

Interactive comment on “Signatures of the self-affinity of fracture and faulting in pre-seismic electromagnetic emissions” by S. M. Potirakis et al.

Anonymous Referee #1

Received and published: 29 May 2014

This paper presents in principle an interesting combination of already published approaches towards understanding the dynamics of seismoelectromagnetic observations : it has to be discussed , but not before some major matters have been taken care of. Because it promotes ideas of a certain school of thought (which I happen to agree partly with), it is bound to attract intense criticism coming from other schools of thought, which it should be able to withstand successfully. I am afraid that the authors have not shown foresight and have left a number of doors open to that kind of criticism, which may even go as far as to discredit this scientific effort as all. The main door - or rather gapping gate - left open to criticism is that they did not demonstrate the properties and

C949

limitations of the data set used in the analysis. The authors have to present strong evidence supporting the quality of data used. In addition they have to present ALL the cases available and to inform the reader where the method supported by the data and where fails and the most crucial why happened so. An extended discussion and analysis on all available data is missing. I force the authors to reorganize their work including analysis of all the records proposed as earthquake forerunners.

In the analysis of Frequency–size [FS] distribution [paragraph 3.3] the authors have to use the new revised expression by Telesca (BSSA, 2012) and not that presented in eq. (12). In accordance, Eq. (10) has to modified/corrected properly.

The methodology used to define “noise” [page 2994] is rather “fuzzy” and has to clearly specified. Please demonstrate how the Anoise level [defined with your methodology] change the FS distribution and the estimated parameters. References to Tsallis pioneering work has to be added. The 3.5 paragraph has to be clear and readable. The authors have to clarify within the present work, what it is a new contribution and what is just repeated from their previous publications. As a general comment a major reorganization of the ms., to this direction is absolutely required. In the possible new version the authors have to clearly present what is the new approach proposed in the present work and what has already presented and to limit their work in the new part. In addition I would like to formulate several questions –suggestions which authors could follow within the possible revision of the manuscript. i) What is the influence of local earth’s heterogeneity as stated in (i) [page 2985] ? ii) In the introduction the authors refer to the existing Laboratory work in the field. The authors have to include the review by Valianatos, F., Triantis, D., Tzanis, A., Anastasiadis, C., Stavrakas, I. Electric earthquake precursors: From laboratory results to field observations 2004, Physics and Chemistry of the Earth 29 (4-9) , pp. 339-351 iii) In the introduction the authors have to add references on precursors as stated by Varotsos results in Tectonophysics iv) At the end of page 2983 the authors state “. . .in the activation of a single faults”. The authors have to justify why single fault and not network of interacting faults. v) Some results of

C950

their analysis has to views in the frame of V. Uritsky, et al., "Critical dynamics of fractal fault systems and its role in the generation of pre-seismic electromagnetic emissions", *Physics & Chemistry of the Earth*, 29, 473-480, 2004. vi) In page 2990 the authors state ". . . .according to our three-stage model. . .". Is that referred as (i)-(ii)-(iii) in pages 2985-2986 ? Please make it clear. vii) They have to present the measuring systems used to record EM signals in fig 1 and 2, along with the appropriate references. viii) At the end of page 2983 the authors state "Finally, we check whether LABORATORY results are also . . .". I believe that at this part the work fails to present a clear view to the readers. The authors have to reorganize the related part of their work.

Interactive comment on *Nat. Hazards Earth Syst. Sci. Discuss.*, 2, 2981, 2014.