High-resolution mapping of pluvial flooding in ungauged agricultural catchments

Response to Reviewer 1

We sincerely thank Reviewer 1 for the careful review of our manuscript and for the insightful and constructive comments provided. These comments have helped us clarify the empirical assumptions underlying our methodology. Our detailed responses are provided below. Reviewer comments are presented in plain text, and our responses are shown in blue.

The reviewed manuscript addresses the evaluation of a high-resolution, spatially distributed hydrological modelling framework designed for mapping pluvial flood hazards in ungauged agricultural catchments. The main objective of the study was to assess the capabilities and limitations of conceptual modelling under conditions of limited calibration data, with particular attention given to the separation of runoff generation and flow routing processes. The Authors compared adjusted SCS-CN formulations, including the Jain method, with two routing algorithms, using data from 39 rainfall-runoff events observed in two nested experimental agricultural catchments.

The topic of the manuscript fits well within the scope of Hydrology and Earth System Sciences, as it addresses important issues related to rainfall-runoff modelling, flood hazard assessment, ungauged catchments, and the practical application of distributed hydrological approaches.

It should be noted, however, that the manuscript presents a modelling approach that is already well known in the literature. Similarly, accounting for the spatial variability of flow paths is a commonly used concept and largely follows ideas presented in the frequently cited study by Grimaldi et al. (2012). Therefore, the manuscript does not show particularly strong methodological novelty.

Nevertheless, the study has considerable practical relevance, as it provides a useful approach to reducing errors in surface runoff simulation in ungauged catchments. The results indicate that the Jain method reduces volumetric bias, and that the distributed approach performs better than lumped modelling. However, model performance remains strongly dependent on rainfall intensity. An important conclusion of the study is also the indication of the limitations of static CN parameterization in representing rapid Hortonian runoff processes, as well as the usefulness of the proposed framework for identifying hydraulic hotspots in ungauged catchments.

Overall, the methods are well described, the results are clearly presented, and the discussion is generally sound and valuable from a practical point of view. I have several comments and suggestions that may help improve the manuscript:

1. Please explain each abbreviation when it is used for the first time in the manuscript.

Thank you. We will amend this.

2. Line 200: Is sheet flow always assumed to occur, for example, in flat forested catchments? In practice, this process is rather rarely observed and usually occurs after heavy rainfall events. Please clarify this assumption.

We thank the reviewer for raising this important physical consideration. We agree that in environments such as flat forested catchments, high infiltration capacities and surface roughness (leaf litter, roots) typically prevent the formation of true physical sheet flow, except during exceptionally heavy rainfall events that exceed the infiltration rate (Hortonian overland flow). However, we would like to emphasize two points to clarify this assumption in the context of our study:

First, our methodology is designed for and applied to agricultural catchments. Unlike undisturbed forested areas, agricultural soils frequently experience compaction (e.g., from machinery), crusting, and periods of sparse vegetative cover. In these agricultural settings, the infiltration capacity is significantly lower, and initial diffuse surface runoff (sheet flow) occurs much more frequently and under less extreme rainfall conditions.

Second, within the standard SCS-TR55 modelling framework, the sheet flow regime is an empirical conceptualization. It represents the initial phase of overland flow before micro-topography inevitably forces the water to concentrate into rills or gullies (shallow concentrated / channel flow). The maximum length of this flow has historically been debated, with values typically ranging between 100 and 300 feet (30 to 90 m). This variability is primarily driven by slope and whether the surface is paved or unpaved. As noted by the USDA-NRCS (2010), while sheet flow can extend further on paved surfaces, it generally occurs within a 100-foot (30 m) radius in unpaved areas. Therefore, applying this 30 m length assumption serves as a standardized, conservative empirical boundary condition to estimate the initial overland travel time across the hillslope grid cells.

To address this valid concern, we will clarify in the manuscript that this 30 m value acts as a theoretical upper bound for the model's empirical framework rather than a strict representation of physical reality for every grid cell.

3. Line 214: What happens if the rainfall duration differs from 24 hours and the return period differs from 2 years? Please explain how the proposed approach can be applied under different rainfall duration and return-period scenarios.

We thank the reviewer for highlighting this point. While not always explicitly clear in standard modelling literature, the P24 term (the 2-year, 24-hour rainfall depth) in the NRCS kinematic wave equation does not represent the specific storm event being simulated. As documented in the fundamental derivations of the TR-55 method (USDA-NRCS, 1986; McCuen, 2004), the equation requires a constant effective rainfall intensity to compute sheet flow travel time. The P24 parameter acts as a localized geographical constant to approximate this baseline intensity. Therefore, our approach remains fully applicable to any rainfall scenario. The parameters of the actual modelled storm (specific duration and return period) are applied later in the workflow to calculate the runoff volume (via the CN method) and for routing, while P24 remains a fixed calibrated constant used solely to determine initial flow velocity. To eliminate any ambiguity, we will briefly clarify this theoretical distinction in the methodology section, ensuring the explanation remains concise so as not to overly lengthen the manuscript.

4. Why were only normal antecedent moisture conditions considered? In some cases, especially for low precipitation events, changing the CN value to wet antecedent conditions may generate surface runoff. Why did the Authors not adjust CN according to antecedent precipitation, for example using P5? Such an approach would make the model more realistic.

We thank the reviewer for raising this point. The conceptual logic that antecedent moisture conditions significantly influence runoff generation is well-founded. However, we deliberately chose not to adjust the CN using the traditional 5-day antecedent precipitation (P5) index due to strong statistical and literary evidence showing that P5 is a fundamentally flawed predictor of actual runoff response. As briefly mentioned in the manuscript (Line 470), the updated official guidelines discourage the use of the discrete P5 table to shift between Antecedent Moisture Classes (AMC I, II, and III) (Hawkins et al., 2008; USDA-NRCS, 2010). Recent studies (Soulis and Valiantzas, 2012; Soulis, 2018; Palli-Gravani et al., 2026, recommended by Reviewer 2) illustrate that P5 is often a poor surrogate for the actual watershed conditions controlling runoff. Watershed heterogeneity and complex antecedent states exert a much stronger influence on runoff estimation than what can be captured by simple antecedent-rainfall classifications. While dynamic soil moisture accounting would certainly improve the model, applying the outdated P5 step-function adjustment would not make the model more realistic, but rather mathematically inconsistent with recent hydrological consensus. We will explicitly clarify this aspect and discuss the limitations of the P5 index in the revised manuscript.

5. Did the Authors consider adding a subsurface flow component? This could increase simulated runoff during smaller precipitation events and would make the representation of runoff generation processes in agricultural catchments more realistic.

We thank the reviewer for this suggestion. While we acknowledge that subsurface flow (interflow) can significantly contribute to flood peaks in certain environments (e.g., steep or highly permeable catchments), we deliberately restricted our model to surface routing for three main reasons:

First, the targeted hazard is rapid pluvial flooding. In compacted or crusted agricultural catchments, these events are overwhelmingly dominated by rapid surface runoff (Hortonian or saturation-excess). While a subsurface component would indeed improve the simulation of the hydrograph's tail or smaller events, it would have a marginal impact on the extreme peak inundation extents we aim to map.

Second, the local catchment physics limit baseflow. The studied catchments are dry, ephemeral headwater systems. Infiltrated water generally percolates deeper or flows to exfiltrate further downstream, outside our study boundaries.

Thirdly, modelling frameworks for ungauged catchments demand parsimony. Introducing a subsurface flow routing component necessitates estimating highly sensitive soil parameters (e.g., saturated hydraulic conductivity and soil depth). In an uncalibrated setting, adding this level of complexity inevitably leads to over-parameterization and high predictive uncertainty. Therefore, we maintain that a parsimonious, event-based surface model remains the most appropriate approach for assessing this specific hazard.

In conclusion, although the manuscript does not provide strong methodological novelty, it addresses a relevant and practically important problem. The paper is generally well prepared and fits the scope of HESS. I recommend that the manuscript be considered for publication after the Authors have addressed the comments listed above.

Thank you for your review!