

Interactive comment on “Coastal flooding risk associated to tropical cyclones in a changing climate. Application to Port of Spain (Trinidad and Tobago)” by Cristina Izaguirre et al.

Anonymous Referee #2

Received and published: 20 July 2017

The authors propose an ambitious paper aimed to provide a methodology to assess present and future (in climate changing due global warming) flooding risk in Port of Spain driven by Tropical Cyclones and relative sea level rise.

The issues faced in the manuscript are therefore very current and important problems. Indeed, the question if and how the future flooding risk in coastal areas will increase in changing climate has many scientific and technical issues still open and all the matter needs to be investigated into further.

The study case proposed is interesting, and I have appreciated the attempt of the authors to propose a methodology that takes into account the entire sequence of the

C1

actions necessary for the assessment of the flooding risk, i.e. depending on the interaction of hazard, exposure and vulnerability. For this reasons I think that the paper addresses relevant scientific and/or technical questions within the scope of NHES

Despite of the above given positive judgments on the paper topic, the manuscript in my opinion, should be very deeply and accurately revisited. Firstly, what is the novelty of the proposal within the current state of art on this topic does not appear clearly as well as, the scientific or technical original contribution of the authors. The only contribution seems to be related to the methodology, but it does not appear particularly original or novel. Moreover, the authors uses a chain of state of art statistical or deterministic models, but no explanation is provide why they have chosen a model rather than others present in the literature. The models used are in most of cases only mentioned or described only in very general terms, as well as, the results of model applications are cursory described. Some of the results of model applications are reported in the session related to data and method and this induces some confusion in the reader.

At this stage the manuscript is more similar to a technical report than to a research article.

My more detailed suggestions are:

- Introduction should be rewritten emphasizing the elements of novelty of the proposal in the framework of the current state of art.
- Paragraph 2. Study site: Port of Spain. The study site description is very rough and is lacking of each detail. There are two possibilities: 1) to remove the paragraph and put the brief description of the study site in the introduction; 2) to rewrite a new paragraph on study site and data in which the main characteristic of the study site and all the climatic, social, and economical data used into the study are described, and they sources and uncertainties are emphasized.
- Session 3 should be focalized on the methodology, but it is a mix of description of the models, data and results. It would be better to separate these aspects.
- In paragraph 3.3.1 hydraulic model calibration and validation is described in very superficial way:

C2

what are data used to calibrate the model, what is the integration domain, mesh size etc etc ? The authors, since they used R-clipper approach, have TC-induced rainfall spatial distribution ,thus, they have the problem to modelling rainfall/runoff; in alternative they can use rainfall hydraulic direct flood modeling. In this last case there are a lot of aspects to highlight as domain size, mesh size, computation times, since there are some limitation at the domain size due to the increase of computation times. - If the issues in paragraph 3.5 are crucial in the economy of the work, the various sources of uncertainty should be discussed before, likely directly into the introduction. In the present form the paragraph is only a list of the possible sources of uncertainty, which moreover are not taken into account or quantified in the proposed study case. In each case, I think that that paragraph is not put in the right position. - Results are discussed in very synthetic way and also the conclusions are poorly discussed. What does study case teach us? What are the limitations of the proposed approach? There is room to improve the methodology? What suggestions about future developments?

Finally, given the importance of the theme, I would like to encourage the authors to submit a revised version of the manuscript, with further and more refined analyses and with a more detailed discussions on methodology, models, analyses of results.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2017-150>, 2017.