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Discussions

Interactive comment on "A probabilistic tsunami hazard assessment for Indonesia" *by* N. Horspool et al.

Anonymous Referee #1

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Review of the paper entitled "A probabilistic tsunami hazard assessment for Indonesia" by N. Horspool et al. submitted to NHESS

The present document contains a review of the discussion paper entitled "A probabilistic tsunami hazard assessment for Indonesia" by N. Horspool and co-authors. The Probabilistic Tsunami Hazard Assessment (PTHA) was first established in the early eighties by Lin and Tung (1982) and later by Rikitake and Aida (1987). Following the 2004 Indian Ocean tsunami and the availability better computing facilities, the use of PTHA has increased much due to the development of Geist and Parsons (2006) (a review of PTHA is given in Geist and Lynett, in press). The present paper uses the PTHA method to quantify the tsunami hazard in Indonesia, and is one of the most elaborate uses of this method published to date. The work is extensive and covers many different

aspects from interpretation of seismic and tectonic, tsunami modeling, and quantification of tsunami probability distributinos. I therefore believe that the present manuscript will be a good contribution to NHESS. Reading the paper however, I find that there are a number of issues, mostly of minor character, that should be addressed prior to publication. The comments are given in detail below. The most general underlying comment is that the paper needs to be even clearer when it comes to communicating limitations of the method (although there is no doubt that the authors tries to communicate limitations as such). In particular, it is important to realize that the use of judgment is still vital in present PTHA as in scenario based methods, and it probably becomes most important for high magnitude earthquakes which we expect to dominate risk (see for instance the review of Løvholt et al. 2012a for a discussion). I also think that the process from source statistics to tsunami hazard could be illustrated better. Through the comments below, I hope to shed light on some additional issues that are less / not discussed. We may only improve our future methods by highlighting its present weak points. To this end, I hope that the comments below will prove beneficial to the paper.

Line-by-line comments:

Page 3425, line 3 – Please check whether the population exposure matches the 2013 report (and not 2009). The papers of Løvholt et al. (2012a; 2014) provides a much more detailed review of the tsunami related work than the UN-ISDR reports, and could preferably be cited in addition.

Page 3425 – line 5 – As most of the fatalities are due to the Indian Ocean tsunami (IOT), you should probably give both fatality numbers (IOT and the rest) to put things in perspective.

Page 3425 – line 9 – Replace "tsunami" with "tsunamis"

Page 3425 – line 10 – Are these high risk or highly hazardous locations? If high risk, please provide references if available.

Page 3425 – line 16 – Maybe rephrase this sentence ". . . scenario database existing . . ."?

Page 3425 – line 23 – A newer and more updated database was presented by Løvholt et al. (2012b) should perhaps be cited.

Page 3425 – line 26 – Actually quantifying the risk rather than the hazard is needed for prioritizing mitigation measures. Given the multidisciplinary nature of this paper, maybe a brief definition of risk and hazard could be given up-front?

Page 3425 – line 28 – Consider citing the two earliest PTHA studies given above in addition to the many newer ones.

Page 3426 – first paragraph – Many authors have previously pointed out the differences between the PTHA and PSHA. I think that the many comparisons between PSHA and PTHA are distracting the main message in the paper, and I suggest cutting them to provide a more compact paper.

Page 3426 – line 12 - Typo - "Futhermore".

Page 3426 – line 12 – PTHA have many benefits over conventional scenario based methods, and most importantly it provides a formalized framework that makes the hazard assessment less arbitrary. Ideally, the PTHA would be superior to scenario-based methods. However, one should also realize that compared to the scenario based method, PTHA may become less transparent due to the large number of scenarios, and that potential errors become more concealed.

Page 3426 – line 23 – Reword "most likely sources", to something like "unit sources that contributes most" etc.

Page 3427 – line 2 – Please note that other PTHA studies such as Gonzalez et al. (2009) consider also extensive inundation mapping.

Page 3427 – lines 5 – 10 – Consider removing the PSHA comparisons, this has been

raised by other authors and defers from the main objectives of the paper.

Page 3427 – line 10 – The use of the term 'tsunami greens functions' is not correct as Greens functions refers to a specific mathematical definition. Instead, I suggest that the authors uses terms such as 'superposition of results from different tsunami simulations' or similar. The document needs to be checked troughout.

Page 3427 – point 1 – Does the authors mean 'fault sources' or just 'fault geometries'? Later, they define unit sources (on the faults).

Page 3427 – point 3 – The abbreviation MFD used later in the text needs to be defined.

Page 3427 – point 3 – More details on how the synthetic earthquakes are constructed should be preferably be given.

Page 3428 – line 10 – 'tsunami' → 'tsunamis'

Page 3428 – lines 14-15 – Experience from the eastern Indonesia may tell a different story than Brune et al. (2010), and historical landslide induced events from for instance the Banda Sea may in fact be more frequent. However, the above argument mentioned by the authors (that probabilities are difficult to determine) is sufficient. Some further support for this is given in the papers by Harbitz et al. (2013) and ten Brink et al. (2014).

Page 3428 – line 16 – In a reassessment of the NGDC data, NGI (2009) found that tsunamis reported due to many low magnitude earthquakes may alternatively be explained as landslides. Unfortunately this discussion was omitted in the later paper (Løvholt et al. 2012) and is somewhat less accessible.

Page 3428 – line 20 – Why separate 'regional' and 'distant'?

Page 3429 – line 16 – This sentence seems immediately unsubstantiated (although it seems intuitively correct). If retained, please elaborate or provide a reference.

Page 3429 – line 22 – typo 'regrssion'

Page 3429 – line 22 – The treatment of Mmax is one example of the use of subjective judgment, and this needs to be stated very clearly. Its possible implications on the results also needs to be explained.

Page 3430 – line 2 – The paper of Geist (1999) is another useful reference.

Page 3431 – line 2 – As mentioned above, please use another term than 'greens function'.

Page 3431 – line 10 – Add 'unit source' ahead of 'subfault'. Besides, a single magnitude unit source would mean that the same shear stiffness (or rigidity) applies throughout the depth? This would underestimate, perhaps severely, the lower magnitude contributions. In that case, the authors need to discuss how a stiffness adjusted unit source is expected to influence their hazard curves.

Page 3431 – line 14 – Which are the 'crustal parameters' explicitly used here?

Page 3431 – line 15 – Please rephrase section head title.

Page 3432 – line 2 – How are the summation made, in terms of maxima or full time series?

Page 3432 – lines 14-18 – Using this method, accuracy is a concern for the smallest and deepest unit sources. As a minimum, the authors should state how many grid cells they use per wavelength for their smallest sources to assess the accuracy.

Page 3432 – line 5 – Is the grid regular in lon-lat (or projected x-y)?

Page 3432 – line 21 – Please note that this is amplitude dependent; the wave may appear as linear at much shallower depth than 100 m. The 100 m depth is probably a conservative choice however.

Page 3432 – line 26 – The authors should note that Greens law become dependent of the terminal depth. This is partly why the use of amplification factors was used by Løvholt et al. (2012a).

Page 3433 – section 2.4 – The difference in aleatory and epistemic uncertainty may not be so clear when dealing with this in practice, as, in the end of the day, they will both be uncertainties that needs to be quantified. Here, epistemic uncertainties (magnitude recurrence etc) are treated via logic trees (discrete weighting), while aleatory uncertainties (computational, geometry, slip realization etc) are treated via continuous probability density functions (PDFs). It seems to me that this binary treatment of uncertainties are selected more based on modelers needs than whether or not the uncertainty is actually aleatory or epistemic. For instance, computational uncertainties may be reduced by better data and more accurate models which are clearly due to lack of evidence. The same apply for modeling the dip angle. Due to our too short observational record, there is an uncertainty in the MFD involved particularly related to the largest magnitudes. Here, such uncertainties are constrained (the PDF is discrete as opposed to the continous for the 'aleatory') by scientific judgment, which has a direct effect on the hazard assessment. It is crucial that the authors point out this. To clarify, one suggestion could be that the authors show observed seismicity from some of their source regions and plot employed MFD with the different weighting parameters on top of that to demonstrate visually how their MFD modeling assumptions comply with data. This will also serve the benefit to show how much modeled magnitudes overshoot the observational record. Finally, it will demonstrate in what magnitude range such judgment is needed, and where data exist.

Page 3434 – lines 12-14 – See comment above. The use of judgment is apparent, and the authors may want to elaborate on the basis on their selection of ïĆś0.2 here.

Page 3434 – line 20 – This is another example of use of a judgment which has large implications for the conclusion. Again, a discussion of (1) why these weighting factors are selected should be discussed, and also their possible implication on the conclusion (this may be given later).

Page 3436 – line 16 – Is the unit source magnitude depth dependent or not?

Page 3437 – eq 2 – please define 'u'.

Page 3439 – line 12 – It seems clear to me that the some of the MFDs used by the authors should be illustrated within the paper (not only the final modeled surface elevation). The link between the formulated temporal source statistics (i.e. MFD), preferably including also seismic and tectonic information (if available), is an essential element of the analysis.

Page 3440 – How does the authors findings comply with the observational record? First, tsunami observations in Java may suggest that the analysis is on the lower side? Slow events have been prominent here, e.g., the 1994 and 2006 events (Tsuji et al., 1995, Fujii and Satake, 2007). 2009 Mentawai is another that must be considered relevant. What about landslides in eastern Indonesia? In certain regions, there are inconsistencies between the observations and the PTHA that may point to landslides. I think the manuscript would benefit from discussing this topic.

Page 3441 – first paragraph – What is the implication of this shielding for major vulnerable cities, such as Padang?

Page 3441 – third paragraph – Does this comply with observations for instance in the Banda Sea? See above comment.

Page 3443 – Based on the comments above, some additional points may be added to the discussion section.

Page 3443- line 25 – I cannot see that tsunami hazard in the east (north of Java) is similar to the west and south based on the hazard maps? Maybe it is better to distinguish between the Java and Sumatra trench and the (North) East?

Page 3444- line 2 – return periods < 1000 year are in my opinion not short. . .

Page 3444 – section paragraph – Are there other examples globally of large sequential ruptures?

Page 3445 – second paragraph – Including landslides in PTHA for landslides is presently more difficult, see reviews of Harbitz et al. (2013) and ten Brink et al. (2014). The use of dispersive models is not the major obstacle, rather it is dealing with much larger uncertainties.

Page 3446 – line 2 – One example is the study of Løvholt et al. (2012b). It is not PTHA but it is relevant nevertheless.

Page 3446 – lines 10-11 – I think the authors strategy of representing slip variability effects through uncertainties is wise given the large computational load required for general slip representations. By the way, others have already quantified this variability to some extent (Geist 2002, McCloskey et al., 2007; 2008, Løvholt et al., 2012).

Page 3446 – section 4.4 – I support the authors that PTHA should be tested. The authors should consult Geist and Parsons (2006) and perhaps Gonzalez et al. (2009) for the comparison with observational data.

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