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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.

Anonymous Referee #1

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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 originally scientific work. 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.

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. The validation of the final results (relative vulnerability index) is not clearly explained, given that there is abundant information of damage from a MINVU.

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 PTVAor 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.

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.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-25/nhess-2018-25-RC1-supplement.pdf

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