REVIEWER 1

RC#01: The authors studies the faultiness close to the Iberia peninsula, in order to estimate the tsunami waveforms that could attack the western coasts of Portugal, Spain and Morocco. Generally, their approach appears interesting, yet there are a number of issues that need to be clarified before the paper can be accepted for publication. […] It is surprising that no mention is made at all in this paper to the historic Lisbon earthquake and tsunami. Such a major event, and its relation to plate tectonics, should at least be mentioned somewhere in the introduction.

> Answer: The manuscript focuses only on the Gloria fault which is believed **not to be** the source of the 1755 earthquake. Nevertheless, to clarify this we added a new sentence and a reference: "The tectonics of this segment of the plate boundary changes from extensional close to the Azores, and transpressive close to the southwest of Iberia, where mega tsunamigenic earthquakes have been generated in the past, namely the 1st November 1755 (Baptista et al., 1998)"

RC#02: In section 2 the authors describe three tsunamigenic earthquakes. However, they provide no indication concerning the records of the inundation heights, etc. This would be of major interest to any readers. Later, the authors state "These results are compatible with the maximum observed amplitudes of the 25th November 1941 tsunami (Baptista et al., 2016)." What exactly are these amplitudes, and exactly how well do they compare?

> Answer: We agree with the reviewer. We inserted table 6 with recorded tsunami amplitudes of the 1939, 1941 and 1975 events. Additionally, we corrected the last sentence of the manuscript to: "Finally, it is worth pointing out that the instrumental observations in the XX century correspond to the realization of low frequency seismic and tsunami events but not to the maximum predicted wave heights"

RC#03: Figure 2 is of very poor quality. It is almost impossible to understand what the authors are referring to regarding the dots (this reviewer has very good eyesight, yet had to put his eyes almost an inch away from the screen to notice anything). The authors could use a different colour (maybe red?) for the dots, and clearly label the segments on the figure?

> Answer: We agree with the reviewer. A new figure 2 is the revised document with a zoom on the Gloria Fault area.

RC#04: P7L4 "The model ran 7200 time steps," How long was this in hours? It would be interesting for the authors to show at least one example of the time history of the wave profile at one point along the coastline (say Lisbon or Cadiz, for the worst inundation height) Otherwise, it is difficult to verify how well the author's model is working.

> Answer: In the manuscript, it is declared a time step of 2.5s. This corresponds to a tsunami propagation time of 5 hours. The sentence on page 7 lines 7-9 was completed accordingly: "The model ran 7200 time steps, corresponding to a Courant Number ~ 0.45 for 18 Points of Interest located along the coasts of Iberia, Africa and the Atlantic islands, with a total propagation time of 5 hours. (see Figure 6 and table 5 for locations)." The model used here was fully tested for the case of the 1941 event in Gloria fault in "Baptista, M. A., Miranda, J. M., Batlló, J., Lisboa, F., Luis, J., & Maciá, R. (2016). New study on the 1941 Gloria Fault earthquake and tsunami. Natural Hazards and Earth System Sciences, 16(8), 1967-1977."

RC#05: Why did the authors choose to show the tsunami amplitude at the 30m line? Somehow, this reviewer would have expected the authors to verify that the model they are using can provide realistic inundation heights, at least at one location. Otherwise, how can they really claim that their model reproduces well historical events (essentially, this is an issue of calibration and verification of their model)

> Answer: The depths of the POIs are very different among them and a function of the morphology and grid cell size. To allow the comparison between the results for different POIs we decided to choose one water depth where non-linear effects are not very significant.

To make our point clear we add the sentence see page 7 line 23 and 24, Concerning the 30 m choice we add the following sentence: "This choice ensures the comparability between wave amplitude estimations for different POIs. On the other hand, we assume that up to 30 m depth the non-linear effects can be neglected."

RC#06: Discussion. It is difficult to accept the hazard analysis presented by the authors, as they only estimated the tsunami wave heights at a depth of 30m. Also, it is strange to talk about sea level rise for the case of tsunamis, and difficult to sea how this would have a great influence, unless the authors are talking about the end of the 21st century (even then: : :)

Answer: In this study we focus on hazard analysis only from the point of view of wave heights. We are aware that these tsunami wave heights cannot be linearly translated to inundation depths. However, the computation of runup and inundation flow depths for inland areas of all POIs require the computation of a significant number Digital Elevation Models and this is beyond to the scope of this manuscript. However, the methodology presented here is not compromised.

We agree with the reviewer in what concerns the comment on sea-level rise and we erased this comment from the conclusions section of the manuscript.

RC#07: L15 "we can expect wave heights between 0.6m and 0.8 m requiring the evacuation of coastal areas." , Normally such small heights would produce little to no damage (based on other events). Obviously the coastline should be evacuated, just in case: : :

Answer: We corrected the sentence in the abstract that now reads: "However, in Morocco, Azores, and Madeira islands we can expect wave heights between 0.6m and 0.8 m leading to precautionary evacuation of coastal areas"

RC#08: "The computation of tsunami waveforms along the coast is a time-consuming process that requires substantial computational resources." Is this really the case nowadays? Somehow it does not feel that the resources needed are that substantial any more, but it could be that this reviewer is used to how things were over a decade ago.

Answer: We partially agree with the reviewer. In fact, the computation of tsunami waveforms is nowadays much faster than it was a decade ago. Nevertheless, we ran 4079 simulations for a large area using only a laptop.

RC#09: In the present work the authors assume a uniform slip. This is a common assumption, though at present some other authors have started to use non-uniform slips. Maybe this can be briefly discussed? Could non-uniform slips result in higher tsunami amplitudes?

Answer: We agree with the reviewer that non-uniform slips can produce higher amplitudes but this is particularly important in case of near field stations. The closest POI to Gloria's western end is Ponta Delgada in the Azores is 120 km away.

Page 3 line 3 now reads: "We consider the simplest case of a rectangular fault with constant slip because of the location of the Gloria Fault in relation to the points of interest, and we use scaling laws to infer the fault parameters"

And in the page 8 lines 22 and 23 we add the following sentence: "In case of studies dealing with near field cases, the variability in slip should be taken into account in the building of the synthetic catalogue"

RC#10: P7L16 This phrase is unclear "Because we ran a linear approach and the depths of the different points of interest are variable," What do the authors mean by "we ran a linear approach"? P8L32

Answer: The model used to compute the empirical Green functions is a non-linear shallow water model, that can also be used to compute inundation. Nevertheless, as the method is based on a superposition principle and POI are reduced to 30m depth the physics used must be described as a linear shallow water approach.

RC#11: "Finally, it is worth pointing out that the instrumental observations in the XX century correspond to the realization of low frequency seismic and tsunami events." It is not clear what the authors mean. Also, note that mentioning records of only one century with regards to tsunami events is potentially dangerous.

> Answer: New table 6 includes a comparison between historical events and maximum wave heights deduced from the approach presented in this paper. Two of the historical well studied events (1941 and 1975) had some impact on harbors. Observed amplitudes are lower than the maximum computed with this approach but nevertheless they correspond to rare events (compare for example amplitudes in table 6 with the distributions in figure 7).

RC#12: Grammar, typos: Generally speaking the language is ok, though at times there are problems with some sentences. Below in an non-exhaustive list of problems found by this reviewer while going through the document.

L5 “known as the Gloria fault”

> Answer: Corrected, now in line 5 reads “as the Gloria Fault”

L8 “a time span of 20,000 years” (is this what the authors are trying to say?)

> Answer: Yes. Corrected to 20 000 years

L29 “Tsunami have several characteristics that differentiate it from other natural hazards.” Maybe “Tsunami waves have several characteristics that differentiate them from other types of natural hazards”

> Answer: Corrected to: “Tsunami waves have several characteristics that differentiate them from other types of natural hazards”

L31 “Moreover, it is not possible to describe tsunami propagation using an “attenuation” law-like,” this is confusing, please improve/rephrase this sentence.

P3L1, delete the comma after “event”

> Answer: Corrected

P3L8 please use 20,000 years (this reviewer prefers this notation, and in any case there should be a space between k and years)

> Answer: Corrected

P3L21 “age difference, between a younger” delete comma after difference –

> Answer: Corrected

P5L12 and following. All parameters in the paper must be in italics. Please check these and the entire paper again.

> Answer: Corrected

P7L19 change to “the corresponding wave heights at 30 m water depth.

> Answer: Corrected

P8 L4 “Along the South Portuguese coast, from Vila do Bispo to Vila Real de Santo António.” Incomplete sentence

> Answer: Corrected. We deleted this incomplete sentence.

REVIEWER 2

RC#13 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. 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.

> Answer: This work is not a Probabilistic Tsunami Hazard study. It only discusses in detail some steps needed to conduct such studies. It focuses on the need to cope with kinematic constrains, particularly in cases where the historical catalogues are too short to allow any realistic assessment of tsunami hazard associated with earthquakes, which is the case of most of the geological environments on Earth. The choice of Gloria Fault is not related with its relevance to tsunami hazard in the NE Atlantic, but because it is a major oceanic transcurrent fault, that gives a good opportunity to include kinematic constrains in tsunami studies. Moreover the section 1 (introduction) presents an overview of the method; section 2 shows why in some cases we have strong kinematic constraints; sections 3 and 4 discuss in more detail the two main steps of the method (earthquake catalogue and tsunami fast simulation). Sections 5 and 6 show the application and results. We see no advantage in changing the organization of the paper.

RC#13: 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.

> Answer: This is out of the scope of this work. This is not a probabilistic tsunami hazard assessment study but an essential step for it. A discussion on uncertainties is provided in the revised text.

RC#14: 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.

> Answer: I simplified geodynamic setting is included in a new inset in Figure 1. Focal mechanisms for the three major earthquakes in the area are also provided in the same figure.

RC#15: 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 especially relevant as the model of Miranda et al. 2014 has not been validated.

> Answer: We understand the point raised by the author in what concerns the geometry used for the computation of the empirical Green functions used here, which is the only difference from what was done in the 2014 paper. Check the answer to RC#4.

RC#16: Finally, the references should be revised as they are misused, omitted and/or forced to support affirmations not always present in the original work.

> Answer: We spotted the main problems mentioned by the reviewer and corrected them in the text. These were found mainly on Table 1.

RC#17: 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.

> Answer: The references were checked and corrected in the revised paper: 1941 from Baptista et al., 2016; 1975 from Lynnes and Ruff, 1985. Two references were deleted accordingly in the revised document.

RC#18: 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.

> Answer: Blaser et al. (2010) (B2010) paper improves on the Wells & Coppersmith (1994), WC94, database, where 176 events were considered, adding 163 earthquakes from other sources. The authors of Blaser et al. (2010) recognize that WC94 scaling laws were dominated by earthquakes in the continents, while the new relationships also contain a significant number of new events along oceanic subduction zones. This is the reason why the two sets of relationships are different from each other. However, the oceanic strike-slips continue to be very poorly represented in this new dataset. None of the events presented in the Blaser et al., 2010 can be considered as a proxy for the Gloria Fault seismotectonic environment. We modified Table 2 to include the Blaser et al. (2010) compilation but the results show the same problems as the other two scaling laws investigated, besides the evident lack of representativeness. The text was changed accordingly and a new reference added. In our opinion the use of a semi-empirical scaling law remains the best approach. However, the investigation of the epistemic and aleatoric uncertainty on this choice is out of the scope of this paper.

RC#19: 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.

> Answer: We added the source proposed by Buforn et al., 1988 to Table 1. The width was considered as 45 km, meaning a complete rupture of the brittle oceanic lithosphere, as estimated for its age. The aspect factor is then 4, identical to the one considered also for the 1941 event. With these constrains the estimated average slip and stress drop are computed. We note that, as consequence, the estimated stress-drop is considerably reduced to 1.1x1010 Pa and the average slip is only 2.5 m. This last result is in contradiction with the tsunami modelling results by Kaabouben et al., 2008, that considered an average slip on the fault of 11 m adequate to reconcile tsunami observations with the source.
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> The low value of stress drop that results from the Buforn et al. (1988) source length is also in contradiction the intra-plate setting of this earthquake and at odds with the stress drop inferred for the 1975 event. In fact, even in the paper of Buforn et al. (1988) one event in the Azores zone has a stress drop close to 10 MPa (their figure 15). The knowledge of the stress drop is also important for the stochastic simulation of strong motion recordings. A recent paper (Carvalho et al., 2015) obtained a mean value of stress drop in the Azores area between 9 and 13 MPa, much higher than the value estimated from the Buforn et al. (1988) proposal. These are the reasons why we do not consider that solution in the evaluation of our proposed scaling law. The text was changed to include these arguments.
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> We don't identify the inconsistencies mentioned by the reviewer. We stand for our semi-empirical scaling law that respects self-similarity in terms of fault aspect ratio and stress drop.

RC#20: 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".

Answer: We agree. The text was changed accordingly.

RC#21: 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.

Answer: The explanation for this is given already in the text: there are not enough events in the area to define a Gutenberg-Richter law (see introductory sentence to paragraph 3.2). A simple lookup on earthquake data providers website may prove this sentence.

RC#22: Moreover, the authors do not explain how they obtain the earthquake activity rate in addition to the b-value.

Answer: the text was changed so that it now reads: "Considering mmin=6.0 we can estimate earthquake activity rate λ. This is done by summing the total seismic moment generated and computing the activity rate that fits exactly the kinematic constrains."

RC#23: 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.

Answer: In fact the text doesn't show the details mentioned by the reviewer and it was changed accordingly. It is explained that a randomly generated synthetic catalog does not comply exactly with the kinematic constrains that we impose on earthquake generation. As can be seen from the figure presented in the paper, the number of high magnitude events, even for such a long-time span of the catalogue, is very irregular. These two results led us to: i) choose the synthetic catalogue with a small deviation from the G-R law at large magnitudes; ii) adjust the time-span to force the closure condition of 100% compliance with the kinematic constrains. All this is better explained in the revised text.

RC#24: 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%.

Answer: The color codes may be a bit misleading to the casual reader, but all the information is given. We choose to saturate the color codes between the extreme values of slip along the fault to emphasize the heterogeneity of the final slip distribution. Taking into consideration the time span of the catalogue, the slip can be converted to slip rates and we obtain for GF1 a variation between 3.35 and 4.85 for 4.0 mm/yr average, for GF2 a variation between 1.5 and 2.65 for 2.0 mm/yr average and for GF3 a variation between 1.2 and 2.75 for 2.0 mm/yr average. The values mentioned by the reviewer are not found in the figure.

The adopted methodology imposes that the average slip rate on each fault segment complies exactly with the kinematic constrains, but this compliance is not observed on individual patches of the fault. Only heterogeneous slip distributions on faults may improve further on this. Another interpretation would be that the fault as a whole is not fully coupled, with patches of different seismic coupling. The methodology used in this paper can be easily adapted to that hypothesis by lengthening the time span of the catalogue so that none of the patches in the fault exceeds the kinematic constrain imposed. We added a note on the text commenting this possibility. The other concerns expressed by the reviewer are addressed by the new version of figure 5.

RC#25: 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.

Answer: We agree that the generation of the synthetic catalogue was poorly explained in the paper and this issue was addressed in the revised paper. But we don't agree that this is the most relevant work presented. In fact, the generation of random magnitudes according to a G-R law is so simple that we didn't think relevant to present much details on that process.

RC#26: 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.

Answer: The model used here was already been used in a number of previous studies, for both forward and inverse modelling. Some of those works are cited here and can be checked directly. The benchmarks can be obtained from the authors. To clarify this point, the following sentence was added to the acknowledgement section: "NSWING code can be obtained from the authors, as well as the corresponding benchmarks".

RC#27: 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.

Answer: see answer to RC#14. There is also a new figure 2, where the events are much better discriminated.

RC#28: 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.

Answer: The figure was changed and improved accordingly.

RC#29: 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.

Answer: We use the truncated Gutenberg-Richter law for the cumulated number of earthquakes (N[m≥M]) and the respective a and b parameters were added to the figure. The distribution of earthquakes on each class is obtained by differentiation the cumulated law and so the parameter a is meaningless in that case. The figure was redone to improve its quality.

RC#30: 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.

Answer: The figure was improved accordingly and the inset deleted.

RC#31: 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.

Answer: Figure 5 is difficult to improve because we need to show the whole dataset. Due to the large number of coastal POI we need to use a large number of symbols. It gives an intuitive assessment of the relative importance of the larger wave heights deduced from the numerical simulation and of its location.

RC#32: In tables 2 and 3 the values of seismic moment should be adequately represented.

Answer: We clarify in the text that the moment magnitude relationship used was taken from Kanamori (1985) and correct a few typos and the representation of the seismic moment.