Response to reviewer' comments

On the manuscript **nhess-2020-132**

revised for publication in

NHESS

We wish to thank the reviewer for their valuable suggestions. We agree with most of the critiques raised during the review process, and we will do our best to incorporate them in the revised paper.

Here is a detailed response [in italics] to each point raised during the review [underlined font].

<u>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".</u>

We thank the reviewer for this suggestion, and we will consider changing the title

<u>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.</u>

We thank the reviewer for this comment: we will modify the Introduction to provide a better framework for this study and highlight the importance of this work. We will improve the literature background to highlight in a more straightforward way what is missing in current research, and what this work is addressing. We will rephrase the Introduction, and we will clarify better adding the text below

"In low-lying coastal areas, the co-occurrence of high sea level and precipitation, resulting in large runoff may cause compound flooding [CF] [Bevacqua et al., 2019]. When the two hazards interact, the resulting impact can be worse than when they occur individually. Both storm surges and heavy precipitation, as well as their interplay, are likely to change in response to global warming [Field et al., 2012].

Major research has been conducted on the assessment of damages to the power system components or other related infrastructures, and proposing design and operation countermeasures and remedies [i.e. Kwasinski et al. 2009; Reed et al. 2010; Abi-Sarma and Henry, 2011; Chang et al., 2007; de Bruijn et al., 2019; Pearson et al., 2018; Pant et al., 2018; Dawson, 2018]. Nonetheless, despite the CF relevance, a comprehensive hazard assessment on critical infrastructure is missing, and no studies have examined CF in the future.

The first step to investigate and assess the impact of CF on the power grid is to perform a systematic risk analysis. To deal with CF coming threats and challenges, there is a need to develop efficient frameworks for exploring a wide range of actual and what-if scenarios in a system that could inform short- and long-term decisions. Scientists must investigate not only how severe these events might be but also how commonly they are likely to occur. We propose a new strategy for providing this information: identify water levels and extent nearby critical infrastructure by observing real-world phenomena and drawing information from simulations.

When a hurricane approaches, providing a few extra hours' notice for infrastructure management is critical. By simulating the impact using possible storm paths, this framework offers more accurate medium-term risk evaluation. It can be used to assess the vulnerability of the infrastructures to current and future events."

a. <u>Nonetheless, an estimation of the expected frequency is fundamental when treating compound events. This aspect is quite lacking in the paper.</u>

We thank the reviewer for this comment. We definitely agree that a frequency estimation is critical in treating compound events. This, however, goes beyond the scope of the current manuscript.

For this work, our aim was to set up a modeling framework and use it to demonstrate the importance of investigating flood impacts due to compound events, based on past hurricane events and synthetic hurricane cases simulated in future climate conditions. We will address this aspect further in the discussion.

b. Many statements are quite imprecise. For example, it is stated that the focus is on coastal power grid substations, but this is not correct.

We thank the reviewer for this comment. Within our study sites, two are more inland [CI3 and CI4] (table 1: see hydrologic distance), nonetheless all the sites are included within the Coastal Area as defined by Connecticut General Statute (C.G.S.) 22a-94(a)

[https://www.cga.ct.gov/current/pub/chap_444.htm#sec_22a-94].

We will clarify this better in the revised manuscript and include the 'coastal area legal boundaries' in Figure 1.

3. 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?

We thank the reviewer for this comment. Due to confidentiality, we cannot provide exact information related to the critical water level for each infrastructure. The presented water depths are indicative numbers, useful to provide a comparison between the various events. In the revised manuscript we will give few comments about this.

4. 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.

We thank the reviewer for this comment. We want to underline that indeed, the aim of the paper was characterizing the risk for the critical infrastructures, hence why we described the water depth at the location. We, however, also investigated the water depths in the whole domain, through the CDFs, and compared the water extent to FEMA maps, to provide an overall hazard assessment. Considering the questions in the Introduction, we will rephrase this chapter, providing a more explicit description.

5. 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.

a. 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).

We thank the reviewer for this comment. We would like to clarify that the hydrologic model was calibrated and validated for the whole Connecticut river basin in the work by Shen and Anagnostou (2017). In the paper, the model was tested for Thompsonville (gauge No. 01184000) with a NSCE of 0.63. Recently, we further validated the model considering hourly flows in Housatonic River and Naugatuck River with a NSCE of 0.69. We will add this part in the manuscript, to clarify the performance of the hydrologic model.

b. 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.

We fully agree with the reviewer on the importance of bathymetry in flood inundation modeling. Unfortunately, for the investigated case studies, we do not have any information about the bathymetry of the rivers.

In general, the impact of inclusion/exclusion of bathymetry data on the model results will vary in its magnitude as a function of river size and flood magnitude [Cook & Merwade 2009]. For larger events in these coastal locations, flood risk is mainly dominated by defence overflow and defence breaching. As a consequence, we did not represent the flow of water in the main channel. Rather, boundary conditions were given as time series of water surface elevation imposed along the defence crests. This means that we do not require detailed bathymetric information in the upstream main channel, thereby considerably simplifying the modelling problem (as also suggested in Bates et al. 2013). As well, the model parameters were calibrated to obtain realistic water depths, as compared to the High-water marks in selected locations, and this allowed us to obtain realistic simulations for Sandy.

Considering the reviewer comments, we will include further clarification about the model validation.

As an example, the following paragraphs illustrate how the proposed model, for such extreme events, provides realistic simulations, even when compared to running the model accounting for bathymetry.

Given the lack of bathymetry data for the case studies, as an example, we applied a Discharge Correction Technique (DCT) to the hydrologically simulated discharge. DCT is based on the assumption that a given flow discharge can be separated in two components: the bankfull discharge, below the assessed water surface, and the discharge exceeding the LiDAR discharge, above the assessed water surface (Bradbrook et al. 2004). To evaluate the bankfull discharge, we considered regional curves (Ahearn, 2004).

Fig R1 shows for hurricane Irene [actual event], a comparison between the CREST-simulated discharge, and the DCT one. The results of the simulation carried out as presented in the manuscript, VS the simulation corrected using the DCT for CI1 is shown in Fig R2.

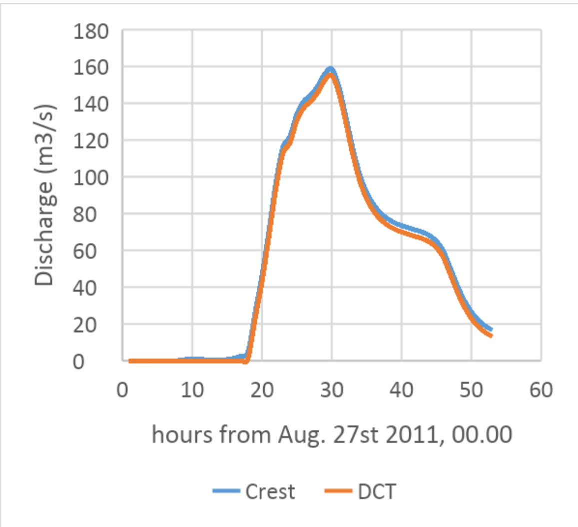

Figure R1: example of DCT as compared to CREST simulated discharge

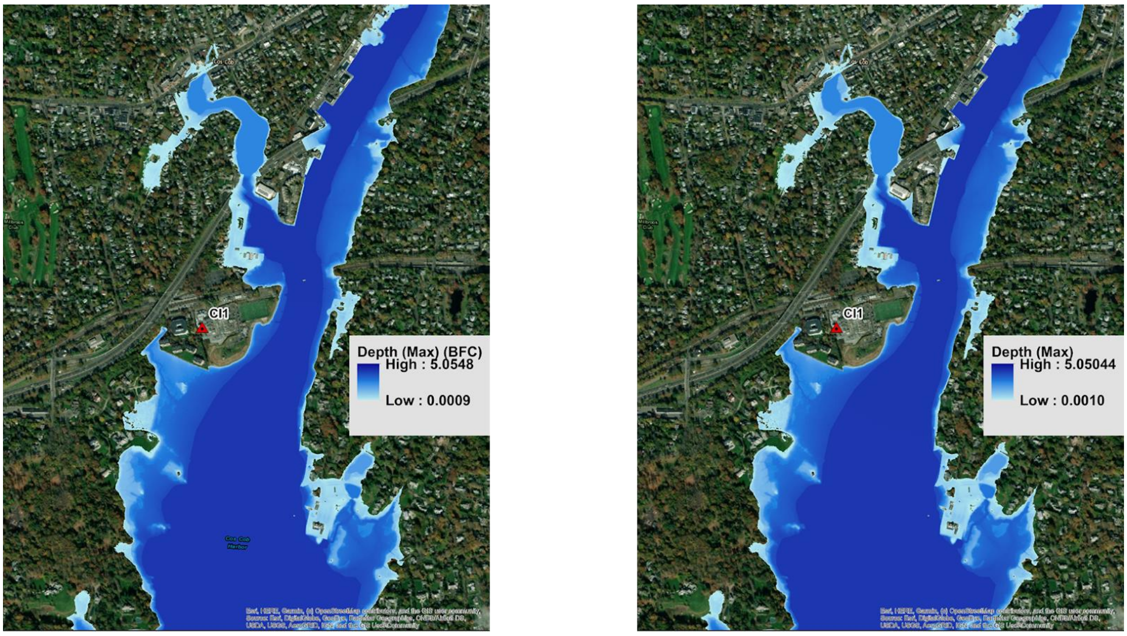

Figure R2: Maximum flood depth during the actual Irene event: left: streamflow with DCT, right: Streamflow without DCT, as presented in the submitted manuscript.

Regarding the model validation, see our response to the below point

<u>6. Figure 4 shows a comparison between modelled and measured 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.</u>

We thank the reviewer for this comment. As validation data, we only have information for Sandy, not for Irene. Hence we based our comparison to that event. In the revised manuscript will clarify better figure 4: to allow for comparison, we evaluate water depth within a 10x10m radius around the high water marks, to avoid issues due to the presence of buildings in the DTM; hence we represented the figure using boxplots.

Regarding the validation of the flood extent, we will provide further assessment in the revised manuscript.

As for the water depth, the most accurate available information for flood extent is only available for Sandy. For this event, CTEco (FEMA,CT DEEP, 2013) provides a map of the storm surge, created from field-verified High Water Marks and Storm Surge Sensor data from the USGS. For Connecticut, the vertical value is water depth above the ground in feet. For comparison purposes, we here provide a visual quality assessment of our model (Fig.R3 a, c), as compared to these maps (Fig. R3 d,e), for two selected locations (CI1 and CI2).

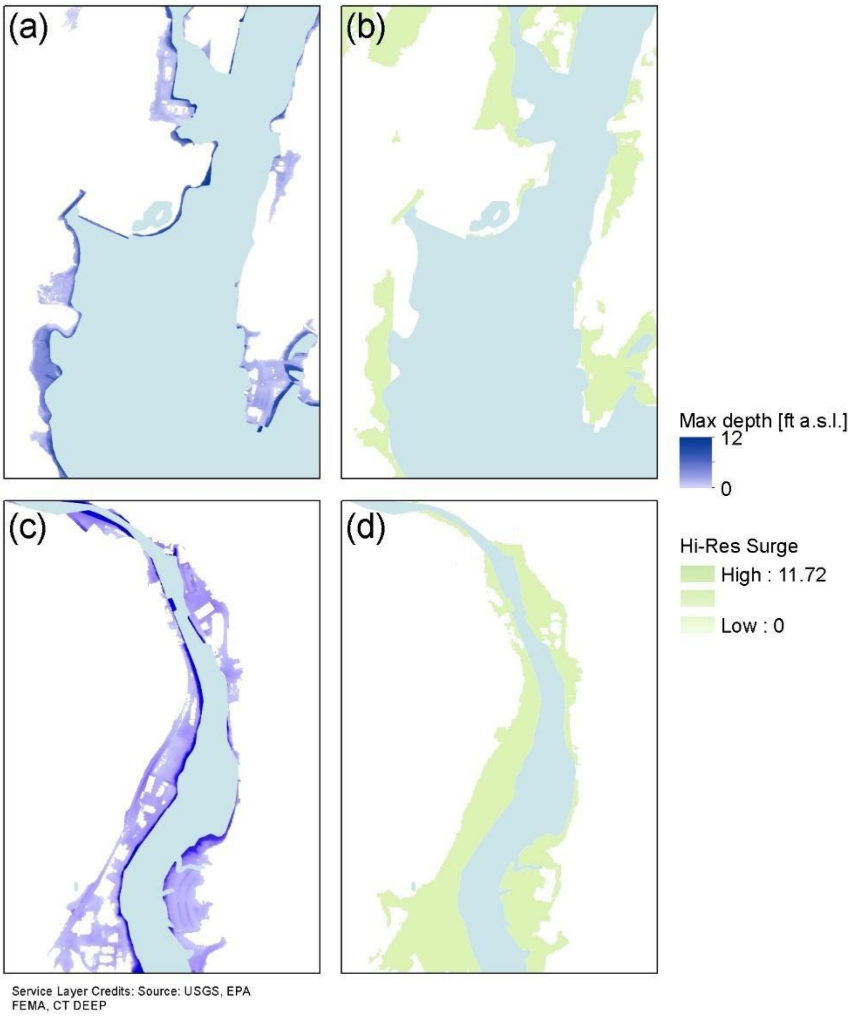

Figure R3: comparison between the results of the proposed model for two selected locations (a,c, CI1 and CI2 respectively) and the maximum surge extent as proposed by CtEco (c,d respectively).

Considering the reviewer comments, we will improve the discussion of the model validation in the revised manuscript.

7. Finally, I agree with the comments raised by the Reviewer 2. In general, the manuscript should be substantially revised and arranged with far greater rigor.

We thank the reviewer for their valuable suggestion. We will proof-read the paper and improve grammar and spelling before resubmitting the revised manuscript.

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 tidesurge-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.

We thank the reviewer for this comment. We will deeply rephrase the Introduction and clarify this part better, explaining the importance of correct setting the downstream 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

We will rephrase this sentence as follows “The modeling of individual flood drivers separately might mischaracterize the true risk of flooding, especially when the effects of compound events are worse than the sum of effects due to single forcing events.”

• l. 56: Barnard et al. 2017 is not present in the Bibliography.

We will double-check all the references and fix them

• l. 73: “in frequency”? The sense of this sentence remains obscure to me.

We will rephrase this sentence. “Some authors have characterized the frequency of compound flooding, and provide approaches to risk assessment based on the joint probability of precipitation and surge (Bevacqua et al., 2019; Wahl et al., 2015).”

• l. 90: please repeat what kind of substations.

We will fix this

• l. 109-111: I cannot recognize subsection a, b, and c in the text.

We will fix this

• l. 157: extent of what? depth of what? (water, of course).

We will rephrase and be more precise

• l. 160: How were the building footprint used in the model? So many different approaches have been proposed. . .

We thank the reviewer for this comment. In the manuscript we will explain more clearly how we approached this.

For the simulations we considered a DTM [bare ground elevation]. To better represent the impacts of urban establishments on inundation dynamics, solid urban features such as houses and buildings which obstruct flow of storm water were added to the bare-earth DTM. To this purpose, we considered the building footprints from CtECO, 2012 and identified positions of buildings and houses in the DEM by increasing the elevation of the pixels inside of the building footprint polygons by an arbitrary height of 4.5 m ~ 15 ft, assuming one-story buildings.

• l. 279: Please explain how cumulative distribution function (CDF) of maximum flood depths were computed.

We computed a Cumulative Distribution Function that describes the probability that a particular value for a random variable will be exceeded. We did this using all the depth values of all the grid of the simulation domain, for the time step when the inundation was maximum. We have treated the depth values as random variables and used the existing function “cdf” in MATLAB to plot the CDF curves.

• In the Bibliography, items are not ordered alphabetically, nor they are given the proper stylisation.

We will double-check all the references and fix them in the revised manuscript

References:

A. *Shen, X., Anagnostou, E. N. (2017),A Framework to Improve Hyper-Resolution Hydrologic Simulation in Snow-Affected Regions. Journal of Hydrology, 552, 1–12. https://doi.org/10.1016/j.jhydrol.2017.05.048, 2017*

B. *Cook A., Merwade, V., (2009) Effect of topographic data, geometric configuration and modeling approach on flood inundation mapping. Journal of Hydrology, 377, 1–2, 20, 131-142* https://doi.org/10.1016/j.jhydrol.2009.08.015

C. *Bradbrook, K., Lane, S.N., Waller, S.G., Bates, P.D. (2004).Two dimensional diffusion wave modelling of flood inundation using a simplified channel representation. Int. J. River Basin Manag. 2, 211– 223*

D. *Bates, P. D., Dawson, R. J., Hall, J.W., Horritt, M.S., Nicholls, R. J., Wicks, J., Hassan, M. A. A. M. (2005). Simplified two-dimensional numerical modelling of coastal flooding and example applications. Coastal Engineering, 52, 793–810.*

E. Ahearn E.A., (2004). Scientific Investigations Report 2004-5160, https://doi.org/10.3133/sir20045160

F. FEMA, CT DEEP (2013). Coastal Hazards Map Viewer Information http://www.cteco.uconn.edu/viewers/coastalhazards.htm#surge