2 April 2021.-

Editorial Board Members
Natural Hazards and Earth System Sciences
Copernicus Publications

Dear Editors:

Thank you for sending us the reports concerning our manuscript with reference number nhess-2020-354. In the resubmitted version we have addressed all comments of the Reviewers, as summarized below.

The changes made in the manuscript are highlighted in blue color.

Finally, we would like to thank the Referees and the Editor for their time and comments that have helped in improving the quality and readability of our manuscript. We hope that our revised text is now suitable for the publication.

Sincerely yours,

Patricio Venegas-Aravena, Enrique Cordaro and David Laroze
Pontificia Universidad Católica de Chile
Universidad de Chile
Universidad de Tarapacá

REPORT 1

Reviewer:

The importance of the Earth Magnetic Field to the dynamic of any action, natural or human, on the Earth is beyond any question. The study of any phenomena or the development of technical work should take into consideration the effect of the Geomagnetic field. Therefor National and International Programs are engaged with the monitoring and study of the Geomagnetic field. Thus in the field of the earthquake risk mitigation the study of the magnetic field variations in relation to the tectonic activity constitute a very promising active domain during the last decades. In this paper the authors analyse the vertical magnetic behaviour close to the latest three main earthquakes in Chile: Maule 2010 (Mw8.8), Iquique 2014 (Mw8.2), and Illapel 2015 (Mw8.3). They try to discriminate the magnetic variations of lithospheric origin from those of planetary origin in the observational data using three methods: The FFT, the Wavelet transform and the daily cumulative number of anomalies, methods. They select quiet space weather days for a time period of one year before and after each earthquake. Their results are very interesting. The paper is very interesting for the earthquake mitigation field scientists, has a very good structure and pay credit to an immense bibliographical bulk of the relative scientific field. It must be accepted for publication in the Journal NHESS. However the submitted manuscript lag a lot in the language quality, due to oversights or English language lag. In the adapted annotated copy I have marked the proper corrections, but I feel that might be more corrections, there for I would suggest that the manuscript should be corrected by a native English speaker, if possible. In concluding I suggest that the paper should be accepted for manuscript should be accepted after minor revision

Authors:

We are delighted that Prof. Michael E. Contadakis considers that the manuscript interesting and can be accepted after the performance of his suggestions. We acknowledge his remarks. The new version of the manuscript has been edited by a native English speaker.

REPORT 2

Reviewer:

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 density 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 ≈ 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 NHESS 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.

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.

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

Authors:

We are thrilled that the Reviewer considers that our study is original and interesting that advance the knowledge in the field of electromagnetic precursors. The references she/he gave us were critical sources to complete our study. In the new version of the manuscript, the aforementioned references have been included in the introduction. Also, we have slightly modified the main text in order to avoid confusions.

Reviewer:

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.

Authors:

Thank you for this comment. The references have been included in the new version of the manuscript.

Reviewer:

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-insolids/vol/14

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

Authors:

In the new version of the manuscript, we have tried to clarified this issue.

Reviewer:

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.

Authors:

This point has been incorporated in the new version of manuscript.

Reviewer:

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

Authors:

Thank for the remark. We have added this reference.

Reviewer:

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. FloresMarquez, "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

Authors:

In the new version of the manuscript, we have added these references.

Reviewer:

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

Authors:

Thank you for this remark. In the new version of the manuscript, this reference has been included.

Reviewer:

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.

Authors:

This reference has been included.

Reviewer:

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 (≈ 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",Îd'ectonophysics 589 (2013), 116-125, DOI: 10.1016/j.tecto.2012.12.020.

Authors:

We have included the corresponding references.

Reviewer:

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.

Authors:

We have included the corresponding reference.

Reviewer:

1) On page 1, line 30, “and” → “an” 2) On page 1, line 39, “gives” → “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, 100.43 Mkm. 4) On page 3, line 18, “indexes” → “indices” 5) On page 5, line 9, “Apr 3, 2014” → “Jan 3, 2014” 6) On page 5, lines 47-48, “but no to bigger” → “but not too bigger” 7) On page 6, line 7, “in surface of earth” → “in the surface of earth” 8) On page 6, line 34, “This mean” → “This means” 9) On page 6, line 48, “march” → “March” 10) On page 6, line 51, “lost” → “loss” 11) On page 6, line 51, “this, is clear” → “this, it is clear” 12) On page 6, line 56, “These” → “This” 13) On page 7, line 6, “anomalies- “ → “anomalies.” 14) On page 8, line 7, “could covers ” → “could cover” 15) On page 9, line 11, “give” → ” 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”

Authors:

Thank you very much for point out this grammar mistakes. Most of them have been fixed. Other sentences, we have fully modified.

Reviewer:

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.

Authors:

Thank you for this remark. We hope that the new version of the manuscript satisfies your requirements and now will be suitable for publication.

REPORT 3

Reviewer:

In this manuscript we have an experimental approach for promoting the variation of vertical component of geomagnetic field as preceding index to large earthquakes. Supported by an extensive literature review, the manuscript is very well written and the discussion following the obtained results is supportive to them. However, in order to provide the interested readers the ability to reproduce the results or to adapt the proposed methodology to their own datasets, the authors must respond to the following suggestions.

a) **authors select continuous wavelet transform (CWT) to detect "rare variations that could not be attributed to space weather in the daily average measurements". Even if the CWT generally is an appropriate method for revealing prevalent variability modes, i thing that the 3D presentation in Fig.5 does not help readers since it is not quite evident the decrease before and the increase after main event. There are more appropriate wavelet based methods that can reveal significant variability changes in a more clear and simple presentation form (per wavelet scale). I suggest the authors to look out and comment the Maximal Overlap Discrete Wavelet Transform(MODWT)(Percival, D. & Walden, A.,2000) or the wavelet coefficients standard deviation (Telesca et, al, 2007)**

b) **Since the authors engage the CWT in their analysis i expected to see a scalogram instead of spectrogram In Fig.6 (especially when the Power Spectral Density is expressed in a.u.). Please justify your choice since scalograms are more suitable than spectrograms to highlight variability in real world signals that exist in different scales.**

c) **If the authors insist to keep the spectrogram , the choice of temporal window and overlap for spectrograms in Fig.6 must be justified (arbitrary choice or after testing? if is the latter, please provide test results briefly)**

d) **Finally the term"Wavelet" in signal processing and data analysis domain refers to the base (mother) wavelet function that used to perform the Wavelet analysis and not to the analysis itself. Please change accordingly.**

Authors:

We are pleased that the Reviewer considers that the paper is well written and the results are coherent. We have tried to clarify the text regarding to the methods, statistic tools, and reproducibility of our paper. We also have organized the relevant days of earthquakes in a new table. Bellow we will be commented point by point the remarks

Reviewer:

authors select continuous wavelet transform (CWT) to detect "rare variations that could not be attributed to space weather in the daily average measurements". Even if the CWT generally is an appropriate method for revealing prevalent variability modes, i thing that the 3D presentation in Fig.5 does not help readers since it is not quite evident the decrease before and the increase after main event. There are more appropriate wavelet based methods that can reveal significant variability changes in a more clear and simple presentation form (per wavelet scale). I suggest the authors to look out and comment the Maximal Overlap Discrete Wavelet Transform(MODWT)(Percival, D. & Walden, A.,2000) or the wavelet coefficients standard deviation (Telesca et, al, 2007)

Authors:

Thank you very much for the remark. In the new version of the manuscript we have explained in more detail our Wavelet analysis as well as the treatment of the signal regarding filtering before to apply the Fourier and Wavelet analysis. Also, we have included the references that the Reviewer was commented. Indeed, we consider that the analysis on of Telesca *et al.* (2007) is a powerful tool to be implemented for our experimental data, and for sure can be analyzed in more detail. However, it is beyond the scope of the manuscript and we will treat future works.

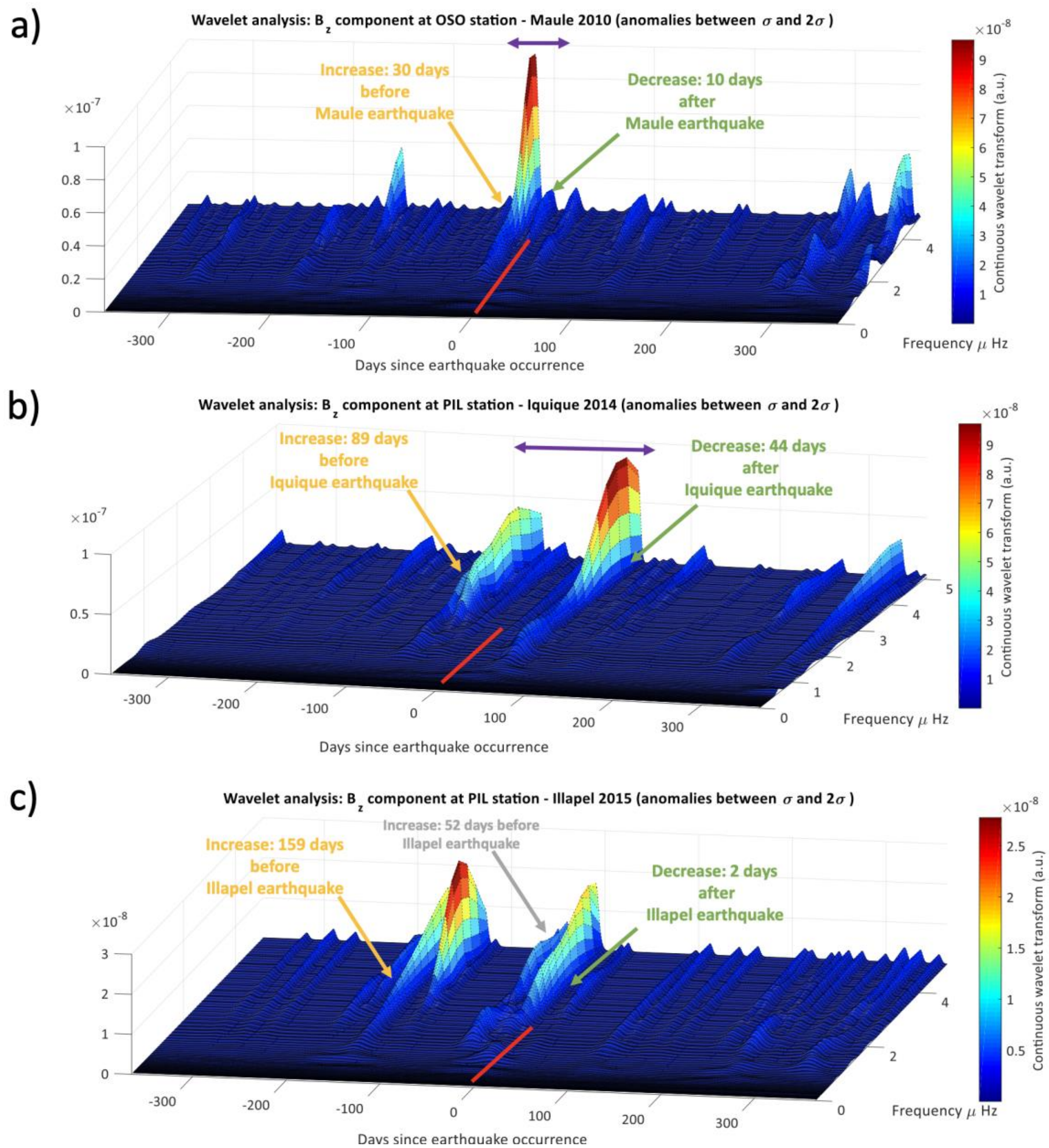
Reviewer:

Since the authors engage the CWT in their analysis i expected to see a scalogram instead of spectrogram In Fig.6 (especially when the Power Spectral Density is expressed in a.u.). Please justify your choice since scalograms are more suitable than spectrograms to highlight variability in real world signals that exist in different scales.

If the authors insist to keep the spectrogram, the choice of temporal window and overlap for spectrograms in Fig.6 must be justified (arbitrary choice or after testing? if is the latter, please provide test results briefly)

Authors:

Thank you for these remarks. In the new version of the manuscript we have added the requested scalograms in the new Figure 5 as it is shown here. Also, we consider that



the spectrogram can also give information. Then, we have also kept. We have discussed in more detail about the methodology.

Reviewer:

Finally the term "Wavelet" in signal processing and data analysis domain refers to the base (mother) wavelet function that used to perform the Wavelet analysis and not to the analysis itself. Please change accordingly.

Authors:

Thank you very much for point out this issue. In the new version it is fixed.