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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.

**R. C. Batac et al.**

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RC - Referee Comment

AR - Authors' Response

RC. It stroke me at the first read, how much the context and references to previous or related works were lacking. There are few references to recent works related to sandpiles and other similar (lattice, mesoscopic) models. I think the authors should not limit their readings to models that explicit mention sandpiles or OFC in their titles, as there is a whole family of other models which share many interesting properties, and are morally very similar, if not exactly mapped to some particular variants of sandpiles.



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In this respect, I am not completely sure that the results presented here are completely novel. They are probably novel to some extent, but should be compared with existing, close-by literature.

Overall, the paper is not badly written, but there are important things that are not easy to grasp, like the precise definition of how things were fitted or "calibrated", and the definitions of the quantities plotted in the figures. I detail what I mean by this in the points below.

AR. We thank the referee for his critical review that helped us in clarifying the context and novelty of the work. Our focus on the early works on the sandpile model, upon which our model is based, resulted in a limited context, i.e. how our model is situated relative to other discrete modeling approaches. In the following, we hope to address the points that he raised to clarify the results and to further highlight the novelty of the work.

RC1.0. About context: The following forest-fire model should, I think, be put in perspective with your work, as it shares some of its ingredients: "Forest-Fire Analogy to Explain the b Value of the Gutenberg-Richter Law for Earthquakes". More recently, the same author (and others, see references inside or citing it), did a paper dealing precisely with how the loading protocol affects the statistics of events in plasticity/fracture: "Avalanche-size distributions in mean-field plastic yielding models". That one is less tightly connected to your work, but it compares two kinds of driving (random and uniform), and it belongs to the family of models using "extremal dynamics" (loading all sites until exactly one is triggered).

In a sense, your protocol is quite similar to doing: - uniform loading at most steps. - with small proba p, trigger the most susceptible site by adding to it the needed stress (and maybe add the a fraction of that amount of stress to every other site). (in extremal dynamics, one adds the same amount of stress to all sites) I think it would actually increase the impact of your paper to connect your protocol and results to these other existing protocols and results. You may want to check out, also, and see for yourself how relevant the following papers are: -"Universality in Sandpiles, Interface Depinning, and Earthquake Models", Paczuski et al (1996). -"Avalanche size distributions in mean field plastic yielding models" - (about SOC, 1 example:) "Dynamic scaling in stick-slip friction" (2005) - "Strain localization and anisotropic correlations in a mesoscopic model of amorphous plasticity", where "extremal dynamics" is used (there are countless others, not every author focuses on the dynamics and uses these words).

AR1.0. While we acknowledge the need for comparison with results from various classes of similar models, we would like to further clarify the protocol used in the paper, which is quite different from what the referee mentioned in his review.

Our protocol is based on the original rules of the sandpile model. Specifically, the external driving or growth is limited to one specific site that is randomly chosen. Unlike the OFC-based classes of models, our protocol does not involve uniform loading at all other sites. This main difference clearly sets us apart from most of the papers he recommended, majority of which require the uniform loading for all the sites.

We believe that, with this clarification on the nature of the protocol used, we have further clarified the novelty of the paper. Still, the references he mentioned have helped us to establish the similarities and differences of our work with others.

ACTION1.0. Wherever possible, we have placed additional references to put the paper into context. Most of the papers we added are the ones suggested by the referee.

Aside from the context-building, other remarks: RC1.1. Several times, you mention the similarity with the original sandpile model as a good thing, in particular the fact that the model is SOC seems to be very positive. For instance in the introduction, you say: "make the model more truthful to the original sandpile design, presenting a clear association with seismicity and SOC.". Why do you consider SOC and sandpiles to be a good thing by itself?

As Fisher noted in his 1998 Review: "Whether critical behavior is considered “selforganized” or not is somewhat a matter of taste: if the systems we are considering are driven at very slow velocity, then they will be very close to critical. In another well known situation, when a fluid is stirred on large scales, turbulence exists on a wide range of length scales extending down to the scale at which viscous dissipation occurs. In both of these and in many other contexts the parameter which is “tuned” to get a wide range of scales is the ratio of some basic “microscopic” scale to the scale at which the system is driven."

I don’t think I could explain better than him: SOC is nowadays, to many people, not a particularly relevant characteristic. Often, what is called SOC is just a model where the critical point is at 0 or infinity. In your case, it is the system size which acts as a limiting size for the avalanche (dissipation occurs only at the boundaries in your model, not in the bulk, if I understood well.) By the way, your driving depends on the system’s state, which in a sense can be seen as a feedback loop... thus giving a weaker "self-"organized structure to the problem.

I think this perspective on SOC and sanpiles should be updated/deleted, as they do not seem to bring anything to the paper. If you persist in liking SOC so much, you should give some explanation of why it is such a good thing that your model has some SOC in it.

AR1.1. The point being raised by the referee is a valid one, to which we also agree. In fact, for this work, the SOC idea has no particular usefulness, as far as the results are concerned. What we would like to highlight, which has not been communicated clearly in our current paper, is the fact that our paper has introduced minimal changes into the original sandpile model, which is the paradigm model of self-organized criticality. That the model is close to the original sandpile rules and may therefore retain SOC characteristics (although, the referee’s point on the feedback is also valid) is just one of our results.

Why, then, a particular emphasis on the closeness to the sandpile model? Apart from the fact that it is one of the earliest discrete models of complexity, the original sandpile model is not able to capture the space and time characteristics of seismicity. Rules based on single triggering at random directions will result in normal (Gaussian) distributions of inter-event distances and times, which is not observed for seismicity. This, we believe, is the reason why subsequent models had to introduce uniform loading at all sites (see RC1.0 and AR1.0), along with asymmetry in the redistribution rules and dissipation in some cases. Here, we introduced a simple bias for a fraction of triggering times (and this fraction is not large, with around 10-3-10-2 recovering similar statistics as the data) and recovered both the interevent distances and time distributions, along with the magnitude (energy) distributions. The simplicity of the change introduced on the original sandpile and its corresponding recovery of the spatiotemporal statistics is one of the strengths of our paper.

ACTION1.1. We revised the paper accordingly to be able to emphasize our motivation and the novelty of our work. The Abstract and the Introduction now removes any mention of SOC; instead, we highlighted the fact that the original sandpile is unable to account for such observations, and explained how our model, with minimal parameters introduced, was able to recover similar statistical features of seismicity.

RC1.2. p.3, line 8: You say: "The number of affected sites in the grid, A, is used as a proxy for the actual energy ...". I think you should count the number of activations, not the number of sites activated once or more. If some sites are activated twice (or more), they should count twice (or more). Given that you put no dissipation, I guess it can occur ... maybe often? If this is indeed what you measured, be more clear. If you did not, you should show both quantities. The number of sites activated (irrespective of the number of activations) represents the area of EQs. The number of activations represents the seismic moment (energy released).

AR1.2. The choice of presenting the number of sites activated (the area A) is made to further strengthen the similarity of the model with the simple sandpile. To us, this simplicity in the model dynamics is still the most important feature of the work.

We understand, however, the referee's concern. Apart from A, many other parameters can be used for characterizing the magnitude or scale of an event. For example, one can use the actual stress value (which we can call S) that was distributed among the neighborhoods during an avalanche event. We have used this metric in a previous work on landslides (Juanico, et al. Geophys. Res. Lett. 35, L19403, 2008), and we have observed that it scales as S âĹİ A3/2.

Here, the referee proposes that we count the number of actual activations (we can call this V). We believe that the motivation for tracking V is the fact that it may be closer to the actual dynamics of energy release during an earthquake event. In the following Figure 1, we show the probability density functions Prob(V) and the behavior of V(A).

Figure 1. (a) Prob(V) shows a similar behavior as Prob(A) [not shown here but present in the manuscript], i.e. it is quite robust to variations in p. (b) The V(A) plot shows a scaling behavior V âĹİ A3/2 for p = 0; at the extreme case of p = 1, the scattergrams show dual scaling, with a second scaling V âĹİ A4/3.

The probability distributions of V behave similarly as those of A (see manuscript) in their robustness to the introduction of p. The obtained scaling exponents are slightly different from those of p(A), however, due to the nonlinear scaling of V(A), as shown in Figure i(b). Near the p = 0 case, the scaling behavior is around VâĹİA3/2 suggesting that V is similar to S as a metric for the energy or volume. In the extreme case of p = 1, the scaling changes to approximately VâĹİA4/3, which can be easily explained by the nature of p; if p = 1, the most susceptible site will be targeted every time, which means that there will be minimal cases of reactivation, because the neighboring sites would always be depleted; the same area, therefore, will correspond to slightly lower volumes.

On the matter of correspondence: Because both the V and the A represent a relative measure of the extent of the relaxation of the system, and in fact show a scaling relation, we believe that both are equally valid representations of the energy being released in an earthquake.

ACTION1.2. We included the above discussion in the revisions.

RC1.3. same place: is used as a proxy for the actual energy or magnitude of replace "magnitude" with "seismic moment", as only this quantity has the dimension of an energy. Magnitude is the log (up to a prefactor) of the seismic moment.

AR1.3. We thank the referee for this correction.

ACTION1.3. We have revised the statement according to the referee's suggestion.

RC1.4. A general, important criticism: it is not clear how much you calibrated to get results to fit experimental data. More precise, yet clear explanation/discussion of the number of degrees of freedom (fitting parameters) in your fits, would be welcomed. Otherwise, the whole point of the paper (i.e. that your fits are rather good and relying on few fitting parameters) is compromised.

In this respect, I find the figures not very clear. In general, the methods should be clearly explained. Using a few more explicit sub-titles along your presentation may help.

AR1.4. This point, which has also been raised by the other referee (see RC2.6), is an important one that we would like to address in our revised paper. Upon reviewing our results, we realized that the term "calibration" might be a bit of a stretch. In fact, for the most part, our paper has presented analogies and similarities, and, although we believe that there is a correspondence between our model parameters and the actual conditions on the ground, we did not attempt to find such an exact relation.

As such, we concur with the second referee's opinion that the procedure we conducted is a simple rescaling of our model results for visual comparison with the empirical distributions. This change in terminology and perspective, we believe, does not diminish the value of our model results. The fact that such a comparison is even possible with just a simple multiplication by a scalar is a testament to the feasibility of the model for capturing the features of the seismicity. Moreover, the rescaling parameters that we used may be explained using actual physical bases, and not obtained arbitrarily. The origin of the rescaling factors is better explained in the revised paper.

ACTION 1.4. We revised the paper to remove any mention of "calibration" and to instead reflect this change in perspective. The origin of the scaling factors for both R and T has also been explained in detail.

RC1.5. Related to my point (1): discuss also maybe, how often (what proba) is it that the site triggered was the most susceptible site of the state after the previous avalanche? By this I mean, if you record the position of the most loaded site after an event, how often is it that the triggering site of the next event is precisely the same site? I suspect this is much larger than p. I think it is good to discuss this, as it is a natural question the reader may have, and it could help relate your model to others.

AR1.5. We thank the reviewer for this helpful insight. As expected, due to the random nature of the triggering, in some instances, the most susceptible site will be targeted even without the action of the targeted triggering probability.

In Figure 2 below, we present the results of sample runs for different p values (grid dimension L = 256, iteration time T = 107), wherein the "natural" triggering of the most susceptible site (i.e. without the action of p) is tracked alongside all the instances of such triggering (i.e. including the targeted cases). The natural triggering is found to be hovering about its expected value, which is [1/L2]T, = 153 where 1/L2 is the random chance of the most susceptible site to be targeted in the grid. The effect of the targeted triggering probability p is found to be order of magnitudes greater than this baseline value.

Figure 2. (red) Baseline values of the natural triggering of the most susceptible site for p = 0 compared with (blue) the total triggering for nonzero values of p.

ACTION1.5. The above discussion is incorporated in the revised paper.

RC1.6. By the way, in your model, is the area VS energy scaling in some way? These features are expected to have multiple scaling behavior, is it the case in your model? I recently published a study of various models, discussing this particular observable as a benchmark of model's quality, i.e. the area-magnitude scaling relationship: "Scaling laws in earthquake occurrence: Disorder, viscosity, and finite size effects in OlamiFeder-Christensen models" This can be a tricky thing to study, and the fact you do not recover expected natural data behaviour for this observable does not discard your model as uninteresting. I am just suggesting this as possible directions for future work.

AR1.6. (See also AR1.2) In Figure i(b), we show the scatter plots of A vs. V. Although we have not investigated the scaling behavior of these two parameters in detail, the plots show that the p = 0 case (original sandpile) follows a single scaling function VâĹİA3/2. On the other hand, the other extreme case of p = 1 shows an asymptotic behavior towards VâĹİA4/3. Visual inspection of p = 1, however, appears to show that the smallest A values follow the VâĹİA3/2 trend, up to a certain value. This preliminary analysis, however, may need to be checked more thoroughly for various p values.

ACTION1.6. As this result may need additional analyses, we leave out the discussion of this result in the revised paper.

RC1.7. figures: do not write PDF, but rather P(A), P(E), P(T), etc. (or Prob(A), etc, as you prefer). It would be more clear.

AR1.7. The original intention was to not use P(A), etc., to avoid any possible confusion of P with the parameter p in the model. But we agree with the referee that the use of a generic "PDF" label is confusing. In this case, we followed the referee's suggestion and used Prob(A), etc., wherever applicable.

ACTION 1.7. All figures that show a probability density plot now has y-axis labels of



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Prob(. . .).

RC1.8. "... observed in the generation of earthquakes, which, despite regional differences, produce universal GR distributions.". This statement is rather controversial, and should be supported at least by a citation. One should not be overly confident with Per Bak's statements, (which were overly enthusiastic about SOC... and sometimes plainly wrong).

Geophysicists are quite interested in knowing the GR law region by region. It has regional differences, and integrating over all regions does not necessarily carry lots of physical meaning.

AR1.8. We acknowledge that the statement may be quite controversial and does not represent the prevailing consensus among researchers in the field. We thank the referee for this comment.

ACTION1.8. We have revised the statement to properly put the result in context.

RC1.9. "However, for the threshold Ath=50 used, we have not seen the power-law regimes due to the ..." I did not understood the definition of quantity $\mathrm{A}_t h. Isitgivensomewhere? Ifnot, giveit. Ifso, makeitmorevisible.$

AR1.9. The point being made here is the fact that in the model, all the avalanches can be recorded down to the smallest possible ones. In contrast, for seismicity, there is a limit to our capability to record the smallest earthquake events; apart from the fact that they are too weak for accurate identification, the GR law predicts that their occurrence will be orders of magnitude greater than the lower-magnitude events. To mimic this limit in the empirical data, we introduced a threshold magnitude Ath, wherein avalanche events A < Ath are removed in the series.

ACTION1.9. We added a discussion of the Ath and the motivation for their use.

RC1.10. "In Figure 2(b)-(d), the we find that the rescaled model statistics for p=0.007 show good agreement". Correct the typo.

AR1.10. We have corrected the typo as noted by the referee.

ACTION 1.10: The text now reads: “In Figure 2(b)-(d), we find that the…” [see also RC2.10]

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/npg-2016-28/npg-2016-28-AC1-supplement.pdf

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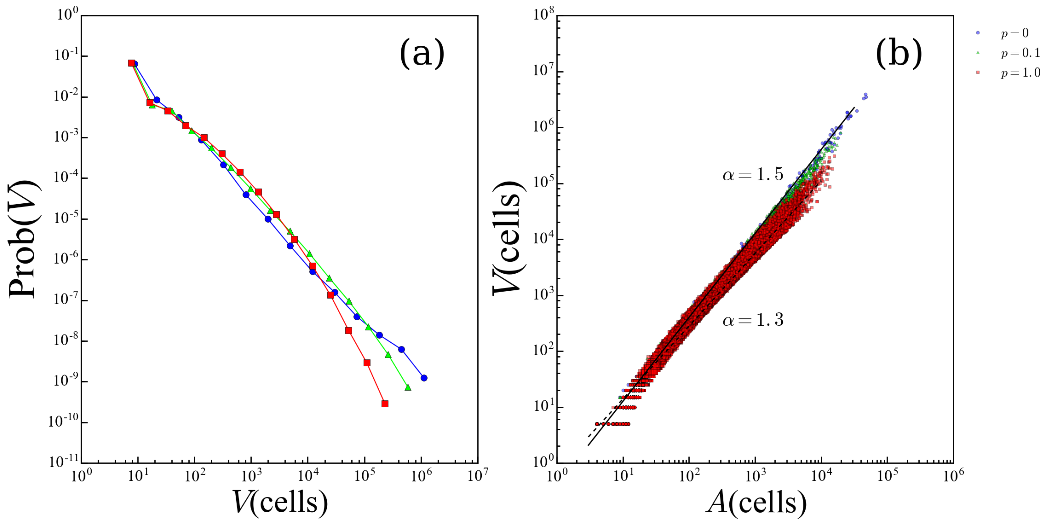


**Fig. 1.**



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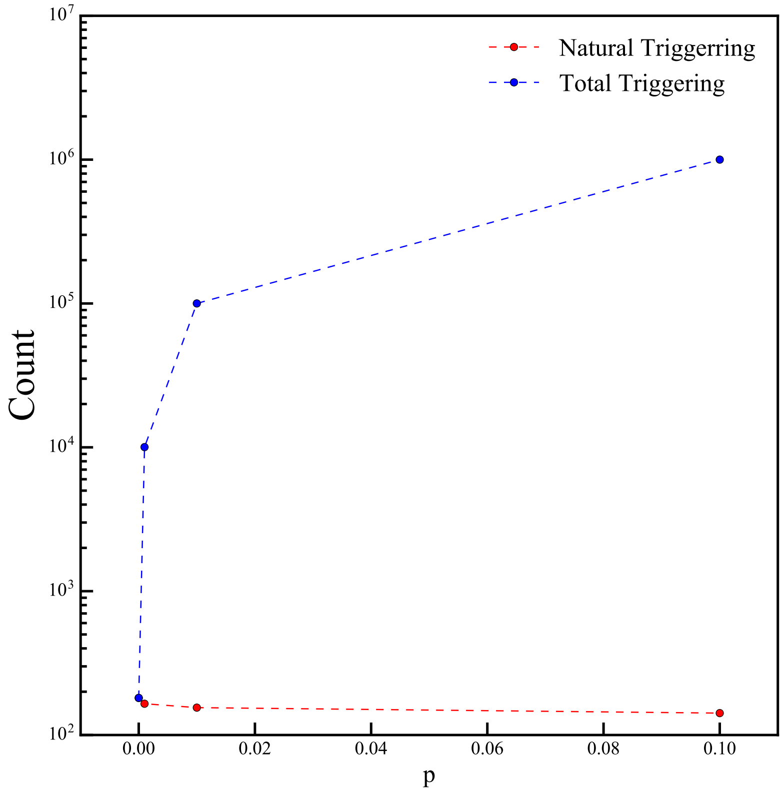
Natural Triggerring
Total Triggering
Count
p
10^7
10^6
10^5
10^4
10^3
10^2
0.00
0.02
0.04
0.06
0.08
0.10

**Fig. 2.**