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Nonlinear Processes in Geophysics Discussions

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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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.

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Discussion paper

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.

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).

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-"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).

Aside from the context-building, other remarks:

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 “self-organized” 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.

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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.

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).

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.

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.

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.

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 Olami-Feder-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.

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.

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.

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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 A_th. Is it given somewhere? If not, give it. If so, make it more visible.

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

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