Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2018-25-AC1, 2018

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Assessing the tsunami building vulnerability PTVA-3 and PTVA-4 models after the 16S 2015 event in the cities of Coquimbo – La Serena (Chile)" *by* Eduardo Fritis et al.

Eduardo Fritis et al.

tlabraca@gmail.com

Received and published: 16 May 2018

Referee: The application looks appealing but the manuscript must undergo substantial changes in order to be fully understood and applicable in other cases. First and perhaps the most profound concern is that I don't quite see what are the research questions the authors are trying to address and what are the novel aspects of their work besides its application in this particular site. The methodology has already been applied in many other sites so the authors should emphasize why this particular application is scientifically interesting. As it is, the study could be useful for local agencies and/or planners as part of their decision making process but cannot be regarded as an

Printer-friendly version

Discussion paper

originally scientific work. Answer: This work presents the results of the use for the first time of the PTVA models in Chile, one of the most tsunamigenic areas of the world. Furthermore, we study a real scenario in contrast with most of the published works in which modeled scenarios are presented. All the above imply a new and relevant approach when compare with previous works in other study areas around the world. Our decision of using the two latest versions of the model lies upon the extraordinary opportunity of having the real damages caused by the 2015 tsunami impact in the study area. This has allowed us validating which of the two versions results in RVI trends similar to the occurred damages. Obviously, our results will be helpful in future urban planning assessment in Chile as decision makers will know which model will have more representative results in terms of vulnerability. Nevertheless, the case study of Coquimbo – La Serena is, to our understanding, a highly valuable contribution to the tsunami risk science.

Referee: Amendments should be done to improve the introduction (some sentences are poorly structured or definitely have no meaning), the methodological aspects which are vaguely presented and on the poor discussion, including sensitivity of the results to various assumptions done with no further explanation. Answer: Using the helpful and exhaustive revision done by the referee in the commented manuscript we will be able to improve the new version including the introduction, the methodology and the discussion both in content and form.

Referee: I doubt if this study can be replicated given the few details in many of the parts of the manuscript. This is especially true for the flooding "scenario" which is vaguely explained. It is unclear what is the (tsunami) model' setup (if there is a model, as this is not clearly explained as well), what are its assumptions and limitations and how the validation is carried out. Given that this is an actual tsunami (not a scenario, as the authors consistently mention), there are readily available records of runups and water depths, as well as numerical models in the literature that provide spatial information of the flooding and from which the authors should take advantage from. Answer: We

strongly disagree with the referee comment. In our opinion our reconstructed scenario of the tsunami flood occurring on September 16, 2015 is more easily replicated by other authors than numerical models as well as more reliable. We have used field inundation height and runup data and we have reconstructed the scenario as other authors have previously done in real tsunami cases (for example, Mas et al., 2012 – NHESS). The scenario was validated calculating the RMSE (root mean square error) that is a frequently used measure of the differences between values predicted by an interpolation and the values actually observed (our field measures). Nevertheless, we will improve the 'Tsunami inundation map' point in the methodology so it is clearer the reconstruction we have carried out.

Referee: The validation of the final results (relative vulnerability index) is not clearly explained, given that there is abundant information of damage from a MINVU. Answer: As the MINVU dataset for the damages occurred in Coquimbo – La Serena is large, we have only used for the validation stage the MINVU final damage classification for the buildings located in Sector 2 – Baquedano, considering it was one of the most damaged by the tsunami. This classification was then compared with our RVI score results.

Referee: The authors should discuss the advantages or withdraws of the used methodology with respect to other approaches which provide much more detail (e.g. fragility curves) and are currently embedded in the common research practice. They should also discuss if the application of these methods (PTVA3 and PTVA4) to a single case is enough to generalize which one is better, as is suggested in the text, and what consequences do the modifications of these methods have on the results (are results sensitive to these modifications?, are there other ways to lump two categories into one?). It is not explained nor justified why the use these two models and disregard older versions of the PTVA or other approaches. The authors seem to be driven by one train of thought but should be a bit more sceptic with the results they obtain. Answer: A brief discussion of the use of fragility curves in the vulnerability assessment when compare

with the PTVA models will be included in the new version of the manuscript. However, we cannot justify why we discard every other single model published for tsunami building vulnerability estimation. We selected the two latest versions of the PTVA model as the version 3 has been widely used (see Vulnerability index calculation section in Methodology) and version 4 claims to improve version 3 (Dall'Osso et al., 2016).

Referee: The manuscript should also improve the poor language which I believe is due to the possibility that authors are nonnative English speakers. The authors should be specific in the use of terminology which is used in a somewhat vague way (e.g. height, runup, water depth and crest to trough amplitude or hazard, vulnerability, impact, risk). I enclose a revised manuscript in pdf format with 74 comments, most of which are related to formal aspects. Answer: The new version of the manuscript will be reviewed by an English native speaker and all the terminology will be checked and defined in the manuscript so it is clear the way the different terms are used in the manuscript. We thank the referee for his/her comments in manuscript that will be considered to improve the new version.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-25, 2018.

Printer-friendly version

Discussion paper