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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Probabilistic hurricane-induced storm surge hazard assessment in Guadeloupe, Lesser Antilles" *by* Y. Krien et al.

Anonymous Referee #2

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This paper is an interesting application of a method developed by Emanuel et al. (2006) and properly credited. The method combines a statistical approach of hurricane tracks generation with a deterministic approach to evaluate hurricane intensity. The authors make use of a data base of synthetic storms due to the work of Kerry Emanuel concerning hurricanes in the North Atlantic Basin to estimate the expected inundation in Guadalupe, Lesser Antilles, which is achieved by computing the effect of each storm by means of the coupled model ADCIRC-SWAN. The paper is clearly exposed and results are shown synthetically in an essential way. The main results are shown in Figures 5 and 6 where the expected inundation level for a 100 year return period and

1000 year return period are shown. There are small misprints and/or imperfections that should be amended 1) it should be convenient to designate the historical events, such as HUGO, DAVID and ALLEN with capital letters throughout all the paper 2) the main reference author Emanuel should be quoted correctly. He is called Emmanuel in the acknowledgments 3) formula (3) in page 8 seems to be wrong. The Holland parameter is dimensionless and the numerator should contain only the square of Vm and not the third power. 4) The Manning's coefficient is not dimensionless and its unit should be specified in Table 1

The main objection to the reliability of the results of the paper is that estimating return period levels of 100 years and 1000 years can be quite an exercise if the statistics are not supported by a sufficient data set. Indeed the work done by the authors is based on a deterministic hydrodynamic model computing waves generated by a storm. But the synthetic storm data set (passed by Emanuel), is said to be computed on the basis of reanalysis of meteo data between 1980 and 2011, which is a time window probably too short to allow inferences over periods of hundred or hundreds of years. Going back to the performance of this method as discussed in the original 2006 paper, one can see that Emanuel et al. show graphs of data of maximum wind speed in given locations (Boston, Miami. . .) computed by means of their method compared to data from historical events covering a period from 1851 (for example see their Figs. 4, 5 and 8) where it is clear that there is substantial discrepancy in the range of the rare events. This practically means that the uncertainties in the range of the long return times are very high. The authors repeat several times that they use an equivalent period of 8000 years to estimate the 100 year return period and of 50,000 years to estimate the 1000 year return period. This tells not enough. They should also specify how many scenarios they study, that is how many storms are equivalent to 8000 years of cyclone activity etc. The authors mention that the storm tracks are computed by means of two methods by Emanuel (Markov process and synthetic wind time series). These two methods provide results that can differ significantly from one another especially in the range of the rare events (see the above quoted paper). Therefore it would be

interesting to show separate inundation maps corresponding to storms computed with one method and with the other. My feeling is that the uncertainty in the final results and therefore their reliability have been largely overlooked, but instead this aspect deserves a lot of attention and discussion. It would be useful to add some uncertainty information to the inundation maps, such as maximum and minimum value of inundation in addition to the average expected value.

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