Response point-by-point to Anonymous Referee #2

The point-by-point answers are in blue color, below each Reviewer's comment (reported in *Italic*).

##General comments##

This paper addresses an important topic, namely the development of onshore probabilistic tsunami hazard assessments and overcoming the related computational challenges. It builds on the work of Lorito et al. 2015 and Selva et al. 2016. A key innovation in this study is efficient filtering of near-field sources based on coseismic deformation, rather than offshore tsunami wave height. Overall, the paper is well written and concisely explains the issues and methods used to overcome them, and is suitable for publication in NHESS with some minor revisions.

In reviewing the paper, my main suggestions (details given below) are:

1. *Siting the introduction more broadly in the PTHA literature. While this paper builds directly on the work of Lorito et al. 2015 and Selva et al. 2016, which is heavily relied upon in the introduction, along with the review paper by Grezio et al 2017, there are a number of additional relevant papers related to PTHA problems that could be cited. In my opinion, this would more neatly place this paper within the broader context of PTHA literature, widening the appeal of the paper. I.e. this paper should be framed as a step forward in PTHA in general, not just an update of the Lorito and Selva methods (although it is that too).*

We fully agree and appreciate the suggestion. Citing Lorito et al. 2015 and Selva et al. 2016 was indeed mandatory. Conversely, using only Grezio et al. 2017 to refer to PTHA was certainly too simplistic. We will improve the bibliography to better frame the paper in the context, also following your specific suggestions below.

As a result, we added the following references, including also those stemming from other comments below:

Brizuela, B., Armigliato, A., and Tinti, S. (2014). Assessment of tsunami hazards for the central american pacific coast from southern mexico to northern peru. Natural Hazards and Earth System Sciences, 14(7):1889–1903.

Burbidge, D., Cummins, P. R., Mleczko, R., and Thio, H. K. (2008). A probabilistic tsunami hazard assessment for western australia. Pure Appl. Geophys., 165(11):2059–2088.

Davies, G., Griffin, J., Løvholt, F., Glimsdal, S., Harbitz, C., Thio, H. K., Lorito, S., Basili, R., Selva, J., Geist, E., and Baptista, M. A. (2017). A global probabilistic tsunami hazard

assessment from earthquake sources. Geological Society, London, Special Publications, 456.
Gailler, A., Calais, E., Hebert, H., Roy, C., and Okal, E. (2015). Tsunami scenarios and hazard assessment along the northern coast of haiti. Geophysical Journal International, 203(3):2287–2302.

Geist, E. L. (2002). Complex earthquake rupture and local tsunamis. Journal of Geophysical Research: Solid Earth, 107(B5):ESE 2–1–ESE 2–15.

Griffin, J. D., Pranantyo, I. R., Kongko, W., Haunan, A., Robiana, R., Miller, V., Davies, G., Horspool, N., Maemunah, I., Widjaja, W. B., Natawidjaja, D. H., and Latief, H. (2017). Assessing tsunami hazard using heterogeneous slip models in the Mentawai Islands, Indonesia. Geological Society of London Special Publications, 441:47–70.

Gusman, A. R., Tanioka, Y., MacInnes, B. T., and Tsushima, H. (2014). A methodology for near-field tsunami inundation forecasting: Application to the 2011 tohoku tsunami. Journal of Geophysical Research: Solid Earth, 119(11):8186–8206.

Harbitz, C., Glimsdal, S., Bazin, S., Zamora, N., Løvholt, F., Bungum, H., Smebye, H., Gauer, P., and Kjekstad, O. (2012). Tsunami hazard in the caribbean: Regional exposure derived from credible worst case scenarios. Continental Shelf Research, 38:1 – 23.

Horspool, N., Pranantyo, I., Griffin, J., Latief, H., Natawidjaja, D. H., Kongko, W., Cipta, A., Bustaman, B., Anugrah, S. D., and Thio, H. K. (2014). A probabilistic tsunami hazard assessment for indonesia. Nat. Hazards Earth Syst. Sci., 14(11):3105–3122.

Løvholt, F., Bungum, H., Harbitz, C. B., Glimsdal, S., Lindholm, C. D., and Pedersen, G. (2006). Earthquake related tsunami hazard along the western coast of thailand. Natural Hazards and Earth System Sciences, 6(6):979–997.

Power, W., Wang, X., Wallace, L., Clark, K., and Mueller, C. (2017). The New Zealand probabilistic tsunami hazard model: development and implementation of a methodology for estimating tsunami hazard nationwide. Geological Society, London, Special Publications, 456.

Satake, K., Fujii, Y., Harada, T., and Namegaya, Y. (2013). Time and space distribution of coseismic slip of the 2011 tohoku earthquake as inferred from tsunami waveform datatime and space distribution of coseismic slip of the 2011 tohoku earthquake. Bulletin of the Seismological Society of America, 103(2B):1473.

2. *Some assessment of the sensitivity to the choices made in the filtering process (i.e. choice of thresholds etc) and whether this has any implication to the broader conclusions. Also whether it is possible for biases to be introduced in this process.*

(the answer below is the same for a similar question from Reviewer 1)

The conceptual explanation traces back to the fact that the two procedures are not equivalent from a physical point of view and we could roughly say that one is in principle “correct” and the other one is “wrong”. Maybe in saying "it is important to distinguish near and far-field sources in the filtering approach" we were not clear enough. What we wanted to stress is that a blind filtering procedure based on offshore tsunami amplitudes produces a non representative selection of the important scenarios, as it could aggregate or even remove important local scenarios.

We try to explain it better below.

In the original procedure by Lorito et al., offshore tsunami amplitudes are supposed to be representative of the coastal inundation, regardless of the source location with respect to the coast. That was reasonable, since it considered either far field scenarios with respect to the coast of Sicily, or scenarios which deformed the coast of Crete Island always in the same direction, since they were all subduction earthquake on the neary Hellenic Arc.

Indeed, offshore tsunami profiles could be strongly misleading when coseismic deformation of the coast occurs, either as coastal uplift or subsidence depending on the causative earthquake. The coseismic displacement induced by local earthquakes can modify the actual onshore tsunami intensity corresponding to the same offshore wave. Hence, near field scenarios must be separately treated, and clustered considering the source similarities, including the co-seismic coastal displacement, rather than the offshore tsunami wave similarity.

We will try to report these “conceptual” arguments in the revised manuscript as concisely as possible.

The tuning of the thresholds in the filtering procedure is a different task, but we note that the same thresholds have been used with and without the correction for near field, so that the differences we found in the results obtained from the two procedures are not in our opinion imputable to those choices.

On the other hand, we can now support such conceptual justification providing the physical explanation of the specific results, based on the new quantity MU (mean uplift) we calculated and described in our introductive general remarks. This also answers to one of your specific comments below.

In general, lower ‘corrected’ hazard means that the predominant effect by local sources contributing to a specific point on the hazard curve - that is to the probability of exceedance for a given intensity threshold - is represented by coastal uplift, which in turn decreases tsunami hazard. In other words, there is a prevalence of clusters represented by scenarios causing uplift. Conversely, higher hazard would correspond to coastal subsidence.

As we said, we investigated this aspect, computing, for different intensity thresholds above 1m, the MU on a random point along the coastline of the inner grid, produced by near field representative scenarios contributing to the hazard at that threshold, weighted by the occurrence probability associated to each scenario (corresponding to the probability of the entire cluster it represents) and normalized to the probability of all of the scenarios contributing to the same intensity threshold.

The obtained positive values, although not representative of the real coastal displacement as averaged on all the scenarios (including that ones which do not produce appreciable coseismic local deformation), indicate that the dominant contribution to the coseismic deformation is an uplift of the coast, in agreement with the percentage differences retrieved between the two approaches.

We hope to have answered in this way to the “significant” concern expressed by the Reviewer. We must acknowledge that this comment made us deepen the analysis and consider our results much more carefully - and indeed we found a bug.

3. *Some comment on whether other metrics besides maximum tsunami height or co-seismic deformation could be relevant in assigning events to clusters.*

Yes, sure. This might be certainly relevant, at least for far-field sources. Storing and using the full waveforms, or considering maybe periods and polarities, or other approaches, can be considered.

Take into account though that this was already briefly discussed in Lorito et al. 2015. It was tested there that after some tuning of the length of the offshore profile of control points, the offshore height profile turned out to be a sufficiently good indicator for approximating the inundation afterwards. We may speculate that this is due to the collective information provided by the maximum heights themselves taken altogether, which then becomes a kind of maximum wave profile. Nevertheless, we will briefly discuss the issue in the revised manuscript, also using the examples you provide in your specific comment.

Vice-versa, as far as near-field sources are concerned, two modelled tsunamis with very similar sources should be quite similar, except in case of a very sensitive dependence on initial conditions - like for the butterfly-effect. We are not totally convinced but we will cautiously mention the issue in the revised manuscript.

In addition, there are several minor areas for clarification to improve the communication of the results, and a few grammatical errors.

##Specific comments##

1. Introduction
As mentioned above, this could benefit from reference to broader PTHA literature, specifically:

- *P2L4: Should also cite other PTHA studies as incremental gains in uncertainty quantification have been made over the past decade or so. Include Burbidge et al 2008; Gonzalez et al 2009; Horspool et al 2014; Davies et al 2017, Power et al 2017 (there may be others).*
- *P2L7-8: These references (Geist and Lynett 2014; Grezio et al 2017) are not the first to emphasise computational approaches to PTHA – see additional references suggested in the above point.*
- *P2L10: Should also reference Davies et al. 2017 regarding uncertainty quantification.*
- *P2L13: Gonzalez et al 2009 should be cited in reference to challenges of PTHA for inundation.*
- *P2L16: Geist 2002 should also be mentioned here.*
- *P2L17. Mueller et al 2014 and Griffin et al. 2015 have both undertaken on-shore tsunami hazard assessments considering heterogeneous earthquake rupture; although neither was fully probabilistic, they should be mentioned here as first steps towards quantifying this uncertainty for inundation hazard. Both also discuss the effect of coseismic displacement on onshore hazard and how this can vary locally, as discussed on P3L2. Here (P3L2) the discussion could be expanded to provide greater justification to your methodological approach to near field hazard.*

These references provide as said a broader context to the paper and we already listed above those we will include in the revised manuscript.

We will improve the introduction accordingly, following all your suggestions, for the different categories, such as: the uncertainty quantification, the computational approaches, challenges for PTHA inundation, rupture complexity and near-field, coseismic displacement and onshore hazard.

- *P1L20: This isn't true. In practice many inundation assessments also use 'representative scenarios' for a range of return periods, not just 'worst credible'.*

We will clarify the statement, adding the mention to "representative scenarios" and some appropriate references - listed above as well, mostly using some scenarios for different representative recurrence times sometimes combined with worst case ones, that is: Gailler et al. 2015; Harbitz et al. 2012; Løvholt et al. 2006; Brizuela et al., 2014.

- ➢ *P1L22: One or a limited range of inundation scenarios get used for much more than 'a first screening' by emergency managers. These scenarios regularly get used to develop emergency management plans, evacuation plans, undertake impact assessments and so on. In my opinion this paragraph severely underplays the utility of scenario hazard assessments. The main problem is that we can't translate the offshore probability to an onshore probability. I expect that even with probabilistic inundation hazard maps, single event scenarios will still be used for a range of emergency management scenario planning purposes – we'll just be in a position to actually say what the probability of the event in terms of inundation hazard is.*

Here we need to disagree a bit; or better, this is not what we meant, since we also wrote: "to realize very detailed assessments of specific scenarios." This goes beyond the "first screening", in our intention. We will clarify this in the revised manuscript, also referring to disaggregation of PTHA for selecting physically meaningful individual scenarios. Instead, we are sorry but we are not sure we understand the statement "we can't translate the offshore probability to an onshore probability.", since in this paper - as well as in other papers from different authors - fully probabilistic inundation maps are presented.

- ➢ *P2L20: Need to clarify that this is talking about onshore PTHA – offshore PTHA are in general computationally affordable (though not cheap!) these days.*

Agree, we will modify the text accordingly.

- ➢ *P2L30: 'while solving all the emerging technical and scientific issues'. This seems a fairly bold claim! Perhaps rephrase.*

We apologise for the misunderstanding: it was intended to emphasize that the work also concerned the implementation of the procedure, which was not trivial. We will rephrase according to your comment.

2. Method outline This section is clear and well-written

3. Implementation of an improved filtering methodology

- ➢ *P5L4-5: How confident are you in the assumption that similar wave heights lead to similar onshore hazard? What about other wave properties such as period, which may be significant in determining onshore behaviour. E.g. Satake et al 2013 showed how inundation from the Tohoku tsunami was variably controlled by long-period components on flat coastal plains and shorter-period peaks in steep coastal areas. While set within a tsunami warning context rather than hazard assessment context, Gusman et al 2014 used two cycles of a tsunami waveform in identifying 'similar' tsunami. I think some of the issues are resolved for near field tsunami in your coseismic deformation filtering approach presented following, but it could still be good to comment on this issue here.*

We generally agree and we have responded to the related general comment 3. above. We nevertheless give some specific answers here, partly repeating our previous answer.

The general assumption that, for a given source, offshore tsunami amplitude profiles are representative of the coastal inundation behind was applied and tested in the previous work by Lorito et. al (2015). On the other hand, we agree that caution must be used as well, since the previous paper did not deepen into any possible specific case.

Indeed we faced for example the problem when treating near field scenarios, as you observed.

We also agree that there might be other issues. As said the wave period is an important property controlling the tsunami impact: in fact, we someway accounted for that by considering a control profile along the target coast, advancing a kind of ergodic hypothesis.

Future developments of our method could take into account a Gusman-like approach, considering the tsunami time history at each point of the control profile, instead of just the maximum wave height. We will nevertheless add a few comments about this in the manuscript adopting the suggested line of reasoning. We thank you for pointing this out.

- *P6L30-35: It is not entirely clear how the distance is measured across the grid of coseismic deformation points, and how the spatial component is handled – perhaps also write the relevant equation to ensure clarity.*

The comparison between the coseismic deformation fields is carried out point-to-point. The squared Euclidean distance is the metric used for the cluster analysis and only the vertical components are taken into account. We will try and rephrase for the sake of clarity.

4. The Milazzo oil refinery

- *P8L28: The abbreviation Mmax is very commonly used to mean the maximum magnitude for a given earthquake source in seismic and tsunami hazard assessment. I would suggest changing this to something else to avoid confusion.*

We will change this MFmax (maximum momentum flux). This will appear as well in the new Figures (those after correcting the results for the bug) in the Supplementary Materials.

- *P9L11: This should be 'overestimates the probability for a given Hmax relative to STEP (3b).*

Ok, we will modify the sentence as suggested.

- *P9L23: Should these be >=, not =, if you're talking about probabilities of exceedance.*

Probability maps are obtained by vertically "cutting" the hazard curves for each point of the grid, i.e. representing on a map the exceedance probability values for a fixed H_{max} . In this sense, the "=" sign is correct. We will add the "(exceedance)" in parentheses before probability for clarity.

- *P9L26-30: Use of phrase 'positive' and 'negative differences' is confusing and makes the meaning of the paragraph somewhat ambiguous. Better to rephrase stating more explicitly which model gives relatively higher/lower hazard etc.*

We agree that the sentences are quite unclear; we will rephrase them referring more explicitly to higher/lower hazard.

- *Also, the difference between results far inland, near the coast and offshore in Figure 4a need to be explained. Why the shift from negative to positive differences at some distance inland from the coast?*

We first need to point out that Figure 4 has changed based on the new results.

We assume that you are referring to the Figure with the differences between the probability maps for steps 3a and 3b and for the intensity threshold of 2 meters (the top right one). We apologise for the confusion and we will add labels where missing to all Figures.

However, note that now this Figure is quite different from before, as it contains more positive values. This is consistent with the new Figure 3c, where the differences are already positive for this threshold and even larger for the 3 metres threshold.

As far as the negative inland values are concerned, note that they occur for very low probability values. So, maybe they shouldn't be overinterpreted. We will however comment all the new Figures based on the new results in the revised manuscript.

- *P10L5: Can anything additional be said about possible biases in the sampling process? Why is it likely that the sampling produced a non-representative selection of the important scenarios? How does this overall affect the strength of you conclusions in comparing the two methods (i.e. could the differences be random rather than systematic).*

What we meant here is that without the correction for near field, namely without a separate treatment for remote and local sources, the filtering procedure provides a non-representative selection of the important scenarios.

This is due to the basic assumption that offshore tsunami amplitudes can be considered representative of the onshore tsunami impact, which surely introduces a bias when the scenarios which deform the coastline are not separated by those that doesn't do it (step (3a)).

For example, admit that 2 different scenarios will both produce 1 meter wave offshore. However, one scenario uplifts the coast of 1 meter, the other one creates 1 meter subsidence. The two inundations will be dramatically different, but the two scenarios would be nevertheless grouped by (3a) under the same cluster.

Therefore, the procedure proposed as step (3b) is in principle the correct one to evaluate site-specific tsunami hazard, when local effects of coseismic deformation can not be neglected.

5. Conclusions

- *P10L10: The statement around the definition of the source scenarios seems a bit strong. I'd suggest removing the word 'fully' as I doubt this has really been done. Aleatory uncertainty applies to both the rate model and the source location, geometry, maximum magnitude etc. I'd suggest putting 'and their mean annual rates' prior to 'exploring source uncertainty'.*

We agree with the comment: the word "fully" here is misleading. Although in principle the proposed methodology allow us for a full exploration of the aleatory uncertainty, some practical limitations are always present in real life. We will correct the text accordingly.

- *P10L19: Suggest 'from offshore wave amplitudes alone'. Also, what about other parameters such as period for non near-field tsunami? This links back to my comments on Section 3.*

Ok, got it, we'll rephrase by saying only that it is unlikely that the assumption holds if there is co-seismic coastal displacement.

Figures:

- *Figure 1: Step 2 should read 'tsunami propagation to offshore points'*
- *Figures 3-5 need labels for parts a), b) etc.*

OK, we added labels where missing in the new figures.

##Technical corrections##

- Throughout: Why use STEP instead of Step?
- P1L11: demonstrate not demonstrated
- P2L25: Rephrase to 'This allows identification of a subset of ...'
- P2L29: Rephrase to 'Here we merge the two approaches of Lorito et al....'
- P3L21: Change 'resume' to 'summarise'. Also P10L9
- P4L8: Change 'enough representative' to representative enough'
- P6L10: Change 'and a separate modelling' to 'and separate modelling'
- P6L34-35: Change to 'while the stopping criterion is based on the Euclidean distance'
- P7L16: Mediterranean Sea (not sea)
- P8L1: Replace 'Namely' with 'That is'; delete 'even'
- P8L25: Please specify the shear modulus used for the Okada calculations

- P9L15: Remove ‘supposedly’
- P10L24: Change ‘has not to be’ to ‘is not’.
- P11L3-4: I think this should read ‘As a consequence, the effect of coastal deformation on tsunami hazard can not be deduced...’
- P11L14: Change to ‘...the approach developed here allows consideration of a very high number...’

We thank you for these technical corrections, which will be all addressed in the revised manuscript. Concerning the Okada calculations, a poissonian solid is assumed with $\lambda=\mu$, so that the Okada results are independent of the shear modulus.