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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Sandpile-based model for capturing magnitude distributions and spatiotemporal clustering and separation in regional earthquakes" *by* R. C. Batac et al.

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RC - Referee's Comments

AR - Authors' Response

RC. 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.

AR. We thank the referee for his/her critical comments that helped improve the readability and further highlighted our important results. In the following, we offer a point-by-point response to his/her concerns.

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In more detail:

RC2.1. 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 universality class).

AR2.1. The referee offers a useful suggestion that we may consider for a future work. In the paper, however, one of our main arguments about the novelty of the work is the fact that it has captured the spatiotemporal signatures of seismicity while introducing very small tweaks in the sandpile model, which does not produce the same spatiotemporal signatures (see also RC1.1, AR1.1). As such, we opted to focus instead on the current model presented, while pursuing further investigations along the referee's suggested direction.

ACTION2.1. A short discussion on the sandpile model's inability to reproduce the statistical signatures of seismicity is added in the Introduction of the paper.

RC2.2. 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

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(and more relevant) references should be cited here, in order to give stronger evidence for the appearence of the exponent 1.6 (given that it appears to fit really well with the experimental data).

AR2.2. The referee raises a valid point, which we have also noted. In the revised paper, we added a short discussion on the possible reason for the occurrence of slightly steeper power-law distributions.

The asynchronicity, which, in here, is defined as the addition of a single trigger value to a randomly chosen (or, in this case, preferred) site, tends to produce isolated regions that are near the threshold value. Because of the lack of global connectivity among such sites (which would have been possible if synchronous updating were used), we observe the preponderance of small avalanche events that affect only small local neighborhoods at a time. This, in turn, leads to a corresponding decrease in the occurrence of very large-area avalanches.

ACTION2.2. The above discussion is included in the revised text.

RC2.4. 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.

AR2.4. We thank the reviewer for pointing out this oversight. The insets in Figure 2 (b)-(d) correspond to shuffled sequences for both the data (colored markers) and the model (black dots), to provide a comparison between the statistical distribution of the data (i.e. earthquake sequences with memory) and a randomized sequence (i.e. no memory), similar to the work done by Batac and Kantz [NPG 2014]. This also leads to the crossover value R^* where the data and the shuffled sequences begin to show similar trends; this, in turn, will be used for creating the conditional distributions Tin = $\{T \mid R \leq R^*\}$ and Tout = $\{T \mid R > R^*\}$.

ACTION2.4. In the revised paper, we emphasized this procedure by removing the insets of the original Figure 2 and plotting the same in a new figure, for clarity.

RC2.5. 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.

AR2.5. The p describes the persistence of the system to target the most susceptible site in the grid, which is most often lies at the vicinity of a previous avalanche. Consequently, this gives preponderance for small spatial separations, thereby producing a bimodal pattern in the trend, similar to what is observed empirically. Unlike previous implementations, however, the p is just a stochastic term, and does not “force” the production of small-distances, by, say, pre-selecting the region to perturb. In essence, we have utilized a property of the sandpile grid; by only imposing that the targeted triggering be at the most susceptible site, we have obtained short characteristic distances because the most susceptible site is oftentimes within the vicinity of a previous collapse.

ACTION2.5. The above discussion is introduced in the revised paper.

RC2.6. 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 world it is an artifact that allows to compare quatitatively two data sets that in principle can only be compared qualitatively but that does not explicitly add any new information to the article.

AR2.6. This point has also been raised by the other referee (see RC1.4), and represents a key idea that needed further elucidation in our paper.

We thank the referee for pointing out that the procedure we conducted is indeed a simple rescaling of the model results for a better visual comparison with the data. How does this change in perspective affect the importance of the results? We would like to think that the model results are still important and novel. If the model results were not in correspondence with those of the empirical data distributions, then we would expect complicated rescaling functions to “match” the model with data. However, for all our results, we find that the model results easily correspond with the empirical data with just a simple multiplication by a scalar, which, in effect, is similar to just converting one cell unit into actual physical length and one iteration into a unit of time. Finally, we found that the scaling factors used for making the visual correspondence can be derived from a physical basis, and not arbitrarily obtained.

ACTION2.6. We provided a change in perspective and removed any mention of “calibration” in the revised text. Instead, we present the results as simple visual comparisons. The choice of the scaling factors used has also been explained in detail.

RC2.7. 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.

AR2.7. For this purpose, we use the discussion given in the Introduction of the paper by Jagla [Phys. Rev. Lett. 111, 238501, 2013] to explain how the exponents are derived.

ACTION2.7. The above reference is cited in the mentioned discussion.

RC2.8. At the beginning of the Discussion (Line 5 page 6): Is "b" defined somewhere?

AR2.8. The GR law is usually given in terms of magnitudes M and introduced in the complimentary cumulative distribution (CCDF) form given by log_10âĄąN=a-bM, where b gives a constant value close to 1 for regions that are seismically active.

ACTION2.8. The above discussion is added in the text.

Typo mistakes:

RC2.9) Line 4-5 page 3 "the stress [...] are transferred" RC2.10) Line 29 page 7 “the we find" RC2.11) Line 2 page 8 "is and intuitive results"

AR2.9-11. We have corrected the specified typo errors.

ACTION 2.9: The text now reads: "the stress [. . .] is transferred. . ." ACTION 2.10. The text now reads: "In Figure 2(b)-(d), we find that the. . ." [see also RC1.10] ACTION 2.11. The text now reads: "is an intuitive result. . ."

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/npg-2016-28/npg-2016-28-AC2-supplement.pdf

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