General Comments

The introduction needs to be revised to highlight the study's novelty. While the authors provide limited background on defining compound flood and the various models used to model it, they do not address the literature gap this study seeks to bridge. Furthermore, the authors do not present a clear research question that the study will address, and this question is not supported by the literature. The introduction only summarizes the methods at the end (which is very confusing and needs to be better restructured); it is not clear how the study will advance the field, given the lack of research questions and the identified knowledge gap. As it is now, it only tells me that the study is comparing two flood models for compound floods, which is not novel. However, if the authors reshaped the introduction according to my suggestions, the novelty this study proposes would be clearer. Furthermore, the authors compare their proposed approach with another study, but fail to explain why their study differs. It is crucial for the reader to understand why the proposed study differs from the literature.

Regarding the simplified approach for estimating compound flood (section 2.2.1), I have concerns about their proposed selection of the compound flood level based solely on the maximum flood levels between the coastal and pluvial/fluvial levels. In the introduction, the authors cited several studies indicating that, in a compound flood event, the flood drivers exhibit nonlinear interactions. This means that when combined, their compound flood level could be lower or higher than the sum of their superposed levels. Thus, by selecting the maximum value between these two drivers, you can under- or overestimate the real compound flood while ignoring the other flood driver that was not chosen. This approach could have benefited from using a nonlinear regression or a machine learning-based approach to estimate the true compound flood value, rather than taking the easy way out and selecting the maximum value as a conservative estimate. Furthermore, the authors comment that the compound flood results from the simple model (L226) have a linear pattern without noise, contradicting previous studies that claim a nonlinear behavior between the flood drivers as their SFINCS results. If the authors plan to keep this, they need to do a better job of justifying this approach and limiting its implications. However, the manuscript lacks a limitations section, which is necessary for studies like this that propose a new flood modeling technique. I would require the authors to include a limitations section before the results or within the discussion that outlines all the simplifications and assumptions used by the authors.

Similarly, the SFINCS model section is missing significant information on model building (see specific comments). Also, neither model (simple or SFINCS) was calibrated/validated

against the various tropical cyclones that have impacted the area, limiting the reliability of the outcomes. Also, it is not clear to me why the authors force the SFINCS and the simple model with two flood drivers at once (coastal with pluvial or coastal with fluvial), since they claimed in the introduction that compound floods can include all three flood drivers. The Result section is poorly written with unnecessary content, such as a repetition of the methods (L230-235), and only set of figures that evaluate the same outcome. The interpretation of these results is very superficial (2-3 sentences per figure), and more content (from figures to text) is needed. However, the discussion has been framed as an extension of the results, as it provides a description and the physical meaning of the results. The discussion needs to connect the study results with the literature and the research gap, but without proper research questions, it is impossible for the authors to cite other studies.

Overall, many of the paragraphs (section 2.2, some in the introduction, etc.) contain statements that were just thrown there without any connection/transition between topics, making it hard to read and follow the author's point. Thus, I strongly suggest that the authors rephrase and strengthen the connection so they can tell a story with the publication rather than just a technical report. However, the approach selected to create the compound flood model using the simple approach may have substantial flaws within the literature, and without further modification of equations 1-3, the manuscript could be difficult to publish.

Specific Comments

- L25-26: rephrase the sentence since the "compound coastal flooding" is used incorrectly, since compound flood implies that coastal and hydraulic flooding are happening. You may want to say that you are focusing on compound flood in coastal areas rather than estuaries, but compound has to also occur in the coastal area, so it might be redundant. The same occurs on L27 and L30, so please fix throughout the manuscript and call it only compound flood and not compound coastal flood.
- L26-27: The sentence talking about compound events (broader than flooding) seems out of context. I would remove.
- L24-38: This paragraph would benefit from improving the reading flow by enhancing the transitions and connections between sentences. Some statements look like they were just thrown there without any coherence. Please improve.
- L39-52: Avoid having a small paragraph with 3 sentences or fewer. Try combining them with their appropriate transition, so your paragraphs are longer (5-8 sentences).

- L46-47: Why are these models less applicable? Give a reasoning for your statement and cite some literature supporting your claim.
- Figure 1: I will suggest adding the river location on panel b and even removing the grey background from this panel since it does not allow to visualize how this inland community could be affected by compound flood through the river. I will also highlight the river/coastline/bay on panels C and D, as it is important for your compound flood. Also, what is the vertical datum reference for the DEM elevation?
- L85-85: I need to see these water features (river, lakes, and bay) commented in Figure 1.
- L100-101: This statement needs support from the literature since it is a big assumption and limitation.
- L127-128: The claim that infiltration is not needed since the POI are in impervious regions is not 100% true, since water will travel from overland and accumulate. For example, if water upstream of the POI is traveling through grass, it should infiltrate some portion before reaching the POI, but in the assumed case, it would not, and more water would be there.
- Table 1: There is no justification or rationale for the values chosen on this table, nor its physical meaning. I am positive about the selected approach for creating the synthetic events. However, this needs to be grounded in the real conditions. For example, the authors have to comment on how realistic it is to have an X mm rainfall with a Y m coastal water level. Also, it is not clear if the rainfall being applied is uniform or not throughout the domain.
- L174-176: The authors need to further justify how realistic it would be to have a relatively high coastal water level be applied uniformly this high for seven days. Why not add tidal conditions or recreate a storm surge hydrograph, which will raise the water level temporarily rather than for the whole 7 days?
- L195-200: Please add MKGE and NSE as a comparison metric. These metrics represent a better comparison with streamflow data.
- L217: It is not clear why the authors selected the mentioned rainfall intensity. Also, what was the temporal distribution of the rainfall, as well as the spatial distribution?
- L238-239: This statement is not correct. For example, the results between the simple and the SFINCS model for the Silsbee POI 1-2 and HWM 1-2 are completely different, but the statement says depth are similar.