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Interactive comment on "Earthquake sequencing: Chimera states with Kuramoto model dynamics on directed graphs" *by* K. Vasudevan et al.

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Interactive comment on "Earthquake sequencing: Chimera states with Kuramoto model dynamics on directed graphs" by K. Vasudevan et al.

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Vasudevan et al. present a numerical analysis of the Kuramoto model in directed networks constructed with real seismicity data in order to model occurrence of sequence of earthquakes. The authors report that synchronous and asynchronous oscillators can coexist stably in such structures, configuring the so-called chimera states. The subject of synchronization of seismically active faults is interesting and well-motivated. In fact, as remarked by the references cited in the manuscript, there are evidences

that such dynamical process can be described as limit cycle relaxations, a fact that naturally motivates the application of the Kuramoto model and can potentially trigger further research on the topic. However, some points should be clarified/modified in order to improve the quality of the manuscript and to deserve publication in Nonlinear Processes in Geophysics.

Major points: 1. It is stated in the manuscript that "a pulsed-coupled or threshold-coupled oscillator that would accommodate the existence of chimera states" is investigated. Later in this section Eq. 1 is presented, which consists in the Kuramoto model where the oscillators are coupled through the sine of the differences of the phases plus a phase lag α (also known as the Sakaguchi-Kuramoto model). However, the model in Eq. 1 is not a pulsed-coupled or threshold-coupled oscillator model, as motivated in the paper. The Kuramoto model does have a variation that is a pulsed-coupled model and it is the so-called Shinomoto-Kuramoto model (see, for instance, Shinomoto et al., Progr. Theor. Phys. 75 (1988) 1105.; Lindner et al., Physics Reports 392 (2004) 321-424) whose equations of motion are given by following the notation in the manuscript. As it is motivated, the excitable behavior of the model seems to be essential for the study of seismic activity, yet this particular characteristic is not present in the model used in the manuscript. This point should be clarified.

(1) We would like to thank the referee for raising the difference between the phase-lag Kuramoto model and the time-delay Shinomoto-Kuramoto model. Our initial intent is to use the time-delay model in the sense of the Shinomoto-Kuramoto model. We acknowledge the mistake in the paper and we 'll correct this in the revised version. In the present paper, we consider the phase-lag model and will be reporting the results of the time-delay model in future publications.

We agree with the referee on the inclusion of the excitable component in the model to reflect on the seismic activity. In the present paper, we consider the inherent plate motion to contribute to the seismic activity. We'll point this restriction out clearly in the revised manuscript.

2. On page 368 it is mentioned that a constant time-delay is assumed for the ensemble of oscillators. The term “time-delay” is also used in other sentences throughout the paper. I believe that the paper is referring to variable α in Eq. 1. However, strictly speaking, the time-delay in the Eq. 1 is constant and equal to zero. In other words, the interaction between the phases takes place at the same time t in the coupling term,I .e.,sin[θj(t)$-\theta$i(t)]. A time delay τ would cause the interaction between the oscillators to be non-instantaneous as sin[θj(t$-\tau$)$-\theta$i(t)], for instance. There is, however, a special case in which the model with time-delay can be approximate to the phase-lag coupling as used in the manuscript. Namely, if τ âĽł $2\pi/\omega$ then φj(t$-\tau$) $\approx$ φj(t)$-\alpha$, where α = $\tau\omega$ (see the discussion in Sec. 8.1 of Panaggio and Abrams, Nonlinearity 28 (2015) R67-R87 for more details and for other interpretations of the phase lag α). Therefore, the term “time-delay” as it is currently used in the manuscript can be misleading for the readers and should be instead refereed simply as “phase-lag”, unless the interaction is truly non-instantaneous, as discussed. In any case, this issue should be clarified in the text.

(2) We’ll revise the manuscript to accommodate the reviewer’s comments.

3. The conclusion section lists the contributions of the paper. While there are in- deed many interesting points regarding the dynamics of the Kuramoto model in directed networks, remarks on potential insights into the dynamics of the real system are almost absent. For instance, what would be the implications of the emergence of chimera states in the synchronization of seismic events?

3. The implications of the emergence of chimera states in the synchronization of seismic events are: (1) understanding the evolving alterations in stress-field fluctuation in fault-zones frequented by earthquakes in the global sense; (2) considering steps to quantify partially or fully the ratio of the number of synchronized oscillators to the number of asynchronized oscillators; (3) establishing the parameter conditions under which the Kuramoto model could yield chimera states; and (4) improving the mathematical model to work towards generating global chimera-state maps for global seismicity

maps.

The hope is that confirmation of chimera states in earthquake sequencing could signal a possible use of a non-linear dynamical model on directed graphs for earthquake forecasting studies.

Minor points: 1. Probably there is a typo in Eq. 6, otherwise the normalization factor in Eq. 1 âĂÍwould be proportional to 1/N2.

(1) There is a typo in equation 5 since there should not be a divisor N. We’ll correct this in the revised version of the manuscript.

2. It is worth mentioning that synchronization of Kuramoto oscillators in directed networks was already theoretically investigated in other papers, e.g., Restrepo et al. Chaos 16, 015107 (2006).

(2) We acknowledge the referee with thanks for pointing this out. We have modified the text accordingly and included the reference of Restrepo et al. (2006).

3. Are there disconnected nodes in the networks used in the simulations? This could possibly lead to asynchronous nodes, which would not be related to the chimera state.

(3)No; they are not disconnected.

4. The concept of recurrence is widely used in the text, however its definition is given only in the end of Section 4.1 in the sentence: “The quiescence period between earthquakes in an earthquake zone is what we interpret here as recurrence period.” Probably the readability of the manuscript would improve if the term is defined right in the introduction section.

(4) We’ll modify the text in the revised version according to referee’s suggestion.

5. The value of α used in the simulations is not mentioned.

(5) The value of alpha is spelled out in the revised text. It is (pi/2) – 0.10.

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/2/C119/2015/npgd-2-C119-2015-supplement.pdf

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Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 361, 2015.