Response to reviews on manuscript egusphere-2025-4099

We appreciate the insightful comments from the editor and reviewers that have remarkably improved the quality of our manuscript. Please find below our point-to-point responses (in blue) to reviewers' comments (**RC**s). Line numbers refer to the marked-up version of the manuscript.

Response to the reviewers

Reviewer 1

The authors present a process-based framework that integrates temperature-conditioned rainfall projection, stochastic storm transposition, and high-resolution hydrodynamic flood modelling to assess future pluvial flood hazards. The topic is relevant to the cope of HESS, and the manuscript is well-written, concise, and straightforward to understand. The work is methodologically innovative and supported by an impressive modelling effort. However, the following points need to be addressed before publication. My suggestion is for a revision of minor to moderate extent.

Reply: We appreciate your recognition of our work, and thank you for the specific comments that we believe helped improve the quality of the manuscript.

RC 1.1 The framework explicitly assumes that changes in short-duration extreme rainfall are primarily thermodynamic, with limited influence from dynamical changes (Section 2). While this assumption is discussed later, it underpins the entire DSQM–SST-AUTOSHED cascade. Can the authors more rigorously justify this assumption for the Beijing region, particularly given evidence of circulation-driven changes in East Asian summer rainfall? A sensitivity or discussion contrasting thermodynamic vs. dynamic contributions would strengthen the credibility of the projections.

Reply: We agree with the reviewer that this assumption requires more explicit discussion. In the revised manuscript, we introduced the following changes (1) Section 2 (**lines 84-88**):

> (i) it primarily represents the thermodynamic processes, i.e., the response of short-duration extreme rainfall to increasing temperatures (Trenberth et al., 2003). The dynamic factors resulting from changes in atmospheric circulation are not explicitly taken into account.

and (2) in the section describing the case study (**lines 185-193**):

As one of the world's largest megacities, Beijing is particularly susceptible to pluvial flooding from extreme storms; this vulnerability is underscored by observed increases in sub-daily precipitation extremes across North China, where Wang et al. (2024) reported positive and accelerated trends in six extreme sub-daily precipitation indexes during 1971–2022, especially after 2001. Studies have shown that thermodynamical factors, such as high temperatures and the urban heat island effect, have played a dominant role in the intensification of local extreme rainfall events (Yang et al., 2025; Zheng et al., 2021). At the same time, dynamic factors, such as monsoon circulation and topography influences, may also affect summer extreme rainfall in the Beijing region (Cui et al., 2025; Zhong et al., 2014). However, the dynamic contributions are not explicitly accounted for in the present study.

RC 1.2 In this study, 1-km CMORPH-based rainfall is employed to drive street-scale pluvial flood simulations. With emerging radar datasets at 500 m or 100 m that more finely capture convective structures, please clarify how the proposed framework would be extended to incorporate these higher-resolution inputs for future flood projection.

Reply: As we mentioned in the text **(line 265)**, our framework is flexible with respect to rainfall resolution and can be readily applied to higher-resolution radar rainfall products when available. Meaning, the same procedure that is demonstrated in the paper using 1 km rainfall as input can be directly applied to finer resolution rainfall; only the input data to the framework needs to be replaced. We slightly revised the sentence in **lines 378-382** to make this point clearer:

However, the proposed framework is flexible with respect to the spatial and temporal resolution of the rainfall fields. Therefore, if rainfall inputs with finer spatial and temporal resolution are available, they can be used directly within the framework, allowing the analysis to be performed at finer rainfall scales without requiring further methodological modifications.

RC 1.3 In the absence of a detailed sewer network, a uniform drainage rate is subtracted over road surfaces. How realistic is this assumption under extreme rainfall events at the 100-year return level? The authors should clarify whether drainage saturation or failure could alter the relative changes in inundation depth and duration under warming.

Reply: We agree that this assumption warrants further discussion, as indeed, the implementation of the actual drainage network could affect the simulations. We have added the following text in the revised manuscript **(lines 254-260)**:

This simplified drainage representation does not resolve the hydraulic behavior of the sewer network. In particular, it cannot represent spatially variable inlet capacity, sewer surcharge, backwater effects, or reverse flow from pressurized manholes and inlets back onto the surface. As a result, it may underestimate local inundation in areas where the drainage system becomes capacity-limited or fails during extreme rainfall. Nevertheless, because the same simplified drainage treatment is applied consistently across all warming levels, the simulations remain useful for comparing relative changes in surface flood response, while absolute local depths should be interpreted with caution.

RC 1.4 The results indicate that storms become more intense but spatially smaller, yet flood depth and velocity still increase. It deserves deeper discussion why reduced storm areas result in more severe pluvial flood hazards.

Reply: The simple answer here is that the magnitude of the storm increases at a higher rate than the reduction in its storm size (Fig. 11), resulting in larger volumes of rainfall per extreme storm with increasing warming that drives more severe floods. We agree that this aspect deserves deeper discussion, and we added the following text in Section 4.1 (lines **349-355**):

> These results indicate that future storms in the BMC are likely to become more intense yet more spatially concentrated under warming. For urban environments, this can still trigger more severe surface inundation when intense rainfall is concentrated over impervious and hydraulically connected parts of the city, such as major roads, underpasses, and low-lying residential areas. Under these conditions, runoff is generated and routed more rapidly, leading to higher inundation depths (Fig. 7) and stronger flow velocities (Fig. 9). By contrast, the reduced storm footprint may partly limit the duration or spatial spread of flooding, which is consistent with the weaker and less monotonic changes found here for inundation duration (Fig. 8).

RC 1.5 The framework is positioned as broadly applicable, yet it is tailored to convective storms and pluvial flooding. Please more clearly delimit the domain of applicability. In particular, under what climatic and surface conditions would the current DSQM–SST–AUTOSHED cascade not be expected to perform well, and how might this influence use by practitioners?

Reply: That is a valid and important question. Although this study focuses on pluvial flooding, the proposed framework is not inherently limited to pluvial applications and could, in principle, be extended to fluvial flooding as well. The rainfall-temperature scaling was applied to the hourly scale and may be extended to longer durations up to daily scales; with coarser temporal scales than daily, the physical Clausius-Clapeyron mechanism might not hold (Marra et al., 2024). Nevertheless, even if the rainfall scaling is done at the hourly scale, the SST can be applied to longer durations (e.g., Zou et al. 2025). The challenge with simulating changes to fluvial flooding is not with the rainfall modeling per scale, but with the fact that our framework generates individual extreme storms, for which fluvial flood assessment typically requires careful specification of antecedent catchment conditions (e.g., soil moisture), which strongly influence runoff generation. Dealing with the antecedent catchment conditions is beyond the scope of our work. We have added the following text in the discussion section (lines **420-434**):

Although the proposed framework is developed for short-duration convective rainfall extremes and urban pluvial flooding, its rainfall-generation component could be used more broadly. The rainfall–temperature scaling is most appropriate at sub-daily durations (Marra et al., 2024), whereas the SST module can also generate rainfall extremes at longer durations (Zou et al., 2025). This opens the possibility of using the generated rainfall fields as input to catchment-scale flood models. For fluvial applications, however, the rainfall module would need to be coupled with a hydrological–hydrodynamic model that properly represents soil water storage, groundwater dynamics, and their interactions with channel routing. Such processes are particularly important in large semi-humid catchments like Beijing, where complex runoff generation mechanisms strongly condition flood response (Cui and Tian, 2025; Cui et al., 2024). In addition, the present framework modifies rainfall spatial structure but does not explicitly account for warming-induced changes in storm temporal structure, which may also affect flood regimes (Hettiarachchi et al., 2018; Nathan, 2025).

RC 1.6 The terms 'regional warming levels' and 'warming scenarios' are used interchangeably. Please choose one primary term, such as 'regional warming levels', and define it clearly once (relative to 1998-2019 baseline). Use it consistently in figures, captions, and text.

Reply: We agree and revised the manuscript accordingly by aligning all terms to 'regional warming levels'.

Reviewer 2

The paper provides an interesting case study of the impacts of climate change on flooding behaviour in a large urban area (300km^2, Beijing), where the key highlight of the paper is the explicit consideration of the changes in spatial characteristics of the rainfall fields under climate change. This is an important topic: the incorporation of morphed rainfall fields derived from SST rainfall fields is a clever approach to a thorny problem, and the case study nicely demonstrates how such information can be used to assess the impacts of climate change on floods over a large urban area. The paper is also well written.

Overall, I think the paper makes a good contribution and would be of interest to readers of this journal. However there are three key aspects that should be given additional consideration by the authors.

I provide some justification for these points below (and given I am citing some of my own co-authored papers in support of these statements it is appropriate that I am identified).

Fadhel, S., Rico-Ramirez, M., Han, D. (2018): Sensitivity of peak flow to the change of rainfall temporal pattern due to warmer climate, J Hydrol Volume 560, 2018, Pages 546-559

Fraehr, N., Wang, Q. J., Wu, W., & Nathan, R. (2024). Assessment of surrogate models for flood inundation: The physics-guided LSG model vs. state-of-the-art machine learning models. Water Research, 252, 121202.

Hettiarachchi, S., Wasko, C., and Sharma, A.: Increase in flood risk resulting from climate change in a developed urban watershed – the role of storm temporal patterns, HESS, 22, 2041–2056.

Nathan, R. (2025). Improving event-based methods for modelling flood risk in a variable and non-stationary climate. Phil Trans Royal Society A, 383: 20240292.

Visser, J. B., Wasko, C., Sharma, A., & Nathan, R. (2023). Changing Storm Temporal Patterns with Increasing Temperatures across Australia. J Clim 36(18), 6247–6259.

Wasko, C., & Sharma, A. (2015). Steeper temporal distribution of rain intensity at higher temperatures within Australian storms. Nature Geo, June, 8–11.

Reply: We thank Prof. Rory Nathan for the careful and thoughtful review of our manuscript, and for the positive assessment of the relevance, originality, and clarity of the work. We particularly appreciate the reviewer's detailed and constructive comments, which help to better position the contribution, clarify its scope, and strengthen the discussion of methodological limitations. Below, we respond to the main points raised.

RC 2.1 There is a need to further qualify the scope and utility of the proposed "framework" (i.e. in places there is a tendency for over-statement),

The title of the paper and high-level messaging presents the work as a "computationally efficient process-informed framework", where the key results are presented as the impacts of climate change on floods of a specific return period. I don't agree that the approach can be referred to as being computationally efficient, at least not for practical purposes, and that references to the approach as being "a proposed framework" is an overstatement, at least as regards the estimation of floods of a given return period. The assessment of the impacts of climate change are restricted to assessing changes in average areal rainfall intensity and the spatial extent of storms, and does not include the impacts of temporal patterns or antecedent soil moisture. Stating these limitations is not intended to be criticisms of the paper – it is entirely appropriate that the authors set the bounds of their research as suits their purposes – but rather these aspects point to the need for the key contributions of the paper to be more clearly stated and qualified.

In my view the strength of the paper is as a case study which demonstrates the impacts of climate change on large floods in a large urban area, where specific attention is given to characterising the impact of higher temperatures on the average areal intensity and spatial extent of rainfall events. The stochastic characterisation of the rainfall fields under climate change (steps I and II in Figure 1) provide a valuable framework for examining the impacts of climate change on the intensities and extents of storms rainfalls of a given return period (as already published in an earlier Water Resour Res paper by Zou et al, 2025), but I don't think the deterministic transformation of these rainfalls into floods using a rain-on-grid hydrodynamic (RoGH) model in the manner demonstrated (Step III in Figure 1) is sufficient for this contribution to be regarded as a "computationally efficient process-informed framework" for assessing flood impacts.

Reply: Reading the reviewer's comments, we realized that some aspects were probably miscommunicated in the text.

First, our framework is not designed to simulate a flood for a given return period (as the reviewer noted). Rather, we simulate the flood response for a given rainfall return period. We believe that the reviewer meant the same, but bring it up for clarity.

Regarding the "computation efficiently" – our approach can offer an efficient solution for urban pluvial flood simulations by leveraging multi-GPU computing clusters, as we further explain in the RC 2.3.

Taking these issues into account, we have properly clarified the computation efficiency of our process-informed framework in the revised manuscript (**lines 274-280**).

After the model validation, we configured AUTOSHED to run the 24-hour storms generated by the SST, using a server with an Nvidia Tesla V100 GPU (32GB) and Intel(R) Xeon(R) Gold 6226R CPU @ 2.90 GHz, for which the results are reported below. Using multiple GPUs, AUTOSHED finishes one 24-hour distributed-storm-driven flood simulation within one hour. This enables high-resolution ensemble simulations that would be prohibitively expensive on CPU-only implementations, although the overall computational burden remains considerable when large rainfall ensembles are propagated through the full workflow.

Considering the boundaries of our approach, we agree with the reviewer that we do not address issues such as soil antecedent moisture conditions and the rainfall temporal patterns in our analysis, which can be meaningful. We have added the following text in the discussion section (**lines 420-434**):

Although the proposed framework is developed for short-duration convective rainfall extremes and urban pluvial flooding, its rainfall-generation component could be used more broadly. The rainfall–temperature scaling is most appropriate at sub-daily durations (Marra et al., 2024), whereas the SST module can also generate rainfall extremes at longer durations (Zou et al., 2025). This opens the possibility of using the generated rainfall fields as input to catchment-scale flood models. For fluvial applications, however, the rainfall module would need to be coupled with a hydrological–hydrodynamic model that properly represents soil water storage, groundwater dynamics, and their interactions with channel routing. Such processes are particularly important in large semi-humid catchments like Beijing, where complex runoff generation mechanisms strongly condition flood response (Cui and Tian, 2025; Cui et al., 2024). In addition, the present framework modifies rainfall spatial structure but does not explicitly account for warming-induced changes in storm temporal structure, which may also affect flood regimes (Hettiarachchi et al., 2018; Nathan, 2025).

RC 2.2 The methodological limitations for estimating floods of a given return period needs qualification and further discussion.

There are two key limitations to the adopted approach which need to be addressed in some fashion, at least in the discussion: a) the weakness of the validation of the RoGH modelling component, and b) the restrictive assumptions used to estimate floods of a specified return period.

In terms of scientific defensibility of the results, the weakest aspect of the paper – whether regarded as a case study or a methodological framework - is the lack of appropriate validation of the RoGH model. The evidence for validation of the SST rainfall model is provided in Table S3 in supplementary material (this gives good confidence in the SST methodology), but no equivalent standard of evidence is provided for the RoGH model. There are two elements to this, namely parameterisation of the routing scheme, and the manner in which flood return periods are estimated. With respect to parameterisation of the routing scheme, the paper compares simulated inundation levels against maximum levels recorded at six underpasses for a single historic event. While these comparisons against maximum levels for a single historical event are very good, particularly for what is assumed to be the largest areas (points 5 & 6), the slight problem here is that the routing parameters were verified using rainfall with a spatio-temporal resolution of 500m and 5min, which is substantially finer that what is used for the SST simulations. Given that model results are dependent on the resolution of both the rainfall inputs and the routing scheme, it would have been better to demonstrate model performance using historical information aggregated to match what is used to derive the key results of interest. Thus, could the authors provide comment on, or evidence for, the dependence of these calibration results on the finer resolution adopted.

Of more importance is that no attempt has been made to demonstrate that the adopted modelling scheme is able to reproduce flood frequency quantiles derived from gauged streamflows (i.e. that the scheme is suited to the estimation of floods with known return periods). We know from Table S3 that the rainfall inputs are suitable for this, but it is a big leap of faith to assume that this guarantees that the adopted scheme can reproduce flood quantiles. The key problematic – and implicit – assumption being made here is that a Y-yr rainfall input to the model will yield a Y-yr flood peak. The limitations of this assumption have been recognised for some decades, and for a recent detailed discussion of this in the context of event-based modelling under climate change see Nathan (2025). The rigorous solution to this problem is to ensure that the SST inputs sample an appropriately wide range of rainfall return periods and to take explicit account of the joint probabilities involved, where the most appropriate validation test is to compare the derived flood quantiles with those based on observed flood data (which is the same comparison as undertaken in Table S3, but for flood maxima). To this end, is there any gauged flood information available in the study area that could be used to compare flood quantiles? (Such data could also be used to assess the simulation of additional historic events). It would be surprising if not, but this is a point that needs to be discussed. If no information is available to do this, then perhaps the easiest solution within the scope of this paper is to make it clear that the flood results are results are based on an ensemble of 100-yr rainfall intensities, they do not necessarily represent 100-yr floods. That is, it should be discussed that the adopted modelling approach is not well suited to estimating floods of a given return period, but rather the results are assumed to be indicative of the expected changes in floods of a notional return period. In terms of assessing defensibility, the nature and availability of gauged streamflow information across the study area should thus be discussed to provide context for the apparent paucity of relevant information. Also, while I appreciate that the code for AUTOSHED is publicly accessible, there does not appear to be any prior published paper or source that full describes the model or evaluates its application to a real world event – the provenance of this model is cited as Li et al (2023b) but as far as I can tell this is merely a conference abstract?

Reply: For the validation of the RoG model, observational data from real flood events are very limited in our study area. Given this constraint, our validation strategy—based on monitored inundation depths at six underpasses—represents the most feasible and informative option available, and it is consistent with common practice for validating urban flood responses in other studies. In the revised manuscript, we have made this point more explicit and also added a discussion on how the spatio-temporal resolution of rainfall inputs may influence the SST-based flood simulations **(lines 258-270)**:

> Based on the estimated parameters, we validate AUTOSHED using the 500 m and 5 min QPE product (Figure S1 in the Supplementary Material) and extract the simulated inundation hydrograph at six underpasses available with maximum inundation depth records (Fig. 5). This event-based evaluation is intended to assess whether AUTOSHED can reasonably reproduce observed pluvial flooding in the study area and, therefore, can serve as a reliable flood simulator of the proposed workflow. According to the comparison results, AUTOSHED achieves simulated absolute depth differences smaller than 5 cm at most sites, except for one underpass, for which an underestimation of 41.4 cm is observed. But this can probably be attributed to the overestimated sewer drainage in the corresponding road zones. Thus, the present validation demonstrates the credible skill of AUTOSHED in translating rainfall fields to local inundation responses.
> We acknowledge, however, that additional uncertainty may arise when the validated AUTOSHED model is forced with the coarser SST-DSQM-based rainfall inputs used for other applications, such as flood-frequency analysis. Aggregating rainfall from 500 m and 5 min to 1 km and hourly resolution can smooth local rainfall peaks and modify short-term rainfall intermittency, potentially affecting the simulated inundation depth, flow velocity, and inundation duration. Nevertheless, the proposed framework is resolution-flexible and can be directly applied with finer rainfall products when such data become available.

Regarding the reviewer comment on the documentation of the AUTOSHED model, we agree that at the time we submitted the manuscript for publication, a non-comprehensive paper describing the model was referenced. We now include an additional reference, demonstrating AUTOSHED to real flood events (lines **162-164)**:

> AUTOSHED has been applied to various real flood events (Li et al., 2025, 2026) and thus has competent capacities in transforming warming-conditioned rainfall forcings to surface inundation fields.

For the estimation of the flood of a specified return period, we believe that there is a misunderstanding here. We are definitely not claiming that "a Y-year rainfall leads to a Y-year flood" in the submitted manuscript. And we will revise the text to ensure this is stated unambiguously (e.g., a 100-year rainfall return level does not imply a 100-year flood). In fact, we only simulate the urban pluvial flood response to a "Y-year rainfall" in our approach, quantifying its inundation and risk implications. For example, Figures 7–10 present "flood hazard under the 100-year rainfall return level," as indicated in the figure titles. In the revised manuscript, we have clarified that these outputs should be interpreted as flood responses to ensembles of rainfall realizations corresponding to a specified rainfall return level (lines 289-290):

> The projected changes in urban pluvial floods can be estimated from the ensemble of AUTOSHED simulations driven by multiple storm realizations associated with specified rainfall return levels and regional warming levels.

Finally, regarding the question of observed flood data, we note that we do not have access to long-series fluvial observations for Beijing city. This is not surprising, as this type of data is generally not available in cities.

RC 2.3 The impracticality of the computational burden needs to be more thoughtfully discussed.

My reasons for stating that the scheme should not be considered as being computationally efficient is that it is stated that the model takes 1 hour to simulate a single 24-hour storm. Thus, with the adopted approach it thus presumably took 50 hours of computational time to derive results for an ensemble of 50 sets of rainfall realizations to derive an approximate estimate of a flood of a given return period (line 268 – I think that's right?). However, to correctly estimate the return period of a 100-yr flood using this scheme it would be necessary to use all 500 rainfall SST realisations which would take 500 hours (21 days). These additional simulations are required as it is expected that rainfalls with average areal intensities ranging from around 10-yr to 500-yr will contribute to a 100-yr flood due to the influence of different spatio-temporal patterns (the authors could easily check this statement by inspecting a simple scatter plot of average rainfall intensity against flood peak for a given location – Fig 6a in the manuscript shows that rainfalls of different spatial attributes with average intensities varying between 30-yr and 200-yr contribute to a the expected probability of a 50-yr rainfall quantile (200mm), and this range will increase markedly when propagated through to flood quantiles). Again, I stress that I am not being critical of what the authors have done – I appreciate the effort and quality of this work – I am making these points so that the authors don't undermine the value of their contribution by making claims that extend beyond the boundaries of their research question. From my perspective the authors have demonstrated just how computationally impractical it is to rely solely on the use of RoG hydrodynamic models – the run times are just too great to solve these problems in a practical fashion with scientific defensibility – and the only useful solutions that I am aware of is to use these models in combination with some form of surrogate model to derive a result (there has been a sharp increase in the number of paper on this topic in recent years, see for example the examples and literature cited in Fraehr et al, 2024), or use low fidelity hydrodynamic models. It would thus be useful if the authors could comment further on the implications and limitations of the computational burden of their RoGH scheme for use in the SST stochastic framework, particularly if used to estimate floods with a given return period.

Following on from this point, one aspect of the paper that I think warrants more nuanced discussion is the exploration of the influence of spatial resolution presented in Section 4.1. This kind of investigation is a valuable addition to the paper as it provides the means to assess the trade-off between computational burden and model accuracy. As present the authors conclude that it is necessary to retain the original high-resolution results. This may be the case, but it could be argued that using a coarser model resolution yields a more accurate estimate of the 100-yr flood because the lower computational overhead makes it is feasible to properly resolve the joint probabilities involved in the making statistical inferences using the ensemble of spatio-temporal patterns over the required range of average rainfall intensities. That is, when a coarse model is deployed in a more rigorous stochastic joint-probability framework the resulting estimate of the 100-yr flood based on the comprehensive analysis of a wide range of rainfall quantiles may be more accurate than the one obtained using a fine resolution model based on only 50 simulations forced by a single rainfall quantile. Thus, it would be helpful if the authors could offer any insights about the relative magnitude of variability represented by the ensemble of spatio-temporal SST rainfall fields compared to the additional epistemic uncertainty associated with the use of a coarser hydrodynamic model. Some indication of this could be obtained (without undertaking additional simulations) by comparing the range of inundation levels associated with the ensemble of SST rainfall fields with the difference in levels associated with different model resolutions.

Lastly, I note that the morphing of rainfall fields used in SST considers the influence of thermodynamic components on areally averaged rainfall intensities and spatial extents. This approach has a lot of merit and is a valuable contribution to the problem, though as far as I can tell the approach, while providing useful evidence for changes in spatial extents of storms (a topic which has received little attention in the past), it doesn't consider changes in the temporal distribution of rainfalls. I can see how this aspect could be incorporated into the morphing scheme, but this aspect does not yet appear to have been explicitly characterised. Temporal distributions have a major influence on flood response (see Hettiarachchi et al, 2018) and there is evidence (Fadhel et al 2018; Wasko and Sharma, 2015; Visser et al, 2023) that temporal patterns are becoming more front-loaded with higher temperatures. The authors could perhaps comment on this additional limitation in Section 4.3.

Reply: For the computational burden of the RoG model, we acknowledge that the RoG model may require more computational resources due to its high spatio-temporal resolution, which is necessary for street-level assessment of pluvial flood hazards. However, the "1 hour running time" mentioned by the reviewer is already considerably faster than many other CPU-based hydrodynamic RoG models (e.g., HEC-RAS). It is important to note that, in contrast with the reviewer assessment, multiple rainfall realizations can be executed in parallel on multi-GPU computing clusters, which can reduce the overall simulation time considerably compared with a purely serial implementation. This means that running an ensemble for a given return period would not result in 21 days of simulation time but likely the simulation can be completed within a day. While the reviewer suggests using a coarser-resolution hydrodynamic model to explore the trade-off between resolution and efficiency, it is important to note that coarser resolutions, e.g., 30 m to 100 m, are generally insufficient for robust urban pluvial flood hazard assessment (Ming et al., 2025; Xing et al., 2022). As a result, improving computational efficiency without sacrificing the spatial detail required for urban applications remains an active research focus in pluvial flood modelling (Cea et al., 2025). However, we appreciate the reviewer's constructive suggestion to combine a coarse model with super-resolution (SR) methods to enable efficient surrogate prediction of high-resolution inundation fields. In the revised manuscript, we have clarified the computational-cost considerations and discussed potential avenues for accelerating simulations, including SR-based approaches (**lines 435-441)**:

> Furthermore, a substantially larger number of hydroclimatic scenarios would be needed for a more comprehensive stochastic flood-frequency analysis, and GPU parallelization of numerical simulations may not be sufficient to support such computational demands. A promising avenue for future work is therefore to integrate the present workflow with emerging physics-informed surrogate models, such as super-resolution models that use deep neural networks to transform computationally efficient coarser-resolution inundation fields into practically useful fine-scale flood maps (Fraehr et al., 2023; He et al., 2023; Herath et al., 2025). Such hybrid approaches may offer a practical way to expand the ensemble size while preserving the spatial detail needed for urban flood-hazard mapping.

Regarding changes in the temporal distribution of rainfall due to climate change, we agree that this is a very relevant point when assessing future floods, and indeed, we assume no changes to the temporal structure of the storm in our approach. We discussed this in our reply above (see RC 2.1) and revised the manuscript to explicitly mention this limitation.

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