

Reviewer #1

This manuscript compares a conventional Atlas-14-based uniform design storm with an SST-based ensemble and examines how the two rainfall representations affect inundation extent, building exposure, and structural damage estimates in the Vermilion River Basin, Louisiana. The topic is relevant, and the attempt to connect rainfall representation with consequence analysis at the building scale is worthwhile. The results are potentially useful, especially the finding that SST realizations identify additional exposed structures and larger upper-tail losses than the deterministic Atlas-14 case. The paper is not ready for publication in its current form. My main concerns are with the framing of the study, the incomplete treatment of the SST literature, and the lack of methodological clarity in the SST implementation. I also think the flood exposure analysis is not yet sufficiently developed. The manuscript currently reads more like a case-study demonstration than a well-supported analysis of “design storm characterization” in a broader sense.

We thank the reviewer for taking the time to provide a thorough and constructive review of the manuscript. We have made major revisions to address the reviewer’s concerns. Below, we provide point-by-point responses to each comment.

Major comments are as follows.

The SST literature review is too limited. For a paper built around SST, the review of previous work is not sufficient. The manuscript cites some core SST studies, but the broader literature on storm transposition, rainfall spatial structure, and basin-scale sensitivity is not adequately covered. This matters because the paper frames itself as addressing a gap in how storm representation affects inundation and damage, yet the introduction does not fully establish that context. In its current form, the novelty is not entirely convincing.

We thank the reviewer for this important comment. We agree that the original Introduction did not provide sufficient context on SST literature. We revised the Introduction to expand the literature review and to better establish the gap that motivates the present study.

Specifically, we added Zhu et al. (2018) to strengthen the discussion of rainfall space-time structure and basin-scale sensitivity. In addition, we added recent SST applications by Perez et al. (2024) and Baer et al. (2026), which extend SST-based analysis toward flood inundation, exposure, and damage estimation. FitzGerald et al. (2026) was also added in the SST-methodology discussion to describe the latest research in the field of objective and reproducible transposition-domain delineation.

We also revised the novelty framing. The manuscript no longer presents the study as a broad evaluation of design-storm characterization. Instead, the revised Introduction

clarifies that the contribution is focused on how selected 100-year, 24 h rainfall representations propagate through a high-resolution hydrologic-hydrodynamic and building-level consequence workflow. The revised text now states:

“Building on prior SST studies of rainfall–flood frequency relationships and rainfall space-time structure (Wright et al., 2014, 2017; Zhu et al., 2018), and by recent extensions of SST toward inundation, exposure, and damage estimation (Perez et al., 2024; Baer et al., 2026), this study addresses a gap at the intersection of rainfall hazard representation, high-resolution hydraulic modeling, and building-level consequence analysis. We evaluate building-level flood exposure and structure damage in the VRB under two contrasting rainfall representations of the 100-year, 24 h event: (1) a deterministic, spatially uniform NOAA Atlas-14 AEP storm, spatially adjusted using Technical Paper 40 (TP40; Hershfield, 1961) depth-area relationships, and (2) an ensemble of 50 SST realizations constructed from gauge-adjusted radar rainfall fields.”

The SST methodology is not described in enough detail for the analysis to be fully convincing. Section 3.1 gives only a general description of how SST was applied. Several choices that could affect the results are either missing or treated too briefly. The discussion of domain selection is particularly weak. The manuscript notes that no exact procedure exists and then states that the domain was selected largely based on familiarity with the region and inclusion of major storms. That may be practical, but it remains subjective, and the implications of that subjectivity are not sufficiently addressed. Overall, the SST setup is not transparent enough at present.

We thank the reviewer for this important comment. We agree that the original manuscript did not describe the SST setup in sufficient detail, particularly the transposition-domain choice, parent-storm selection, storm-arrival model, parent-storm screening, and realization count. We have substantially revised Section 3.1 to make the SST implementation more transparent.

The revised manuscript now states that SST was implemented using RainyDay with the NCEP Stage IV gauge-adjusted radar rainfall archive for 2002–2020. We added a clearer explanation of the transposition domain shown in Fig. 1b, including that it was selected to include the VRB and surrounding areas with broadly similar rainfall-producing mechanisms while also being large enough to provide a sufficient catalog of historical parent storms. We also now explicitly acknowledge that SST domain selection is an important methodological choice because it controls which historical storms are available for transposition and where they may be repositioned.

To address the reviewer’s concern about subjectivity, we revised the text to state that SST domains have historically often been defined using regional and meteorological judgment, but that objective and reproducible domain-delineation methods are an active area of research. We added FitzGerald et al. (2026), who introduced an L-moments-based hypothesis-testing framework for identifying statistically homogeneous storm-transposition regions, and we now state that applying alternative objective domain-delineation procedures was outside the scope of the present study. This makes the domain-selection limitation explicit rather than treating the selected domain as uniquely definitive.

We also added implementation details for the SST sampling procedure. The revised Section 3.1 now states that parent storms were identified as 24 h rainfall events over the SST domain with a minimum temporal separation of 24 h, that the initial catalog contained 350 parent storms, corresponding to approximately 19 storms per year, and that annual storm counts were represented using a Poisson storm-arrival model parameterized from the catalog frequency. We further added that 26 events with less-than-optimal radar rainfall quality were excluded before stochastic resampling, and that 50 independent SST realizations were carried forward because this number balanced stochastic sampling of plausible 100-year storm placements with the computational expense of the full hydrodynamic and consequence-analysis workflow.

The revised manuscript text now includes:

“The second rainfall representation was generated using stochastic storm transposition (SST) with RainyDay (Wright et al., 2017) and the NCEP Stage IV gauge-adjusted radar rainfall archive for 2002–2020. SST treats historically observed storms as space-time rainfall fields that can be repositioned within a broader transposition domain assumed to have similar extreme-rainfall characteristics. The SST domain used in this study is shown in Fig. 1b and was selected to include the VRB and surrounding areas with broadly similar rainfall-producing mechanisms, while also being large enough to provide a sufficient catalog of historical parent storms for transposition. We acknowledge that SST domain selection is an important methodological choice because the selected domain determines which historical storms are available for transposition and where they may be repositioned.”

and:

“Parent storms were identified from the Stage IV record by selecting 24 h rainfall events over the SST domain while enforcing a minimum temporal separation of 24 h between storms. The initial catalog contained 350 24 h parent storms, corresponding to

approximately 19 storms per year over the Stage IV archive used in this study. The extracted storms were then inspected, and 26 events with less-than-optimal radar rainfall quality were excluded before stochastic resampling.”

We hope that these revisions provide the missing SST setup information and clarify the implications of domain-selection subjectivity.

The title is broader than the actual analysis. The paper is presented as a study of the “effect of design storm characterization,” but in practice it compares two specific storm representations: one Atlas-14-based uniform storm and one SST-based ensemble. That is a narrower question. The manuscript does not clearly state which storm characteristics are actually under investigation. As a result, the framing promises more than the analysis delivers.

We thank the reviewer for this comment. We agree that the original title and framing were broader than the analysis performed. The manuscript is not intended to evaluate all aspects of design-storm characterization, but rather to compare selected 100-year, 24 h rainfall representations in the VRB with a baseline spatially uniform NOAA Atlas-14 design storm and an ensemble of SST-derived rainfall realizations. We have therefore revised the title and manuscript framing to align more directly with the actual analysis.

The title has been changed from:

“Effect of Design Storm Characterization on Flood Exposure and Structure Damage Estimates: A Case Study in South Louisiana, USA”

to:

“Flood Exposure and Structure Damage under Deterministic Design-Storm and Stochastic Storm Transposition Rainfall Scenarios: A Case Study in South Louisiana, USA”

We also revised the Introduction, Methods, Discussion, and Conclusions to state more clearly which rainfall characteristics are being compared. The primary comparison is between: (1) a deterministic, spatially uniform Atlas-14 rainfall field adjusted using TP40 depth-area relationships and distributed temporally using the alternating-block method, and (2) 50 SST-derived rainfall realizations that preserve observed storm spatial structure and allow storm placement to vary within the SST domain. We also added Atlas-14 sensitivity simulations to evaluate the effect of selected deterministic design-storm assumptions, specifically rainfall-depth uncertainty using the upper 90% confidence-bound Atlas-14 depth and temporal peak-placement assumptions using 25% and 75% alternating-block peak timing.

The revised Introduction now states:

“We evaluate building-level flood exposure and structure damage in the VRB under two contrasting rainfall representations of the 100-year, 24 h event: (1) a deterministic, spatially uniform NOAA Atlas-14 AEP storm, spatially adjusted using Technical Paper 40 (TP40; Hershfield, 1961) depth-area relationships, and (2) an ensemble of 50 SST realizations constructed from gauge-adjusted radar rainfall fields.”

The Conclusions were also revised to avoid broad claims about design-storm characterization in general and to keep the findings tied to the selected rainfall representations evaluated in this case study. These revisions align the title, analysis, and final claims more closely with the scope of the work.

The Atlas-14 vs. SST comparison is not fully controlled. I was not fully convinced by some of the causal interpretation. The two setups differ in several respects at the same time, not only in spatial rainfall variability. This makes it difficult to attribute the reported differences to one factor alone. The manuscript later emphasizes the role of localized rainfall cores, but the comparison itself does not isolate that effect clearly enough. At minimum, this issue should be acknowledged more carefully in the interpretation of results.

We thank the reviewer for raising this important point. We agree that the Atlas-14 and SST simulations differ in multiple ways and that the original manuscript attributed the differences too strongly to rainfall spatial variability alone. We have revised the manuscript to clarify that the comparison is not a fully controlled attribution experiment. Instead, it is a comparison of selected rainfall representations of the 100-year, 24 h event: a baseline spatially uniform Atlas-14 design storm and 50 SST-derived rainfall realizations, with additional Atlas-14 sensitivity simulations used to contextualize selected deterministic design-storm assumptions.

To address this issue, we revised the Methods, Results, Discussion, and Conclusions to use more careful language such as “rainfall representation,” “rainfall-forcing differences,” and “storm placement,” rather than implying that all differences are caused only by spatial rainfall variability. We also added explicit sensitivity simulations for the Atlas-14 side of the comparison. These include the upper bound of the NOAA Atlas-14 90% confidence interval and two alternative temporal peak-placement cases with the alternating-block peak shifted to 25% and 75% of the 24 h storm duration. The revised Methods now state:

“To contextualize the baseline Atlas-14 scenario within conventional design-storm uncertainty, we also conducted three additional Atlas-14 sensitivity simulations. First, we

simulated the upper bound of the NOAA Atlas-14 90% confidence interval using the same TP40 depth-area adjustment and 50% alternating-block temporal pattern as the baseline Atlas-14 case. Second, we evaluated sensitivity to temporal peak placement by repeating the baseline Atlas-14 rainfall depth with the alternating-block peak shifted to 25% and 75% of the 24 h storm duration. These additional Atlas-14 simulations were not used to redefine the primary comparison, but they provide a sensitivity check on how rainfall-frequency uncertainty and temporal-pattern assumptions affect exposure and damage estimates under a spatially uniform design-storm framework.”

We also added a new rainfall-volume comparison in Section 4.2 to avoid attributing the Atlas-14/SST differences solely to spatial rainfall structure. The revised Results now explicitly state that the baseline Atlas-14 scenario has a larger TP40-adjusted areal rainfall depth than the mean SST realization:

“The total structure damage produced by Atlas-14 is approximately \$0.477B, slightly exceeding that of the SST 90th-percentile composite. This similarity is partly explained by rainfall volume: the TP40-adjusted Atlas-14 areal rainfall depth is 310 mm, whereas the mean basin-average 24 h accumulation across the 50 SST realizations is approximately 238 mm. However, the SST ensemble also shows that rainfall volume alone does not determine damage. Across the 50 SST realizations, basin-average rainfall-depth rank does not closely match total structure-damage rank (Fig. S1), indicating that storm placement relative to developed flood-prone areas also affects losses.”

We further revised the interpretation of the SSTmax damage result to avoid overstatement. The revised Results now state:

“In this sense, the uniform Atlas-14 storm represents a widespread high-volume design event, as shown by its larger basin-average rainfall depth relative to the individual SST realizations (Fig. S2). However, the weak relationship between basin-average rainfall accumulation and total structure loss across the SST realizations indicates that spatial storm placement also contributes to the loss divergence.”

Finally, we added the Atlas-14 sensitivity results to Section 4.2:

“These tests were motivated by the fact that the Atlas-14 and SST simulations differ not only in spatial rainfall structure, but also in how rainfall depth and temporal distribution are defined.”

and:

“The upper 90% confidence-bound Atlas-14 storm increased total structure damage to approximately \$0.738B, while the 25% and 75% peak-timing variants produced

approximately \$0.426B and \$0.495B, respectively. These results indicate that Atlas-14 rainfall-depth uncertainty has a larger effect on structure damage than the tested temporal peak-placement changes. However, all Atlas-14 sensitivity cases remain below the SSTmax composite damage estimate of approximately \$0.923B.”

The flood exposure analysis is not fully convincing in its current form. This part of the paper needs to be stronger. The “exposure gap” is defined in a one-sided way, focusing on buildings inundated in at least one SST realization but not under Atlas-14. That framing is useful, but it is also selective. Exposure is mostly treated as a binary outcome, and the analysis does not yet provide a sufficiently balanced picture of how exposure differs across the two hazard representations. Since flood exposure is a central part of the paper, this weakens one of the main arguments.

We thank the reviewer for this helpful comment. We agree that the original exposure-gap framing was too one-sided because it focused only on structures inundated by SST but not by Atlas-14. We revised both the Methods and Results to make the exposure comparison two-sided and more transparent.

In the revised Methods, we now define three exposure groups: buildings inundated by both the baseline Atlas-14 simulation and SSTmax, buildings inundated by SSTmax but not by Atlas-14, and buildings inundated by Atlas-14 but not by SSTmax. The revised Methods state:

“To identify how exposure differed between rainfall representations, we classified structures into three groups: buildings inundated by both the baseline Atlas-14 simulation and SSTmax, buildings inundated by SSTmax but not by the baseline Atlas-14 simulation, and buildings inundated by the baseline Atlas-14 simulation but not by SSTmax. We refer to the SSTmax-only group as the SST-exclusive exposure gap because these structures are missed by the baseline uniform Atlas-14 storm but become inundated when storm placement and spatial rainfall variability are represented through the SST ensemble. The Atlas-only group was also quantified to provide a balanced comparison of disagreement in both directions.”

We then revised Section 4.3 to report the reverse disagreement explicitly. The revised Results now state:

“Next, we evaluated disagreement between the baseline Atlas-14 inundation map and the SSTmax composite in both directions. This two-sided comparison is important because a deterministic design storm can also inundate structures that are not inundated in the SSTmax composite. In this case, however, the Atlas-14-only group was small, containing

312 buildings and contributing approximately \$2M in structure damage. Approximately 50% of the Atlas-14-only buildings had inundation depths of 0.03 m or less, and 75% had depths of 0.06 m or less, which explains their limited aggregate damage contribution (Figs. S5–S7). Because the reverse disagreement was small in both building count and damage, the remainder of this section focuses primarily on the larger SST-exclusive exposure gap: buildings inundated in at least one SST realization but not under Atlas-14.”

We also added supplementary figures to support this two-sided comparison. These figures show the inundated-building counts for SSTmax, Atlas-14, the overlap set, and the Atlas-14-only set; the corresponding structure damage; and the inundation-depth distribution for the Atlas-14-only buildings. This allows the reader to see that the Atlas-14-only group exists, but is small relative to the SST-exclusive group and contributes only limited aggregate damage.

We also strengthened the exposure analysis beyond a binary flooded/not-flooded classification by connecting exposure groups to inundation depths and structure damage. For the SST-exclusive group, the revised text reports that 11,518 structures were inundated in at least one SST realization but not under the baseline Atlas-14 scenario, with aggregate structure damage of approximately \$110M. We also clarified that the median inundation depths for these structures are shallow, but that the aggregate damage is meaningful because the group contains many buildings and includes a subset of structures experiencing deeper inundation.

The conclusions are somewhat overstated. The study considers one basin, one storm duration, one return period, one deterministic Atlas-based scenario, and 50 SST realizations. That is enough for an interesting case study, but not enough to support broad claims about low-gradient basins in general. The conclusions should stay closer to what has actually been demonstrated here. There is also still some misalignment between the title, the analysis, and the final claims.

We thank the reviewer for this comment. We agree that the original Conclusions made claims that were broader than the case-study design supported. We have revised the title, framing, Discussion, and Conclusions so that the manuscript now stays closer to the actual analysis: selected 100-year, 24 h rainfall representations in one basin, with a primary comparison between a baseline Atlas-14 spatially uniform design storm and 50 SST-derived rainfall realizations, plus selected Atlas-14 sensitivity simulations.

First, we revised the title to better match the analysis. The original title, “Effect of Design Storm Characterization on Flood Exposure and Structure Damage Estimates,” implied a

broader evaluation of design-storm characterization than the study performed. The revised title is:

“Flood Exposure and Structure Damage under Deterministic Design-Storm and Stochastic Storm Transposition Rainfall Scenarios: A Case Study in South Louisiana, USA”

Second, we revised the Conclusions to remove broad statements about low-gradient basins in general and to keep the findings tied to the VRB case study. We removed language such as “systematically underestimate” and reframed the conclusion around the selected rainfall representations, the common hydrodynamic and damage-assessment workflow, and the specific numerical findings obtained in the VRB. The revised Conclusions now begin:

“This study compared building-level flood exposure and structure damage estimates produced by selected 100-year, 24 h rainfall representations in the Vermilion River Basin (VRB), south Louisiana. The primary comparison was between a baseline spatially uniform NOAA Atlas-14 design storm, adjusted using TP40 depth-area relationships, and 50 stochastic storm transposition (SST)-derived rainfall realizations generated from gauge-adjusted radar rainfall fields. Additional Atlas-14 sensitivity simulations were used to evaluate how rainfall-depth uncertainty and temporal peak-placement assumptions affected the baseline deterministic design-storm result. All scenarios were evaluated under a common hydrodynamic and damage-assessment framework.”

We also revised the numbered findings so that they are more directly supported by the analysis. The first conclusion now combines the previously separate exposure and severity findings, noting that SSTmax inundated 11,518 structures not inundated by the baseline Atlas-14 storm, corresponding to approximately \$110M in additional structure damage, and that SSTmax produced approximately \$0.923B in structure damage compared with approximately \$0.477B for the baseline Atlas-14 scenario. The second conclusion reports the spatial pattern of the differences, including the KDE clustering and HAND results near Lafayette–Scott and Abbeville. The third conclusion adds a practical recommendation, stating that spatially variable rainfall scenarios can complement deterministic design-storm simulations when evaluating building-level flood consequences, localized pluvial flooding, exposure hotspots, and mitigation priorities.

Minor comments

1. Some basic information on the SST setup is still not stated clearly enough in the methods.

This comment was addressed in response to Major Comment 2. We substantially revised Section 3.1 to provide additional SST setup details, including the rainfall archive

used, SST domain description, parent-storm identification, storm-separation criterion, parent-storm catalog size, storm-quality screening, and realization count.

2. The basis for using 50 SST realizations is not explained clearly enough in the main methods section.

This comment was addressed in response to Major Comment 2. Section 3.1 now explains that 50 SST realizations were carried forward to balance stochastic sampling of plausible 100-year storm placements with the computational expense of running the full hydrodynamic and consequence-analysis workflow.

3. The definition of the “100-year, 24-hour” SST storms is still not fully transparent.

This comment was addressed in response to Major Comment 2. We revised Section 3.1 to clarify that the SST analysis was conducted for 24 h rainfall and that the SST rainfall fields used in the hydrologic-hydrodynamic simulations were selected from the resulting 1% annual exceedance probability SST scenarios.

4. The Atlas-14 implementation appears highly simplified and needs clearer justification.

This comment was addressed in response to Major Comments 3 and 4. We revised Section 3.1 to clarify that the baseline Atlas-14 case represents a conventional spatially uniform design-storm implementation, with the point rainfall depth obtained from the NOAA Atlas-14 gridded product, adjusted to an areal depth using TP40, and distributed over 24 h using the alternating-block method. We also added Atlas-14 sensitivity simulations to evaluate rainfall-depth uncertainty and temporal peak-placement assumptions.

5. The temporal storm assumption for the Atlas-14 case is treated too casually given the topic of the paper.

This comment was addressed in response to Major Comment 4. We added two Atlas-14 temporal sensitivity simulations in which the alternating-block peak was shifted to 25% and 75% of the 24 h duration. These simulations showed that temporal peak placement had minor effects on the deterministic Atlas-14 damage estimate.

6. The differences between the two rainfall-forcing approaches are not presented very clearly.

This comment was addressed in response to Major Comments 3 and 4. We revised the Introduction, Methods, Results, and Conclusions to more clearly distinguish the two primary rainfall representations: a spatially uniform Atlas-14 design storm and spatially

variable SST-derived storm realizations. We also clarified that the comparison is not a fully controlled attribution experiment because the two approaches differ in rainfall volume, temporal construction, spatial structure, and storm-placement representation.

7. Terminology is not fully consistent throughout the manuscript.

We revised terminology throughout the manuscript for consistency.

8. The KDE analysis is under-documented.

We revised the Methods and Results to better document the KDE analysis based on this comment and comments from other reviewers. The manuscript now states that a Gaussian KDE was applied using a fixed bandwidth factor of $h=0.2$, applied consistently across flood maps, and that sensitivity tests with comparable bandwidth factors changed the smoothness of the KDE surfaces but did not alter the dominant cluster locations or interpretation.

9. The HAND-based interpretation remains somewhat qualitative.

We revised the HAND section to make the interpretation more cautious and transparent. The revised text describes HAND as a terrain-based diagnostic rather than a definitive causal explanation.

10. The manuscript would benefit from language polishing and some tightening of the Results section.

We revised and tightened the Results section to reduce repetition, improve transitions among subsections, and remove overstated causal language. We also edited the manuscript throughout for terminology consistency, clearer case-study framing, and closer alignment among the title, analysis, results, and conclusions.

Reviewer #2

The authors present a comparison of flood inundation and damage estimates from 100-year storms developed using two independent techniques: the conventional design storm approach using Atlas 14 rainfall frequency estimates, and fifty realizations of rainstorms from stochastic storm transposition (SST). The manuscript is well written and the findings are useful. I do have some suggestions, including at least one new hydrologic/hydraulic simulation and some additional discussion that could improve the manuscript prior to publication. Of particular note in this study is the demonstration of localized high rainfall rates driving flooding in “intermediate areas” that are neither adjacent to rivers nor in upland areas. This is important. It is striking how different the basin-average SST depths are so much less than the Atlas 14 depths, but the flood damage resulting from the former are greater. I have some comments below to improve the related discussion.

We thank the reviewer for taking the time to provide a thoughtful and constructive review of the manuscript. We have revised the manuscript to address the reviewer’s suggestions. Below, we provide point-by-point responses to each comment.

One notable if understandable omission from the works cited is the new study by Baer et al. (2026), which was recently accepted for publication in NPJ Natural Hazards (see preprint citation below; the paper will likely be published shortly, providing a better citation to used). That paper has broadly similar goals, but addresses one of the shortcomings of this manuscript (described below) in that it provides an (almost) fully probabilistic deployment of SST, allowing it to use both SST and design storm methods to estimate average annual loss. The authors will need to cite the Baer et al. study. It can also facilitate description of the main limitation of this study. Another omission is Perez et al. (2024), who used inundation and damage together with SST to examine the relationships between rainfall and flood responses.

We thank the reviewer for this helpful suggestion. We have added both Baer et al. (2026) and Perez et al. (2024) to the manuscript. Since Baer et al. (2026) has now been published in *npj Natural Hazards*, we cite the published article rather than the earlier preprint.

In the revised Introduction, we added discussion of Perez et al. (2024) as a recent example of extending SST-based flood-frequency analysis beyond rainfall and discharge frequency toward inundation and exposure-related flood responses. We also added Baer et al. (2026), which provides an important recent comparison of design-storm and SST-based flood hazard and risk estimates, including a more complete probabilistic treatment of damage.

We also used Baer et al. (2026) to clarify a limitation of the present study. Specifically, we now state that our analysis does not constitute a full probabilistic SST-to-risk workflow and does not estimate complete flood-frequency curves, damage-frequency curves, expected annual damage, or average annual loss. Instead, the present study compares selected 100-year, 24 h rainfall representations using a detailed hydrodynamic and building-level consequence-modeling workflow. This distinction has been added to the Discussion to better position our study relative to the broader probabilistic SST framework demonstrated by Baer et al. (2026).

The revised Introduction now includes:

“Recent SST applications have extended this framework beyond rainfall and discharge frequency analysis toward flood inundation, exposure, and damage estimation. Perez et al. (2024) advanced the process-based flood frequency analysis–SST (FFA-SST) framework by coupling RainyDay-generated synthetic storms with a spatially resolved hydrologic model to estimate frequency curves for peak flow, basin inundation fraction, and population flood exposure. Baer et al. (2026) further demonstrated the implications of spatiotemporal rainfall variability for flood risk by comparing design-storm and SST-based estimates of flood hazard and expected annual damage. These studies highlight the value of SST for evaluating the non-unique relationship between rainfall frequency, inundation, exposure, and losses, while also showing that broader probabilistic SST-to-risk analyses can require large numbers of hydrologic, hydraulic, and damage simulations.”

In my opinion, the main limitation of this study is that it doesn’t fully explore the capabilities of SST (or other stochastic rainfall methods) to examine the translation of rainfall into flood response and damage in probability terms. To do that, one would generate a large number (1,800+ in the Baer et al. case) of rainfall scenarios of varying magnitudes/AEPs, run them all through the hydrologic/hydraulic/damage estimation chain, estimate resulting damage AEPs, and then compare rainfall and damage AEPs. To be clear: I don’t think that the authors need to do this. But they should acknowledge that it is something they didn’t do, that it would be a useful extension of their work, cite appropriate studies (such as Baer et al. and Perez et al.), and explain why they didn’t go this route. One reasonable justification for not doing it is that the computational burden of thousands of hydraulic simulations is oftentimes too great to be feasible. This was the main factor in both Baer et al. and Perez et al. that limited the number of simulations they could run.

We thank the reviewer for this important comment. We agree that the present study does not fully explore the broader capability of SST to translate rainfall frequency into flood-

response and damage frequency in a probabilistic framework. We have revised the manuscript to acknowledge this limitation explicitly, cite the relevant recent studies, and explain why that broader workflow was outside the scope of the present analysis.

In the revised Introduction and Discussion, we now cite Perez et al. (2024) and the published Baer et al. (2026) study to better position our work relative to recent SST-based flood-hazard and risk analyses. We clarify that Perez et al. (2024) extended the FFA-SST framework from peak-flow estimation to flood extent and population exposure, while Baer et al. (2026) compared design-storm and SST-based estimates of flood hazard and expected annual damage. We also state that the present study complements these efforts by focusing on a detailed engineering-design case: how a 100-year, 24 h deterministic design storm and 50 SST-derived rainfall realizations propagate through a coupled HEC-HMS/HEC-RAS and building-level consequence workflow.

We also added a limitation paragraph explaining that the present study does not estimate complete flood-frequency curves, damage-frequency curves, expected annual damage, or average annual loss. The revised Discussion now states:

“The 50 SST realizations used here therefore represent a computationally feasible sample of plausible 100-year, 24 h SST rainfall scenarios, not an exhaustive sample of all possible SST storm placements or rainfall-frequency outcomes. Unlike full FFA-SST or probabilistic risk workflows, this study does not estimate complete flood-frequency or damage-frequency curves, expected annual damage, or uncertainty bounds across many return periods. Such broader workflows require many more hydrologic, hydraulic, and consequence simulations than were feasible with the detailed HEC-HMS/HEC-RAS and building-level damage workflow used here.”

We further explained the computational reason for not pursuing the full probabilistic workflow. The coupled HEC-HMS/HEC-RAS model used in this study provides a detailed representation of channels, hydraulic structures, and storage areas needed to simulate complex flood regimes in the VRB, but that level of detail restricts the number of simulations that can be executed in a reasonable timeframe. We now note that simpler models may support hundreds or thousands of simulations, but can simplify hydraulic controls, storage areas, and channel bathymetry that are important for the VRB application.

These revisions clarify that the present study is not intended to replace a full probabilistic SST-to-risk analysis. Instead, it evaluates a computationally feasible, detailed hydrodynamic and building-level consequence workflow for selected 100-year, 24 h rainfall

representations, while identifying a broader multi-AEP SST-based damage-frequency analysis as an important future extension.

I recommend that the authors carry out one or two more simulations; using the upper (and less importantly, lower) bound of the 90% confidence interval from Atlas 14. This would help contextualize the results even further in terms of conventional design storm uncertainties.

We thank the reviewer for this suggestion. We added an Atlas-14 sensitivity simulation using the upper bound of the NOAA Atlas-14 90% confidence interval. This simulation used the same TP40 depth-area adjustment and the same 50% alternating-block temporal pattern as the baseline Atlas-14 case. After TP40 adjustment, the upper 90% confidence-bound rainfall depth was 389.3 mm. We focused on the upper confidence-bound case because it is the more relevant test for contextualizing whether conventional design-storm rainfall-depth uncertainty could approach the larger SST damage estimates.

The revised Methods now state:

“To contextualize the baseline Atlas-14 scenario within conventional design-storm uncertainty, we also conducted three additional Atlas-14 sensitivity simulations. First, we simulated the upper bound of the NOAA Atlas-14 90% confidence interval using the same TP40 depth-area adjustment and 50% alternating-block temporal pattern as the baseline Atlas-14 case. Second, we evaluated sensitivity to temporal peak placement by repeating the baseline Atlas-14 rainfall depth with the alternating-block peak shifted to 25% and 75% of the 24 h storm duration. These additional Atlas-14 simulations were not used to redefine the primary comparison, but they provide a sensitivity check on how rainfall-frequency uncertainty and temporal-pattern assumptions affect exposure and damage estimates under a spatially uniform design-storm framework.”

The revised Results report that the upper 90% confidence-bound Atlas-14 storm increased total structure damage to approximately \$0.738B, compared with \$0.477B for the baseline Atlas-14 case. The 25% and 75% peak-timing variants produced approximately \$0.426B and \$0.495B, respectively. All Atlas-14 sensitivity cases remained below the SSTmax composite damage estimate of approximately \$0.923B. These results are now shown in Figs. S3–S4 and discussed in Section 4.2.

Regarding the useful analysis of the importance of localized rainfall rates: it really seems to me like these findings are driving home the issue of pluvial vs. fluvial flooding, and how the

former is an important contributor to flood damage that is omitted from FEMA floodplain assessments. Of course, that opens up the necessity to cite some sources from the growing pluvial flooding literature. In other words, the authors seem to be describing an important and increasingly recognized phenomenon, one that SST and other space-time stochastic rainfall methods can address, that design storms manifestly fail to address, and they should use the right terminology when describing it.

We thank the reviewer for this helpful comment. We agree that the localized rainfall-driven flooding identified in the manuscript is closely related to pluvial flooding, particularly where intense rainfall placement produces building-level inundation outside the main river corridor. We revised the manuscript to use pluvial-flooding terminology more consistently and added citations to studies that connect pluvial flood hazard with stochastic space-time rainfall representation.

Specifically, we added Simões et al. (2015) and Mediero et al. (2022) to the Introduction to better connect the SST findings with the broader pluvial-flooding literature. The revised Introduction now states:

“This issue is closely related to pluvial flooding, where intense local rainfall and limited drainage capacity can produce inundation away from the main river corridor; previous studies have shown that stochastic space-time rainfall methods can be useful for representing such rainfall-driven flood hazards in urban areas (Simões et al., 2015; Mediero et al., 2022).”

We also revised the Results and Discussion to describe the additional SST-driven impacts as localized pluvial flooding in developed areas. For example, Section 4.2 now states:

“This observation emphasizes that a portion of the additional SST-driven damage arises from localized pluvial flooding in developed areas, where intense rainfall placement can increase building-level inundation outside the main river corridor. These losses are therefore not only a function of basin-wide flood severity, but also of how spatial rainfall organization interacts with locally flood-prone terrain.”

In the Discussion, we added:

“This interpretation is consistent with pluvial-flooding studies showing that spatially and temporally variable rainfall can strongly affect urban inundation patterns and that stochastic rainfall-based approaches can help identify pluvial flood hotspots that may not be captured by deterministic design-storm assumptions (Simões et al., 2015; Mediero et al., 2022).”

Minor comments:

L9: “Annual” should not be capitalized

Corrected. We changed “Annual” to “annual.”

L75: we didn’t use Stage IV in Wright et al. (2014). We used a radar rainfall dataset developed ourselves using the HydroNEXRAD algorithm

Corrected. We revised the Introduction to avoid stating that Wright et al. (2014) used Stage IV.

Generally: details on the SST “configuration,” such as the transposition domain, etc. are missing. I imagine that stuff will be included in the publicly available data and so perhaps it isn’t necessary to describe it in the paper. You should highlight that these details will be available (perhaps you already do in the paper and I missed it).

Addressed. We revised Section 3.1 to provide additional SST configuration details directly in the manuscript, including the Stage IV archive period, transposition-domain rationale, 24 h parent-storm definition, 24 h storm-separation criterion, parent-storm catalog size, storm-quality screening, and the basis for using 50 SST realizations. We also plan to provide these materials with the codes used.

L164 and L172: Wright et al. (2014) is not the correct citation for RainyDay. It should be Wright et al. (2017).

Corrected. We now cite Wright et al. (2017) when referring specifically to RainyDay.

L168-169: Please cite our ongoing work on objective transposition domain delineation: (preprint) FitzGerald et al. (2025)

Addressed. We added FitzGerald et al. to the SST domain-selection discussion. Since this work is now available as the published Journal of Hydrology article, we cite the published version as FitzGerald et al. (2026) rather than the earlier preprint.

Fig 4 caption and/or figure: make clear that “count” refers to number of inundated structures

Corrected. We revised the Fig. 4 caption to clarify what count means.

L351-354: I would recommend leaning heavily into this striking quantitative result. For example, I think you can gain the readers’ attention if you contrast the basin average rainfall differences (238 vs. 310 mm) and flood damage differences quantitatively in the paper’s abstract.

Addressed. We revised the Abstract to highlight this quantitative contrast. The revised Abstract now states that although the TP40-adjusted Atlas-14 rainfall depth was larger than the mean basin-average SST accumulation across the 50 realizations (310 mm vs. 238.1

mm), the SSTmax composite produced substantially greater total structure damage than the baseline Atlas-14 scenario (\$0.923B vs. \$0.477B).

L381: “localized extreme rainfall over highly populated areas” this is a clear example of where using the “pluvial flooding” terminology is appropriate.

Addressed. We revised the Results, Discussion, and Conclusions to use “localized pluvial flooding” where appropriate. We also added pluvial-flooding citations, including Simões et al. (2015) and Mediero et al. (2022), to connect the findings with the broader literature on stochastic rainfall representation and pluvial flood hazard.

L402: I think you mean “spatial”, not “special”

Corrected. We changed “special” to “spatial.”

Regards,

Daniel Wright

References:

John A. Baer, Antonia Sebastian, Lauren E. Grimley et al. Neglecting Spatiotemporal Rainfall Variability Underestimates Flood Hazard and Risk, 23 January 2026, PREPRINT (Version 1) available at Research Square [<https://doi.org/10.21203/rs.3.rs-8593870/v1>]

FitzGerald, Benjamin and Wright, Daniel B. and Yan, Lei and Dietrich, Alyssa Hendricks and Sebastian, Antonia, An L-Moments-Based Hypothesis Test to Identify Homogeneous Storm Transposition Regions. Available at SSRN: <https://ssrn.com/abstract=5292988> or <http://dx.doi.org/10.2139/ssrn.5292988>

Perez, Gabriel, Ethan T. Coon, Saubhagya S. Rathore, and Phong V. V. Le. “Advancing Process-Based Flood Frequency Analysis for Assessing Flood Hazard and Population Flood Exposure.” *Journal of Hydrology* 639 (August 2024): 131620. <https://doi.org/10.1016/j.jhydrol.2024.131620>.

Wright, D. B., Ricardo Mantilla, and Christa D. Peters-Lidard. “A Remote Sensing-Based Tool for Assessing Rainfall-Driven Hazards.” *Environmental Modelling & Software* 90 (2017): 34–54. <https://doi.org/10.1016/j.envsoft.2016.12.006>.

Reviewer #3

This study compares flood exposure and structure damage estimates produced by a conventional spatially uniform NOAA Atlas 14 design storm against an ensemble of 50 stochastic storm transposition (SST) realizations in the Vermilion River Basin, a low-gradient inland-coastal watershed in south-central Louisiana. The topic is relevant and the methodological framework is well-developed. The use of a calibrated coupled HEC-HMS/HEC-RAS model, the HAND-based topographic analysis, and the exposure gap quantification are meaningful contributions. The paper, however, requires moderate revisions before it can be considered for publication. The comments are provided below.

We thank the reviewer for the thoughtful and constructive review. Below, we provide point-by-point responses to each comment.

1. The abstract leads with methodology rather than the problem motivating the study. A clearer statement of why this comparison matters for flood risk management and engineering practice would strengthen the opening.

We thank the reviewer for this helpful comment. We revised the Abstract so that it now begins with the flood-risk management and engineering motivation rather than the methodology. The revised opening emphasizes that flood hazard maps and damage estimates guide floodplain management, mitigation planning, and engineering design, but often rely on spatially uniform design storms that suppress the spatial organization and intensity gradients present in observed storms. The revised Abstract now begins:

“Flood hazard maps and damage estimates guide floodplain management, mitigation planning, and engineering design, but they commonly rely on rainfall frequency analysis to define annual exceedance probability (AEP) storms, commonly referred to as design storms. Current engineering practice typically employs spatially uniform design storms derived from point-based gauge statistics; however, this approach suppresses the spatial organization and intensity gradients present in observed storms.”

We also revised the Abstract to emphasize the main quantitative findings, including the contrast between the TP40-adjusted Atlas-14 rainfall depth and mean SST basin accumulation, the corresponding difference in total structure damage, and the additional buildings inundated in the SST realizations but not under the baseline Atlas-14 scenario.

2. The introduction is long and could be streamlined without losing substance. Several paragraphs covering prior work (lines 40–70) overlap in their message. The specific contribution of this paper relative to Wright et al. (2013, 2014, 2019) and Zhu et al. (2018) should be stated more sharply. It is not immediately clear whether the novelty lies in the hydraulic model fidelity, the damage estimation framework, or the HAND-based diagnostic, or all three.

We thank the reviewer for this helpful comment. We revised the Introduction to reduce overlap in the literature framing and to state the contribution of the paper more sharply relative to prior SST studies. The revised Introduction now distinguishes earlier SST work on rainfall–flood frequency relationships and rainfall space-time structure from more recent SST applications involving inundation, exposure, and damage estimation.

Specifically, we added discussion of Zhu et al. (2018), Perez et al. (2024), and Baer et al. (2026), and clarified that the contribution of the present study lies at the intersection of rainfall hazard representation, high-resolution hydraulic modeling, and building-level consequence analysis. The revised Introduction now states:

“Building on prior SST studies of rainfall–flood frequency relationships and rainfall space-time structure (Wright et al., 2014, 2017; Zhu et al., 2018), as well as recent extensions of SST toward inundation, exposure, and damage estimation (Perez et al., 2024; Baer et al., 2026), this study addresses a gap at the intersection of rainfall hazard representation, high-resolution hydraulic modeling, and building-level consequence analysis. We evaluate building-level flood exposure and structure damage in the VRB under two contrasting rainfall representations of the 100-year, 24 h event: (1) a deterministic, spatially uniform NOAA Atlas-14 AEP storm, spatially adjusted using Technical Paper 40 (TP40; Hershfield, 1961) depth-area relationships, and (2) an ensemble of 50 SST realizations constructed from gauge-adjusted radar rainfall fields.”

We also clarified that the HAND analysis is used as a diagnostic tool to interpret where the largest Atlas-14–SSTmax exposure differences occur, rather than as the sole novelty of the paper.

3. Section 3.1: The SST domain selection is described as relying on "familiarity with the study region," and the authors acknowledge that no formal procedure exists for this selection. This is a meaningful source of uncertainty that deserves more explicit discussion. How sensitive are the results to reasonable alternative domain definitions? This should be addressed either in the methods or in the limitations section.

We thank the reviewer for this important comment. We agree that SST domain selection is a meaningful source of uncertainty and should be discussed more explicitly. We revised Section 3.1 to describe the selected transposition domain more carefully and to acknowledge that the SST results depend on this choice.

The revised Methods now state that the SST domain is shown in Fig. 1b and was selected to include the VRB and surrounding areas with broadly similar rainfall-producing mechanisms, while also being large enough to provide a sufficient catalog of historical parent storms for transposition. We also now explicitly state that SST domain selection determines which historical storms are available for transposition and where they may be repositioned.

To place this limitation in context, we added FitzGerald et al. (2026), who introduced an L-moments-based hypothesis-testing framework for identifying statistically homogeneous storm-transposition regions. The revised Section 3.1 now states:

“We acknowledge that SST domain selection is an important methodological choice because the selected domain determines which historical storms are available for transposition and where they may be repositioned. Historically, SST domains have often been defined using regional and meteorological judgment; however, objective and reproducible methods for domain delineation are an active area of research. For example, FitzGerald et al. (2026) introduced an L-moments-based hypothesis-testing framework for identifying statistically homogeneous storm-transposition regions. Applying and evaluating alternative objective domain-delineation procedures was outside the scope of the present study, which focuses on how the selected rainfall representation affects modeled inundation and structure damage estimates.”

We also expanded the limitations discussion to clarify that the SST results are conditional on the selected transposition domain and parent-storm catalog. The revised Discussion now states that alternative domain choices could change the storms available for transposition and therefore the resulting SST rainfall fields. We did not conduct a formal sensitivity analysis using alternative SST domain definitions because doing so would require rebuilding the parent-storm catalog, resampling SST rainfall fields, and rerunning the full hydrodynamic and building-level consequence workflow for each candidate domain, which was beyond the computational scope of this study.

4. Section 3.1: The number of parent storms selected for the SST catalog is not stated. The total count should be reported explicitly, along with the years from the 2002–2020 archive that contributed the most events.

Addressed. We revised Section 3.1 to report that the initial SST catalog contained 350 24 h parent storms from the 2002–2020 Stage IV archive. We also added that, among the 50 routed 100-year SST realizations used in this analysis, the contributing parent events began on 29 August 2012, 16 October 2006, 12 August 2016, 9 March 2016, 25 October 2015, and 29 August 2017.

5. Section 3.2: The acceptability criterion for model calibration, requiring that at least 50% of evaluated gauges meet performance thresholds for each event, is relatively lenient. The rationale for this threshold should be justified, and the actual performance statistics (PPCC, RMSE, PBIAS) for calibration and validation events should be reported, at minimum in a summary table.

We thank the reviewer for this comment. We agree that the 50% event-level screening criterion required additional justification and that the calibration/validation statistics should be reported more transparently. We revised Section 3.2 to explain that the 50% threshold follows the Louisiana Watershed Initiative model-performance framework under which the Vermilion regional model was developed and reviewed.

We also clarified that the 50% criterion is most relevant for discharge because only two discharge gauges were available; therefore, for discharge metrics, meeting the criterion can correspond to one of two gauges satisfying the target. For stage/WSE, however, the model generally exceeded this minimum threshold, with most calibration and validation events meeting the target metrics at 80–100% of evaluated stage gauges.

To make this transparent, we added two supplementary tables. Table S1 summarizes the number and percentage of gauges meeting the LWI target criteria for each calibration and validation event, while Table S2 reports the corresponding station-level ranges of PPCC, RMSE%, and PBIAS across the evaluated gauges. These additions show both the event-level pass rates and the remaining station-level variability.

6. Section 3.2: The antecedent soil moisture condition is set to 36.8% of maximum storage for all 51 simulations. This value appears precise but its derivation is not explained. Is this a calibrated parameter, a median of observed pre-event conditions, or an assumed value? This should be clarified.

We thank the reviewer for identifying the need to clarify this value. We revised Section 3.2 to explain that the 36.8% value is not directly measured volumetric soil moisture and should not be interpreted as such. It represents a fixed initial deficit equal to 36.8% of the maximum deficit in the deficit-and-constant loss formulation.

The revised manuscript explains that this value was derived from the validated LWI initial-deficit framework, which relates the initial deficit ratio to cumulative precipitation during the 12 days preceding the simulation start. Within that framework, the selected value represents an intermediate antecedent condition within the range of calibration and validation events, rather than an unusually wet or unusually dry initialization.

We also clarified that the same initial deficit was applied to the baseline Atlas-14 simulation, the additional Atlas-14 sensitivity simulations, and the 50 SST realizations so that antecedent wetness was held fixed while evaluating differences among the selected rainfall forcings.

7. Section 4.1: The discussion of the lognormal distribution parameters is informative, but the claim that the dispersion parameter $\sigma_{\ln}$ is "largely governed by the basin's topography" is stated as a conclusion without supporting analysis. A brief justification or citation would strengthen this claim.

We thank the reviewer for this comment. We agree that the original statement attributing the stability of $\sigma_{\ln}$ largely to basin topography was stronger than the analysis directly supported. We revised Section 4.1 to remove that unsupported interpretation and to describe the fitted distribution parameters more cautiously.

The revised text now states that $\sigma_{\ln}$ varies only modestly across the Atlas-14 and SST composite scenarios, indicating that the fitted depth distributions maintain a similar right-skewed spread between shallow and deep inundation depths. We now interpret this as showing that rainfall representation can change how many buildings flood and shift the

distribution toward greater depths, without fundamentally altering the relative dispersion of flooded-building depths across the fitted distributions.

We also revised the later interpretation to state that the fitted depth distributions retain a broadly similar right-skewed shape across scenarios, while the distribution center and upper-tail behavior are more strongly influenced by rainfall spatial distribution and storm placement. This avoids presenting topographic control as a demonstrated conclusion without a supporting analysis.

8. Section 4.2: The Atlas-14 scenario produces approximately \$0.48B in damages, which the authors describe as comparable to the SST 90th-percentile composite. This alignment is then attributed to the smoothing effect of percentile composites. However, a more direct explanation, that Atlas-14 represents a relatively high areal rainfall depth after TP40 reduction (310 mm) while the average SST basin accumulation is only 238 mm, is introduced later in Section 4.1 and not clearly integrated into the Section 4.2 discussion. The relationship between total rainfall volume and damage ranking across the 50 SST realizations should be examined more explicitly.

We thank the reviewer for this important comment. We agree that the original discussion did not clearly integrate the rainfall-volume explanation with the damage comparison in Section 4.2. We revised Section 4.2 to explicitly state that the baseline Atlas-14 scenario represents a relatively high-volume areal rainfall input compared with the mean SST realization, and that this partly explains why Atlas-14 damage is comparable to the SST 90th-percentile composite.

The revised text now states:

“The total structure damage produced by Atlas-14 is approximately \$0.477B, slightly exceeding that of the SST 90th-percentile composite. This similarity is partly explained by rainfall volume: the TP40-adjusted Atlas-14 areal rainfall depth is 310 mm, whereas the mean basin-average 24 h accumulation across the 50 SST realizations is approximately 238 mm.”

We also added an explicit analysis of the relationship between rainfall volume and damage ranking across the 50 SST realizations. Specifically, we compared basin-average 24 h rainfall accumulation, rainfall-depth rank, damage rank, and total structure loss across the SST ensemble. The revised Results now state:

“However, the SST ensemble also shows that rainfall volume alone does not determine damage. Across the 50 SST realizations, basin-average rainfall-depth rank does not closely match total structure-damage rank (Fig. S1), indicating that storm placement relative to developed flood-prone areas also affects losses.”

We further added a supplementary figure comparing basin-average rainfall accumulation and total structure loss, with the baseline Atlas-14 scenario shown for reference. This figure shows that although Atlas-14 has a larger basin-average rainfall depth than the SST

realizations, the relationship between basin-average rainfall accumulation and total structure loss within the SST ensemble is weak. The revised Section 4.2 now states:

“In this sense, the uniform Atlas-14 storm represents a widespread high-volume design event, as shown by its larger basin-average rainfall depth relative to the individual SST realizations (Fig. S2). However, the weak relationship between basin-average rainfall accumulation and total structure loss across the SST realizations indicates that spatial storm placement also contributes to the loss divergence.”

9. Section 4.3: The median inundation depths for the exposure gap structures (0.08 to 0.13 m) are described as "significant," resulting in "a significant amount of potential losses." This language is imprecise. At those shallow depths, depth-damage functions typically produce modest damage fractions. The authors should clarify what fraction of the \$110M is attributable to shallow inundation versus the subset of structures experiencing deeper flooding, or at minimum avoid characterizing these depths as significant without qualification.

We thank the reviewer for this comment. We agree that the original wording was imprecise and overstated the importance of the median inundation depths. We revised Section 4.3 to avoid describing the 0.08–0.13 m median depths as “significant” without qualification.

The revised text now states:

“Median inundation depths for the SST-exclusive structures are shallow, ranging from 0.08 to 0.13 m across the frequency groups. Although these median depths correspond to modest damage fractions for individual structures, the aggregate loss reaches approximately \$110M because the group contains many buildings and includes a subset of structures experiencing deeper inundation. Thus, the SST-exclusive exposure gap is important primarily as an aggregate building-level loss signal, not because the median inundation depth for each frequency group is large.”

10. Section 4.5: The HAND analysis is the most diagnostic element of the paper and is well-executed. However, the bandwidth h used in the KDE analysis (Equation 5) is never reported. A single bandwidth was stated to have been applied across all maps, the value should be stated and its selection justified.

We thank the reviewer for this helpful comment. We revised the KDE methods description to report the bandwidth value and justify its use. The revised manuscript now states that the KDE analysis used a Gaussian kernel with a fixed bandwidth factor of $h=0.2$, applied consistently across all evaluated flood maps so that differences in KDE intensity reflected differences in inundated-building patterns rather than differences in smoothing assumptions.

We also added that sensitivity tests using comparable bandwidth factors produced sharper or smoother density surfaces but did not change the locations of the dominant inundation clusters or the interpretation of the Atlas-14–SSTmax differences.

The revised Methods now state:

“A Gaussian KDE was applied using a fixed bandwidth factor of $h=0.2$, which was used consistently for all evaluated flood maps so that differences in KDE intensity reflected differences in inundated-building patterns rather than differences in smoothing assumptions. Sensitivity tests using comparable bandwidth factors produced sharper or smoother local density surfaces but did not change the locations of the dominant inundation clusters or the interpretation of the Atlas-14–SST differences.”

11. Section 5: The discussion largely restates the results rather than interpreting them in the context of engineering practice. A more focused discussion of what these findings imply for FEMA floodplain mapping, NOAA Atlas 14 applications, or design standards in low-gradient coastal watersheds would strengthen the paper's practical contribution.

We thank the reviewer for this helpful comment. We revised Section 5 to move beyond restating the results and to discuss the practical implications for engineering flood-risk assessment. The revised Discussion now clarifies that the findings do not imply that Atlas-14 or FEMA floodplain products should be replaced. Rather, they suggest that deterministic design-storm simulations and regulatory floodplain products may need to be complemented by spatially variable rainfall scenarios where localized pluvial flooding and building-level consequences are important.

The revised Discussion now states:

“The practical implication is that spatially uniform design storms can provide useful basin-wide design information, but they may not identify localized pluvial exposure in developed areas where storm placement interacts with local drainage and terrain-transition zones.”

We also added:

“In the context of the present study, this suggests that FEMA floodplain maps and design-storm-based hazard products remain important regulatory and planning tools, but they may not fully capture rainfall-driven pluvial exposure outside the main river corridor.”

Finally, we revised the Discussion and Conclusions to emphasize the practical use of SST-based or other spatially variable rainfall analyses as a complement to deterministic design-storm simulations for identifying exposure hotspots, upper-envelope damage concentrations, and locations where mitigation or drainage improvements may be sensitive to rainfall placement.

12. Section 5, line 505: There is a typographic error, "(SST and Atlas-14-based uniform design storms)" is missing an opening parenthesis and reads awkwardly.

Corrected. We revised the sentence to fix the missing parenthesis and improve readability.

13. The conclusions are presented as a numbered list. While the findings are clearly stated, points 1 and 2 are closely related and could be combined. The practical

recommendation, what practitioners should do with these findings, is absent from the conclusions and should be added.

We thank the reviewer for this helpful suggestion. We revised the Conclusions to combine the previously separate exposure and damage findings into one conclusion and added a practical recommendation as the final conclusion.

The revised Conclusions now include three focused points. The first combines the exposure and structure-damage findings, including the 11,518 SSTmax-only structures, approximately \$110M in additional structure damage, and the difference between baseline Atlas-14 and SSTmax total structure damage (\$0.477B vs. \$0.923B). The second summarizes the spatial diagnostic findings from the KDE and HAND analyses, emphasizing that the largest differences were concentrated near Lafayette–Scott and Abbeville and in intermediate HAND zones. The third provides the requested practical recommendation, stating that practitioners should use spatially variable rainfall scenarios as a complement to deterministic design-storm simulations when evaluating building-level flood consequences.