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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.

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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?”
This is a good question here. Why do we not use frictional contacts? Why do we use only cohesion inside the material here? On the surface or the shallow depth, the rock/fault rupture behavior follows the Mohr-Coulomb criterion, the friction plays a very important role. But, at depth, the rock behaves more like ductile. What’s the depth of the transition zone between brittle and ductile? In engineering field, when the depth is larger than ca. 1km (the Figure below), the rock property starts to become more ductile (e.g., the lateral stress ca. 20 MPa responds to at the depth of ca. 1 km in Fig. 13 of Hoek and Martin, 2014). In the geological field, a very recent paper shows the exact transition zone is about the seismic depth ca. 10-40 km, based on the temperature distribution (Molnar, 2020). The seismic depth becomes shallow for a hot environment. Thus, we consider the relatively high temperature along the interface in the subduction zone. We set the contacts of elements only with cohesion. So, in our model, we set that the shear rupture along the faults is not affected by the friction or the normal stress.

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*

spatiotemporal 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?"

This item involves three questions: 1. the loading rate is quasi-static state. 2. How to use the discrete element models to address the complex spatiotemporal slip behavior? 3. Could the DEM simulations be different if we change the number of particles? Let's answer them one by one.

How to make sure that the loading rate is quasi-elastic state in the model? In Section 2.2.2, we scale our model to the reality. Thus, after scaling, our modeling results are comparable to the natural deformation. Since the natural deformation is quasi-static deformation except the co-seismic moment which we did not consider here, the deformation in our model is also quasi-static.

How to use the discrete element models to address the complex spatiotemporal slip behavior? Any numerical method could be used to address the slip issue, if it could be comparable to the natural behavior. Other numerical methods, such as the FEM, generally use the precise equations to simulate the deformation to understand the main factors controlling the main deformation. But in the past decades, the homogenous FEM is impossible to simulate the complex slip sequence in the earth. By scaling and validation, the discrete element method is able to simulate the slip sequence, and the results could be comparable to the nature. Thus we need to scale and calibrate the model, described in Sections 2.2.2 and 2.2.3, to simulate the natural deformation.

Could the DEM simulations be different if we change the number of particles? The simple answer is Yes. If we change the number of particles only, the simulation results might be changed. Thus, we need scale and validate the model. Before we simulate the real problem, we need make sure that the model is able to simulate the natural behavior and validate the deformation of the behavior. After the scaling and validation, with these settings including the particle size, the model is able to be used to simulate

the natural deformation.

3. "Model 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 du- rations, 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."

When we do the numerical simulation, one of fundamental procedures is to scale the model first. Then, we could be able to make sure that our modeling results could be possible comparable to the natural deformation. Why do we scale the model's particle to the 1 m? We could scale the particle to the even smaller size, which would result in significant calculation time-consuming.

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?"

Figure 5 shows the material mechanical behavior. Figure 5e shows the mechanical behavior of the inside material, and Figure 5f shows the mechanical behavior of the interface between the slab and the overriding plate, which is different from a cyclic behavior. Note that here we just show the material behaviors.

"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."
The point P1 is very close to the surface, which is not stable. We did not analyze the seismic cycle only based on the data of this point. We determine the seismic cycle along the megathrust based on the data of the P2 and P3, since they represent displacement on the surface. We discuss the megathrust events based on the displacement along the slab (Points A-K, represented in Figure 10) instead.

"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 required. 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?"
We defined big and small events based on the rupture length, i.e., if the rupture length is ca. 100 km, we decide it as a big event; if the rupture length is ca. 50 km, we mark

it as a small event. We also confirm their size according to the displacement and velocity fields along the slag, represented in Figures 10-16. Thus, our model shows a recurrence interval of ca. 140 years for big event (i.e., megathrust earthquake).

5. "Control experiment missing In the last line of your abstract you conclude the pres ence 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."
We include the case without seamount in the text (Section 2.2.4) and the supplementary material (Supplementary Fig. S2).

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."
We add description the assumption and limitation at the end of the paper, described that 'As we know that no model is perfect. The more data constrain, the more reliable the model would be. At the Sumatran subduction zone, the best available data could only provide us limited constrain to our model. We expect more observations in the future, and then we could further improve our model to contribute more helpful information.'

7. "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?"

The slab is rigid, thus the thickness of the slab does not affect the results but affect the calculation time. The parameters of the model are listed in the Table 1.

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

As we know, seafloor irregularities are quite common in subduction systems. Even though some places do not have a subducting seamount, roughness such as basins along the slab may be another factor controlling the seismic cycle. With different roughness distribution, the subduction zone might show various earthquake behaviors. Again, we include discussion on the case without seamount in the text (Section 2.2.4) and the supplementary material (Supplementary Fig. S2).

“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.”

We carefully reviewed each figure and still expect each could make our story clearer. Thus, we prefer to keep all of the figures.

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Figure caption

Figure Left: the tendency envelop line from peak strengths of the triaxial compression tests on Lac du Bonnet granite (modified from Figure 13 of Hoek and Martin, 2014; Lau and Gorski, 1992); Right: strength versus depth through the brittle-ductile transition (modified from Figure 1 of Molnar, 2020)

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