

Interactive comment on “Long-term magnetic anomalies and its possible relationship to the latest Greater Chilean earthquakes in the context of the seismo-electromagnetic theory” by Enrique Guillermo Cordaro et al.

Anonymous Referee #2

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General Comments: In this manuscript (ms), the authors present experimental evidence -concerning the vertical component of the geomagnetic field- that supports the existence of long-term anomalies preceding the strong earthquakes that took place in Chile during the last decade (cf. these are the (magnitude) M8.8 Maule 2010, the M8.2 Iquique 2014, and the M8.3 Illapel 2015 earthquakes). They use the Fast Fourier Transform, the wavelet transforms and the daily cumulative number of anomalies methods during quiet space weather time during one year before and after each earthquake in order to filter out space influence. They find a pre-seismic raise of power spectral den-

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sity in the mHz range, supported also by the wavelet method, before each earthquake. They also find that the cumulative anomalies method reveals an increase 50-90 days prior to each Chilean earthquake. The authors provide evidence that similar changes have been observed before the M8.2 Mexico 2017 earthquake. Finally, they suggest a model based on fracture mechanics for connecting their experimental observations with the seismo-electromagnetic theory.

My opinion is that the results presented are original and interesting that advance our knowledge in the field of electromagnetic precursors. The ms is professionally written, convincing, and easy to follow but unfortunately the authors did not manage to relate their findings with the pre-existing literature.

For example, the pioneering work of Varotsos and Alexopoulos:

P. Varotsos and K. Alexopoulos, "Physical properties of the variations of the electric field of the earth preceding earthquakes, I." *Tectonophysics* 110 (1984), 73-98, DOI: 10.1016/0040-1951(84)90059-3

that stimulated international interest on the so-called VAN method and provided the basic properties of electromagnetic earthquake precursors is not mentioned although it is earlier than any other work mentioned on the subject. Moreover, the recent results of this method, see for example

N.V. Sarlis, P.A. Varotsos, E. S. Skordas, S. Uyeda, J. Zlotnicki, T. Nagao, A. Rybin, M.S. Lazaridou-Varotsos, and K.A. Papadopoulou, "Seismic Electric Signals in seismic prone areas", *Earthquake Science*, 31 (2018), 44-51, DOI: 10.29382/eqs-2018-0005-5

and

P.A. Varotsos, N.V. Sarlis, and E.S. Skordas, "Phenomena preceding major earthquakes interconnected through a physical model", *Annales Geophysicae* 37 (2019), 315–324, DOI: 10.5194/angeo-37-315-2019

and references therein are also ignored. Additionally, references like

Q. Huang, "Rethinking earthquake-related DC-ULF electromagnetic phenomena: towards a physics-based approach", *Nat. Hazards Earth Syst. Sci.* 11 (2011), 2941–2949, DOI: 10.5194/nhess-11-2941-2011

and

J. Zlotnicki, V. Kossobokov, and J.-L. Le Mouel, "Frequency spectral properties of an ULF electromagnetic signal around the 21 July 1995, M=5.7, Yong Deng (China) earthquake", *Tectonophysics* 334 (2001), 259-270, DOI: 10.1016/S0040-1951(00)00222-5

which explicitly state the existence of pre-seismic electromagnetic anomalies in the ultra-low frequency range (especially \approx mHz in Zlotnicki et al. 2001) are just not mentioned. This problem should be solved before the publication of the present manuscript. More details are also given below in the **Specific Comments**.

Since the reported findings are well supported and original, the ms certainly merits publication in NHESSD upon appropriate amendments on the points raised above. Thus, I suggest that the authors should update their references by commenting on related results found by other scientists (which as it will become clear below support the present findings) and resubmit their ms.

Specific Comments:

1. In the introduction, lines 52-54 of page 1 and first line of page 1. The authors' claim: "failed to conclude that it is possible to use seismological data as a predictive tool (Geller, 1997). Besides, when less classical methods (e.g., electromagnetic methods) have been used some decades ago, conclusive results have not been obtained either (see the debates of Varotsos et al. (1996) and Hough (2010))." is based on outdated literature which is not thorough and fails to follow the state of the art in the field. For example, according to

R. Musson, "Predicting the Unpredictable: The Tumultuous Science of Earthquake Prediction", *PHYSICS TODAY* 63(11) (2010), 46-47, DOI:

10.1063/1.3518213

Hough (2010) “is rather US-centric, as even the author admits. There is little discussion about the development of earthquake prediction in Japan, China, or Russia. Briefly mentioned is Greece’s VAN project (named for the three seismologists who pioneered it), which uses seismic electrical signals to predict earthquakes. However, that classic case—it led to a great debate in the 1990s among seismologists about whether earthquakes could be predicted—deserved a more detailed exposition.” and hence should not be used as providing evidence for VAN (see Ref. Varotsos and Alexopoulos (1984) mentioned earlier in **General Comments**) or for the attempts of other countries like Japan, China, or Russia (e.g., see Sarlis et al. (2018), Huang (2011), Zlotnicki et al.(2001) mentioned earlier in **General Comments**).

As concerns recent results on the existence of statistical significance in the use of seismological data based earthquake prediction methods see:

N. V. Sarlis, E. S. Skordas, S.-R. G. Christopoulos and P.A. Varotsos, "Natural Time Analysis: The Area under the Receiver Operating Characteristic Curve of the Order Parameter Fluctuations Minima Preceding Major Earthquakes", *Entropy* 22 (2020), 583, DOI: 10.3390/e22050583

while of electromagnetic precursors (including those invented by VAN) see:

N.V. Sarlis, “Statistical Significance of Earth’s Electric and Magnetic Field Variations Preceding Earthquakes in Greece and Japan Revisited”, *Entropy* 20 (2018), 561, DOI: 10.3390/e20080561

and

P. Han, J. Zhuang, K. Hattori, C.-H. Chen, F. Febriani, H. Chen, C. Yoshino, S. Yoshida, “Assessing the Potential Earthquake Precursory Information in ULF Magnetic Data Recorded in Kanto, Japan during 2000–2010: Distance and Magnitude Dependences.” *Entropy* 22 (2020), 859, DOI: 10.3390/e22080859.

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As a result, the authors should rephrase their claim in view of the literature provided above.

2. In the list of references mentioned in the lines 16 to 20 on page 2, the following references which are related with ground observations of magnetic anomalies before strong M6.5 or larger earthquakes and hence very closely related with the findings of the present ms:

P. Varotsos, N. Sarlis, and E. Skordas, "Electric Fields that "Arrive" before the Time Derivative of the Magnetic Field prior to Major Earthquakes", *Physical Review Letters* 91 (2003), 148501, DOI: 10.1103/PhysRevLett.91.148501

and

N. Sarlis and P. Varotsos, "Magnetic field near the outcrop of an almost horizontal conductive sheet", *Journal of Geodynamics* 33 (2002), 463-476, DOI: 10.1016/S0264-3707(02)00008-X

are missing and should be included.

3. On page 2, lines 37-38, since the term PSC has been also used in a similar context (piezo-stimulated currents) in previous research on the field of solid state physics and earthquake precursors, e.g., see pp. 417-420 of

P. Varotsos and K. Alexopoulos, *Thermodynamics of Point Defects and their relation with the bulk properties*, Eds. S. Amelinckx, R. Gevers, and J. Nihoul, North Holland (1986) pp. 474, <https://www.sciencedirect.com/bookseries/defects-in-solids/vol/14>

the authors should proceed to a clarification to avoid readers' confusion.

4. On page 4, lines 30-33, the authors explain why they study the vertical magnetic field component. Sarlis and Varotsos (2002) -mentioned above- provides evidence on the importance of the vertical magnetic field as an earthquake precursor.

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5. On page 5, lines 49-50, the results of the authors are compatible with the dates reported on Table II for the earthquake precursors studied in
N. V. Sarlis, S.-R. G. Christopoulos, and E.S. Skordas, "Minima of the fluctuations of the order parameter of global seismicity", Chaos 25 (2015), 063110, DOI: 10.1063/1.4922300
6. On page 7, lines 14-26, the authors discuss the magnetic anomalies before the M8.2 Mexico 2017 earthquake. Their findings are compatible with the date (27 July 2017) identified for the precursors found in
N.V. Sarlis, E.S. Skordas, P.A. Varotsos, A. Ramirez-Rojas, and E. L. Flores-Marquez, "Identifying the Occurrence Time of the Deadly Mexico M8.2 Earthquake on 7 September 2017", Entropy, 21 (2019), 301, DOI: 10.3390/e21030301
Moreover, the mentioned, in line 22, margins of 50-90 days are compatible with those found in the VAN method for Seismic Electric Signals (SES) activities
P. Varotsos and M. Lazaridou, "Latest aspects of earthquake Prediction in Greece based on Seismic Electric Signals", Tectonophysics 188 (1991) 321-347, DOI: 10.1016/0040-1951(91)90462-2
see also the related discussion in
S.-R. G. Christopoulos, E. S. Skordas, N. V. Sarlis, "On the Statistical Significance of the Variability Minima of the Order Parameter of Seismicity by Means of Event Coincidence Analysis", Applied Sciences 10 (2020), 662, DOI: 10.3390/app10020662
7. On page 8, lines 10-11, in the list of References there Varotsos and Alexopoulos (1984) as well Zlotnicki et al. (2001) should be included.
8. On page 8, line 17, the values of a few tenths of nT or smaller as a precursory signal in the vertical magnetic field component is also anticipated according to the model of Sarlis and Varotsos (2002) mentioned above.

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9. On page 9, lines 21-32. This paragraph should also include the fact that the dates identified in Figures 8 (Feb 6, 2010 and Jan 8, 2014) and 9 (\approx Jul 19, 2017) as those marking the onset of increase of the magnetic anomalies are very close to the appearance dates of the seismological precursors identified by Sarlis et al. (2015) (for Maule 2010 and Iquique 2014 earthquakes, see the first two columns of their Table II) and Sarlis et al. (2019) (for Mexico 2017 earthquake) (cf. the papers Sarlis et al. (2015) and Sarlis et al. (2019) are those mentioned previously in points 5 and 6). Moreover, as mentioned the margins of 50-90 days is compatible with the lead time of SES activities of the VAN method. Such a coincidence is also compatible with the one found in

P. A. Varotsos, N. V. Sarlis, E. S. Skordas, and M. S. Lazaridou, "Seismic Electric Signals: An additional fact showing their physical interconnection with seismicity", *J. Geophys. Res.* 589 (2013), 116-125, DOI: 10.1016/j.tecto.2012.12.020.

10. On page 10, lines 1-8, the authors may also consider at this portion the results found by Sarlis (2018) and Han et al. (2020) mentioned above.

Technical Corrections:

1) On page 1, line 30, "and" \rightarrow "an" 2) On page 1, line 39, "gives" \rightarrow "give" 3) On page 2, line 43, the readers would benefit if the authors add the expression 100.43M so that "thousands of kilometers" becomes "thousands of kilometers, 100.43Mkm. 4) On page 3, line 18, "indexes" \rightarrow "indices" 5) On page 5, line 9, "Apr 3, 2014" \rightarrow "Jan 3, 2014" 6) On page 5, lines 47-48, "but no to bigger" \rightarrow "but not too bigger" 7) On page 6, line 7, "in surface of earth" \rightarrow "in the surface of earth" 8) On page 6, line 34, "This mean" \rightarrow "This means" 9) On page 6, line 48, "march" \rightarrow "March" 10) On page 6, line 51, "lost" \rightarrow "loss" 11) On page 6, line 51, "this, is clear" \rightarrow "this, it is clear" 12) On page 6, line 56, "These" \rightarrow "This" 13) On page 7, line 6, "anomalies-" \rightarrow "anomalies." 14) On page 8, line 7, "could covers" \rightarrow "could cover" 15) On page 9, line 11, "give" \rightarrow "gives" 16) On page 9, lines 55-58, please rephrase the sentence because it is incomprehensible. 17) On

page 14, lines 59-60, please place the reference at its correct position at alphabetical order. 18) On page 27, in Figure 10, please define “cte” (if it means constant const. is enough) 19) On page 28, Table 1, 4th column, “Atmospheric Deep” → “Atmospheric Depth”

Summary: As the ms reports original and interesting results, I will be glad to suggest publication for a revised version in which the points mentioned above will be appropriately addressed by the authors.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2020-354>, 2020.

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