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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Recent seismic activity at Cephalonia island (Greece): a study through candidate electromagnetic precursors in terms of nonlinear dynamics" *by* S. M. Potirakis et al.

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I would like to start my evaluation of the present manuscript of Potirakis et al. with the disclaimer that I have been collaborating with some of the authors of this work on another recent study on pre-seismic electromagnetic emissions, comprising different data and methods. As such, my viewpoint might be considered as not completely independent from those of the authors. However, I have not been involved in the present work and have done my best to present a fair and critical evaluation of the new material.

The manuscript is based on pre-seismic MHz electromagnetic recordings as well as seismicity data prior to two recent earthquakes at Cephalonia Island (Greece). The

systematic existence of distinct electromagnetic signatures prior to at least a certain not yet fully specified class of earthquakes is still a subject of ongoing debates, even though a lot of observational evidence has been provided during the last years. Accepting the latter findings, it is valuable to study the dynamical properties of such emissions and, more precisely, their temporal changes prior to earthquakes in order to contribute to a better understanding of the underlying processes in the solid ground. It is important to note that the present work is far from claiming that relevant dynamical signatures suggested as earthquake precursors can be systematically applied as early warning signals of upcoming events - rather, they should be used as a posteriori diagnostics. In order to avoid possible confusion raised by the utilization of the term "percursor" in the title of the paper, I would recommend to make this point even more explicit in the introduction of the manuscript.

Accepting the general rationale of the present work as being valid, the analysis presented here is scientifically sound and appears technically correct. In this spirit, the identified congruence between critical behavior in EM emissions and foreshock activity and the first-time observation of tricritical behavior in MHz emissions prior to major earthquakes are two important results of the present study that help further shaping our view on earthquake preparation processes in the considered region. Therefore, I recommend publication of this work in Nonlinear Processes in Geophysics, taking into account a few specific comments that are detailed below:

Scientific comments:

1. In all applications of MCF, I wonder about the fitting procedure and model selection. Equation (3) presents a statistical model to be fitted to data that does not provide direct access to proper parameter estimates by simple regression in log-log space. Instead, proper parameter estimates for p2 and p3 would require a "clean" maximum likelihood approach. What is the authors take on this? In particular, one could formulate the identification of critical windows as a model selection problem of comparing the statistical models with p3=0 and p3>0 by means of suitable penalized-likelihood criteria or simi-

lar approaches. I don't find any details on the parameter estimation in the manuscript, but think that since the distinction between the latter cases is an important part of the present analysis, the best and most robust statistical methodology should be applied at this point.

2. For a self-sustained description of the NT method in Sect. 2.2, some minor points should be added to this section: (i) What exactly is Phi (p. 1597, l. 12)? I don't find a corresponding explanation. (ii) Please provide an explicit definition (with equation?) of <D>. (iii) The "theoretical estimation"(?) of the normalized power spectrum (p. 1598, l.10) is not fully clear. Please provide a few more details. (iv) The introduction of a magnitude threshold to NT (p. 1598, l.14) comes very ad hoc; some brief motivation/explanation/background would be desirable.

3. In Sect. 3, the authors study "stationary" time series segments. How has the stationarity been tested? Just by visual inspection or in a strict mathematical sense?

4. One very interesting fact is the observation of VLF anomalies for the same earthquakes as studied in the present work (Skeberis et al., 2015). I would be curious to learn about the authors' opinion on whether (and how) this kind of signal could be integrated with their four-stage model. Which stage could be accompanied by such seismic-ionospheric disturbances, and under which general conditions?

Technical comments:

1. The third and second last paragraphs of the Introduction provide a very (probably unusually) detailed summary of the findings of the present paper, which would better fit to the conclusions section. In the introduction, much less details should be given.

2. p.1593, l.5: Terming "critical phenomena" as a "model" might be a wording that one could discuss about. Some minor rephrasing of the corresponding sentence would help avoiding possible misunderstandings.

3. p.1593, l.13: What exactly is meant by "multiply" here?

4. p.1593, l.16: The terms short vs. long range correlations are typically related to a distinction between exponential and algebraic (power-law) decay of correlations with increasing distance. Is this what is meant here, or do the authors simply refer to increasing spatial correlation lengths?

5. p.1595, ll.17-18: "forming the distribution" sounds a bit strange.

6. The term "laminar distribution" (p. 1599, l. 13, as well as several figure captions) is short but rather imprecise. I recommend using a longer but precise term here.

7. The fourth paragraph of Sect. 3 is an almost literal repetition of the second one with just numbers changed. Just concentration on the differences between the two signals would allow shortening the results on the second one (Fig. 3) considerably. In the same spirit, it is not necessary to have almost identical figure captions in all figures using the MCF. Just give all details once and then refer to the caption of the first of these figures, emphasizing only the differences.

8. Some sentences in the conclusions are literal repetitions from the introduction (e.g., the disclaimer regarding the four-stage conceptual model). I strongly recommend avoiding such self-repetitions. Content-wise recapitulation of results is okay, but just copy and paste sentences should be avoided.

9. In Fig. 1, it is really hard to see the different symbols in front of the green background. Just using the land contours without filling would present a much better visualization option. The same also applies to Figs. 8, 9 and 11.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1587, 2015.