Interactive comment on **"Insight into earthquake sequencing: analysis and interpretation of time-series constructed from the directed graph of the Markov chain model"** ***by*** **M. S. Cavers and K. Vasudevan**

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The authors extend some previous work on a Markov chain approach to describing the spatio-temporal organization of seismicity. Specifically, they consider a given partition of a global catalog of seismic events and analyze the sequence of zone types in this data set to infer transition probabilities between different "states" of global seismicity.

The method used in this work is quite complex and involves multiple steps from data preparation over appropriate coarse-graining to analysis, and the reader is sometimes a bit lost by the associated number of details. A clearer structure of the methodological section 2 together with some more details on the underlying methodological approaches introduced in previous works (particularly regarding the authors previous papers as well as the notion a "network of recurrences") would be very helpful. At the present stage, it is sometimes quite hard to follow the description of the methodology, since important details are either not given (just as a reference to the literature) or found only later in the text (e.g., regarding the choice of optimal time scale Δt). I think that section 2 should be restructured accordingly, maybe some substructure could already help.

Response: A comprehensive summary of our previous work on the selection of Δt and definition of network recurrences is now included in the methodological section 2 is now included.

Beyond this general observation, the work presented here is interesting and (as far as I was able to assess) scientifically sound, so that I would recommend its further consideration for publication in Nonlinear Processes in Geophysics after a thorough revision. Some specific recommendations given below might help the authors clarifying their methodology and results:

1. Do the intrinsic mode functions of EEMD indeed provide an orthogonal basis set? From the classical EMD method, this is not obvious to me.

Response: The orthogonality test carried out, following Huang et al. (1998), suggests that the IMFs resulting from the EEMD here are not orthogonal. We plan additional work to examine this.

2. Please clarify already in the beginning of the introduction what specifically you mean by a "state" in the present case.

Response: Done

3. How unique and/or reliable is the classification of seismic events into the different zones as used in this work?

Response: We would direct the referee to the works of Bird (2003), DeMets et al. (2010), and Kagan et al. (2010) on the classification of seismic events. We have now included the reference of DeMets et al. (2010) paper in the revised paper. The simplification of 52 plate boundaries described by Bird(2003) into 5 major zones by Kagan et al. (2010) is the basis of the present work. It is possible but cumbersome to use the 52 plate-boundaries for the construction of the Markov-chain transition probability matrix for an optimal Δt for a detailed analysis. We defer this to future studies.

4. Please provide some details on the choice of $\Delta t = 9$ days. Just referring to the selection rules of previous work appears insufficient.

Response: We have included this in the revised methodology section.

5. Please add a brief description of the construction mechanism of the "network of recurrences". What does "recurring events in the record-breaking sense" mean?

Response: We have included this in the revised methodology section.

6. When referring to distances (below Eq. (3)), do you mean geodesic distances on the sphere?

Response: We use great-circle distances on the sphere.

7. It is not fully clear what Fig. 2a shows. Please clarify this in the end of section 2.

Response: Done.

8. As suggested by the title of the manuscript, the presented analysis is based on a graph structure which probably varies with time (i.e., the transition probabilities between different states are not constant). It might be interesting to further exploit the potential of classical network approaches for quantification of these temporal changes (or even take a temporal network perspective). This is clearly beyond the scope of the present work, but might be a recommendation for future work.

Response: It is a great idea. We'll consider this in our future work.

9. In section 3.1 (first paragraph), the authors write that EMD "has been recently proposed as an adaptive time-frequency analysis method". This statement mixes the actual purpose of EMD (time-scale decomposition) with the Hilbert-Huang transform based on the derived modes. Some minor

rephrasing would clarify this point.

Response: We'll clarify this in the revised manuscript.

10. From the presented material, it is not fully clear to me what is the advantage of the ensemble approach over classical EMD in the present analysis.

Response: For any non-linear and non-stationary time series with data intermittency, i.e. missing values for some time samples, an ensemble approach to EMD was suggested by Wu and Huang (2004, 2009). Their method allows one to add Gaussian white noise to the time-series before any EMD run. By carrying out this for many realizations of the white noise and by summing intrinsic mode function results from them, there will be a cancellation of the white noise, and at the same time, a final set of intrinsic mode functions for analysis.

Regarding Eq. (4), what type of white noise has been added here? Gaussian noise would violate the non-negativity condition for xsstf and might thus be a bad choice. In turn, if not using Gaussian white noise, how to determine a reasonable variance(-like) property to describe the noise level?

Response: We have not investigated it in this paper.

11. How is the state-to-state transition frequency matrix defined for each IMF separately (last paragraph of section 3.1)?

Response: It is somewhat misleading to use the term "transition-frequency" for the static display of the intrinsic mode functions. We have modified the text to reflect this.

13. What is the interpretation of the two main findings made based upon the Hilbert- Huang amplitude spectrum (last paragraph of section 3.1)?

Response: We include our interpretation in the last paragraph of section 3.1.

14. In the definitions of the Fano factor (Eqs. (6) and (8)), the nominator should include a variance. I don't see why the present formulation corresponds to such a variance.

Response: Our equation strictly follows the previous definition by Telesca et al. (2007). Reference: L. Telesca et al., Space-time fractal properties of the forest-fire series in central Italy, Communications in Nonlinear Science and Numerical Simulation, 12, 1326-1333 (2007).

15. It would be an asset if the authors could draw a link between the results based on Fano and Allan factor analysis and some known findings on the scaling characteristics of earthquake sequences in both space and time.

Response: We have modified the text to include additional information on the usefulness of the Fano factor and the Alan factor in understanding the scaling characteristics of earthquake sequences.

16. p.412, l.21: "top right corner grid" seems to be a quite imprecise statement. Maybe you could refer to Fig. 2B for clarity.

Response: Corrected in the revised manuscript.

17. Fig. 4: Is the unit of the y-axis really "Hertz" (=sec−1)?

Response: Corrected in the revised manuscript.

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