

# ***Interactive comment on “Development of a methodological framework for the assessment of seismic induced tsunami hazard through uncertainty quantification: application to the Azores-Gibraltar Fracture Zone” by Vito Bacchi et al.***

**Anonymous Referee #4**

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Review of “Development of a methodological framework for the assessment of seismic induced tsunami hazard through uncertainty quantification: application to the Azores-Gibraltar Fracture Zone” by Bacchi et al.

This paper describes a methodology for tsunami hazard analysis using uncertainty quantification techniques and meta-models, and then presents an application of the methodology based on an example application to the Azores-Gibraltar Fracture Zone.

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I find the methodology that has been developed interesting and that publication of a significantly revised manuscript would be useful for other researchers interested in similar techniques. However, the demonstration application shows that the methodology is incomplete for practical purposes for reasons given below, and the authors need to acknowledge and address some of these issues.

Specific points:

1) The presented application to the Azores-Gibraltar Fracture Zone takes very little in the way of constraints or weightings from what is known about the geology, geophysics and seismology of the area. A better indication of how prior knowledge can be included in such a study is something that I think will be needed before the presented methodology can be put into practical use. Related to this, is a lack of connection to the time-frame associated with the hazard, eg the return period associated with a water level at the study sites, something that is necessary for many applications of tsunami hazard.

I think the authors are aware of the current limitations of their results, and they repeatedly mention that this is '... not an operational assessment ...'. There could be some differences in interpretation about what 'operational' means, and I think it would be better if they could be more explicit about this, eg at the end of the Introduction they might consider something like: '... not an operational assessment of tsunami hazard along the French Atlantic Coast, and no reliance should be placed on these results for practical purposes'. With something like that, and more acknowledgement of the current limitations and areas for more work, the authors would be on safer ground that this manuscript is just an introduction to the 'meta-model' concept.

2) The methodology is hard to follow, and in particular, the relationship between the various different databases in Table 5 and how they are used in the analysis, was difficult to understand. I suggest the authors try to construct a flow diagram or other type of figure to explain this graphically (could be somewhat similar to Figure 1).

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3) The Antoschenkova (2016) reference is key to understanding the design database, but this is just a conference presentation and I could not find the information needed to assess the suitability of this work to the purpose to which it is applied. If the information is not published elsewhere, I suggest a summary be included here – perhaps as an Appendix.

4) It seems a pity to me that all of the study sites are located along the French coast. In particular the sensitivity analysis is, in my view, one of the most interesting parts of the paper, but it would be really nice to see how the sensitivity applies to sites closer to the source region.

5) I'm presuming that the magnitudes in Table 5 are calculated after the uniform sampling of length, width and slip and applying constraints to these. And that the distribution of magnitudes is just what falls out from this process (and not linked to any assumption about earthquake statistics – see point 1). This needs clarification (unless it was mentioned and I missed it).

6) Non-uniform slip distributions have been shown to be influential in estimating tsunami hazard (see eg Geist and Dmowska, 1999), but the study here is (as far as I could tell) all based on uniform slip modelling. It is not at all obvious to me how this might be included in the methodology presented here, and it would be nice if the authors could comment on this.

Similar to this is the approximation of the rupture by a rectangular plane, which may be suitable for low magnitude earthquakes but will not be realistic for ones at the upper end of their study range. Again, this does not seem like an easy thing to incorporate.

7) The authors may find that some of their problems with finding suitable wave height statistics could be improved by working with log-normal distributions. A starting point for this would be to search for 'Aida-k parameter' (original paper Aida, 1978).

8) The paper needs extensive copy-editing to improve the English.

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Aida, I., 1978. Reliability of a tsunami source model derived from fault parameters. *Journal of Physics of the Earth*, 26(1), pp.57-73.

Geist, E.L. and Dmowska, R., 1999. Local tsunamis and distributed slip at the source. In *Seismogenic and tsunamigenic processes in shallow subduction zones* (pp. 485-512). Birkhäuser, Basel.

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