculate on their existence. In our previous papers, we have shown that many reported local pre-earthquake changes hypothesized to be connected with the impending earthquake are not precursors. You can refer to our papers on TEC precursors reported in the References section of our submission, as well as to our publications on ULF magnetic precursors (see, e. g., Masci and Thomas, 2015, doi:10.1002/2015JA021336 and references therein).

Then, in order to deny our findings, and supporting those of Nenovski et al. (2015), Referee#1 shows three figures from his team where they report the Delta TEC analysis at the time of L’Aquila earthquake using GPS measurements from 32 Italian receivers. Unfortunately, he does not specify where these results have been published. Even if we think that the figures provided by the reviewer are irrelevant since we do not know the method they used to produce them (Is it the same method as used in Nenovski et al.? ), we are going to comment on these figures.

In brief, Figure 1 shows the location of the 32 GPS receivers. Figure 2 shows a positive anomaly in a map of interpolated Delta TEC data. The anomaly occurs on 5 April 2009 at 04:00 UT, and it is extended over a large area in Central Italy. Figure 3 shows Delta TEC calculated using AQUI GPS receiver.

Contrary to what is stated by Referee#1, the three figures do not support the result of Nenovski et al. (2015), but confirm our results. More precisely:

- Referee#1 reports just a single map. Note that, the positive Delta TEC values over Central Italy shown in Referee#1's Figure 2 has been identified at 04:00 UT of 5 April 2009. On the contrary, the positive TEC anomaly reported by Nenovski et al. (2015) starts to appear many hours after the anomaly shown in Referee#1's Figure 2, more precisely 6 hours before the main shock (about at 19 UT). Therefore Referee#1's Figure 2 does support the Nenovski' findings. Instead, similarly to Referee#1's Figure 2, Figure 4 in our submission shows positive Delta TEC values at 04:00 UT of 5 April 2009. However, we can note that positive Delta TEC values are present at 04:00 UT in all the days reported in our figure, from 29 March to 7 April. Thus, a single map (that identifies a specific point in time) is not compelling for demonstrating that an anomaly highlighted in the map is really seismogenic without considering the period before and/or after the anomaly occurrence, as shown in Figures 4 and 5 of our submission.

Referee#1 claims to have found similar positive Delta TEC values during the hours before the main shock as well. Similarly to that of Referee#1, our analysis shows positive Delta TEC values during the hours before the earthquake. However, as previously stated these positive values are part of the daily modulation in Delta TEC that shows similar maxima, and in the same hours, to what is observed on the earthquake day throughout the investigated period.

- Concerning Figure3 by Referee#1, it is important to note that the 32 GPS receivers he used in the Delta TEC analysis include the AQUIC receiver. As stated by Nenovski et al. (2015, page 245) GPS TEC data from AQUIC receiver are unreliable due to calibration problems caused by gap in data records (see also our submission, page 2, line 19). Note that this was the reason why a first version of the Nenovski's paper was rejected by an AGU journal. With a more careful reading, Referee#1 would have noted that this is also the reason why Nenovski et al. (2015) have not used the AQUIC receiver for calculating Delta TEC. In conclusion, the positive anomaly in Delta TEC from AQUIC GPS receiver shown in Referee#1's Figure 3 is flawed due to calibration problems.

Still, Referee#1 claims that:

- 1) *We can present more results for ionospheric anomalies over L'Aquila but it seems that's enough to demonstrate that Nenovski is right and the authors of this paper are wrong.*

This statement is not supported by evidence or peer-reviewed studies.

- 2) *I'd like to underline the poor level of discussion. The main argument of the authors is to refer the publications of somebody without own calculations or arguments.*

Again speculation. Referee#1 should note that in section 2 of our submission we discuss the characteristics of the Delta TEC anomaly reported by Nenovski et al. (2015) taking into account recent literature on earthquake-related effects in ionospheric TEC. Then, in section 3, we report our own independent analysis using the same GPS data analysed by Nenovski et al. (2015) showing that their hump-like shape in Delta TEC is not anomalous. Again, Referee#1 avoids to comment on our results.

Concerning our own analysis of TEC data, Referee#1 can also refer to our recent JGR-SP papers (see doi:10.1002/2014JA020822; doi:10.1002/2016JA023652).

- 3) *For example, speaking on the physical mechanism of seismo-ionospheric coupling they claim: many researchers are sceptical of the reliability of these mechanisms, and I can say with the same level of confidence that many researches support this mechanism. This is not the argument.*

Similarly to Nenovski et al. (2015), we have briefly introduced the state of the art in the topic of the proposed generation mechanisms for seismo-ionospheric coupling. Then, we have discussed the mechanism that according to Nenovski et al. (2015) may support the Delta TEC anomaly they observed. This mechanism is the possible generation of electric currents during the hours before the 6 April 2009 earthquake (see Nenovski et al., 2015, page 265). Referee#1 should have noted that:

- We show that studies of data records from L'Aquila area (see Biagi et al., 2010; Masci, 2012b; Masci and Di Persio, 2012; Masci and De Luca, 2013; Villante et al., 2010) have identified no anomalous magnetic or electric effects during days to hours before and after the 6 April earthquake that might be hypothesized to have seismogenic origin (see our submission, page 4, lines 26-29).
- We refer to our recent GRL paper (doi:10.1002/2016GL069759) where we have shown that there is no evidence that might support the generation of an underground electric current in correspondence of the 6 April main shock, when the rupture occurred and the vast majority of mechanical energy was released (see our submission page 4, lines 29-31, page 5 lines 1-2).

Bearing these two points in mind, can we still assume that electric currents were generated before and during the L'Aquila earthquake?

- 4) *The poor quality of illustrations should also be mentioned, for example the Figure 4 if to compare with our Figure 3.*

We disagree the criticism on the poor quality of our Figure 4. On the contrary, as previously shown, we note that the quality and the scientific meaning of Referee#1's Figure 3 is apparent. The positive anomaly shown in the Referee#1's Figure 3 is identified over a large area including only three GPS receivers (untr, m0se, AQUI). Taking account the calibration problems of AQUI data, can them be used to obtain a valid map?

Still, we note a second a positive anomaly over Northern Italy. Can this second positive anomaly be connected with the earthquake as well?

- 5) *Summarizing I would say that such type of publications are very dangerous: they create the new style of not science but fighting without strong arguments basing only on authority of somebodies publications. Concerning the results of the authors themselves, they look very poor and my conclusion is to reject this paper.*

Again speculation. We are not creating a *new style of not science*. Comments on published results are part of the scientific debate. Instead, we think that reports of invalid precursors as well as the Interactive Comment of Referee#1, where we see no science, are very dangerous both for the scientific community and for society in general. Their responsibility is that these reports motivate the idea that in the future we will be able to predict earthquakes. This idea is not supported by the current scientific knowledge. As we previously said, the aim of our research is not to reject the idea that one day earthquake prediction will be possible. However, the persistence in encouraging

the publication of flawed earthquake precursors, or to try to hinder their denial without provide strong arguments, as we can see in the Referee#1's Interactive Comment, is unscientific.

In conclusion, Referee#1 provided a biased report where he tries to deny our results by means of unsupported arguments, showing very questionable results from his team, and using many unnecessary and sometimes offensive statements. Still, Referee#1 shows poor knowledge both of our submission and of the paper by Nenovski et al. (2015), as well as conflict of interest with the Nenovski's team.

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