Interactive comment on "*Assessing Climate Change-Induced Flood Risk in the Conasauga River Watershed: An Application of Ensemble Hydrodynamic Inundation Modeling*" by Tigstu T. Dullo et al.

The authors would like to thank the reviewer for the insightful and constructive comments. We have reviewed the comments and provided our responses herein. The reviewer's comments are presented first followed by our response.

Anonymous Referee #1

The manuscript by Dullo et al. titled, "Assessing Climate Change-Induced Flood Risk in the Conasauga River Watershed: An Application of Ensemble Hydrodynamic Inundation Modeling" presents a systematic approach for evaluating the impact of climate change on exacerbating the future flood risk across a large watershed. First, a hydrologic model is used to simulate streamflow corresponding to multiple climate projections and second, a high-resolution hydrodynamic model (TRITON) is used to simulate the flood inundation extents corresponding to the streamflow values for different scenarios. I appreciate how thoroughly the modeling is conducted and described in the text. I particularly like the authors' approach to quantify flood frequency estimates at a grid-level. Overall, this is a strong paper but requires additional discussion and justification. Please see below for comments that are intended to improve the quality of the manuscript:

R1.1. My major concern is the absence of a variable roughness distribution based on different land use types for current and future periods. The hydrodynamic model assumes a fixed channel and floodplain roughness which may not be reflective of future land use variability. Therefore, the study evaluates climate variability from a hydrologic perspective, but only uses the modified streamflow to drive the same hydrodynamic model. Climate change is strongly linked to human-induced land use change and therefore, the land use variability must be reflective in the future simulations. Similarly, channel roughness can also vary spatial from upstream to downstream in large channels. Please comment on why this is not incorporated and how this might influence results.

Our response:

Thank you for the comment. The surface roughness values were selected based on the Whitfield County Flood Insurance Study (FIS, 2007; reference listed in the manuscript), which reported a range of main channel and floodplain Manning's n values. This is discussed under sensitivity analysis section (section 3.2). Further, to obtain representative roughness values, a couple of flood simulations were performed by adjusting the Manning's n values within the main channel and floodplain until a satisfactory agreement was achieved between the simulated and reference FEMA flood map. This is discussed in detail under flood model evaluation section (section 3.3).

We understand the reviewer's concern about the absence of variable roughness values based on land use in current and future periods. To model future land use and the corresponding surface roughness, one may use land use forecasts such as USGS's FOREcasting SCEnarios of Land-use Change (FORE-SCE) model for the Contiguous United States (CONUS) developed by Sohl et al. (2018). However, this dataset is only available at 250 m spatial resolution. Resampling this dataset to a 10 m spatial resolution will likely introduce more interpolation errors and may not adequately represent the spatial variability of land use patterns. This in turn will add additional uncertainty and hence requires an even more comprehensive task to characterize its impact.

The main focus of this manuscript is to evaluate the impacts of climate change on flood inundation extent and electricity infrastructures. Incorporating additional factors such as land use land cover change (LULCC) would increase the dimension of scenarios and require expanded ensemble simulations. These would require more computing resources and creates difficulty in data management as the total number of outputs increase significantly. Although we were unable to incorporate the suggested change in this study, the reviewer's comment is very essential in enhancing the analysis and model accuracy. As such, we have included additional information in section 3.3 to discuss the limitations of our current inundation modeling approach, such as missing variable Manning's n values and simplified river bathymetry correction. These limitations are provided as references for the enhancement of inundation modeling in future applications.

R1.2. Is the initial depth modification a proxy for antecedent conditions? How would the results change if depth variability in Manning's n is considered? Usually, the channel and floodplain roughness reduce with increasing depth following an exponential function. This has been applied previously in GSSHA and ICPR (https://doi.org/10.1029/2019WR025769). Please comment on how the results might be impacted having not incorporated a depth-variable roughness distribution.

Our response:

The initial water depth values represent the starting water surface elevation along the main channel (water course).

Dynamic variability of Manning's n value was not a part of the current study, as our TRITON model does not simulate this phenomenon at this time. However, as the reviewer suggested, if depth variability in Manning's n could be considered, it is likely that the channel and floodplain roughness would change and likely increase in part due to incorporating additional flow turbulence during bankfull flows (Morvan et al., 2008; Christelis et al., 2016; Bellos et al., 2018), as well as additional losses from complex floodplain flows that occurs during high flow events. However, the authors are hesitant to comment on the results without providing any evidence as this would lead to speculation but not likely affect the scope and outcome of the current study. We have discussed this in the limitations of our current inundation modeling approach and also included the suggested Saksena et al. (2019) reference in the revised manuscript.

While it may be worthy to investigate the potential impacts by considering depth variability in Manning's n, it is unlikely to impact the main objective of the study which is to demonstrate the applicability of a computationally intensive ensemble inundation approach to study the climate change impacts on flood regimes, floodplain regulation standards, and the vulnerability of existing infrastructures. The point by the reviewer is well taken and will be considered in future studies.

R1.3. I know the LP3 distribution works well for streamflow, but I am not sure of its applicability for flood depths. I would assume using a log-normal distribution for curve fitting flood depths would be more optimal. Can the authors provide a comparative analysis of the two distributions? Did the authors consider different distributions for curve fitting? Please comment.

Our response:

Thank you for the insightful comment. Indeed, although the Log-Pearson type III (LP3) distribution was recommended by Bulletin 17C (England et al., 2019) for streamflow, it may not be the optimal choice for flood depth. However, given the community's familiarity with LP3, we still decided to test the applicability of LP3 in this study. Our goodness-of-fit tests suggested that LP3 can still be a reasonable choice for flood depths.

Based on the reviewer's suggestions, we have conducted an evaluation by randomly selecting 679 locations in the study area and comparing the fittings of both LP3 and Log-Normal (LN) distributions. These locations were identified by sampling at 500 m interval along the streams in the domain, which resulted in a total of 851 points. Out of the 440 simulations, if any of these points were not wet (i.e., the depth is not greater than 10 cm) for 30 or more simulations, they were excluded from further analysis. This resulted in 679 points within the computational domain for the next step.

Using the Anderson-Darling (AD) goodness-of-fit test ($\alpha = 0.05$), we found that LP3 is accepted in more locations than LN. Additionally, we also used Akaike information criterion (AIC) to evaluate the suitability of both distributions and we found that LP3 can outperform LN in more locations. The results indicated that the LP3 can be an even more suitable choice than LN, for our study area (Table R1).

Table R1 – AD and AIC comparison between LP3 and LN distributions at 679 locations

	Log-Normal	Log-Pearson Type III
AD p-value > .05	590	636
AD p-value ≤ .05	89	43
Suitability based on AIC	235 locations	444 locations

It must be noted, however, that our goal in this study is not to identify the most suitable choice of distribution for flood depth. Therefore, there can be other more suitable distributions than the two tested herein. Given the good performance of LP3, we believe that it's sufficient for the purpose of our study. These additional analysis and clarification have been included in Section 3.5 of the revised manuscript.

The distribution fitting (LP3 and LN), Anderson-Darling k-sample test and AIC calculations were conducted using Python 3 and SciPy libraries (Hovey & DeFiore, 2003; Salvosa, 1930; Scholz & Stephens, 1987; Virtanen et al., 2020; Vogel & McMartin, 1991).

R1.4. Lines 420-424: This result resembles what has been reported in Dey et al. 2019 (https://doi.org/10.1016/j.jhydrol.2019.05.085). Please add a statement highlighting this similar finding. Additionally, this study also highlights the impact of incorporating an optimal channel shape. In the manuscript, the authors have modified the channel bottom, but this may not be entirely reflective of the bathymetric configuration of the streams. While channel shape and sinuosity may not impact 1D models where channel conveyance volumes are more important, this may be essential in 2D models. Please discuss the potential limitations of the approach adopted in this study.

Our response:

Thank you for the comment and suggested reference. We have revised the manuscript to highlight the similar findings from other studies. Further, we have included statements such as "*Although this simple approach is unlikely to adjust the channel bathymetry to its true values, it can improve the channel conveyance volume that is lost in the base DEM.*" in section 3.3 to discuss our model limitations. The suggested Dey et al. (2019) reference has been included.

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