Comment to the authors

Authors: Kutschera *et al.*

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The manuscript submitted by Dr. Kutschera and the colleagues investigates the potential tsunami hazard due to strike-slip earthquakes in the Húsavík-Flatey Fault Zone in North Iceland. Their proposal of tsunami scenarios based on two different tsunami modeling approaches, one-way linked tsunami model and the 3D fully-coupled (earthquake–tsunami) model, are informative to understand how similar and different these results are. The authors also try the simulation with differently assumed simple and complex fault geometries, which yielded different results. They suggest the importance of consideration of detailed fault geometry for tsunami scenarios and that strong fast-arriving acoustic waves may be used to notice the tsunami potential in advance.

The manuscript is very well-prepared and deliver useful information in an appropriate way. I here list up numbers of comments, but these are just for more concise and fair descriptions. I hope these will be helpful for the authors to improve the manuscript. After this minor revision, I can recommend the publication of this article.

[Moderate comments]

1. When the authors discuss on the 2018 Palu tsunami, only the strike-slip earthquake itself is attributed to the huge tsunami, but there is still possibility of the landslide origins. It would be fairer to mention previous studies which proposed the landslide tsunami origins somewhere in introduction or discussion (for example, around Lines 33–33, 318–320, or 396–397).

2. When the authors compare the two tsunami modeling approach, a one-way linked or 3D fully-coupled ones, it sounds that the one-way linked approach is somewhat underrated. For example, the dispersion property can be included into the one-way linked model by using an appropriate dispersive tsunami model, but in the present manuscript, it sounds that the dispersion can incorporated only by the 3D fully-coupled model (I know that this is not the authors' intention). The non-dispersion is due to the shallow-water wave model. Please keep fairness to compare the one-way linked and the 3D fully-coupled model.

3. I think both two tsunami models you use here do not consider tsunami runup or inundation to coasts.

Is my understanding correct? The classical one-way linked model can easily incorporate the runup/inundation, as done many models such as Baba et al. (2015). How about the 3D fully-coupled model? I guess it requires sophisticated modeling of a moving land/ocean boundary. Please discuss on this somewhere (maybe in Discussion), because the runup/inundation can be also important aspects for the tsunami hazard assessment. (I understand that this would be out of your scope, and comparison at tide gauges without inundation is useful enough. I comment on this because it would be very informative for tsunami researchers.)

ref) Baba, T., Takahashi, N., Kaneda, Y., Ando, K., Matsuoka, D., & Kato, T. (2015). Parallel Implementation of Dispersive Tsunami Wave Modeling with a Nesting Algorithm for the 2011 Tohoku Tsunami. *Pure and Applied Geophysics*, *172*(12), 3455–3472. https://doi.org/10.1007/s00024-015-1049-2

[Minor comments]

Lines 37–47: The tectonic setting is better to described as a figure. it is difficult to understand the tectonics only from the text. I recommend that the authors put a figure with a broader area including the main tectonic setting, with notations of TFZ, NVZ, and the fault motion direction with average velocity of the plate boundary. These will be helpful for readers who are not familiar with the region.

Line 50: *The strongest historically recorded M 7 event in 1755 caused extensive damage and may have generated a series of oceanic waves (i.e., a tsunami) that hit the coastline (Stefansson et al., 2008; Þorgeirsson, 2011; Ruiz-Angulo et al., 2019)*

Would you please explain this part more clearly? Did the referenced studies show any evidence of tsunamis (such as historically documented record or tsunami sediment), or just speculated based on the large fault offset found on seafloor?

Line 59: *calculate a recurrence interval of 32 ±24*

What are the unit? Year?

Figure 1. See my comments on Lines 37–47. This figure needs to include the information of more broad tectonic setting.

Lines 74–81:

The detailed information, on such as SeisSol, GMM, or parameter differences, does not fit here in Introduction. I recommend that the authors introduce what they will show just more briefly here and move the details to in Sect 2 or 3.1. It will guide readers more smoothly to the details.

Line 90: *We model one-way linked (cf. Sect. 2.4) and fully-coupled (cf. Sect. 2.5) tsunami scenarios (Abrahams et al., 2023)*
"One-way linked and fully-coupled tsunami scenarios" sounds strange to me. If we say "tsunami scenarios", it would be possible tsunami *cases* by different source models, as you say in the following paragraph like "We use six earthquake scenarios." Please consider replacing the "tsunami scenarios" by "tsunami models", "tsunami approaches" or "tsunami computation methods".

Line 131–132: *It also agrees with assuming a 90◦dipping fault system.*
How about "It also agrees with our assumption of a 90° dipping fault system"?

Line 153: *6 to 10 km is the inferred locking depth for the HFFZ (Metzger and Jónsson, 2014).*
Based on what is the locking depth estimated? Please clalify.

Figures 3:
Three panels in the figure are shown too small. Please consider showing the panels vertically in a column., or "a) on the top row, and b) and c) on the bottom row.

Line 165:
Here you explain SeisSol. You may remove the description of SeisSol in Introduction and explain the details here.

Line 233: *However, we observe dynamic rake rotation (°æ20◦) near the surface during the rupture (Fig. 6).*
It is interesting that there happens a rake orientation. Could you please explain how this happens?

Figure 6, A1, A2:
The panels are very small. Please enlarge the figures, by showing them vertically, three panels of a) in 1st column, and those of b) in the 2nd column. The same problem happens in Fig. A1 and A2. Please consider revising them, as well.

Figure 7:

Why are the Simple cases at t=120 and 600s show very broad ssha (faint green and blue)? This looks weird, and is very contrasted to the complex cases. Please check they are modelled correctly and, if true, explain what they represent.

Figure B2:

The color bar is flipped. Please revise it.

Lines 305–310 (Related to my moderate comment 2):

As you know, the non-dispersive character is not because of a one-way linked tsunami model, but just because of the "linear long-wave model" employed. It would be fair to note that the dispersion can be simulated in a one-way approach as well.

Lines 333–335:

In difference to a proposed dominance of off-fault deformation in strike-slip tsunami generation in Palu Bay (Ma, 2022), the effect of offfault plasticity is likely small in our simulations. We find that off-fault deformation contributes only about ∼3 % of the total seismic moment.

Could you please mention possible reasons of the difference? Is it because of fault geometry, rupture model, or other parameters?