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Solid Earth
Discussions

Interactive comment on "Discrete element modeling of a subduction zone with a seafloor irregularity and its impact on the seismic cycle" *by* Liqing Jiao et al.

Anonymous Referee #3

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Review for "Discrete element modeling of a subduction zone with a seafloor irregularity and its impact on the seismic cycle" by Jiao et al. submitted to EGU Solid Earth.

In this paper Jiao et al. develop a discrete element model (DEM) to simulate earthquake cycles in Sumatra, where a seamount is present at depth. They simulate 400 years of subduction to provide values for earthquake cycle characteristics that they suggest are comparable to actual Sumatra observations and to investigate the role of seamounts. This is a very relevant topic. However, I list major comments on e.g., model scaling, convergence and calibration that make the findings in its present form very hard to accept as a meaningful contribution.

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All models have simplifications and limitations and no models are perfect. Despite that we can still learn many things from them if used appropriately. Important conditions for that are that key findings should not change distinctly depending on numerical or setup choices and that these limitations should be clearly described and discussed. Since both conditions are not met and conclusions and key findings are not well supported by reliable results and methods, I regret to conclude that I can not recommend the manuscript for publication in Solid Earth. A very significant revision of the manuscript addressing the major comments below with a shift of focus to more reliable findings would be needed before I can recommend this for publication.

Major comments

1. Friction and healing I am not very familiar with DEM models, but when analysing the equations I do not get really confident that equations and procedures are based on what we think we know about earthquakes based on laboratory experiments (e.g., Dieterich, 1979; Marone et al., 1998). A friction (pressure dependence) and friction variation seems to be missing and there is no explanation of what governs "slip" or nucleation, propagation and arrest during an earthquake. Instead the normal force between particles is set to "gradually decrease" according to the equation line 116, while healing to obtain cyclic behaviour is introduced through cohesion. - Could you please explain your frictional and cohesion procedures and the equation at l. 127 further and provide justification with references to (laboratory) studies? - How does your weakening and strengthening behaviour relate to known frictional formulations such as rate-and-state friction or slip weakening? - What governs earthquake slip (i.e., nucleation, propagation and arrest)? - Why do you not use frictional contacts, which are used in classic DEM simulations, as you write? - Why do you use only cohesion? Why (i.e., through what processes) would the cohesion or residual strength of a new fracture increase very rapidly on time scales of tens of years?

2. Model convergence - l. 90-95: You describe you use a non-viscous damping to facilitate convergence towards a quasi-static equilibrium, which needs to be used with

caution to prevent any bias. How do you in this paper ensure results are correct and not biased? Could you show evidence to convince us of that?

- A key results is the presence of different rupture lengths. However, it is known that complex spatio-temporal slip behaviour is introduced artificially if results are not well resolved (i.e., they are inherently discrete) (Rice, JGR, 1993). Could it be that this problem also arises in your DEM simulations? What happens if you (significantly) increase the number of particles you simulate?

3. Model scaling Scaling is not adequately explained and justified. This makes it difficult to understand why observables are off and what the meaning is of the numbers you provide as key results for the Sumatran subduction zone in the abstract. - L. 153: What is the reason you need to scale your model? - Because the particles are too large? - You scale down resolution of your particles to 1m only? Why 1m? Why not smaller? However, since your other dimensions are set to natural values, I would think that you can no longer tune the spatial dimensional component in your models. - Because you can not achieve realistic elastic parameters? - Or?

Your unrealistically low elastic stiffness parameters limit slip rates, lengthen slip durations, limit stress build up rate and thereby affect recurrence intervals, and likely affect stress transfer and slip profiles. Even though these numbers are affected by your numerical parameters, your key findings still refer to the numbers of e.g., recurrence intervals. Moreover, this limitation is not discussed or even mentioned.

4. Model calibration; realistic events? statistical significance? - You propose you can simulate earthquake cycles, because tests in Fig. 5f show a cyclic behaviour. However, the stress increase and decrease periods have about the same duration. That is very far off from earthquakes or a short coseismic period versus a long inter seismic period. Why would the precise values you derive for key seismic cycle characteristics have a direct meaning for the Sumatran subduction zone?

- Furthermore, Fig. 8c shows that deformation that most resembles earthquake-like cycles only occurs near the dip of the wedge (within 30 km of the trench; P1). Traditionally, this up dip zone is thought the be mostly aseismic, whereas seismic slip mainly occurs deeper in the seismogenic zone. However, where I estimate the seismogenic zone to be (P2,P3) cycles are either hardly visible in vertical surface displacements.

- It seems you base your key results of a 140 year recurrence interval mentioned in the abstract on the occurrence of three events identified visually by lines in Fig. 8c. For that smaller events visible in P1 and P2 are ignored. What thresholds and justification did you use to select events? How do these decisions affect your results? Since you provide actual numbers as a key results I think accurate definitions and justifications for “nucleation” and “ruptures” are needed.

- Even if these could be justified somehow, do you think two recurrence intervals is enough statistics to suggest recurrence intervals in Sumatra are 140 years?

5. Control experiment missing In the last line of your abstract you conclude the presence of seafloor irregularities significantly affects rupture events along the slab. However, you did not do a control experiment in which no seamount is present. Hence it is very difficult to determine what and how large this impact really is.

6. Discussion on limitations or assumption missing All models have simplifications and limitations and no models are perfect. To the very least these limitations should be described clearly and accurately. However, I miss a specific section discussing the large amount of assumptions and limitations in this paper.

Moderate comments:

Setup - A rigid slab of 10 km as a general thickness for oceanic slabs (l. 144) seems very thin, but depends on what you refer to as “rigid”? Could you be more specific here? - In the setup I do not understand where your seismogenic zone is and what parameters are used to model this?

Four domains match You suggest the four domains you identify “rigorously match”

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those defined in seismic observations (Lay et al., 2012). However, in this case they seem to be related to the seamount that is present at a location that can make similar subdivisions. Yet seamounts are not present at most other locations where this depth distribution still exists. Hence it seems unlikely to me that this is a meaningful comparison that is based on a resemblance in physical processes. Without causation I think such conclusions are not well founded. If you want to suggest this, I think a control experiment without seamount is needed (see below).

Suggestion near future event The recurrence interval of your large events is 140 years. The last lines of the conclusion you suggest that in case if you have observed a splay fault rupture a megathrust eathquake happen in the near future, defined as “less than ca. 100 years”. If it is 100 years, it is about 40 years after the previous event, which is still very early in the cycle. Hence suggesting that events can be imminent is meaningless. Moreover, because you only have a statistics of 2 large cycles.

Figures I also think you use many figures to present your results. A good portion of those are not needed. Focusing the presentation of the results in distinctly less figure would make the story your paper much easier to read.

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