

Interactive comment on “Analysis of geomagnetic measurements prior the Maule (2010), Iquique (2014) and Illapel (2015) earthquakes, in the Pacific Ocean sector of the Southern Hemisphere” by Enrique G. Cordaro et al.

Anonymous Referee #1

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The article “Analysis of geomagnetic measurements prior the Maule (2010), Iquique (2014) and Illapel (2015) earthquakes, in the Pacific Ocean sector of the Southern Hemisphere.” by Cordaro, Venegas-Aravena and Laroze, is dealing with a very interesting topic, however the manuscript, in its present form, present several weaknesses. Apart from a number of typos or draft version leftovers here and there (e.g., “Ester Island”, “Figura”, “a (6:00 - 05:00 Local time. . .”, etc.), one could highlight the following major issues: Part of section 2 is just a repeat of information given in introduction. The authors should carefully revise both section 1 & section 2 in the direction of a more

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concise manuscript. The authors should use Z/Z^* (e.g., doi: 10.1002/2014JA019789, doi: 10.1007/s10712-012-9215-x) or P_z/h (e.g., doi: 10.1002/2013JA019530, doi: 10.1080/19475705.2014.895965) instead of Z (or in parallel to Z), because from the literature these quantities seem to be most efficient in exposing precursory anomalies from ground-based magnetometers. The authors should also show the variation of Dst (probably also A_p) over the studied time periods. For all seismic events the authors should study the analyzed signals (e.g., Dst , K_p , B_z , Z/Z^* , P_z/h , the derivatives of the magnetic field, etc.) over time periods of the same duration or explain why they don't do that. The readers would expect to have convincing information at hands in order to be persuaded that the revealed anomalies in the magnetic field recordings are not related to some global phenomenon. The whole part of Fourier analysis (both Fourier spectrum analysis and Spectrograms) is not convincing and probably should be excluded from the paper or carefully re-written. Some problematic points are the following: (a) it is not clearly stated which are the time periods over which each spectrum of Figs 3a-c was calculated; (b) for the peak at 5.154 μHz claimed to be related to the Iquique earthquake in Fig. 3d it is not clear why it presents intensity changes over the different studied time periods and what does this mean. (c) The claimed findings resulting from Fig. 4 are not supported by Fig. 4, while the white drawings (dashed lines and arrows) on the figures of Fig. 4 is not clear why have been employed (what are they highlighting). In section 4, the authors should explain why they adopted the specific filter and how did they ended-up with the used factors. Also, it is not clear how the anomalies were extracted from the measurements. The authors mention that "The data considered are for periods $Dst < 10$ nT and only quiet magnetic data (6:00 - 05:00 Local time. . .". How did they selected the time period 16:00-05:00 as a quiet one and what do they mean by ". . .for periods of $Dst < 10$ nT. . .", how precise in time is the specific discrimination, what happens around (but close to) the periods of $Dst < 10$ nT? Finally, the authors have to bear in mind that there is recently a criticism regarding the formula proposed by Dobrovolsky et al. (1979) for the calculation of the region of precursory deformation, while the notion of critical radius is often used instead (e.g.,

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doi:10.1002/2016JA023652, doi: 10.1002/2014JA019896, doi: 10.1785/0120040181, doi: 10.1029/98JB00792). Moreover, the original work of Dobrovolsky et al. (1979) itself indicates that some precursors may appear in narrower zones than others, while the wideness of the zone depends on the strain level (see Fig. 5 and Eq. (C) of Dobrovolsky et al., 1979). For example if the strain level is 10^{-6} , then an EQ of M8 corresponds to a radius of the order of 500 km and not of the order of 2700 km as calculated by the formula that the authors used. Therefore, their results may accept intense criticism, especially as regards the 2015 Illapel earthquake.

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