

# Sandpile-based model for capturing magnitude distributions and spatiotemporal clustering and separation in regional earthquakes

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**RC – Referee Comments**

**AR – Author Response**

**Response to Comments: Referee #1 François P. Landes** (francois.landes@gmail.com)

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. 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.

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  $S \propto A^{3/2}$ .

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 i, we show the probability density functions  $\text{Prob}(V)$  and the behavior of  $V(A)$ .

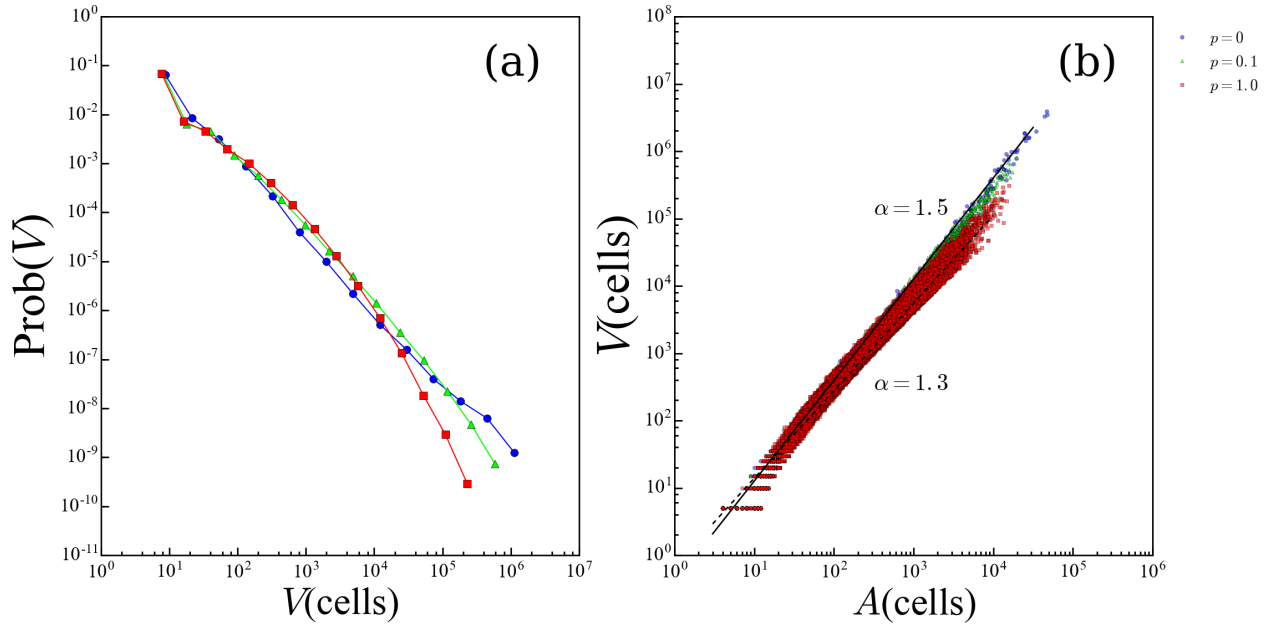


Figure i. (a)  $\text{Prob}(V)$  shows a similar behavior as  $\text{Prob}(A)$  [not shown here but present in the manuscript], i.e. it is quite robust to variations in  $p$ . (b) The  $V(A)$  plot shows a scaling behavior  $V \propto A^{3/2}$  for  $p = 0$ ; at the extreme case of  $p = 1$ , the scattergrams show dual scaling, with a second scaling  $V \propto A^{4/3}$ .

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  $V \propto A^{3/2}$  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  $V \propto A^{4/3}$ , 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 ii below, we present the results of sample runs for different  $p$  values (grid dimension  $L = 256$ , iteration time  $T = 10^7$ ), 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  $[1/L^2]T = 153$  where  $1/L^2$  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.

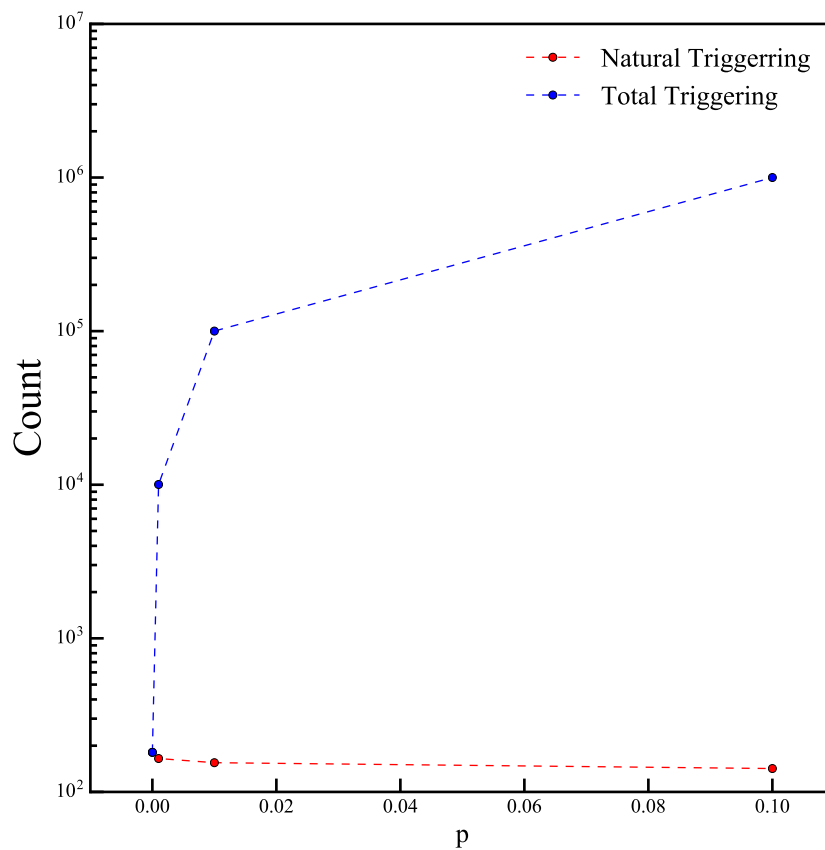


Figure ii. (red) Baseline values of the natural triggering of the most susceptible site for  $p = 0$  compared with (blue) the total triggering for nonzero values of  $p$ .

**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  $V \propto A^{3/2}$ . On the other hand, the other extreme case of  $p = 1$  shows an asymptotic behavior towards  $V \propto A^{4/3}$ . Visual inspection of  $p = 1$ , however, appears to show that the smallest  $A$  values follow the  $V \propto A^{3/2}$  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  $\text{Prob}(A)$ , 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  $\text{Prob}(A)$ , etc., wherever applicable.

**ACTION 1.7.** All figures that show a probability density plot now has y-axis labels of  $\text{Prob}(\dots)$ .

**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  $A_{th}=50$  used, we have not seen the power-law regimes due to the ..." I did not understand the definition of quantity  $A_{th}$ . Is it given somewhere? If not, give it. If so, make it more visible.

**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  $A_{th}$ , wherein avalanche events  $A < A_{th}$  are removed in the series.

**ACTION1.9.** We added a discussion of the  $A_{th}$  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**]



## Response to Comments: Referee #2 Anonymous

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.

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.

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 (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).

**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  $T_{in} = \{T \mid R \leq R^*\}$  and  $T_{out} = \{T \mid R > R^*\}$ .

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 quantitatively 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  $\log_{10} N = a - bM$ , 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...”