## Summary

The manuscript describes improvements made to an existing hydrodynamic model, COAWST, that will now include a uniform drainage rate as a volumetric source to represent the urban drainage system and infiltration. The improved model was tested over the Jamaica Bay watershed (NYC) during Hurricane Ida's impacts. Results are compared with limited high-water marks, and a sensitivity analysis was performed to see the system performance under the variation of the storm impacts.

## General Comments

The research work proposed by the authors is interesting and might be suitable for the selected journal. However, the manuscript needs substantial changes to reach the expected standards in top-tier journals like this one. Thus, if my comments are addressed, maybe the manuscript will be more suitable for publication.

First, there seems to be a misunderstanding of the terminology and hydrologic concepts throughout the manuscript. For example, the title states pluvial and compound flooding, but compound flooding may already include pluvial drivers. I suggest the authors follow a nomenclature of pluvial, fluvial, and coastal flood drivers and specify in their scope that they will be considering only the pluvial-coastal interaction. Thus, they can reference this as a compound flood. See line 65 for another example of a pluvial compound. Please revise all the terms used in the paper.

Second, the manuscript format is not suitable. For example, when researchers introduce a new model or modification, they should add a limitation section before the results so the reader is aware of this upfront. However, despite the study's many limitations, the authors only summarize in the last paragraph of the discussion. I think the authors have more than enough to have a robust limitation section. Also, the introduction lacks a clear research gap/motivation for the work. So far, I can interpret from the introduction the importance of flood modeling but not what others have done regarding pluvial-coastal flood modeling in urban settings and the rationale for making these improvements to the COASWT model. Thus, I urge the authors to give some context to the previous works in this field, present the knowledge gap, and discuss their research questions that will bridge this gap. Also, Lines 82-88 disrupt the story-telling flow that all peer-reviewed journals should have, especially in the introduction. Thus, please rephrase. Furthermore, the description and figures of Ida’s event should be moved to section 2.1. In the results section, the authors repeat too much or/and give too many details when describing the results.

Lastly, my biggest concern with the manuscript is the substantial simplification of the hydrologic process the authors did for the model improvement. I can support a simple

approach, but the authors need to then justify their selection. Furthermore, the authors have many vague statements in the discussion and conclusion section that are not properly supported by the results and/or the model simplification. For example, I would not suggest stakeholders and decision-makers use the results of this model to provide flood-resilience measures to avoid Ida's flood impact since the authors simplified the storm drainage system and infiltration as a uniform drainage rate over the entire basin on the model. Also, their rainfall addition is not included in the governing equations as a source code and only as a volumetric addition, which can produce different hydrodynamic behaviors expressed by previous studies. At a bare minimum, the authors should compare their approach with other rain-on-grid models. I think the authors need to defend and justify their selected approach with more detail.

## Specific Comments

- L12: replace all with most, remove highly, and replace vulnerable with prone.
- L19: RMS is not define before using its acronym.
- L32: remove "and motivation" from the section header.
- L49-51: the statement needs support from a reference.
- L61: how about also the overestimation? Studies have shown that not accounting for all the physical processes correctly can result in both under and overestimation.
- L71: ICPR model is now called StormWise, so please include this name for future reference of the readers.
- L73-74: Is it crucial for compound flood simulations to have 3D hydrodynamics? The authors claim this as crucial and even one of their novelty, but they fail to provide evidence of such need in the field of the compound flood. Please justify why this is needed with cited literature.
- Figure 1: The watershed is barely noticed on panel A, so increase the line width. The authors can use the USGS HUC watershed shapefiles for this. Also, I am almost certain that the authors do not have copyright permission to include a figure from Wikipedia as the one in panel b, so please make your own figure. When you make your own figure, please describe what the color points mean. Lastly, this figure should be moved to Section 2.1.
- Section 2.1: the authors should include a brief summary of other storms that affected the watershed and its response to the system, such as Hurricane Sandy.
- L126-128: does this mean that the rainfall component is not directly integrated into the governing equation as a source term? Please explain better and justify your approach. Several authors have included rainfall on coastal models by modifying their governing equations, like Dresback et al. (2022) and Santiago-Collazo et al. 2024).
- L129-132: the authors should comment more about the limitations their selected approach for drainage rate affects accuracy and real-life scenarios. For example, the spatial variation of the stormwater infrastructure, the temporal-varying drainage

rate of the system during the event, and the backwater flow preventer structures placed typically in the sewer outlets, to mention just a few.

- L132-133: I disagree with the author's statement that the model does not need to route the remaining runoff toward the ocean for their study. However, they claim the importance of their model for compound flood assessment, but the first point where you exhibit compound flood in coastal urban cities is the drainage outfall and how it propagates inland through the system. Obviously, the authors' proposed model is far from capturing this, but they still claim this (L363-364 ). I will be careful with their statements, especially if they cannot properly back up their statement with results.
- L150: the authors should give more details about the hot start simulation they performed to initialize the model.
- L156-157: the authors should locate these gauges on a Figure. The reader should not wait until the results to find the gauge. The same occurs with other gauges mentioned in the methods section.
- L158: if you are using coastal gauge water levels to represent storm surge conditions, then you are not modeling storm surge directly on your model with a wind and barometric field pressure. This isn't very clear to me and will not be clear to the reader. Please rephrase. Also, if the answer to my question is true, the authors are not modeling storm surge directly; rather, they are just propagating a coastal flood inland. This will need clarification throughout the entire manuscript to change what you call “simulating storm surge” to “simulating coastal flood.” The reader will expect that if you are simulating storm surge, then you are applying wind stress to the ocean domain in your model and not just a water level at the boundary condition.
- L155-160: Authors should add a figure of the model domain with a clear description of the model boundary conditions. This will add the explanation in this paragraph.
- Figures 2-3: combine them into a single figure with two subpanels. There are many similar instances of this, such as Figures 4-5. Please try to achieve this since the fewer figures, the better for the reader.
- L170-174: would this mean that you only simulated pluvial flooding and not compound flooding since your coastal flood was minor during this event? If true, the authors should be more clear on their terminology and manuscript language. The reader will expect a “good amount” of both flood drivers to call the event a compound flood. Please be more clear with this earlier in the manuscript. I want to say that the authors should have selected a different event, such as Hurricane Sandy since Ida did not bring significant coastal floods. Furthermore, the justification of selecting Ida as the event due to data for calibration is not supported since you cannot accurately calibrate a whole watershed with only a handful of high-water marks, especially if they are in places where water does not accumulate greatly. The reader will question why you did not model Sandy, so please include a justification for this and why Ida was best. If your purpose was to simulate extreme rainfall, Ida is fine, but if you wanted to simulate a compound flood event, then Ida was not ideal.

- L173: quantify the adjective “far below”. Similarly, the authors have many vague adjectives that need quantification, such as “significant,” “accurate,” and “good.” What for the authors is far below could be different to me.
- L180-186: I consider this too much detail about the radar rainfall source. The authors should use only one sentence and cite a reference unless they extensively use rainfall for their model, which is not the case, I believe.
- L189: how does this 70 mm/hr of rainfall compare with other events at the basin? For example, the authors could compare this with return period values from the NOAA Atlas 14.
- L192-193: remove this sentence.
- Figures 4-5: include the basin boundary on the map so we can assess visually the amount of rainfall that falls inside.
- Figure 6: Why show six days of rain if everything happens on a single day? I consider even the graph unnecessary since it gives the impression that the authors are simulating uniform rainfall using the values on this graph.
- L212: what do the selected values of drain rate physically mean for the system? Do they have any physical justification for being selected, or are they just random values that work?
- L221: all of the urban areas the same? The authors are using a typical value of CN for their whole area. However, areas do change, and the soil type and antecedent conditions also play a role in the curve number value selected. The authors should talk more about this since it is crucial for their results, and not just say everyone uses 90 and that it. I strongly recommend the authors compute their own weighted average CN for their watershed using all the parameters needed since that would be more defendable than the current approach. In my opinion, the authors have been too simple on the hydrologic side of the study, which is crucial for any compound flood simulation.
- L222: Can the authors confirm the rainfall amount in 3-hr, using rain gauges within the basin instead of only depending on the radar data, which will not always be 100% accurate? For example, authors could use the CoCoRHAS network.
- L225-226: this is another big limitation that is not addressed in the manuscript. There are many models out there that use the spatially-varying rainfall to generate spatially-varying runoff instead of just a single runoff volume for the entire basin. For example, how would the results change if the authors used the average rainfall over that basin instead of the maximum value? Those are key questions that need to be answered within the sensitivity analysis to justify the approach.
- L226-227: While I agree with the authors' statement, it is not appropriate for their study due to the gross simplification of uniform rainfall and drainage rate. Designing stormwater infrastructure requires more than this since as you keep going downstream towards the outlet, your main sewer line will start receiving more runoff than upstream or other collecting pipes with a smaller catchment area.
- L234: if you only shifted the rainfall field, means that you did not shifted the wind field. Thus, you compound simulation is not well represented. Futhermore, this

statement confirms for me that you did not simulate storm surge over the domain as my previous comment suggests.

- L238-240: this information should be in section 2.1. Also, why not use the return period values at NYC and use the Northeaster states? A local comparison could benefit since a 10-yr return period is not extreme.
- Figure 7: combine with Figures 4-5.
- L257: why not synchronize the peak storm surge with the high tides also? You already had low storm tide conditions. The suggestion made by the authors would not affect your compound flood since the storm tide conditions were already low. This can be shown in the results. If the author manipulates the water level boundary conditions to coincide the peak storm surge with high tide, then they could have a more significant compound flood in the model than what they have now.
- L268: are you referring to the "no infiltration" to the "drainage rate" component of the research? The authors never computed infiltration in the method section. I know that, in theory, it is the same sink term, but this could confuse the readers, so be consistent with your terminology.
- L269: it is technically impossible to see the flood depths to vary spatially since you apply uniform rainfall, drainage rate, and runoff. What you see is a deeper flow in low-lying areas since it tends to accumulate, but a varying rainfall, drainage rate, and runoff (which is more realistic) will show varying floods.
- L270: what do the authors mean by Derit?
- L271: why do the authors label the high-water marks as empirical? Are they were not field surveyed? Explain more.
- L273-275: remove the sentence since this is obvious when comparing data to high-water marks.
- L275: the location of the high-water marks with respect to the deeper flood locations is another limitation of the study that the authors should highlight in the corresponding section.
- L286: I would not call what the authors did a hydrologic model.
- L287: Why did the authors select these drain rates? Justify and put into context what these values might represent. For example, an X-in pipe is flowing full. That would make it easier to relate to the real world.
- L296: the authors mention here that the Cn calculations were used using hourly rainfall, but in the respective section, they said they use the maximum value of rainfall over the basin within a 3-hour time window. Therefore, these arguments confuse the reader. Please decide what hourly runoff or a single value you used.
- L305: what do the authors mean by "non-zero depth HWMs"?
- L314-317: can you reference someone else who has followed a similar flood classification? Also, why is important to classify them as this? Also, I would expect the authors to change the color scale on the results to use this classification rather than the numeric values. If not, then what is the purpose of the classification in the first place?

- Figure 11: why not include the histogram insert in Figure 9? Also, the authors should combine this figure with Figure 9.
- L361: it is not clear what is the second scenario within the context of the sentence.
- L363-364: the authors do not have the necessary results to support this claim. For example, if the authors have run three different flooding scenarios (rainfall only, storm tide only, and compound flood), then they could comment on how important are each of those flood drivers in their modeling approach and in the compound flood assessment.
- L376-377: I disagree with the authors. While simple models could be useful, the authors fail to show this on the manuscript. Thus, it results does not support the claim.
- L381: the authors use the concepts of vulnerability and risk throughout the entire manuscript. However, they really only focus on the hazard component. To talk about risk the authors need to talk about the socio-demographics and the community's exposure. I will move away from these concepts since it is not the main focus of the study and replace it with the term hazard.
- L388: can this only be because tides? Authors should show results with and without rain and tides to see how each driver interacts and they could make a similar statement.
- L389: the authors talked about coastal flood zones but failed to define them. I will suggest citing Bilskie and Hagen (2018).
- L391-393: the authors do not support this statement with their current results.
- L396-398: There are already models that exist and are similar to the proposed approach but without the simplification taken here. For example, ICPR (known as StormWise) can include stormwater infrastructure, spatially-varying rain, and runoff, and include coastal boundaries similar to the author's approach.
- L404 and 406: the word "run off" should be together like "runoff".
- L423: the authors should not claim the sediment transport and erosion component of the model since it was not tested.