

Interactive comment on “On a reported effect in ionospheric TEC around the time of the 6 April 2009 L’Aquila earthquake” by Fabrizio Masci et al.

Anonymous Referee #3

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The active attempts to find the physics parameters, which are sensitive enough for detection of earthquake (EQ) preparation phase by available instrumentation, are underway more than half a century. Unfortunately, until now, there is no any reliable method in EQ prediction area in spite of intensive efforts with use of multiple techniques and printing of numerous publications. The main problems there are a low signal-to-noise (or interference) ratio and lack of sufficient statistics. The single “successes” at EQ precursor finding (usually post factum) are mainly a product of a wrong data interpretation based on above mentioned causes. So, at present, it is not enough to study a single EQ event and associated precursors but to test the promising technique on convincing statistics. F. Masci is known as a conscientious critic of EQ researchers which published results have weakness or errors at data processing at insufficient signal/interference ratio. Such a processing often leads to effect of “a wishful thinking”

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at interpretation procedure. The manuscript “On a reported effect in ionospheric TEC around the time of the 6 April 2009 L’Aquila earthquake” by Fabrizio Masci et al., 2017 presents in clear and persuasive form the drawbacks of Nenovski et al., 2015 article. For example, from Figs. 2, 4, 5 (Masci et al., 2017) it is seen the low signal-to-noise ratio of obtained data, so the result of precursor existence is unstable and is dependent on algorithm of data processing. Obviously, that the value of elevation angle threshold for GPS satellites, number of GPS receivers and their distance to EQ epicenter should be theoretically substantiated otherwise results will be depend on the preferences of every researcher. The similar remarks should be pointed out about possible EQ precursor source. For example, for radon ionization hypothesis the radon concentration against vertical and horizontal distances to EQ epicenter should be theoretically substantiated (at least by modeling) depending on geological structure, atmospheric/ionospheric conditions, winds etc. Naturally, such “an ideal goal” is hard for achievement, but without it realization we will have the constant fruitless discussions and very moderate progress in EQ forecasting. In my opinion the manuscript Masci et al., 2017 is worthy of publication in NHESD.

Minor comments. The authors should more carefully give the references. For example: Line 270. It should be year “... 2015.” instead of “... 2105.” Line 278. Please remove spare comma. Line 281. It should be year “... 2007.” instead of “... 2004.” Line 294. It should be year “... 2015.” instead of “... 2105.” Line 300. It should be year “... 2013.” instead of “... 2103.” Line 306. It should be “... Earth Sci.,114, 414 ...” instead of “... Earth Sci.,114,414 ...”. Line 339. It should be year “... 2015.” instead of “... 2105.”

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