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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Tsunami hazard assessment in El Salvador, Central America, from seismic sources through flooding numerical models" *by* J. A. Álvarez-Gómez et al.

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Reply to referee #1 comments.

R#1_1- Validation of the historical cases they model with deep water measurements from DART systems whenever available, particularly for far-field scenarios. This would add to the credibility of their sources.

In the case of far-field scenarios, we model recent events from the seismic sources computed with different methodologies by the referenced authors (details in the manuscript). We consider that the used sources are of sufficient quality by themselves

and does not need to be validated again. The credibility of the sources rely on the quality of the data and the methodology used to invert the seismic sources. The comparison with DART data could validate the propagation numerical model, but the model used in this work for wave propagation (COMCOT) has been widely used and validated before as is referenced in the text.

R#1_2- Validation of the 2012 El Salvador event with inundations results from UNESCO/IOC field survey.

In fact the referee is proposing a validation of the numerical model used with data from the recent event occurred offshore El Salvador. This event was a 7.3 magnitude thrust faulting earthquake. Most of the tsunami inundation occurred in the Jiquilisco Bay area, where no detailed data on the bathymetry exists and consequently the required inundation simulation cannot be done. The tsunami was modeled by the NOAA with the MOST model and the wave elevation results were incompatible with the observations, probably due to the low seismic rupture velocity of the event and the complexity of the rupture compared with the oversimplification of the NOAA sources. In order to compare a model with the observations of inundation for an event we need a high resolution bathymetry and topography. This data does not exist for the area and the comparison is impossible.

R#1_3- Discussion regarding any possible discrepancies in the characterization of the local sources with those of NOAA's propagation data base for the area (see Gica, 2008).

The NOAA's sources database (Gica et al., 2008) is based on "unit" sources with 100 km in length and a width of 50 km; producing Mw 7.5 earthquakes for 1 m of displacement. The strikes and dips used by Gica et al. (2008) are obviously compatible with the used in our work, as both reflect the geometry of the subduction interface. In fact Gica et al. (2008) do not characterize in detail the subduction in Central America (they do not characterize in detail any subduction for the NOAA's database) as they are generating

the unit sources in a world-wide basis with similar characteristics around the globe. A “unit source” is by definition an oversimplification of the actual source geometry and is merely a tool to develop a short-term forecast wave propagation model.

The objectives of our work are different from those of the NOAA’s forecast model, and our characterization of the subduction is much more detailed than the characterization of Gica et al. (2008). We think that the discussion of the differences between both set of sources is pointless because they are not comparable.

R#1_5- Inclusion of current speed in the presentation of results. Currents during a tsunami have proven to reach damaging levels even without associated flooding.

We agree with the referee, and although the current speed is implicitly included in the Human Instability Hazard, we will include new maps of current speed in figures 5, 6 and 7 of the final version (modified figures in the supplement of this comment).

R#1_Of less importance, but still requiring the attention of the authors would be the following two issues:

R#1_1- The authors should provide a definition for the term "human instability hazard" which they use throughout the paper and estimate in the calculations but is not obvious to the reader what is meant by it.

Already corrected in the previous revision and actually included in the discussed text.

R#1_2- In equation 1, the authors refer to Strassers’s relation relating Mw to rupture area, but the equation seems to only contain L (is L length?) how do you obtain total area from the relationship?

Already corrected in the previous revision and actually included in the discussed text.

The rest of minor observations of the referee #1 are not referring to the present version of the text as its comments have been already corrected previously and uses line numbers that are not present in the current discussion manuscript.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C745/2013/nhessd-1-C745-2013-supplement.zip

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 2127, 2013.