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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Current and Future Climate Compound-Event Flood Impact on Coastal Critical Infrastructures" *by* Mariam Khanam et al.

Anonymous Referee #2

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The paper describes a modelling study aimed at assessing flooding hazard at eight specific sites in the coastal areas of Connecticut as a consequence of compound flooding events, considering both current and future climate conditions.

Major points

1. The title is awkward to read. I suggest something as "Flood impact on coastal critical infrastructures considering compound flood events in current and future climate".

2. The Introduction is quite general and not specific enough. What the Author describes as a "a dynamic framework to project the combined hazard" is nothing else that a hydrological model and a hydrodynamic model run in cascade and

forced with both actual and synthetic data. Nonetheless, an estimation of the expected frequency is fundamental when treating compound events. This aspect is quite lacking in the paper.

Many statements are quite imprecise. For example, it is stated that the focus is on coastal power grid substations, but this is not correct. No information is given about the chance of malfunctioning of power grid substations due to flooding. Are these substations built up to tolerate a given water depth? The paper only deals with the water depths at eight locations in which power grid substations are present, which is quite another (preliminary) issue. Moreover, at the end of the Introduction, two main questions are reported. First, it is said that the present work forms the basis on which to address these two questions (which is correct), then it is said that these questions are investigated, which is incorrect.

3. Model calibration/validation. I'm not an expert of meteorological models, so I'm not commenting on. But for what concerns hydrological and hydrodynamic models, I have substantial concerns.

   As for the hydrological model, the use of information on land use, land cover, and imperviousness ratio does not imply that an overparameterized model (as all spatially explicit and hyper-resolution model are) provides reliable results. The fact that the model was successfully verified in river basins within Connecticut, where all the watersheds simulated in this study reside, does not assure the model reliability in different river basins. Indeed, it is common that different rivers in the same country show very different hydrological behaviours. Calibration and validation should have been performed for the rivers considered in this study, and for the actual events (Sandy and Irene) the outcome of the model should have been compared with some measured data (no measured data within all the modelled domain seems quite an unrealistic picture).

4. It is simply unacceptable that a riverine model is set-up using LiDAR data also for

the submerged channel beds. Bed elevations MUST be corrected using proper bathymetric data (multibeam, cross sections, etc.) to obtain reliable results. Contrarily to what the Authors stated, it cannot be concluded that neglecting submerged channel bed, which results in an underestimation of channel conveyance capacity, would lead to an overestimation of the flood extent. A channel with a lower capacity can also confine an inundated area, whereas a greater conveyance capacity can cause further flooding as well. Furthermore, the model is validated considering water depth only, and not flood extent.

5. Figure 4 shows a comparison between modelled and measures water depth. Considering that two real flooding events (Sandy and Irene hurricanes) were simulated, I was expecting a comparison for these two events. Modelled water depths are reported in the figure using boxplot (instead of single values referring to these two real hurricane events), but it is not said from which set of simulations these boxplots are derived from.

Finally, I agree with the comments raised by the Reviewer 1. In general, the manuscript should be substantially revised and arranged with far greater rigour.

Minor points

- l. 55: “riverine models cannot capture the risk from tide-surge-SLR effects”. In what a sense? While it is true that, traditionally, one looks at the river or at the coast one at a time, riverine models can naturally capture the risk induced by tide-surge-SLR on flooding in the form of higher free-surface elevations for tailwater effects, when forced with proper downstream boundary conditions. Moreover, if the riverine model includes floodable areas adjacent to the coast, the same hydrodynamic model can be used to assess coastal flooding too, it’s only a matter of boundary conditions.

- l. 56-57: Depending of what is meant for “riverine models”, “the modelling of individual flood drivers separately mischaracterizes the true risk of flooding” is not a rigorous statement, as what the Authors affirms is true only when the effects of compound events are worsen than the sum of effects due to single forcing events.
- l. 56: Barnard et al. 2017 is not present in the Bibliography.
- l. 73: “in frequency”? The sense of this sentence remains obscure to me.
- l. 90: please repeat what kind of substations.
- l. 109-111: I cannot recognize subsection a, b, and c in the text.
- l. 157: extent of what? depth of what? (water, of course).
- l. 160: How were the building footprint used in the model? So many different approaches have been proposed. . .
- l. 279: Please explain how cumulative distribution function (CDF) of maximum flood depths were computed.
- In the Bibliography, items are not ordered alphabetically, nor they are given the proper stylisation.

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