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Comment on nhess-2021-183

Anonymous Referee #1

Referee comment on "Characterisation of fault plane and coseismic slip for the May 2, 2020, Mw 6.6 Cretan Passage earthquake from tide-gauge tsunami data and moment tensor solutions" by Enrico Baglione et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-183-RC1, 2021

General comments

The NHESS manuscript with title "Characterisation of fault plane and coseismic slip for the May 2, 2020, Mw 6.6 Cretan Passage earthquake from tide-gauge tsunami data and moment tensor solutions" by Baglione *et al.* reports the authors' findings for the source that generated the May 2, 2020, south Crete tsunami. The authors use the nearest two tide gauge recordings of the tsunami that are available and perform a source inversion analysis. The parameter space used in the inversion is constrained using focal mechanism solutions provided by several agencies. Utilizing the inversion methodology of Romano *et al.* (2016, 2020), the authors solve for the best fit and the average of the top 5th percentile out of 41,310 realizations. The authors do not make an assumption on the preferred nodal plane and instead solve for that too. Even though the results do not clearly point to one nodal plane or the other, the analysis leads to a very interesting discussion on the uncertainty of the causative faults for tsunamigenic earthquakes along the Hellenic Arc. The work presented is of high scientific quality and the results are well presented, however, some basic elements of the source inversion were perhaps not properly accounted for. The numerical mareograms at the two tide gauge locations (Green's functions) were downsampled every 1 minute to extract time series of the same output frequency as the tide gauge signals. However, the numerical mareograms should have been averaged in 1-min time windows to produce time series equivalent to the tide gauge signals. Averaging the signal leads to smaller amplitudes and a time shift. This is critical to the source inversion methodology - more concerns on the source inversion methodology are provided in the specific comments below. This study is important to be published as it provides an analysis that is of interest to the scientific community. Therefore, I recommend that the manuscript is accepted after the specific are addressed.

Specific comments

- Tide gauges do not typically report instantaneous water level measurements. Instead, tide gauges average water level values sampled at a higher rate (than the data output rate). Therefore, downsampling the numerical mareograms every minute (line 190) does not produce an equivalent signal to the tide gauge (averaged) recording. Moreover, the sampling period typically corresponds to the output signal period, thus introducing a time shift of typically half the sampling period. These factors should be taken into account in the inversion and should lead to different results.

- The choice of including the Kasos tide gauge signal in the source inversion or not is not straightforward since the signal to noise ratio is so low. Fig. 8e-g show that particularly for the first 2-3 waves (up to minute 44), the signal to noise ratio is about 1 to 1. Lines 210-215 of the manuscript explain how the Kasos tide gauge signal is assigned a smaller weight, but still its inclusion is questionable when the quality of the recorded data is so poor. Adding random noise with a higher percentage of the clean synthetic waveform amplitude variance to the Kasos tide gauge in the test of Section 2.4 should provide an estimate of how much the low signal to noise ratio of the Kasos tide gauge affects the inversion results.

- Lines 190-191: The authors state that "We assumed linearity of the slip amount and the tsunami to obtain the scenarios for different slip values". Tide gauges, unlike deep water pressure sensors typically used for linear source inversions, are located in the nearshore where waves are clearly nonlinear. I'm having trouble believing that the linearity assumption is valid without testing it at each (tide gauge) location where it is used. Since the linearity assumption is key to the slip inversion, the authors should include a separate subsection in section 2 where the linearity assumption is verified.

- The wave period of this particular tsunami is relatively small (2-5 min), and water depth values at the source region reach ~3000-4000 m. Thus, wave energy in the source region is certainly contained in the intermediate (kh~1) water range (outside the kh<π/10 shallow water range). Since a shallow water model was used, frequency dispersion was not considered per se in this study. Early wave arrival of the Green's functions is considered in the form of a time shift together with the inaccuracies of the bathymetry etc, but considering frequency dispersion in the form of a fixed time shift is not equivalent to resolving frequency dispersion through higher order terms in the governing equations. This is an epistemic uncertainty that can be alleviated with the use of a dispersive model, although such an undertaking would be very computationally demanding for such a large number of simulations. A short discussion on the effect of frequency dispersion for this particular (small) tsunami event should at least be included in the Data and Methodology section.

- The source rupture area was fixed and the slip magnitude was varied in the source inversion. Also, the authors did not use the seismic moment as a constraint to try different combinations of rupture length, width and slip magnitude. I believe that was done to save computation time since the slip magnitude was accounted for as a linear perturbation of the Green's functions. The use of scaling laws to compute the fault rupture area and derive the initial conditions for the hydrodynamic simulations does not guarantee an agreement with the tsunami recordings. While in this case the authors produce an excellent agreement with the Ierapetra tide gauge recording, I'm not sure whether other source parameter combinations can produce equally good results. The expected implications in the inversion of using scaling laws to fix the rupture length and width should be briefly discussed after line 129.

Technical corrections

- "tide-gauge" should be written as "tide gauge".

- Line 77: Ebeling *et al.* (2012) is another reference for the 1948 earthquake and tsunami event:

Ebeling, C.W., Okal, E.A., Kalligeris, N. and Synolakis, C.E., 2012. Modern seismological reassessment and tsunami simulation of historical Hellenic Arc earthquakes. *Tectonophysics*, **530**, pp.225-239.

- Line 119: use "topography-" instead of "topo-…".

- Lines 162:163: describe the model governing equations in addition to the numerical scheme.

- Lines 172-173: the nautical charts the authors refer to were produced by the Hellenic Hydrographic Service. The issue date of the nautical charts used should also be mentioned in the text because it is an important piece of information.

- Lines 222-226: this is a difficult concept which I did not fully grasp and I believe needs to be explained/presented better.

- Line 237: what is the definition of ||aj|| here? The square root of the sum of the squares of all (seven) parameters?

- Line 270: it was not immediately clear to me what "resolution test results presented in Section 2" refers to. Better refer to them using the title of section 2.4, i.e. synthetic test.

- Lines 338-339: difficult to read. Sentence needs to be rewritten.

- Line 342: "The choice…is not sufficient for discriminating…" needs to be rephrased.

- Lines 397-398: the moment magnitude values resulting from the inversions can also be presented in Table 2.

- Figure 11: What is the sampling period of the W and HG numerical mareograms plotted here? Also, it is difficult for the reader to distinguish the magenta from the red curves.