NHESS-142 Development of a methodological framework for the assessment of seismic induced tsunami hazard through uncertainty quantification: application to the Azores-Gibraltar Fracture Zone

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Comments by Luis Matias, University of Lisbon

Recommendation

It is my recommendation that this paper cannot be accepted for publication. There are some major considerations that I will describe below to support my conclusion. Additional comments follow that could help the authors if they wish to resubmit a new paper.

Major comments

As I understand the paper, the authors use the “uncertainty quantification technique” to derive “meta-models” that could be used to make an extensive exploitation of the parameter space without requiring the time-consuming process of tsunami modelling. The authors present two applications of the methodology, one addressing the sensitivity analysis and the other as a contribution to tsunami hazard evaluation. While the methodology described seems interesting, the first application to sensitivity analysis suffers from several shortcomings and the application to tsunami hazard evaluation is seriously flawed.

As the authors mention in the paper, there are two main procedures for tsunami hazard evaluation, the probabilistic method (PTHA) and the deterministic method (DTHA). The authors explore a few scenario databases to apply the methodology that allegedly exploit randomly (with uniform probability) the range of source parameters considered. There is no probability attached to the geological realization of each scenario and so the results presented in the paper should be included in the second type of evaluations, DTHA. This is clear when the authors compare the present study with the work published for the Indian Ocean by Roshan et al. (2016). As the authors mention, DTHA studies rely on the study of “maximum credible scenarios”. The databases used in the paper make a random exploitation of the model space which gives origin to a large number of scenarios (over 50,000 in one case) where only a few of them, not identified, would classify as “maximum credible scenarios”. In my opinion, this approach precludes any statistical use of the results as a contribution to DTHA.

The major problem I found in the paper is related to the characterization of tsunamigenic earthquakes that is used for the Azores Gibraltar Fracture Zone (AGFZ), that I prefer to mention here as the Azores Gibraltar Plate Boundary (AGPB). The authors arbitrarily divide the AGPB into two domains, “oceanic” from 18ºW to 10ºW and “continental” from 10ºW to 7ºW. In latitude both domains span from 34ºN to 40ºN. According to the authors, the “oceanic” domain is characterized by “thin oceanic crust” while the Eastern domain is characterized by “thickened oceanic to continental crust”. This classification is at odds with the knowledge of the deep structure of the Gulf of Cadiz and the Gloria Fault zone as given by Martínez-Loriente (2014) and Batista et al. (2017) that show that such a simplistic classification is inappropriate.

The authors consider that the maximum magnitude allowed for the “oceanic” domain is 8.3, being 9.3 for the “continental” domain. There is no explanation whatsoever for the arbitrary attribution of these values. There is no reference in the literature for this area that proposes such a high value of magnitude. It is in contradiction with the known studies for THA and Seismic Hazard Assessment (SHA) and it should be properly explained. The highest magnitude considered for the AGPB is the one estimated for the 1st November 1755 earthquake that, at most, as an inferred magnitude of 8.8 (e.g. Johnston, 1996). The strong discrepancy between the two zones as regards the maximum magnitude is not explained in the text. In fact, given the uncertainty today on the probable source for the 1755 earthquake, the authors recognize that the “oceanic” database does not contain one of the proposed sources for this event, the Gorringe Bank. The authors try to mitigate this problem by adding an ad-hoc extra database of scenarios for that particular source, thus violating the basic principle in their origin, make a random choice of parameters that would include all possible sources of extreme events. The “oceanic” database clearly fails to attain this objective.

The heterogeneity of the AGPB has been captured by all SHA and THA studies made in the area, most of them not considered in the paper, e.g. Vilanova and Fonseca (2007), Woessner et al. (2015) for SHA or Omira et al. (2015), Omira et al. (2016), Matias et al. (2013) for THA. The criteria used in these works take into consideration the best knowledge on seismology and seismotectonics, the same considerations that should guide the development of the tsunamigenic databases used in the paper. Instead the authors choose to build random scenarios over an all-encompassing range of source parameters. True, the geological knowledge of the area is still incomplete and there are uncertainties or disagreements, for example, on the identification of the source for the 1755 event. This fact, however, does not justify the replacement of an “educated” database supported by expert opinions by a purely random source distribution.

As regards the source of the largest historical earthquake and tsunami reported for the area, the 1st November 1755 event, the authors identify the difficulty to find in the literature a consensual solution. Some of the more relevant references to that problem were not considered in the paper (in chronological order here), Baptista et al. (2003), Terrinha et al. (2003), Gracia et al. (2003), Barkan et al. (2009) and Cunha et al. (2010.). These and other studies show how artificial is the split between "oceanic" and "continental" source areas with very different maximum magnitudes (8.3 and 9.3 respectively). The derived databases cannot be considered as representative or as including the maximum credible tsunamigenic sources in the AGPB as claimed.

Another shortcoming in building the databases is the absence of an explanation on the way that the parameters length, width, slip and magnitude are explored, since they must be related by the equations (15) and (16). Equation (15) also depends on the value attributed to the shear modulus in the equation, but no mention to that is found in the paper. Contrary to what is inferred from the text and Table 5, the magnitude is not uniformly sampled between the limits indicated. This is clear from the histogram presented in figure 9a. A clear explanation on the procedure used to relate these 5 parameters must be clearly presented, as for example is done for the AGPB by Matias et al. (2013) and Baptista et al. (2017). If other scaling laws are used in a similar process these should be clearly discussed in the paper.

As I understand, the purpose of the adopted methodology was "to build a numerical database of a wide bunch of the maximum possible tsunamis generated by the AGFZ". It is demonstrated in the paper that the most influential source parameter to the maximum water height observed at the cost is the average slip in the fault, that scales with magnitude, if earthquake physics are respected. Then we should expect that only the largest magnitude events would contribute to the desired hazard parameter. It is then hard to understand what is the contribution to the proposed investigation of events with magnitude < 8.0 that constitute 50% of the database (figure 9a). Furthermore, the authors recognize that most of the hazard is generated by "well located-oriented" faults. Since the authors explore randomly the full source parameter range, it is difficult to be sure that these "well located-oriented" faults are caught by the databases, when a small set of sources have the largest magnitudes tested. If we sample the strike every 30º, the dip every 10º, the rake every 30º and the epicentre coordinates every 1º, the depth every 5 km, we arrive at ~400,000 possible sources for each magnitude level. If we allow for some variability in length, width and slip for each magnitude level, then more than 10^6 scenarios are possible. With only a few thousand scenarios randomly selected we should not expect to find the "well located-oriented" faults that are responsible for the highest water level at the coast.

As regards the depth of the tsunamigenic sources (presumably below the sea bottom, but not clear) the authors mention that “the maximum oceanic depth is considered to be of the order of 10 km whereas the continental crust can attain 30 km depth”. This consideration is translated into the values provided in Table 5. The authors thus disregard the evidence that recent microseismicity investigations in the area show that the brittle lithosphere extends at least to 50 km depth (Geissler et al., 2010, Grevemeyer et al., 2016, Grevemeyer et al., 2017, Silva et al., 2017). The large focal depth found in the area is interpreted as representing the rheological properties of very old oceanic lithosphere that forms most of the AGPB. This rheology as consequences for the scaling laws appropriate for the generation of large tsunamis in this domain as discussed in Matias et al., 2013 and Baptista et al., 2017. The paper does not explain also how the conflict between a shallow focus and a large vertical width is solved when selected by random?

The AGPB is an area prone to tsunamis and so the investigation of the resulting tsunami hazard is an important objective. However, the authors fail to recognize the synthesis tsunami catalogues already published for the area, while discussing the merits of the built database (Baptista et al., 2009 and Kaabouben et al., 2009).

About the methodology, as I understand it, one “meta-model” must be built for each of the desired outputs to be investigated. In the paper 4 tide gauge locations where chosen for testing and only one hazard parameter elected, the maximum tsunami water height. Nowadays, following the development of the tsunami warning systems, both national and regional, THA studies, deterministic or probabilistic, are applied to very close observation points at the coast, typically a few tens of km, to account for the high variability in tsunami propagation and coastal effects (e.g. Molinari et al., 2016, Roshan et al., 2016). Nowadays, to mitigate the massive computing power required to generate the large number of tsunami scenarios required for PTHA, authors have resorted to parallel computing using GPU and to the concept of Elementary Source or Empirical Green Function (e.g. Molinari et al., 2016 or Baptista et al. (2017). Applying the “uncertainty quantification technique” to reproduce the results that are nowadays required, with an extensive set of observation points (or forecast points) may be as computer intensive as the traditional approach of tsunami propagation for a large number of scenarios. The paper fails to present the advantages of the methodology regarding a realistic application.

The paper applies the “uncertainty quantification technique” to make a sensitivity analysis aiming to identify the source model parameters most relevant for each of the four target sites. A general classification on the most relevant parameters is given and a simple interpretation is presented. However, a more detailed analysis would be much more relevant. What are the epicentres and magnitudes that contribute most to the highest hazard levels in each site? This is a common exercise in Probabilistic Seismic Hazard Assessment called disaggregation. My expectation is that this analysis would show that the most relevant contributions would come from unrealistic geological sources, even taking into consideration the uncertainties ascribed to the seismotectonic framework of the AGPB.

The authors use systematically the moment statistics of gaussian distribution to characterize the several variables discussed in the text. However, the authors recognize that many of these quantities have long tailed distributions, as expressed in table 4 or in figure 9a. The gaussian distribution doesn’t seem the most appropriate model for the distribution of extreme values as desired. This question should be properly addressed in a more formal way in the paper.

The authors claim to obtain maximum water height estimates for the 4 locations identified in the text that are close to the coast line. The representative water depths at those points are 8, 15, 28 and 40 m. It is well known that the shallow water equations used for tsunami propagation loose their precision close to the coast and at water depths lower than 50 m. It is usually recommended that shallow water results should stop at that water depth (e.g Kamigaichi, 2009). From that point higher resolution models or empirical relationships should be used to estimate wave heights at the coast or shallower waters (ibid.). This issue should be addressed in the paper.

Other comments

The mathematical notation needs to be carefully revised. In particular vectors/matrices and scalars should be clearly identified, for example using bold type. The meaning of variable names and its capitalization should be kept throughout the paper.

The mention of bibliographic references should follow the journal standards and double “(())” should be avoided.

One disadvantage of the “meta-model” approach is that it adds an extra layer of uncertainty to the uncertainties already due to the source parameter space variability. This issue is addressed in the paper, but it may ne undesirable in practical applications.

The list of references is clearly incomplete for many of the subjects addressed.

The numbers on the tables do not follow the order they are referenced in the text.

The numbers on the figures do not follow the order they are referenced in the text.

The paper quotes extensively the Antoshchenkova et al. (2016) work that corresponds to an abstract to a conference. This should be avoided by presenting the main results of that work in the paper or as a supplement, otherwise many sentences in the paper cannot be verified by the reader.

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