Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2017-57-RC2, 2017

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Synthetic Tsunami Waveform Catalogs With Kinematic Constraints" *by* Maria Ana Baptista et al.

J. A. Alvarez-Gómez (Referee)

jaalvare@geo.ucm.es

Received and published: 11 April 2017

Review:

The authors present a probabilistic tsunami hazard estimation methodology based on a synthetic earthquake catalog. They apply the methodology to the North East Atlantic, and specifically to the Gloria Transform Fault generated earthquakes. The tsunami propagation model used is based on Empirical Green Functions (EGF). The authors use EGF to save computational time, using a database of precomputed propagations (110 x 40 – 4400 – unit cell propagations), and adding linearly the needed EGF for each seismic event.

Below I expose the main concerns with the work in more detail, but based on the lack of detailed explanations and discussion on the methodology proposed, the misuse

of some references and concepts, and the total absence of uncertainties estimations (epistemological and aleatory) I cannot recommend the acceptance of the work on its present form, and major revisions are needed.

In brief the expected major revisions are:

The structure of the text must be improved. The work presents a new methodological approach but seems a case study for the Gloria Transform Zone. I suggest to present first the methodological approach from a theoretical point of view, and then an example of application to the Gloria Transform and its results.

The authors should revise the use of scaling relations and bibliographical data to obtain the fault parameters. They should incorporate the associated aleatory uncertainties on their calculations, and a logic-tree approach if needed for some parameters. The influence of the selections made in the results should also be discussed.

The tectonic setting and assumptions done is key for the synthetic earthquake catalog. The authors should show in a figure the kinematic constrains and the modeled faults.

The authors should incorporate an estimation on the accuracy of the tsunami simulation results comparing their propagation results to some historical event. As there are three tsunamigenic historical events (as described in the text) the authors could choose one of them to estimate the accuracy of the model. This is specially relevant as the model of Miranda et al. 2014 has not been validated.

Finally, the references should be revised as they are misused, omitted and/or forced to support affirmations not always present in the original work.

Main concerns:

In Table 1 the 1975 earthquake data is referred to Buforn et al., 1988 and Argus et al., 1989. The latter does not describe the 1975 event, and does not provide any seismotectonic data, and the parameters shown for this event in Table 1 does not coincide with the parameters of Buforn et al. (1988). The parameters for the 1941 event does

not coincide with the data provided by Udias et al. (1976) or Lynes and Ruff (1985). The Baptista et al. (2011) is a reference to a conference communication abstract so I suppose that they simply use the original Udias et al. (1976) focal mechanism parameters.

The scaling relations used in the work are the Wells & Coppersmith (1994) and Stirling et al. (2002). Both works are based on surface ruptures in continental lithosphere. I recommend the use of other scaling relations that already incorporates oceanic lithosphere events like Blaser et al. (2010) or Allen et al. (2017) relations. These relations use the subsurface rupture length, so there is no need to use the ad-hoc relation of Length – Subsurface Length used by the authors. The relation of Manighetti et al. (2007) is used but forced to fit the dimensions assumed for the earthquakes in the zone; if different dimensions are used, as for example the 200 km proposed by Buforn et al. (1988) for the 1975 earthquake, then the Dmax diminishes and then would fit without forcing the Manighetti et al. (2007) relation. There are inconsistencies in the use of relations and dimensions, and being this aspect crucial to the tsunami generation it should be adequately justified and discussed and an error estimation on the results should be presented.

The concept of seismic coupling is misused in section 3.2; the seismic coupling is the proportion of tectonic slip produced seismically vs aseismically. If GF1 is fully coupled it is reasonable to assume that GF2 and GF3 are also fully coupled, then the seismic coupling should be also 1. The authors in fact are talking about a slip distribution between two parallel faults, absorbing each the 50% of the total tectonic slip. This should be clearly explained and avoid the confusion with the concept “seismic coupling”.

The authors use a generic b-value of 0.98 from a global analysis (Bird and Kagan, 2004) and I wonder why they do not simply calculate the b-value from the seismic catalog of the area, which is the appropriate approximation. Moreover, the authors do not explain how they obtain the earthquake activity rate in addition to the b-value. In

general the method to generate the synthetic earthquake catalog is not well explained, only a paragraph is used, and resolved with “We made several runs to obtain coherence with the GR model and with the nominal slip rate.” which seems like a trial and error until the results coincides with what we expected. This is not adequate for a serious research and the limitations and implications of the selected methodology to generate the synthetic catalog should be discussed.

The location of the events along the fault segments seems incoherent. As seen in Figure 5 there are areas of the fault with much higher accumulated slip, which means that there are sections with around 4 mm/yr while there are others with 2 – 3 mm/yr, which is tectonically inconsistent if the seismic coupling is 100%.

In general the method to generate the synthetic catalog is not well explained, neither discussed or justified. Being this part the most relevant of the work it is not acceptable.

The tsunami propagation code is not described, just cited as Miranda et al., 2014. In Miranda et al. 2014 the modeling does not include the Coriolis terms or bottom friction and no comparison with historical tsunami observations is done. I have doubts on the accuracy of the results as they are not compared to any observed tsunami in the area and the model is not conveniently described in the text.

Figures:

The figures in general needs to be improved.

Figure 1 I suggest the use of focal mechanisms representation for the three main events shown. This figure should be used also to present the tectonic setting with the main geological structures and the plate kinematics. The modeled structures (GF1, GF2 and GF3) could also be shown.

Figure 2 The synthetic catalog events cannot be distinguished. I suggest the use of a conditional symbol with the size of the circle proportional to the size of the modeled event. The modeled fault sections GF1, GF2 and GF3 should be shown.

Figure 3 The three events highlighted (stars) should be specified. Which one corresponds with the 1975 event? And with the 1941 event? The yellow color on a white background is not a wise decision.

Figure 4 The figure need to be reworked. The gray background and the extremely long titles of each subfigure should be changed. An a and b for each subfigure is recommended.

Figure 5 What is the vertical axis in the figures? Width? What are the parameters of the fault sketch? Are they used anywhere? If so they should be described in the figure caption, if not the sketch should be deleted.

Figure 6 This figure can be greatly improved, and/or simplified. On its present form is almost useless due to the accumulation of symbols and its similarity.

Tables:

In tables 2 and 3 the values of seismic moment should be adequately represented.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2017-57, 2017.