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Natural Hazards and Earth System Sciences
Discussions

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.

Anonymous Referee #1

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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

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Discussion paper

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:

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.

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.

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.

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.

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