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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Assessment of tsunami hazard for the American Pacific coast from southern Mexico to northern Peru" *by* B. Brizuela et al.

Anonymous Referee #1

Received and published: 13 August 2013

Review of the paper "Assessment of the tsunami hazard for the American Pacific coast from southern Mexico to northern Peru", by B. Brizuela, A. Armigliato, and S. Tinti.

General comments:

The present paper discusses tsunami hazard quantification over a section of the Pacific coastline covering southern Central and norther South America. Only local earthquake sources are considered (i.e. non-seismic and far field source are omitted), and the hazard evaluation is regional. The tsunami hazard in the region is previously little studied, and the paper demonstrates it is significant. The study is therefore timely. The

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applied methods seems reasonable for a first order approach for providing mean estimates. However, I feel that there paper includes a number of points that needs major improvement before the paper could be accepted for publication. The most important improvement pointsÂÍ to be made are in part linked to discussion and benchmarking of the hazard methodology (1), and in part linked to the clarity of the presentation (2). Below, I provide some elaboration to points 1 and 2, these are partly repeated also under the specific comments. In addition, there are some smaller improvement pionts listed below, including corrections.

1 - The authors utilize databases extensively, including tsunami and earthquake databases as background for statistics, i.e. Magnitude - Frequency distributions. Key results are hazard maps for 50, 100, and 500 year return periods. At the shortest return periods, run-up up exceeding 4-5 m are observed. However, these results are not compared against run-up distributions from publicly available databases that span several houndred years (although the more recent events are covered better). This opportunity of comparing the results against field data, at least for the 50 year return period, is not utilized. Secondly, the results for the smallest return periods seems somewhat high. A worst case approach is taken by assuming shallow rupture, but this approach may be biased for the smaller magnitude earthquakes. I strongly suggest that the authors provide a more in depth discussion on the applicability and strengths / weaknesses of the model using the present case. As the applied method appears to be a hybrid of more conventional methods such as worst case scenarions and PTHA, a discussion of how the applied method related to these is recommended. A short paragraph was found in the conclusion section, but this is not sufficient. Finally, citations to the work of other groups may be given somewhat more emphasis as this is presently a bit one sided.

2 - Although parts of the paper appears well written, the presentation needs clear improvement. This is due to an unclear presentation and structure of the paper rather than due gramatical errors. Several explanations are spread into different parts of the

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paper. Moreover, essiential input such as source parameters must be explicitly given. I would recommend the authors to review the paper to provide a more clear presentation, and perhaps reduce the length. Avoid the use of repetitions and ambigous statements. I have provided some examples below.

Specific comments:

Abstract line 15: First you discuss "a statistical" approach. Next, you say that "A deterministic approach is then used". Please reformulate, i.e. clarify that the statistical input is used as input to the deterministic approach that are conducted for different return periods.

Page 2984, line 23: Please review if this (area and population) is relevant information, as you are investigating a coastline section. In general, the paper would benefit from cutting some superfluous information to reduce its length.

Page 2985, line 7: It seems possible to me that a landslide could have triggered this tsunami as its magnitude is low given the strength of the tsunami? The authors may wish to make a point out of this.

Page 2985, lines 15-20: The statements made here are partly conflicting those above, i.e. that tsunamis are partly forgotten hazard in this area. Please clarify.

Page 2985, line 22: Revise wording of sentence starting with "The need of setting ..."

Page 2985, line 27: You should state your main assumptions here as well, i.e., that you solely look at local earthquake sources.

Page 2986, line 1-6: The description of the hazard methodology is confusing (see above comment for abstract). The introduction is generally lacking a discussing of other hazard methodologies. Examples of other methods include for instance worst case scenario based methods (e.g. Lorito et al., 2008), or PTHA (e.g. Geist and Parsons, 2006; Power et al., 2007; Gonzalez et al., 2009)

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Page 2986, line 9: Please provide a reference to the Catalogue.

Page 2986, line 13: I would be careful with using the term "GR law", rather use "GR relation".

Page 2988, Section 2.1: This section also review past work on tsunamigenesis, consider renaming.

Page 2988, line 18: Please elaborate on why the Pacific is more tsunamigenic.

Page 2989, line 23: Please provide reference to the Imamura-Iida scale.

Page 2900, line 1-2: Is it realistic that all past tsunamis are caused by earthquakes? Experience from other regions with limited earthquake potential may indicate that landslide sources are underreported (e.g. Løvholt et al., 2012a).

Page 2994, lines 3-10: Different GR relations have been used by others. Some reasoning for the suggestion of the present GR-relation is suggested, see e.g. Geist et al., (2009)

Page 2994, line 20: Sentence starting with "The main idea ...". Please revise wording or remove sentence.

Page 2994, line 21: Sentence starting with "The occurrence ..." and next sentence doest not make sense to me. Please revise wording to clarify. Here, the authors repeat above statements which is confusing to the reader.

Page 2995, line 1: If the fault parameters are deduced from the magnitude the reference should be given. But such references are stated later in the text. The paper would highly benefit from a more compact presentation of the method.

Page 2995, line 10-11: Here, the authors should preferably include reference to other groups in addition to their own.

Page 2996, line 6-7: Please explain more explicity how the activity rate is reduced.

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Page 2996, line 19: Again, the method should be explained more explicity. For instance, long near shore sources would have practically no spreading, whereas short distant sources are attenuated. Please elaborate.

Page 2997, line 7: The methodology follows closely many of the procedures previously published by Løvholt et al. (2012ab), and a reference must be included, either here or above.

Page 2998, lines 10-13: The reader should be notified that there may be substantial corrections to this assumption.

Page 2998, lines 14-19: This is partly stated above, repeated statments such as this should be avoided.

Page 2999, eq 5.: A reference to page or equation number in C&G would be helpful to the reader.

Page 3000, A discussion regarding the validity may be done for instance by performing a (simple) comparison with run-up data from a database such as NGDC. As some conservative assumptions are made, there be implications on the results that needs to be discussed here.

Page 3000, line 25-30: These assumptions should be placed up front.

Page 3001, line 10: The authors assume shallow rupture, which indeed characterise tsunami earthquakes in addition to low rigidity. However, the rigidity is rather large (50 GPa). This is a strange assumption given that the earthquake scenarios are shallow, and hence this is underconservative. However, it may balance other conservative assumptions. In general, the authors should explicitly state their earthquake parameters as the method would be more transparent.

Page 3001, line 20: Do you mean in this region? Globally, there are many more examples.

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Page 3002, line 25: Note that Gonzalez et al. (2009) found that local earthquakes tend to dominate the hazard. The authors may want to make a point of this.

Page 3003, lines 18-25: As mentioned above, PTHA should also be discussed.

Some technical corrections:

Abstract line 10 - Remove "first" and "then".

Page 2986 line 25 - Remove "at the moment".

Page 2988 line 13 - Remove "little"

Page 2995 line 26 - Replace "not so numerous" with "limited"

Page 2994 line 4 - Reword sentence starting with "They tend to be ...", i.e. "The profiles shown in Fig. 10 are almost equally ..."

Page 3001 line 1 - Replace "I deed" with "indeed"

References:

Geist, E., and T. Parsons (2006), Probabilistic analysis of tsunami hazards, Nat. Hazards, 37, 277–314

Geist, E.L., Parsons, T., ten Brink, U.S., and Lee, H.J., (2009), Tsunami Probability, in Bernard, E.N., and Robinson, A.R., eds., The Sea

González, F. I., et al. (2009), Probabilistic tsunami hazard assessment at Seaside, Oregon, for near- and far-field seismic sources, J. Geophys. Res., 114, C11023, doi:10.1029/2008JC005132.

Lorito, S., M. M. Tiberti, R. Basili, A. Piatanesi, and G. Valensise (2008), Earthquake-generated tsunamis in the Mediterranean Sea: Scenarios of potential threats to Southern Italy, J. Geophys. Res., 113, B01301, doi:10.1029/2007JB004943.

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and present tsunami hazard in eastern Indonesia and the southern Philippines, J. Geoph. Res., 117, B09310, doi:10.1029/2012JB009425

Løvholt, F., Glimsdal,S.,Harbitz,C.B.,Zamora,N.,Nadim,F.,Peduzzi,P.,Dao,H.I., Smebye, H., (2012b). Tsunami hazard and exposure on the global scale. Earth-Science Reviews 0012-8252. doi:10.1016/j.earscirev.2011.10.002

Power, W., Downes, G., Stirling, M., (2007). Estimation of tsunami hazard in New Zealand due to South American Earthquakes. Pure and Applied Geophysics 164, 547–564.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 2983, 2013.