10 January 2017

Dr. Richard Gloaguen
Editor
Nonlinear Processes in Geophysics

Dear Dr. Gloaguen:

We are attaching herewith our revised manuscript npg-2016-028 titled **Sandpile-based model for capturing magnitude distributions and spatiotemporal clustering and separation in regional earthquakes**, along with our response to the referees' comments.

We carefully considered all the points raised by the referees, which, we believe, improved the general readability and presentation of our results. More importantly, one of our major change for this version is on the interpretation of the measures used for the avalanche event sizes. While still presenting the avalanche area distributions as was done in the previous works we have cited, we noted that the number of activations is a better reepresentation of the energy being released in the earthquake events.

For this round of reviews, we have attached our point-by-point responses to the referees' comments, a list of our major changes, and a detailed list of corrections from our previous version. We hope that the revised manuscipt will be better suited for possible publication in the journal *Nonlinear Processes in Geophysics*.

We thank you for your consideration.

Sincerely,

Rene C. Batac
Antonino A. Paguirigan
Anjali B. Tarun
Anthony G. Longjas

Manuscript Number: npg-2016-28

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

Rene C. Batac, Antonino A. Paguirigan Jr., Anjali B. Tarun, and Anthony G. Longjas

Response to Referee Reports

RC – Referee's Comments

AR – Authors' Response

Referee #1: Dr. Francois Landes, francois.landes@gmail.com

I think the authors have substantially improved the paper. There are still a few points that need to be improved, but the overall quality is now much more satisfactory.

We thank the referee for agreeing to review the paper again. His suggestions have significantly improved the quality and overall presentation of the paper, both in this version and the previous one. We have incorporated additional revisions to the paper to address his further comments below.

RC1.0. General comment:

Please use floating point notation (e.g. 1.5 instead of 3/2), when you have no prediction or argument for the expoenents to take these simple rational values. Otherwise it is misleading, as usually when an exponent is *exactly*U equal to a ratinal number, there is (relatively) simple argument explaining that result. In your paper I believe most exponents are irrational numbers.

AR1.0. We agree with the referee's comment, and revised the paper accordingly. We have, in fact, conducted additional work to check whether the "nice-looking" rational exponents will come out from theoretical origins. As most of these works are still being pursued, and will not be included in the paper, we will use the empirical fits reported in decimal form.

ACTION1.0. In the text and in the figures, the exponents are reported in decimal form instead of fractions.

RC1.1. [In response to previous review item 1.0]

Ok, I see you added some litterature, I haven't compared in detail but reading the whole paper I found it more clear.
I see you cited Landes and Lippiello 2016, this is nice but not very necessary: please do not feel like you should cite me because I'm refereeing. Cite if you truly believe it is relevant.
I think you would gain readership by further putting things into context in a precise way, comparing your results quantitatively with other model's, but that may also be for a separate publication, it's your choice. Now the reader is not lost, I think.

AR1.1. On this account, many of the additional papers cited for breadth have been suggested by the referee in the previous review, for which we are thankful. We agree that these citations have placed the work in a better context, while still focusing on the simplicity of the approach.

We have cited Landes and Lippiello (2016) because it introduced us to the different physical quantities associated with the area A and activations V. Prior to the review, we only thought of these model metrics as two different ways of representing the relative magnitude of an event in the grid. Through this paper, though, we have seen that these two quantities may actually be associated with area and seismic moment, respectively. The citation, therefore, is important. In fact, we have revised our interpretation of the results based on this suggestion [see **RC1.4** and **AR1.4**].

We sincerely hope that the revised paper will be more understandable to a wider readership. We again thank the referee for his critical assessment.

RC1.2. [In response to previous review item 1.1] Ok, good.

AR1.2. We thank the referee for clarifying the association (or lack of it) with self-organized criticality (SOC) in the previous review item 1.1. In the revised paper, we preserve the discussion of the main motivation of our model, which is to be able to simulate the statistical distributions of earthquakes through a sandpile-based model.

RC1.3. [In response to previous review item 1.2]
Thank you for this nice discussion and adding these itneresting results.
You should add one or two tentative fits and their corresponding power-law exponents in Fig 4.

AR1.3. The new results are based on the referees suggestions. Figure 4(a) now includes a power-law $V^{-1.45}$ as a guide to the eye. We revised the paper as suggested.

ACTION1.3. The new Figure 4(a) is revised to include a power-law guide to the eye. The repeated use of the parameter α, already used in Figure 1 to denote the Prob(A) exponent, is also avoided; in Figure 4(b), the V vs. A scaling is now represented by the scaling exponent δ.

RC1.4. [In response to previous review item 1.2] About your last comment on 1.2, a remark:
you say that V and A are equally representations of the energy .... but it's like saying that velocity v and kinetic energy E_k of a system are equally valid representations of its temperature: instead, we have |v|^2 ~ T ~ E_k, not |v|~T~E_k.
Exponents change if you use a variable other than the correct one (or not proportional to it).
If P(A) ~ A^-1.6 and V~A^1.5 for instance, using P(A) dA = P(V) dV , you get (I think) P(V)~V^-((1.6-1.5+1)/1.5)=V^-1.4.
I do not think it is crucial for your results that the exponents match very well: the key result is that you have bimodal statistics in both space and time distributions which appear as a result of introducing p. So even if the exponent of fig 4 is not very close to the famous 5/3=1.6666666 you wish for, it's ok, your paper is worthwhile (amyway the "true" value of b is very debated).

AR1.4. This comment, along with the analogy presented, made us appreciate the difference between the two measures we have presented [see also **RC2.2** and **AR2.2**]. Interestingly, the Prob(V) plots appear to follow the trend of V^(-β), where β is between 1.4 to 1.5, as predicted by the referee from the scaling arguments above; we have presented the β=1.45 in Figure 4(a) as a guide to the eye.

Therefore, in the revised paper, we stick with the presentation of Figure 1, where the earthquake energy distributions are placed side by side with Prob(A). This is in keeping with the earlier works on sandpile-based approaches, most of which have tracked only the area A. For Figure 4, on the other hand, a more mechanistic measure of the actual energy released is obtained through V. As the referee noted, the slight differences in exponents are not an issue; the goal is not to completely replicate the exponents. But it is worth noting that β is still close to the obtained empirical exponent.

ACTION1.4. The above-mentioned points are highlighted in the revised paper.

RC1.5. [In response to previous review item 1.3]

Ok... So you elected to call M (and sometimes m) the magnitude and m (?) its threshold... why not use m always and m_th for the threshold ?

AR1.5. We did as the referee suggested.

ACTION1.5. We used m_{th} to denote the threshold magnitude and m for the magnitude in the revised paper.

RC1.6. [In response to previous review item 1.4]

Ok, excellent, this point is now much clearer to me and clearer in the paper.

AR1.6. We thank the referee for this comment.

RC1.7. [In response to previous review item 1.5]

I appreciate your work, but cannot find this discussion in the revised paper. Where did you include (part of) this discussion?

Let me add a comment for you:
what I was trying to exaplain is that because of this effect (of Fig ii of your reply), your model may be described by "count", the y-axis of Fig ii, instead of the proba p. Let me call "count" C here. Using C as parameter is completely equivalent to using p.
Using C as control parameter, it becomes obvious that as soon as C > 10^5, i.e. 1000 times its baseline value, what you are actually doing is a quasi- extremal dynamics, since you are almost always picking this site.
For lower values of C (in the range p~0.007 I guess C is much closer to its baseline value), you are not doing extremal dyanmics, but since you load all sites at random almost equally, your loading protocol is in effect quite similar to uniform loading.

I just noticed this fact while reading your paper and I think one needs to study this matter carefully in order to compare with other loading protocols.

It is not necessary to have this discussion in full in your paper, a short comment to let the reader realize this fact will be enough.

AR1.7. We have incorporated a clearer discussion of this fact in the revised paper. Although we did not include the Figure from our previous response, we discussed how the the effect of p can also be studied further to provide a comparison between the model and existing protocols.

RC1.8. [In response to previous review items 1.7-1.10] OK
[for 1.9]: Thanks, this is now very clear when reading the paper, and furthermore one understands why it is important to threshold (relative to interevent time statistics). I learned something new, thank you !

AR1.8. We thank the referee for this comment.

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Response to Referee Reports

RC – Referee's Comments

AR – Authors' Response

Referee #3: Stefan Hergarten, stefan.hergarten@geologie.uni-freiburg.de

Sorry for not taking part in the first round of review, although I was asked. Therefore my review mainly refers to the question whether the authors addressed the numerous concerns raised by the two first reviewers appropriately. I feel that they did in general, but in my opinion there are still two important points that require more clarification.

We thank the referee for participating in the review of our paper, and, in the process, reading and commenting on the previous versions of the manuscript and the responses. In our responses below and in the revised manuscript, we attempt to address his concerns.

RC2.1. Points 1.4, 2.2 and 2.6 of the original reviews refer to the role of the parameters used for calibrating the model and the relationship to established models in this field. If I understood the model correctly, $\nu = 0.25$ (at $p = 0$) should reproduce the original BTW sandpile model, while $\nu \to 0$ (at $p = 0$) should correspond to the OFC model in the conservative limit. Both cases are characterized by scaling exponents in the event size distribution lower than the values found in this manuscript, and those found here fit much better to real earthquakes. The finding of larger exponents between the two limiting cases is interesting and unexpected, but it should be shown clearly, perhaps with a figure showing the dependence on ν. In this context it should also be made clear that the finding is not a spurious effect of an exponentially decreasing event size distribution.

AR2.1. We thank the referee for raising this important concern again.

To clarify the parameters of the model, we note that the driving rate ν is the value of the external trigger that is added to a specified cell in the grid every time step. In the Bak, Tang, and Wiesenfeld (BTW) sandpile, each cell can have only discrete states $\sigma \in \{0, 1, 2, 3, 4\}$, where $\sigma_{max} = 4$ is an unstable one, i.e. at σ_{max}, the site will "topple" to redistribute stress to its nearest neighbors. Because of the discrete nature of the BTW sandpile, its ν can only be discrete, in this case $\nu = 1$, or ¼ of the σ_{max}. The 2D BTW sandpile produces avalanche size distributions with power-law exponents of around -1.0; the 3D BTW, on the other hand, recovers a power-law exponent of -1.3.

With continuous-state sandpiles, however, the scaling exponent tends to be different from the BTW case, i.e. it is not straightforward to expect the same exponent using the same ratio ν/σ_{max}. Lübeck (Phys. Rev. E, 56,1590-1594, 1997), using the Zhang sandpile that corresponds to our $p = 0$ case, presented a trend for the scaling exponents of the avalanche size distributions for different values of the driving rate, and found that while very small driving rates (« 1/32) tend to preserve the scaling exponent, a hallmark of SOC, the exponents (~1.3) are different from those obtained in the BTW (~1.0). Moreover, for driving rates above 1/32, there is a nontrivial trend for the scaling exponent; the exponents can go higher than the baseline value.

A similar model by Piegari, et al. (Geophys. Res. Lett. 33(1), L01403, 2006) showed the effect of the finite driving rates on the shape of the avalanche size distribution. Their model is another continuous-valued cellular automata with similar rules as ours, although they incorporated the degree of conservation *C* and assymmetrical neighbor redistribution fractions *f* (as in the OFC). Their model has shown a crossover from pure power-law to normal distributions as the driving rate is increased. The intermediate power-law regime tends to get steeper as the driving rate is increased; interestingly, their power-law behaviors for very small driving rates also start from exponents higher than the BTW case of 1.0. According to them, "Such a behavior is to be expected since for strong driving rates all internal correlations are washed out." In one of their presented results (Figure 2 inset of their paper), the exponent they obtained for the case of $\nu = 5\times10^{-3}$ (comparable to our $\nu = 0.001$) approaches 1.6 as the level of conservation is increased. This behavior, in fact, has been verified by our previous work (Juanico, Longjas, Batac, Monterola, Geophys. Res. Lett. 35, L19403, 2008) both in model results (ours was for the conservative case $C = 1$) and in actual sand avalanches in the lab.

As noted in the last part of **RC1.7**, more detailed studies are needed to obtain the correspondence between the proposed model and other existing discrete models of seismicity. Because this may require further analyses, we added a note on how to further establish where the model stands in light of the previous implementations.

ACTION2.1. A shorter version of the discussion above is added into the revised manuscript.

RC2.2. The relationship to earthquake rupture area and seismic moment of the model properties (points 1.2 and 1.3) is still not very clear. A should correspond to the rupture area, and V to the seismic moment.

AR2.2. [see also **RC1.4** and **AR1.4**] We have revised our view of our results, accounting for these recommendations by the referee. While we still provided the statistical distributions of *A*, in keeping with the previous sandpile based models, we now mention in the text that *V* is a better parameter for providing and analogy with the earthquake energy *E*.

Provided that the authors can clarify these points and demonstrate that their results on the scaling exponents are nor a spurious effect, I think that the manuscript brings some progress into the understanding of such simples models in the context of earthquakes, so that I would recommend publication then.

We again thank the referee for appreciating the value of our work.

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List of Changes

0. General Changes
    - Exponents reported in decimal values instead of exact fractions [see **RC1.0**]
    - Notation: Magnitudes are now represented by m (instead of M) and threshold magnitudes by m_{th} (instead of m) in text and figures [see **RC1.5**].
    - Changed the citation of Batac, Acta Geophysica from 2015 (online publishing) to 2016 (print publishing).
1. Section 1: Introduction
    - Fixed some citation issues (\citet and \citep issues)
    - Removed "avalanche energies deemed to be analogous to earthquake energies" [see **RC1.4**, **RC2.2**].
2. Section 2: Model Specifications
    - Added the description of V as one of the quantities being tracked for representing the event size.
3. Section 3: Model Results
    - In the first paragraph, a discussion of the scaling exponents of other continuous-state sandpiles are presented, along with the difference with the BTW exponent [see **RC2.2**].
4. Section 4.1: Energy Distributions and the Gutenberg-Richter Law
    - Explained that the distribution of A is presented in keeping with the earlier models that tracked only A and not V [see **RC1.4**, **RC2.2**].
    - Introduced the parameter V as the better analogous quantity to the earthquake energy E [see **RC1.4**, **RC2.2**]
5. Section 4.3: Temporal Separation of Earthquake Events
    - Separated the discussion of data and model results, for clarity.
6. Section 4.4: Model Advantages and Insights on Empirical Modeling
    - Added a discussion of the implications of the model and comparisons with other models [see **RC1.7**, **RC2.2**].
7. Figures 1-3
    - Made consistent in colors and notations
8. Figure 4
    - Panel (a): Added a power-law with $\beta = 1.45$ as guide to the eye [see **RC1.3**, **RC1.4**]
    - Panel (b): Changed the exponent from α to δ to avoid confusion (α is already used in the avalanche size distribution scaling) [see **RC1.3**, **RC1.4**].
9. Figure 5
    - Legend entries updated to clearly indicate the data and the shuffled sequences.
10. Acknowledgments
    - Added our acknowledgment of the editor and referees for the useful comments.