Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-41-RC1, 2020

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 #1

Received and published: 8 May 2020

General comments:

This paper presents the DEM simulation of the subduction process with an indenter on the subducting plate. The sequence of thrusting events and associated pre- and post-processes are examined as the source of the periodic earthquake system. The numerical simulation successfully reproduces the splay and backstop thrust formation with large- and small-scale slip events which are consistent with seismic observations. The analysis of deformations in the different locations and timings are presented. From the results, the author pointed out the significant role of sea surface roughness in the seismic cycle.

The impact of seafloor irregularity on the seismic cycle is still not well understood. Therefore, this type of numerical study should be interesting for geodynamics, geology, and seismic researchers, because it is difficult to access the plate boundary by the direct observation.

However, the current manuscript lacks the presentation and supporting data for the novelty needed for Research articles. I agree that the author successfully reproduces the characteristic feature of repeating earthquakes events. It is, however, difficult to find the new findings from this DEM simulations. The indication of a significant role of sea surface roughness to the seismic cycle is not new. For example, what are the key parameters, dynamics, and geometry for the successful fit to the time interval length scale, thrust angle of the observation? How robust your simulation result is? What are the new mechanisms not found in earlier studies? Without such quantitative constraints or significant findings from numerical simulation, this simulation result is just the ad-hoc result to reproduce realistic-like event. I suggest more simulation runs with different parameter sets for these findings and then we can get more understanding of the role of seafloor irregularity on the seismic cycle. You might already perform such simulation studies, but it was difficult to know in the present manuscript. Although I encourage the author to continue the work in this direction and I am willing to review this manuscript again, according to the lack of novelty and insufficient presentation, it is difficult to recommend this paper for publication in "Solid Earth" in the present form.

Detail comments:

1. In introduction or Methodology: Please review more studies with other numerical approaches for earthquake cycles in geological scale, such as [1]. What is the advantage of DEM over them?

2. In line 70: Recently 3D DEM simulations are performed in geodynamics (e.g. [2]). Please refer such earlier 3D DEM studies and discuss the 3D effect in the Sec. 4 or Sec. 5.

3. In line125 “As for the normal force”: This is about tangential force.

4. In line 125: Please give a number for equations

5. In line 125: Failure envelope in Fig. 2 c (Fˆmax_s) should be constant when c (Table 1) and A_int are the constant. I guess A_int is not constant (= pi*min(R_A, R_B)ˆ2). Please correct or explain this.

6. In Sec 2.2.1: How to initialize the particle position? If random, what was the sensitivity of the result against the different random set?

7. In Sec 2.2.3: What was the target maximum stress? I guess 180kPa but the calibration result in Fig.5 seems not to be consistent with that.

8. In line 215: Delete multiple Fig. 8.

9. In line 218: Fig 8 is difficult to see because of color choice (black and dark purple).

10. In line 229: ST is already defined.

11. In line 235: Why 0.4m, not 0.3m? Are there any objective reasons?

12. In line 227: Please confirm that the periodic big event is independent of random numbers for initial particle size and location. Since simulation is only for 400 years, it is difficult to justify the robustness of these observations.

13. In Sec 3.1: In the sand-box experiment (e.g. Dominguez et.al. 1998), the shadow area was found in the back of the indenter. In your simulation, it is difficult to see such a region. Please explain why?

14. In line 268: Separation by ST seems to be intuitive from Fig.8. What is the new finding from Fig. 10. I feel redundant.

15. In line 280 “very similar”: Please compare the integration value of the slip in that area to avoid illusion. To me, areas in fig 10 are different between after BE3 and BE1 and are more similar in after BE2 and after BE1.

16. In line 281: I think it’s a jump of discussion. The same size of slips does not mean the complete release of energy. This is just the biggest observed slip event. Since you perform numerical simulation, you should show the quantitative evidence (value) rather than qualitative evidence (image).

17. In lines 321-325: It is difficult to follow the discussion. Which timings are involved in SG1?

18. In line 327: This is already clear from Fig 8 and Fig 9. What is new?

19. In line 359: These subevents show clear changes of curves and not difficult to be identified.

20. In line 392: Coseismic behavior shows clear spikes.

21. In line 393: Why unstable can explain smaller peaks?

22. In line 339: This conclusion is weak. A similar conclusion is already argued by earlier sand-box and seismic studies. Please focus on the new finding which your DEM revealed.

References:

[1] Y Van Dinther, et.al. J. Geophys. Res. Solid Earth 118 (12), 6183-6202, DOI:10.1002/2013JB010380

[2] M. Furuichi, et.al Scientific Reports (2018), 8, 8685, DOI:10.1038/s41598-018-26534-x

---

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-41, 2020.