First of all, we want to thank the reviewer for the insightful detailed comments and recommendations. Specific responses to the reviewers' comments are added below.
Note: Below is our response (italics) to each reviewer's comment (regular font)

1. The articles needs desperately a discussion section separated from the results. Right now, everything is cramped into a single section that makes difficult to take the main points out.

Response: We will add a separate discussion section as the reviewer suggested and state the main points clearly for the readers.

2. I would also add as a minimum a paragraph in the conclusion section (if not its own standalone section) about the framework limitations. This is crucial in any prediction framework so readers are aware of it when taking decision.

Response: We will revise the conclusion section and add more information on the limitations of the framework.

3. The introduction (and other sections) have too many small paragraphs (1-3 sentences) that disrupts the reading of the manuscript. I will suggest to combine paragraphs that convey the same message.

Response: We will try to revise the introduction section as per the reviewer's suggestion. We will try to make sure of the flow of reading by grouping together consistent messages.

4. Did the authors had a minimum years of data treshold when selecting the gauges? It seems imperative to have one, since a gauge with 5 years of data will yield very different results than one with 50 years of data. Also, what are the general statistics of the gauge data? For example, what is the mean length of record, amount of cross sections measurements, etc.

Response: For this work, for the gages data, we followed Slater's 2015 workflow and used the system they provided accompanying the published work. Overall, one must consider that for our model we focus on sudden changes of the stage discharge, and not their persistence in time. We evaluate the stage discharge relationship based on measured river properties, as by Slater's 2015 work. Following the referenced work, we detected and excluded sites featuring artificial controls at the gauging station that could impede the natural adjustment of the channel's shape. Additionally, we eliminated all field measurements conducted at a different location or potentially different location, along with those taken in icy conditions, as these factors could impact the accuracy of channel geometry measurements. Our selection process retained only sites with comprehensive time series data, and as per Slater's et al. 2015 work, only kept gages with 99.7% completeness in streamflow records and 40 channel cross-section measurements. We will clarify this in the revised manuscript.

Specific Comments:

Line 44: is not clear the statement. Since during a flood event, flow within the channel can change due to external factors, stormwater discharge o compounding flood at coastal estuaries.

Response: The reviewer is correct, many other parameters could be the direct cause of change, also considering for example debris flows or mass movements. Nonetheless, these could also be triggered by the storm properties themselves. We will add some comments on this in the revised manuscript.

Line 64: the "secondary channel" that is referring in Figure 1 should be highlighted in the figure itself to help the reader understand the point.

Response: We will revise the figure for better understanding.

Figure 1: The figure needs a north arrow and scale bar. I also strongly suggest the authors to use a GIS platform to enhance the quality of the figure. The figure also needs a location map.

Response: We will add the mentioned specifics to the figure.

Line 93 and 95: both sentences start with "Despite some limitations …" Please rephrase.

Response: We will rephrase the sentences.

Line 106: I will summarize all the gauges selected with their corresponding ID in a text file (or any other format file) and upload it to a repository for easy sharing. Then, the reader could see exactly which stations were selected. This helps the open data statement in the research community.

Response: Thank you for the suggestion. We will do this.

Line 110: did the authors downloaded also discharge values from the NWS or it was just flood stages as it is mentioned in this line? If the cross section data (width and depth of the river) were obtained from the USGS, why not also use their created flow-stage curves. My biggest question is from where the authors obtained the discharge values for the creation of their rating curves, since NWS only provides stage level whereas USGS provides both stage and discharge in most gauges.

For this work we followed Slater's 2015 workflow, and used the system they provided accompanying the published work. We evaluate the stage discharge relationship based on measured river values, as by Slater's 2015 work. We will clarify this in the manuscript.

Figure 2: I do not support have several lines of text if the figure caption just to describe the different climate regions in the map. Also, the authors also explain the abbreviation in the results section when talking about it. Thus, I strongly recommend having a nomenclature section that summarizes all of these, including the variables from table 1. Then, the reader can easily find it.

Response: We will revise this.

Line 161: the statement of the reason for change in capacity (deposition) has been already mentioned in Line 159. Please rephrase or remove.

Response: We will revise this.

Line 177: why that the authors only focused on a very narrow range of years for their storm event? This seems like a big limitation, especially since the latest year of the record was a decade ago. The authors needs to justify their selection as a minimum.

Response: We want to thank the reviewer for this comment. We would like to draw the reviewer's attention to the fact that in our study we have used a published dataset (Shen et al. 2017) of storm events and the properties of the events. Most of them were calculated properties. This dataset was from 2002-2013. Our framework showcases the intercorrelation of the different event properties that can affect the channel changes and that can be of any timelines. Here we have established a framework that can be used for extended timelines. Researchers can use the trained model with additional years of data, if they have available the same storm properties proposed by Shen for more recent events.

Table 1: there are some variables that have their "unit" column empty. For example, Peak , Q2, etc. This might be a typo since if the variable does not has unit the authors specify with a dimensionless or N/A. Also, the table is too long for a peer-review article. I strongly suggest dividing the table into three separate ones, one for each variable type. There are also some variables like BFI_AVE that their description is a quarter of the page due to being squeezed in the small column width. I would suggest the authors to place the long variable description as a footer in the table or in the appendix as part of the nomenclature section.

Response: *We will revise the table as per the reviewer's suggestion.*