

Interactive comment on “Sandpile-based model for capturing magnitude distributions and spatiotemporal clustering and separation in regional earthquakes” by R. C. Batac et al.

Anonymous Referee #2

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I think that overall the article is well written and the presented results are sufficiently clear, even though some passages are poor of references and some of the methods that were used are not fully explained.

In more detail:

1] The authors refer to the Zhang sandpile, introducing a deterministic toppling rule (the stress from the toppling site is equally redistributed between its nearest neighbors). It is known that the deterministic sandpile exhibits anomalous multi-scaling because of the breakdown of ergodicity caused by the existence of many toppling invariants [Bagnoli et al. 2003 Europhys. Lett. 63 512]. (If it isn't too annoying) I would suggest to choose a random toppling rule instead (that guarantees the model to belong to Manna

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universality class).

2] Why is the exponent of the avalanche size probability density functions ($\alpha=1.6$) so different from the exponent found for 2-dimensional sandpiles under synchronous updating rule ($\alpha=1.26$) -even in the limit of $p=0$ - ? I think that this should be discussed or some literature should be cited. The only cited article with respect to this issue is Paguirigan et al., 2015, which deals with the introduction of sinks leading to non-conservation. I would suggest to clarify how does this relate to asynchronous updating rule. The other cited article (Lubeck, 1997) compares the static and dynamical properties of Zhang sandpile (the same that is considered here) with those of the Abelian sandpile model of Bak, Tang, and Wiesenfeld, stating that the exponents of the avalanche probability distribution are the same. I think that some more (and more relevant) references should be cited here, in order to give stronger evidence for the appearance of the exponent 1.6 (given that it appears to fit really well with the experimental data).

4] In Figure 2 (b)-(d) I guess that black dots represent shuffled data but I think that it should be written explicitly for better readability.

5] The authors state and show in Fig. 2 that the distribution of the spatial distances between events becomes bimodal for some values of p , stating that "bimodality [in the experimentally observed distributions] is due to the difference in the characteristic times of the correlated aftershock sequences and the independent mainshocks". I think it would be useful that the authors discuss why such distribution becomes bimodal in the model, for some intermediate values of p .

6] I think that the "calibration with real-world data" procedure is not perfectly clear. I would suggest to explain the procedure in more detail. As it is, I can imagine it is only a way to plot simulations results and experimental data on the same plot, in other words it is an artifact that allows to compare quantitatively two data sets that in principle can only be compared qualitatively but that does not explicitly add any new information to

the article.

7] In Line 3 page 6 the authors write: "The GR law [...] can be shown to be equivalent to an energy E CCDF ...". I think it would be useful to cite here where it is shown.

8] At the beginning of the Discussion (Line 5 page 6): Is "b" defined somewhere?

Typo mistakes:

9] Line 4-5 page 3 "the stress [...] are transferred"

10] Line 29 page 7 "the we find"

11] Line 2 page 8 "is and intuitive results"

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