

Final authors response to Referee 1

Hydrological spatial coupling within an abandoned Mediterranean terraced microcatchment

MS No. egusphere-2026-2711

Summary

This manuscript reports on four years (2021–2024) of continuous data collection on precipitation, soil moisture, and outlet discharge from a 4.7-hectare burned terraced microcatchment in Mallorca. The authors examine 186 rainfall events, including both runoff-producing and non-runoff events, to explore how rainfall characteristics and antecedent soil moisture affect runoff generation and how landscape position shapes the relationship between local soil moisture and catchment discharge.

Overall, I found this to be an interesting and worthwhile contribution. The dataset itself is especially valuable. Multi-year, event-based monitoring in abandoned Mediterranean terraced catchments is uncommon, particularly in systems displaying extremely low runoff coefficients and little or no baseflow. Equally important is the deliberate inclusion of non-flow events, which are often excluded from similar studies despite providing essential information on threshold behaviour and catchment response. This is, in my opinion, one of the strongest aspects of the study.

The manuscript fits well within the current connectivity literature, and the Introduction provides a useful overview of key concepts and previous research. Overall, the analyses are clearly presented and the figures are informative.

However, several of the principal conclusions appear to extend beyond what the current analyses can robustly support. Many results rely on descriptive comparisons between event groups, whereas the manuscript often adopts predictive or mechanistic language. There is considerable potential to strengthen the paper through additional analysis and a more cautious interpretation of the findings. The issues outlined below are all addressable, and resolving them would substantially improve the manuscript.

Authors response

We thank the reviewer for the careful and constructive assessment of the manuscript and for recognising the value of the multi-year event-based dataset and the deliberate inclusion of non-flow events. We agree that the submitted manuscript did not always distinguish clearly enough between descriptive associations, predictive relationships, and mechanistic interpretation. The revision therefore combines additional analyses with a more explicit separation between directly observed relationships and process hypotheses, as detailed in the responses below.

General comments

General comment 1. Interpretation of soil moisture metrics

One aspect that deserves further consideration is the interpretation of maximum soil moisture and ΔSM as indicators of runoff occurrence.

In the manuscript (see Abstract, Sections 3.2 and 4.1.2), maximum soil moisture is presented as the strongest indicator of runoff, whereas antecedent soil moisture appears less important. However, both maximum SM and ΔSM are measured during the rainfall event and therefore evolve concurrently with runoff. They are thus not independent predictors, but responses to the same rainfall forcing that produces runoff.

Could the authors discuss this distinction more explicitly? In particular, to what extent do maximum SM and ΔSM provide information beyond rainfall magnitude itself?

Interestingly, one of the most informative findings may instead be that antecedent soil moisture—the only true pre-event state variable—shows relatively weak predictive capability in this catchment. This differs from several earlier studies (e.g., Penna et al., 2011; Llorens et al., 2018) and may deserve greater emphasis.

To better support the conclusions, I encourage the authors to examine whether soil moisture metrics retain explanatory value once rainfall characteristics are considered. For example:

- What is the correlation between total precipitation and ΔSM or maximum SM?*
- Would a multivariate logistic regression, or a comparable classification model, including rainfall amount, rainfall intensity, duration, and antecedent soil moisture identify variables that independently explain runoff occurrence?*
- Does the inclusion of soil moisture significantly improve model performance relative to rainfall variables alone?*

The Introduction refers to developing a “probabilistic understanding” of runoff occurrence, yet the manuscript does not currently include an event-probability model. Such an analysis could provide a much stronger quantitative basis for the conclusions.

Authors response

We thank the reviewer for this important comment. We agree that antecedent SM, maximum SM, and ΔSM should not be interpreted in the same way. Antecedent SM is a pre-event state variable, whereas maximum SM and ΔSM progress during the precipitation event and therefore form part of the soil-water response to the same forcing that may generate storm flow. We have consequently reassessed the analytical treatment so that pre-event state and within-event response are considered separately. The revised manuscript will make this distinction explicit.

We first quantified the dependence of the within-event SM metrics on precipitation forcing. Total precipitation was very strongly correlated with ΔSM at both 10 cm ($\rho = 0.915$) and 50 cm ($\rho = 0.822$), and was also strongly correlated with maximum SM at 10 cm ($\rho = 0.766$) and 50 cm ($\rho = 0.648$). These relationships show that both metrics, particularly ΔSM , contain substantial information already associated with total precipitation. They do not imply that maximum SM or ΔSM contain no additional information about the event response. However,

because both are measured during the event and evolve concurrently with storm-flow generation, any additional predictive discrimination obtained by including them would not represent an independent pre-event control. We therefore do not use them to quantify incremental predictive value beyond rainfall forcing. Maximum SM is instead interpreted as the soil-water state attained during the event, whereas Δ SM represents the magnitude of event-scale wetting.

To address the reviewer's suggestion for an explicit probabilistic analysis, we fitted binomial logistic regression models of storm-flow occurrence using total precipitation, $IP_{\max 15}$, storm duration, and antecedent SM as candidate predictors. Diagnostic assessment indicated a nonlinear relationship between total precipitation and storm-flow probability on its original scale, so total precipitation was natural-log transformed, whereas the remaining predictors were retained untransformed. All predictors were subsequently standardised, and all additive combinations of the four candidate variables were compared using AICc, allowing the complete candidate model set to be evaluated rather than prescribing a single model structure a priori.

For the complete January 2021–December 2024 record, the top-ranked model at both depths contained total precipitation, $IP_{\max 15}$, and antecedent SM. Total precipitation received essentially complete summed Akaike weight (>0.99 at both depths), antecedent SM received substantial support (0.79 at 10 cm and 0.82 at 50 cm), $IP_{\max 15}$ received intermediate support (0.54 and 0.59), and storm duration received lower support (0.38 and 0.35). Four models had Δ AICc ≤ 2 at each depth, so we interpret the model set as indicating relative support among predictor combinations rather than identifying a uniquely optimal model. The corresponding model containing total precipitation and $IP_{\max 15}$ but excluding antecedent SM had Δ AICc values of 3.04 at 10 cm and 3.65 at 50 cm relative to the top-ranked model, indicating that antecedent wetness contributed additional information in the complete record.

The best precipitation-only model for the complete record contained total precipitation alone. Relative to this model, the best model from the complete candidate set had AICc values lower by 2.95 units at 10 cm and 3.51 units at 50 cm. Repeated five-fold cross-validation showed that the corresponding gain in discrimination was modest, with mean AUC increasing from 0.917 to 0.929 at 10 cm and from 0.918 to 0.926 at 50 cm. Thus, although antecedent SM received substantial support in the complete record, total precipitation remained the most consistently supported predictor, and expanding the precipitation-only model to include $IP_{\max 15}$ and antecedent SM produced only a modest improvement in out-of-sample discrimination.

Support for antecedent SM was strongly influenced by the exceptionally wet November 2021 sequence. When all events beginning in November 2021 were excluded, the top-ranked model at both depths contained only total precipitation and $IP_{\max 15}$. The summed Akaike weight for antecedent SM declined to 0.27 at both depths, whereas that of total precipitation remained >0.99 . The best model from the complete candidate set was then identical to the best precipitation-only model, and allowing antecedent SM in the candidate set did not improve cross-validated discrimination.

These analyses lead us to revise the interpretation of the submitted manuscript. Precipitation forcing, particularly total precipitation, emerges as the most robust general predictor of storm-flow occurrence. Antecedent wetness provides additional information mainly under the exceptionally wet conditions represented by November 2021. Maximum SM and Δ SM remain informative for characterising the within-event soil-water response, but they are no longer interpreted as independent predictors or controls of storm-flow occurrence.

Accordingly, the revised Methods will describe the correlation analysis, binomial logistic modelling of storm-flow occurrence, AICc-based comparison of candidate models, cross-validation, and the November 2021 sensitivity analysis, and the corresponding Results will report the model support and predictive performance outcomes summarised above. The Abstract, Results, and Discussion will explicitly distinguish antecedent SM as a pre-event state variable from maximum SM and Δ SM as within-event response metrics, and the submitted wording identifying maximum SM as the “strongest predictor” of storm-flow occurrence will be removed.

General comment 2. Event independence and temporal clustering

The dataset covers four years, but most runoff-producing events occurred within a relatively short period, particularly during autumn and winter 2021. November 2021 alone contributed a large proportion of the total runoff.

This raises an important question concerning statistical independence. To what extent do the reported differences represent general catchment behaviour rather than the unusual hydrological conditions associated with this period?

It would therefore be helpful if the authors could:

- report the number of runoff and non-runoff events for each season directly in Table 2;*
- discuss the implications of the strong temporal clustering;*
- evaluate, if feasible, whether the principal conclusions remain similar when the November 2021 events are excluded from the analysis.*

Such a sensitivity analysis would strengthen confidence that the reported relationships reflect general catchment behaviour rather than one exceptional hydrological period.

In addition, some seasonal groups contain very few runoff events. Although the statistical tests may be suitable in principle, the limitations associated with small sample sizes should be discussed and seasonal significance interpreted cautiously.

Authors response

We thank the reviewer for highlighting the temporal clustering of storm-flow events. Following the audit of the event catalogue, the occurrence dataset comprises 32 storm-flow and 154 non-flow events (186 events in total), while complete discharge records are available for 31 storm-flow events. The seasonal distribution is 17 storm-flow and 64 non-flow events during the wet period, 5 and 23 respectively during drying-down, 1 and 33 during the dry period, and 9 and 34 during wetting-up. These sample sizes are now reported directly in Table 2. We also acknowledge that the small number of storm-flow events in some hydrological periods limits period-specific inference and requires cautious interpretation of seasonal statistical significance.

Table 2: Seasonal and overall event-scale comparison of precipitation and soil-moisture metrics between storm-flow and non-flow events during 2021–2024. Data are presented as mean $\pm$ standard deviation. Soil-moisture metrics at 10 and 50 cm depth represent the mean across all sensors installed at the corresponding depth. Event counts (n) are reported as storm-flow/non-flow events.

Metric	Hydrological period	Storm-flow events	Non-flow events	Test	BH-adjusted p
Storm duration (h)	Study period ($n = 32/154$)	14.5 ± 14.4	6.3 ± 8.5	MWU	<0.001

	Wet ($n = 17/64$)	13.4 ± 9.5	8.2 ± 7.4	MWU	0.013
	Drying-down ($n = 5/23$)	17.4 ± 20.9	4.7 ± 4.6	MWU	0.087
	Dry ($n = 1/33$)	1.0	3.2 ± 2.9	-	-
	Wetting-up ($n = 9/34$)	16.4 ± 19.1	6.9 ± 13.7	MWU	0.089
	Study period	28.8 ± 20.7	6.5 ± 8.5	MWU	<0.001
Total P (mm)	Wet	23.8 ± 20.1	8.1 ± 11.2	MWU	<0.001
	Drying-down	30.1 ± 10.5	3.7 ± 3.1	MWU	0.001
	Dry	49.0	4.6 ± 3.8	-	-
	Wetting-up	35.2 ± 25.4	7.5 ± 7.9	MWU	<0.001
	Study period	28.1 ± 28.2	7.2 ± 9.0	MWU	<0.001
IP _{max} 15 (mm h ⁻¹)	Wet	17.3 ± 18.3	5.8 ± 4.8	MWU	<0.001
	Drying-down	32.6 ± 36.4	3.3 ± 2.4	MWU	0.004
	Dry	119.2	9.4 ± 11.0	-	-
	Wetting-up	35.8 ± 19.8	10.5 ± 13.4	MWU	<0.001
	Study period	23.5 ± 4.3	21.3 ± 4.5	MWU	0.012
Antecedent SM 10 cm (%)	Wet	26.5 ± 1.9	24.4 ± 3.4	MWU	0.009
	Drying-down	18.3 ± 1.7	19.7 ± 2.1	MWU	0.131
	Dry	-	15.7 ± 1.4	-	-
	Wetting-up	20.6 ± 3.8	21.7 ± 4.2	MWU	0.472
	Study period	26.3 ± 5.1	23.9 ± 4.4	MWU	0.010
Antecedent SM 50 cm (%)	Wet	30.1 ± 2.1	27.3 ± 3.1	MWU	<0.001
	Drying-down	21.4 ± 2.8	22.9 ± 2.8	Student t	0.306
	Dry	-	18.5 ± 1.2	-	-
	Wetting-up	22.1 ± 4.4	23.1 ± 3.7	Student t	0.488
	Study period	29.1 ± 2.4	22.9 ± 4.8	MWU	<0.001
Average SM 10 cm (%)	Wet	30.3 ± 2.0	26.0 ± 3.4	MWU	<0.001
	Drying-down	27.1 ± 2.2	20.0 ± 2.1	MWU	0.002
	Dry	-	17.1 ± 2.3	-	-
	Wetting-up	27.9 ± 2.1	23.9 ± 4.1	Student t	0.013
	Study period	31.9 ± 3.2	24.9 ± 4.9	MWU	<0.001
Average SM 50 cm (%)	Wet	33.3 ± 2.4	28.3 ± 3.6	Student t	<0.001
	Drying-down	29.9 ± 3.3	22.9 ± 2.9	Student t	<0.001
	Dry	-	19.6 ± 2.7	-	-
	Wetting-up	30.5 ± 3.3	24.7 ± 4.7	MWU	0.003
	Study period	36.8 ± 3.7	25.1 ± 6.2	MWU	<0.001
Maximum SM 10 cm (%)	Wet	37.3 ± 3.8	28.6 ± 5.1	Student t	<0.001
	Drying-down	36.2 ± 3.2	21.5 ± 3.4	Student t	<0.001
	Dry	-	18.8 ± 4.1	-	-

	Wetting-up	36.1 ± 4.1	26.6 ± 5.4	Student t	<0.001
	Study period	37.9 ± 3.5	26.3 ± 6.1	MWU	<0.001
	Wet	37.8 ± 3.0	29.9 ± 5.0	Welch t	<0.001
Maximum SM 50 cm (%)	Drying-down	38.6 ± 3.2	23.2 ± 3.1	Student t	<0.001
	Dry	-	21.1 ± 5.3	-	-
	Wetting-up	37.6 ± 4.7	26.3 ± 5.8	MWU	<0.001
	Study period	13.0 ± 4.9	3.9 ± 4.5	MWU	<0.001
	Wet	10.8 ± 4.3	4.4 ± 4.1	MWU	<0.001
ΔSM 10 cm (%)	Drying-down	17.4 ± 5.0	1.9 ± 3.0	MWU	0.001
	Dry	-	2.8 ± 4.1	-	-
	Wetting-up	14.9 ± 3.7	5.2 ± 5.8	MWU	<0.001
	Study period	11.5 ± 5.7	2.4 ± 4.4	MWU	<0.001
	Wet	8.0 ± 3.7	2.6 ± 3.9	MWU	<0.001
ΔSM 50 cm (%)	Drying-down	16.2 ± 5.3	0.4 ± 0.8	MWU	0.001
	Dry	-	2.2 ± 4.9	-	-
	Wetting-up	15.7 ± 4.7	3.4 ± 5.7	MWU	<0.001

MWU = Mann–Whitney U test; Student t = Student’s two-sample t -test; Welch t = Welch’s two-sample t -test. No inferential comparison was conducted for the dry period because only one storm-flow event occurred. P -values were adjusted using the Benjamini–Hochberg false-discovery-rate procedure separately for each hydrological period and for the overall analysis, across all metrics. Statistical significance was established at adjusted $p < 0.05$. Full statistical diagnostics are provided in Supplementary Table S5.

The reviewer is also right that the storm-flow events are not distributed evenly through the four-year record. Although individual precipitation events are separated according to the event-definition criteria, the hydrological states represented by those events are strongly clustered in time. November 2021 is particularly influential. It contained 15 precipitation events, of which 10 generated storm flow and 5 did not, and therefore accounts for 10 of the 32 storm-flow events in the complete occurrence catalogue. These successive events occurred during an exceptionally persistent wet period and consequently provide disproportionate representation of one hydrological state within the complete record. This is particularly relevant when evaluating antecedent soil-water conditions, because repeated events during such a sequence can occur under persistently elevated pre-event wetness.

To evaluate whether the principal inference about storm-flow occurrence depended on this exceptional period, we repeated the probabilistic occurrence analysis after excluding all precipitation events beginning in November 2021. As detailed in our response to General Comment 1, the dominant precipitation signal remained. After exclusion, the top-ranked model at both depths contained total precipitation and $IP_{\max 15}$. In contrast, antecedent SM was no longer retained in the top-ranked models and allowing it in the candidate model set did not improve cross-validated discrimination.

This sensitivity analysis changes an important aspect of the interpretation of the complete-record analysis. Considered alone, the complete record would support the conclusion that antecedent SM contributed predictive information beyond precipitation forcing. The November 2021 exclusion shows, however, that this additional support is strongly dependent on the exceptionally wet sequence represented by that month. We therefore no longer interpret antecedent SM as a generally informative predictor across the full range of monitored

conditions. Instead, the results indicate that evidence for an additional predictive contribution of antecedent SM is concentrated in the exceptionally wet November 2021 sequence, whereas precipitation forcing, particularly total precipitation, remains the more robust general predictor of storm-flow occurrence. We also avoid extending this result to all wet conditions or inferring a defined antecedent-SM threshold.

Maximum SM and Δ SM are now treated separately from this predictive interpretation because both develop during the precipitation event. Their storm-flow/non-flow contrasts remain useful for describing the soil-water state attained and the magnitude of event-scale wetting, but they are not used to infer an independent pre-event predictive effect or included in the November-exclusion occurrence models.

The revised manuscript will report the storm-flow and non-flow sample sizes for each hydrological period directly in Table 2, describe the temporal concentration of the event record and its implications for inference, and report the November 2021 exclusion analysis in the Methods and Results. The Discussion will distinguish the precipitation relationships that remain robust after exclusion from the contribution of antecedent wetness, whose support is concentrated in the exceptional wet sequence, and will explicitly temper period-specific interpretation where the number of storm-flow events is small.

General comment 3. Comparison of absolute soil moisture thresholds

I also encourage the authors to reconsider the interpretation of absolute soil moisture thresholds across different monitoring locations.

The manuscript states that factory calibration is suitable for identifying thresholds across sites. Using identical sensors certainly facilitates temporal comparisons at an individual location; however, direct comparison of absolute volumetric water content among sites with contrasting stone content, carbonate content, and fine-earth fractions is more challenging.

Could the authors clarify why they consider absolute values directly comparable under these conditions?

An alternative would be to express thresholds in normalized terms, for example using site-specific percentiles or scaling relative to each sensor's observed range. This would retain the spatial interpretation while reducing uncertainty associated with absolute calibration. If site-specific calibrations are available, including them would considerably strengthen these comparisons.

Authors response

We agree that using identical probes with factory calibration does not justify direct comparison of absolute volumetric soil-water content among monitoring positions with contrasting soil properties. We therefore no longer consider the absolute values directly comparable across positions for the purpose of identifying spatial soil-water thresholds. No site-specific calibrations are available for the individual monitoring locations.

To address this concern, we recalculated the spatial soil-water analyses using a sensor-specific relative-wetness framework. For each SM sensor, we constructed an empirical reference distribution from its available 2021-2024 observations at 15-min resolution. Each observation

was then expressed as its percentile within the corresponding sensor-specific distribution, after which the event-scale metrics used in the spatial analysis were calculated from the percentile-transformed series. Each value therefore describes the position of a sensor within its own observed wetness distribution rather than assuming that the same factory-calibrated volumetric value represents an equivalent hydrological state at different monitoring positions. Δ relative wetness is expressed in percentile points.

We then repeated the spatial event analysis using these relative-wetness metrics. For the three illustrative 10-cm positions highlighted in the main text, the revised analysis relates the maximum relative-wetness percentile and Δ relative wetness reached during each precipitation event to whether that event produced measurable storm flow at the microcatchment outlet.

The principal spatial contrast remains evident without relying on absolute volumetric values. Storm-flow events at the High-Connectivity Low-Gradient Hillslope and High-Connectivity Upslope-Edge Terrace 1 positions were concentrated near the upper end of their respective sensor-specific wetness distributions, whereas storm-flow events at the Disconnected Mid-Terrace position occurred across a broader range of local relative-wetness states. The pattern for Δ relative wetness was less spatially uniform and more strongly seasonal. We therefore interpret maximum relative wetness as the relative soil-water state attained locally during the event and Δ relative wetness as the magnitude of event-scale local wetting, rather than treating either as a position-specific threshold.

The revised manuscript will remove the absolute soil-moisture thresholds of the first submitted manuscript rather than replace them with new formal “relative thresholds”. The Methods will describe the sensor-specific percentile transformation used for the spatial analyses, and the corresponding revised Figs. 6-7, shown in our response to Specific Comment 8, replace the submitted absolute-SM versions, and the Results and Discussion will report and interpret the spatial patterns in terms of sensor-specific relative wetness and event-scale wetting. The revised text will also state explicitly that storm-flow occurrence refers to the integrated response measured at the microcatchment outlet and will remove interpretations that rely on direct comparability of absolute volumetric soil-water contents among monitoring positions.

General comment 4. Assessment of hydrological coupling

I encourage the authors to reconsider how hydrological coupling is evaluated in Section 3.3.2.

The current analysis focuses on periods when outlet discharge is already occurring ($Q > 0$) and examines the associated range of local soil moisture conditions. While this provides useful information about the conditions under which runoff was observed, it is less clear whether it fully characterizes hydrological coupling itself.

Could the authors discuss how often similar soil moisture states occurred without measurable outlet flow? Including this complementary perspective would help distinguish whether the observed variability reflects differences in coupling strength or simply the temporal evolution of soil moisture during runoff events.

Would it also be possible to complement the current analysis with event-scale lag analysis, cross-correlation, or soil moisture–discharge hysteresis analysis (e.g., following Zuecco et al., 2016)? Such analyses would provide a more direct assessment of the timing and strength of hydrological coupling and would considerably strengthen this section.

Authors response

We agree that the submitted analysis, which considered SM conditions only while outlet discharge was occurring, did not by itself provide a sufficiently complete assessment of local-to-outlet hydrological coupling. We therefore expanded the analysis in two complementary directions that address the reviewer's questions directly: (i) a wetness-state overlap analysis, evaluating how often local soil-water states associated with measurable outlet flow also occurred when no measurable outlet flow was present; and (ii) an event-scale onset-lag analysis, evaluating the timing of detectable local SM response relative to outlet-discharge onset.

First, we compared sensor-specific relative-wetness distributions during $Q = 0$ and $Q > 0$ for all 11 SM sensors within the 186 precipitation-event response windows. For consistency with Sect. 3.3.2, the results presented below focus on the three illustrative 10-cm monitoring positions, while corresponding results for the complete sensor network will be provided in the Supplement.

We compared the complete $Q = 0$ and $Q > 0$ distributions and used, as a simple descriptive measure of their overlap, the proportion of $Q = 0$ observations that reached or exceeded the median relative wetness observed during $Q > 0$. At High-Connectivity Low-Gradient Hillslope, High-Connectivity Upslope-Edge Terrace 1, and Disconnected Mid-Terrace, median relative wetness during $Q > 0$ was approximately the 99th, 99th, and 98th sensor-specific percentiles, respectively. Nevertheless, 8.0 %, 14.6 %, and 15.2 % of $Q = 0$ observations, respectively, reached or exceeded these reference wetness states. Thus, very wet local conditions were not exclusive to periods with measurable outlet flow. The full distributions showed the clearest separation between $Q = 0$ and $Q > 0$ at High-Connectivity Low-Gradient Hillslope, weaker separation at High-Connectivity Upslope-Edge Terrace 1, and the greatest overlap at Disconnected Mid-Terrace (Fig. GC4-1). The median $Q > 0$ wetness was used only as a descriptive reference for quantifying overlap and was not interpreted as a hydrological threshold. Because successive 15-min observations are temporally autocorrelated, these timestamp-scale comparisons were interpreted descriptively rather than treated as independent replicates for inferential testing.

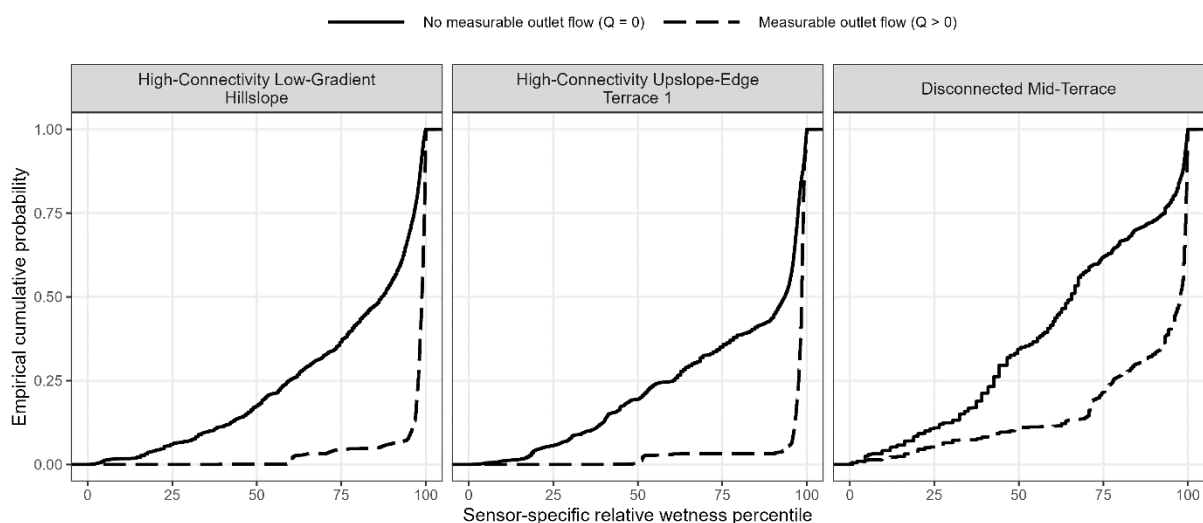


Fig. GC4-1. Empirical cumulative distributions of sensor-specific relative wetness during periods without ($Q = 0$) and with ($Q > 0$) measurable outlet discharge within precipitation-event response windows at the three illustrative 10-cm monitoring positions.

Second, we added an event-scale onset-lag analysis for the 31 storm-flow events with complete outlet-discharge records. The pre-event local SM baseline was defined as the median value during the preceding 60 min. Detectable local SM response onset was defined as the first increase of at least 1 volumetric-water-content percentage point above this baseline that persisted for two consecutive 15-min observations. We repeated the analysis using 0.5- and 2-percentage-point criteria to evaluate sensitivity to the response definition.

Because onset lags from the three illustrative positions were measured for the same storm-flow events, differences among positions were evaluated using a Friedman repeated-measures test. The inferential comparison was restricted to the 19 events for which valid onset lags were available at all three positions. Following the omnibus test, pairwise differences were evaluated using paired Wilcoxon signed-rank tests with Holm correction for multiple comparisons.

Onset lags differed significantly among the three positions (Friedman test, $\chi^2(2) = 33.30$, $p < 0.001$; Fig. GC4-2). Considering all valid event-level lags available at each position, median lags were -15 min at both High-Connectivity Low-Gradient Hillslope and High-Connectivity Upslope-Edge Terrace 1, indicating that detectable SM response typically preceded outlet-discharge onset by approximately one 15-min interval. At Disconnected Mid-Terrace, the median lag was +60 min, indicating that detectable local response typically followed outlet-discharge onset.

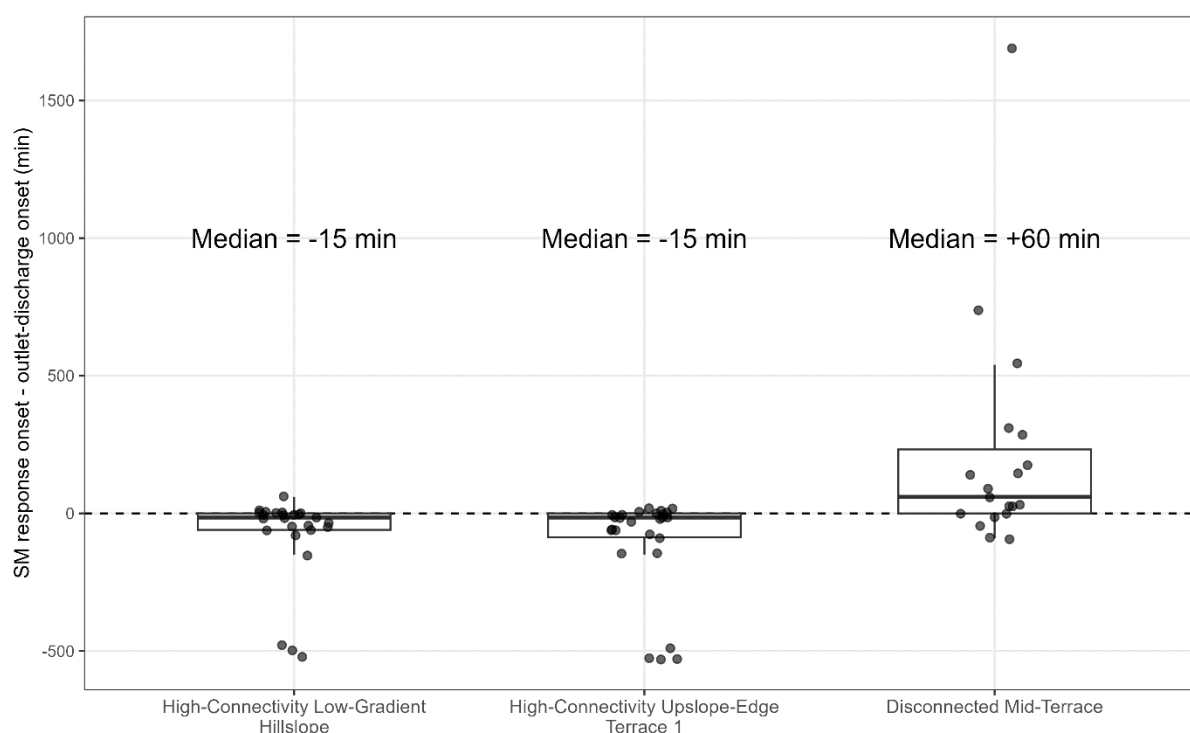


Fig. GC4-2. Event-scale lag between detectable local soil-moisture response and outlet-discharge onset at the three illustrative 10-cm monitoring positions. Negative values indicate that detectable local SM response preceded outlet-discharge onset, whereas positive values indicate that outlet discharge preceded detectable local SM response. All valid event-level lags are shown.

For the 19 complete paired events, onset lags at Disconnected Mid-Terrace were significantly later than at both High-Connectivity Low-Gradient Hillslope and High-Connectivity Upslope-Edge Terrace 1 (paired Wilcoxon signed-rank tests, Holm-adjusted $p < 0.001$ in both cases). Disconnected Mid-Terrace responded later than High-Connectivity Low-Gradient Hillslope in

18 of the 19 events, with one tie, and later than High-Connectivity Upslope-Edge Terrace 1 in all 19 events. The two high-connectivity positions also differed significantly (Holm-adjusted $p = 0.013$), although this contrast was smaller. High-Connectivity Low-Gradient Hillslope responded later in 9 events, at the same 15-min interval in 9 events, and earlier in 1 event. The overall pattern of earlier responses at the two high-connectivity positions and later responses at Disconnected Mid-Terrace was retained when the response criterion was varied between 0.5 and 2 percentage points, although the number of detectable responses at Disconnected Mid-Terrace decreased as the response criterion became more restrictive.

We also examined the alternative temporal approaches suggested by the reviewer, including event-scale cross-correlation and SM-Q hysteresis following Zuecco et al. (2016). For the present dataset, the onset-lag analysis provided the clearest and most directly interpretable event-scale measure of relative response timing and was therefore selected for inclusion in the revised manuscript.

Taken together, these analyses provide an operational assessment of hydrological coupling through two observable dimensions: the degree of overlap between local wetness states occurring with and without measurable outlet flow, and the relative timing of detectable local SM response and outlet-discharge onset. Both dimensions varied among the monitored positions. We interpret these differences as evidence of contrasting local-to-outlet hydrological correspondence, while recognising that neither analysis demonstrates direct transfer of water from a sensor location to the outlet or identifies the physical flow pathway involved.

The revised Methods will describe the wetness-state and onset-lag analyses and their associated criteria, including the event-level statistical comparison of onset lags, and Sect. 3.3.2 will be rewritten to report wetness-state overlap and response timing rather than relying on contemporaneous $Q > 0$ observations. The response figures shown here document the analyses performed to address this comment. The corresponding Results will be reformulated accordingly, and a redesigned Fig. 8 will present the two analyses for the three illustrative positions, with corresponding analyses for the complete sensor network provided in the Supplement. The Discussion will distinguish these observed state and timing relationships from hypotheses concerning internal water-transfer pathways.

General comment 5. Role of wildfire in the study

The wildfire provides important context for the study site, but its role within the analyses appears more limited than suggested by the Abstract and Introduction.

The manuscript discusses post-fire hydrological processes, including soil water repellency, infiltration changes, and the post-fire disturbance window. However, monitoring began approximately eight years after the 2013 wildfire, and the analyses do not explicitly include burn severity, recovery status, or comparisons with unburned conditions.

Could the authors clarify the intended role of wildfire within the manuscript? If wildfire is primarily included as background context, I suggest moderating the framing in the Abstract and Introduction so that it more closely reflects the analyses that were performed. Alternatively, if information on burn severity or recovery patterns is available, incorporating it into the analyses could further strengthen the manuscript.

Authors response

We agree that the role of wildfire should be clarified so that the framing more closely reflects what is evaluated by the present analyses. The 2013 wildfire remains an important component of the recent environmental history of the microcatchment and therefore provides relevant context for interpreting the landscape in which the hydrological monitoring was conducted. However, the 2021-2024 observations analysed in the submitted manuscript begin approximately eight years after the wildfire and are not used to isolate the causal effect of wildfire or to reconstruct a post-fire hydrological recovery trajectory. We will therefore moderate the wildfire framing where the submitted manuscript implies that specific post-fire processes are evaluated by the present dataset.

Previous work in the same catchment nevertheless provides evidence that the initial post-fire hydrosedimentary signal evolved substantially during the first years after the 2013 wildfire. García-Comendador et al. (2017) documented a strong concentration of sediment export and responses associated with more distant, fire-affected source areas during the early post-fire period, followed by a decline in subsequent years. García-Comendador et al. (2020) subsequently showed that suspended sediment during the first monitored events originated predominantly from burned surfaces, whereas the contribution from burned hillslopes progressively decreased through the study period. These findings indicate attenuation of the initial wildfire-related sediment-source signal during the first post-fire years, although they should not be interpreted as demonstrating complete hydrological recovery. They provide useful site-specific context for recognising that the 2021-2024 monitoring period represents a later post-fire stage than the immediate disturbance period.

Accordingly, discussion of processes such as post-fire water repellence, reduced infiltration capacity, or generic post-fire recovery trajectories will be removed or substantially reduced where these mechanisms are used to explain the observed 2021-2024 hydrological behaviour. The manuscript will retain the 2013 wildfire as contextual framing and as part of the recent disturbance history of the study area, while avoiding attribution of the measured hydrological patterns to wildfire effects that were not directly assessed.

We also do not propose adding burn severity as an explanatory variable. Even if spatial information on the 2013 wildfire were available, the present monitoring design does not provide the temporal or reference framework required to isolate a burn-severity effect from the other controls considered in this study. Incorporating such a variable would therefore risk implying a level of causal attribution that the dataset cannot support.

The Abstract and Introduction will be revised so that wildfire remains part of the environmental and landscape context of the study but is not presented as an explanatory factor tested by the analyses. The Study Area will retain the relevant wildfire history, while the Discussion and Conclusions will avoid attributing the observed hydrological behaviour to post-fire processes or recovery dynamics that were not directly evaluated.

General comment 6. Interpretation of runoff generation mechanisms

The Discussion presents several plausible explanations for the observed behaviour, including subsurface lateral flow, fill-and-spill processes, terrace storage, and activation of slower subsurface pathways.

These interpretations are certainly consistent with the observations, but the available measurements consist primarily of soil moisture at two depths and outlet discharge. Without complementary observations such as piezometers, tracer experiments, runoff pathway measurements, or direct observations of overland flow, it may be difficult to distinguish among the different runoff-generation mechanisms.

I therefore encourage the authors to present these mechanisms more explicitly as hypotheses supported by the observations rather than definitive interpretations.

In addition, the two high-intensity dry-season runoff events appear particularly interesting. Could these events represent a different runoff-generation mechanism, perhaps involving infiltration-excess runoff, compared with the wetter-season events? Discussing this possibility may strengthen the paper by highlighting the state-dependent nature of runoff generation within the catchment.

Authors response

We agree that the available observations do not allow individual runoff-generation mechanisms to be identified unequivocally. The monitoring design provides precipitation, local SM dynamics at two depths, and integrated outlet discharge, but it does not directly measure surface runoff pathways, lateral subsurface fluxes, saturation extent, infiltration capacity, or internal water transfer.

We will therefore revise the Discussion so that lateral subsurface flow, fill-and-spill behaviour, terrace storage, saturation-excess runoff, infiltration-excess runoff, and slower subsurface pathways are treated as process hypotheses that may be consistent with the observations rather than as mechanisms demonstrated by the dataset. This distinction is also consistent with the revised coupling analysis, as wetness-state correspondence and onset timing strengthen the observational basis for local-to-outlet coupling but do not identify the pathway by which water reaches the outlet.

Following the event-catalogue audit, the revised catalogue contains one storm-flow event classified within the dry period, on 3 September 2024. The submitted manuscript referred to two dry-period events because it also included the 14 August 2024 runoff response, for which precipitation data from the headwater station are unavailable. That event will therefore be excluded from the canonical event catalogue used for event-scale analysis in the revised manuscript.

The 3 September 2024 event delivered 49.0 mm of precipitation in approximately one hour and had the highest $IP_{\max 15}$ observed during the study period (119.2 mm h^{-1}). Its occurrence during the dry hydrological period under exceptionally intense rainfall is compatible with an infiltration-excess contribution. However, SM data were unavailable for this event, so its antecedent soil-water state and event-scale wetting response cannot be evaluated. We will therefore discuss infiltration excess only as a plausible process hypothesis rather than as a demonstrated mechanism.

More broadly, the contrast between this high-intensity dry-period event and the wetter soil-water states associated with many storm-flow events during the wet periods suggests that runoff generation may be state dependent. Under dry conditions, sufficiently intense precipitation may produce storm flow through mechanisms different from those operating when catchment

storage and soil-water availability are higher. The latter conditions are compatible with storage-dependent, saturation-related, or subsurface contributions, but the available observations do not allow these mechanisms to be distinguished uniquely.

General comment 7. Discharge measurements and event classification

Given the very small annual runoff volumes reported for this catchment, it would be helpful to provide additional information regarding discharge detectability and the consistency of the discharge measurements throughout the monitoring period.

The manuscript notes that the stage sensor was replaced in January 2023, but it is not entirely clear whether the two sensors were cross-calibrated or whether the minimum detectable discharge remained unchanged. Because the classification of events into runoff and non-runoff depends directly on detecting measurable discharge, additional clarification would strengthen confidence in the event catalogue.

Specifically, it would be useful to:

- define the stage or discharge detection threshold used for event classification;*
- describe how consistency was ensured across the sensor replacement;*
- discuss whether the exceptionally low runoff observed during 2022–2023 could have been influenced, even partially, by measurement detectability.*

Similarly, the 27 August 2023 event appears to be one of the few runoff-producing storms during the dry season. Although discharge could not be quantified, acknowledging more explicitly how its exclusion may influence the seasonal analyses would improve transparency.

Authors response

We agree that the event-classification procedure and the continuity of outlet-flow detection should be described more explicitly. No single numerical stage or discharge threshold was imposed for classifying storm-flow occurrence. Instead, events were classified from quality-controlled outlet hydrographs. A storm-flow response was identified when precipitation was followed, within the defined response window, by a coherent event-related rise in stage above the zero-flow baseline. Isolated low-amplitude deviations that did not develop into a discernible hydrograph response were classified as non-flow. Once a response was identified, stage was converted to discharge using the rating relationship for the fixed 2.5-foot H-flume. The same operational classification criterion was applied throughout the study period.

After an unexpected malfunction, the CS451-L pressure transducer was replaced by an OTT PLS in January 2023. The OTT PLS is the pressure transducer currently used throughout the research group's hydrometric network in the Balearic Islands, which comprises more than 50 gauging stations. The CS451-L had also been used by the group for more than 10 years at several stations with reliable performance. Because the failure was unexpected, no period of simultaneous operation was available for direct cross-calibration, and we will state this explicitly. Both sensors were installed at the same outlet measurement point and referenced to the same zero-flow level of the H-flume, and the same stage-discharge rating relationship was used throughout the monitoring period. The malfunction was identified before any storm-flow event occurred during the affected interval. Nevertheless, in the absence of overlapping

measurements, we cannot demonstrate that the two sensors had identical practical detection limits.

We find no evidence that the very low runoff observed during 2022-2023 was primarily caused by reduced measurement detectability. The very low runoff during 2022 was recorded entirely with the original CS451-L sensor, while low-amplitude but coherent storm-flow responses were also detected after installation of the OTT PLS. To illustrate this, Fig. GC7-1 shows low-amplitude confirmed storm-flow responses recorded with each instrument. Among confirmed storm-flow events with complete quantitative discharge records, these were the lowest-peak-discharge events within each sensor period, with peak discharges of only 0.057 L s^{-1} for the CS451-L and 0.086 L s^{-1} for the OTT PLS. These observations show how those small outlet responses were detectable with both sensors, although the absence of an overlap period prevents direct comparison of their practical detection limits.

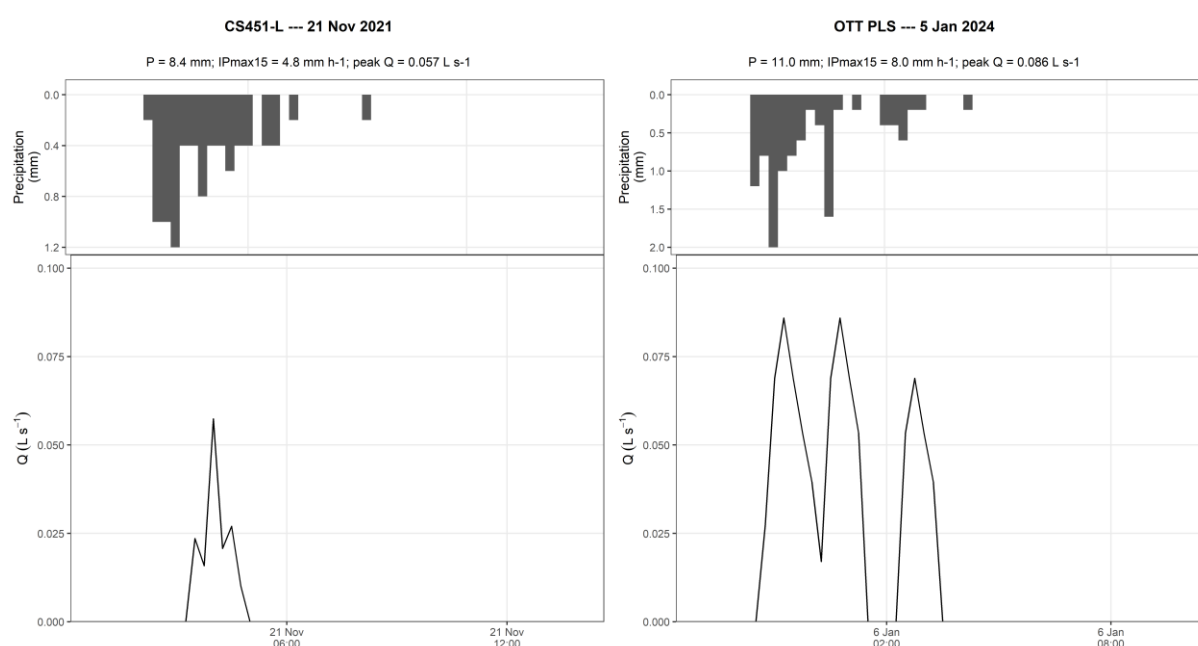


Fig. GC7-1. Examples of low-amplitude confirmed storm-flow responses recorded before and after replacement of the outlet stage sensor in January 2023. The 21 November 2021 event was recorded with the CS451-L and the 5 January 2024 event with the OTT PLS. These events were the lowest-peak-discharge confirmed storm-flow events with complete quantitative discharge records and no measurable outlet discharge during the preceding hour within their respective sensor periods.

Following the event-catalogue audit, the 27 August 2023 event is retained as a confirmed storm-flow event because an outlet response was observed in the available real-time telemetry. The complete local discharge record was subsequently lost, however, so its discharge magnitude and runoff volume cannot be reconstructed. The revised seasonal classification assigns this event to the wetting-up period rather than to the dry period. It is therefore retained in the wetting-up storm-flow count and in analyses based on storm-flow occurrence but excluded from analyses requiring a complete quantitative discharge hydrograph. Its exclusion consequently does not alter the seasonal classification of occurrence analysis, but quantitative runoff summaries for the wetting-up period do not include this unknown discharge contribution.

The revised Methods will describe the hydrograph-based event-classification criterion explicitly, the absence of a fixed numerical stage or discharge threshold, the common zero-flow reference and rating relationship, and the January 2023 sensor replacement without an overlap

period. The Results and associated captions will distinguish consistently between confirmed storm-flow occurrence and availability of a complete quantitative discharge record, particularly for the 27 August 2023 event.

General comment 8. Sensitivity of the event catalogue

The conclusions of the study depend on several methodological choices used to define individual rainfall events, including the minimum rainfall threshold, the event separation criterion, and the time window used to associate rainfall with runoff.

These choices are reasonable, but it would be valuable to know how sensitive the principal findings are to alternative definitions.

Would it be possible to perform a brief sensitivity analysis using, for example, different minimum rainfall thresholds (1, 2, or 5 mm)? Demonstrating that the principal conclusions remain similar under alternative event definitions would increase confidence in the robustness of the results.

Authors response

We agree that the robustness of the event catalogue to methodological choices should be evaluated. The 1 mm minimum precipitation criterion used in the submitted manuscript was selected because it represented the smallest precipitation amount associated with a measurable SM response exceeding the stated sensor accuracy ($\pm 1\%$). Following the reviewer's suggestion, we repeated the principal event-scale analyses using minimum total-precipitation thresholds of 1, 2, and 5 mm. To isolate sensitivity to this criterion, the six-hour event-separation criterion and the six-hour post-rainfall response window used to associate precipitation events with outlet response were retained unchanged across the three catalogues.

Increasing the minimum precipitation threshold reduced the catalogue from 186 events at ≥ 1 mm to 149 events at ≥ 2 mm, and 97 events at ≥ 5 mm. All 32 storm-flow events were retained under all three definitions because the smallest precipitation event associated with measurable outlet storm flow delivered 5.8 mm. The alternative thresholds therefore progressively removed small non-flow events without changing the observed storm-flow event set.

The principal storm-flow/non-flow contrasts were robust to this filtering. Total precipitation, $IP_{\max 15}$, maximum SM, and ΔSM continued to show clear differences between the event classes under all three minimum-precipitation thresholds. Antecedent SM also remained higher for storm-flow than non-flow events at both depths under each threshold, although these contrasts were weaker than those for precipitation and the within-event SM metrics. Storm duration was the main exception, as its contrast weakened as progressively smaller events were removed and was no longer supported at the 5 mm threshold.

The probabilistic storm-flow-occurrence analysis showed the same robustness. For the complete record, the top-ranked model at both 10 and 50 cm consistently contained total precipitation, $IP_{\max 15}$, and antecedent SM at thresholds of 1, 2, and 5 mm. Total precipitation retained very high summed Akaike support under all three thresholds, while antecedent SM retained substantial support in the complete record. When all events beginning in November 2021 were excluded, however, the top-ranked model consistently contained only total precipitation and $IP_{\max 15}$ at both depths and under all three precipitation thresholds. The

summed Akaike weight for antecedent SM declined to approximately 0.26-0.27 across these sensitivity analyses, reproducing the same loss of support identified using the original ≥ 1 mm catalogue.

These results indicate that the principal event-scale interpretation is not driven by the inclusion of very small precipitation events. Precipitation forcing remains the most consistently supported general control of storm-flow occurrence, while the additional support of antecedent wetness in the complete record, and its strong dependence on the exceptional November 2021 sequence, is reproduced under all three minimum-precipitation thresholds.

The revised Methods will retain and clarify the rationale for the 1 mm minimum precipitation criterion and describe the ≥ 1 , ≥ 2 , and ≥ 5 mm sensitivity analysis, including that the event-separation and post-rainfall response criteria were held constant. The detailed event counts, storm-flow/non-flow comparisons, and model-support results under the alternative thresholds will be provided in the Supplement. Because the sensitivity analysis confirms rather than alters the interpretation obtained from the canonical ≥ 1 mm catalogue, we will retain the main Results focused on that catalogue rather than adding a separate sensitivity-results section.

General comment 9. Structural versus hydrological connectivity

The manuscript classifies monitoring locations using the Index of Connectivity, which is based primarily on surface topography and sediment transport pathways.

The Discussion, however, largely interprets the observed behaviour in terms of subsurface storage and lateral flow processes. Because these processes are not necessarily represented by a topography-based connectivity index, it would be helpful to discuss this distinction more explicitly.

Could the authors elaborate on why the Index of Connectivity is considered an appropriate proxy for structural hydrological connectivity in this terraced environment? Alternatively, framing these classes primarily as topographic positions rather than hydrological connectivity classes may avoid potential ambiguity.

Authors response

We agree that the submitted manuscript did not distinguish sufficiently clearly between terrain-based structural connectivity and event-scale hydrological behaviour. We will therefore clarify the meaning of the Index of Connectivity (IC)-based structural classification and revise the interpretation of the hydrological observations so that structural connectivity, hydrological coupling, and possible flow mechanisms are not conflated.

We nevertheless consider the IC an appropriate terrain-based metric for characterising structural connectivity in this terraced environment. The IC describes the spatial organisation of landscape elements according to contributing area, slope, downslope routing, and surface impedance, and therefore captures how terrain and surface configuration condition the continuity of downslope pathways. This is particularly relevant in abandoned terraced landscapes, where terrace benches and walls, breaks in slope, vegetation, and surface roughness reorganise those pathways and generate strong contrasts in structural connection and disconnection. Importantly, IC-based connectivity analysis has already been applied within the same Mediterranean terraced landscape to characterise how terracing, terrace degradation,

vegetation, and terrain configuration organise spatial connectivity (Calsamiglia et al., 2017, 2018; Estrany et al., 2019). We use these studies as site-specific support for the structural interpretation of the IC, rather than as evidence that the IC itself measures event-scale functional hydrological connectivity. This distinction is consistent with the broader hydrological-connectivity literature, in which landscape structure conditions the organisation of flow pathways but the actual activation of hydrological connections depends on event and antecedent conditions (Bracken et al., 2013; Moreno-de-las-Heras et al., 2020).

On this basis, we will retain the connectivity-based monitoring-position names. Terms such as “high-connectivity” and “disconnected” refer specifically to the IC-derived structural connectivity of the terrain and surface-routing configuration at those positions. They do not indicate that hydrological transfer between a particular monitoring position and the outlet was directly observed, nor do they imply a particular surface or subsurface flow pathway. The structural classification was established independently from the event-scale SM and outlet-discharge observations analysed in this study.

We agree with the reviewer that the submitted Discussion extended this structural classification too directly into interpretations involving subsurface storage, lateral flow, and specific runoff-generation pathways. As described in our response to General Comment 6, this is the aspect that requires revision. Lateral subsurface flow, fill-and-spill behaviour, terrace storage, and related processes will therefore be presented only as possible mechanisms compatible with the observations rather than as processes inferred from IC values or demonstrated by the monitoring network. The IC characterises terrain and surface organisation relevant to structural connectivity, but it does not resolve subsurface pathways or identify the mechanism through which water reaches the outlet.

The distinction between structural and functional connectivity also helps explain why the hydrological relevance of a given structural configuration can vary among events. Structural organisation is comparatively stable over the timescale of individual precipitation events, whereas hydrological connections can emerge, persist, or disappear depending on precipitation forcing, antecedent wetness, and catchment storage. Hydrological studies have shown that the area producing a local response is not necessarily equivalent to the area effectively connected to the stream or outlet at a given time, and that these connected contributing areas can expand and contract as hydrological conditions change (Jencso et al., 2009; Nippgen et al., 2015). We use this effective contributing-area concept only as a conceptual framework for interpreting event-dependent hydrological connectivity. Our monitoring network does not delineate, map, or quantify the effective contributing area of So na Vidala, nor does it establish which parts of the microcatchment supplied water to the outlet during individual events.

Within those limitations, however, our observations do provide information on the dynamic relationship between local hydrological responses and the integrated outlet response. As described in our response to General Comment 4, we evaluate hydrological coupling through two complementary observations: the correspondence between local relative-wetness states and outlet-flow states; and the timing of detectable local SM responses relative to outlet-discharge onset. These analyses do not demonstrate continuous water transfer between individual monitoring positions and the outlet, but they quantify whether and when local hydrological responses correspond with the microcatchment outlet response. We therefore use hydrological coupling as an observationally constrained expression related to the functional component of hydrological connectivity, while avoiding the stronger claim that functional flow pathways or source-to-outlet connections have been directly identified.

The revised analyses are also broadly consistent with the independently defined structural setting. In the onset-lag analysis, the two monitored positions classified as having high structural connectivity showed earlier median detectable SM responses relative to outlet-discharge onset than the Disconnected Mid-Terrace position, while the Q-state analysis identified corresponding spatial differences in the local relative-wetness states associated with measurable outlet flow. We interpret these patterns as consistent with, rather than as validation of, the IC-based structural classification, because neither analysis identifies the physical pathway through which water was transferred.

The revised Introduction and Methods will therefore define more explicitly the distinction between IC-derived structural connectivity, hydrological coupling, and functional connectivity, and will strengthen the justification for applying the IC in this terraced landscape using both established site-specific literature (Calsamiglia et al., 2017, 2018; Estrany et al., 2019) and the broader hydrological-connectivity framework. The connectivity-based monitoring-position names will be retained, with their structural meaning stated explicitly at first use and in the relevant figure captions. The Discussion will use the effective contributing-area concept only to explain conceptually why the part of the landscape hydrologically connected to the outlet may vary among events, while stating explicitly that its spatial extent was not measured in this study. The Discussion will also separate the observed local-to-outlet state and timing relationships from hypotheses concerning surface or subsurface flow pathways and will no longer infer specific subsurface mechanisms from the IC classification.

General comment 10. Consistency of terminology

The manuscript discusses several closely related concepts, including hydrological spatial coupling, hydrological coupling, functional connectivity, and hydrological connectivity.

Although these terms are introduced in the Introduction, they appear to be used somewhat interchangeably throughout the manuscript. Establishing a consistent terminology and applying it throughout would improve clarity and help readers distinguish among the concepts being discussed.

Similarly, the manuscript appropriately highlights the value of including non-runoff events. However, since studies such as Saffarpour et al. (2016) and Kaffas et al. (2025) have also incorporated non-runoff events, it may be more accurate to describe this as an area that remains relatively underexplored rather than presenting it as a completely novel aspect of the study.

Authors response

We agree that the submitted manuscript used several closely related terms without distinguishing their roles consistently enough. We will therefore simplify the terminology and establish explicit usage rules that will be applied throughout the manuscript.

Hydrological coupling will be the principal operational term for the event-scale relationship evaluated from our monitoring data. It will refer to the correspondence between soil-water dynamics at an individual monitored position and the integrated outlet response, assessed through local wetness-state correspondence with outlet-flow state and through the timing of detectable local SM response relative to outlet-discharge onset. We will no longer use “hydrological spatial coupling” as a separate term. Spatial differences among monitoring

positions will instead be described directly as spatial variation in hydrological coupling. This avoids assigning two terms to the same operational relationship and makes the terminology more consistent throughout the manuscript.

Structural connectivity will be treated separately and will refer to the comparatively stable terrain and surface configuration represented by the IC. As explained in our response to General Comment 9, the connectivity-based monitoring-position names will be retained, but terms such as “high-connectivity” and “disconnected” will be defined explicitly as referring to IC-derived structural connectivity, not to the observed strength of event-scale hydrological coupling at those positions. Hydrological behaviour will therefore be evaluated from the observed wetness-state and timing relationships rather than inferred from the structural labels.

Hydrological connectivity will be retained as the broader literature concept describing water-mediated linkage among landscape elements. Functional connectivity will refer to the dynamic, event-dependent component of that broader concept, but it will not be presented as a directly measured variable or result of this study. As discussed in our response to General Comment 9, the observed hydrological coupling provides information relevant to this functional component because it quantifies whether and when local hydrological responses correspond with the integrated outlet response. However, it does not demonstrate continuous source-to-outlet water transfer, identify functional flow pathways, or delineate the area effectively connected to the outlet.

Terminology implying internal hydrological pathways beyond what was measured will also be removed or reformulated. In particular, the isolated use of “subsurface connectivity” in the Conclusions is neither operationally defined nor supported by a direct measurement and will be removed. Expressions such as “dynamic activation of connectivity” will likewise be revised where they imply that activation of an internal pathway was directly observed. Where interpretations involving lateral flow, storage, or pathway activation remain useful, they will instead be presented explicitly as hypotheses consistent with the observations rather than as measured connectivity.

Regarding non-flow events, we agree with the reviewer that their inclusion should be framed as an underexplored rather than unprecedented aspect of event-based hydrological analysis. This was also the intended framing of the submitted manuscript, as the Introduction already stated that relatively few studies had explicitly incorporated both storm-flow and non-flow events within a unified analytical framework and cited Saffarpour et al. (2016) and Kaffas et al. (2025) as examples. We will retain this framing and review the surrounding wording to ensure that no statement can be interpreted as claiming methodological novelty.

The revised manuscript will therefore undergo a manuscript-wide terminology audit across the title, Abstract, Introduction, Methods, Results, Discussion, Conclusions, figures, and captions. Hydrological coupling will be used as the sole operational term for the observed local-to-outlet relationship, while spatial differences will be described as spatial variation in hydrological coupling rather than through the separate term “hydrological spatial coupling”. The manuscript title will be revised accordingly to “Spatial variability in hydrological coupling within an abandoned Mediterranean terraced microcatchment”. Structural connectivity will refer specifically to the IC-derived terrain configuration, whereas hydrological connectivity and functional connectivity will retain their broader conceptual meanings without being presented as directly measured quantities. Unsupported terminology implying observed internal pathways will be removed or qualified. The framing of non-flow events will retain the underexplored

interpretation already present in the submitted manuscript while making the distinction from methodological novelty fully explicit.

Specific comments

Specific comment 1 – L28–29 / L254–258

Mean annual precipitation is given as 491 mm for 2013–2024 in Section 2.1 and 496.9 ± 189.6 mm for the study period in Section 3.1.1. These values are not necessarily inconsistent, but the corresponding periods should be stated side by side once so that readers do not interpret them as contradictory.

Authors response

We agree that the reference periods should be made explicit. The 491 mm yr⁻¹ value characterises the longer 2013–2024 period, whereas 496.9 ± 189.6 mm yr⁻¹ refers specifically to the four-year monitoring period (2021–2024). We will state the relevant period alongside each value so that they cannot be interpreted as contradictory.

Specific comment 2 – L215–216

Please provide a stronger hydrological justification for using calendar years. In a Mediterranean regime, the wet season straddles the calendar-year boundary; “to include a larger number of events” is not, by itself, a hydrological justification and complicates comparison with studies using hydrological years.

Authors response

We agree that the submitted justification was incomplete. Calendar years are not intended to represent independent hydrological cycles in this Mediterranean setting, but they are used as the temporal framework for descriptive annual summaries.

The principal analyses do not depend on annual boundaries. Storm-flow occurrence is analysed at the individual-event scale and within the four hydrological periods (wet, drying-down, dry, and wetting-up) defined from the observed evolution of soil moisture, precipitation, and atmospheric water demand. These hydrological periods vary in timing and duration among years and may cross both calendar-year and conventional hydrological-year boundaries. They therefore provide the process-relevant temporal framework for the analyses, whereas imposing a fixed hydrological-year boundary would not alter the event classification or better represent the observed seasonal transitions.

Within the January 2021–December 2024 study period analysed, retaining calendar years also allows all observed events to be included within four complete annual summaries. Restricting the record to complete hydrological years would reduce the number of events represented in those annual units without providing an advantage for the event-scale or hydrological-period analyses. We therefore retain calendar years for annual description, while making clear that the hydrological interpretation is based on the complete event record and on the observed hydrological periods rather than on calendar-year boundaries.

We also acknowledge the reviewer's point regarding comparison with studies reporting hydrological-year statistics. The revised manuscript will therefore state explicitly that annual values are calendar-year summaries and will avoid treating them as directly equivalent to hydrological-year water balances reported elsewhere.

Sect. 2.3 will be revised accordingly. The original statement that calendar years were used simply "to include a larger number of events" will be replaced by an explanation that calendar years are used for descriptive annual summaries and to retain the complete 2021-2024 event record within those summaries, whereas the process-based analyses are conducted at event and observed hydrological-period scales.

Specific comment 3 – L219–223

The seasonal delineation uses percentiles of the same soil moisture record that is later analysed within seasons. This introduces a degree of circularity that should be acknowledged. Please also specify how the transitional-period boundaries were operationally defined, for example by visual inspection or through an algorithmic procedure, so that the approach is reproducible.

Authors response

We agree that the seasonal-delineation procedure requires a more explicit description and that using SM both to help delineate hydrological periods and subsequently to characterise those periods introduces a degree of dependence.

The 25th and 75th percentiles of network-mean SM were used as quantitative reference levels for relatively dry and wet states, but they were not applied as automatic thresholds defining transition dates. The final boundaries were assigned manually by joint visual inspection of the daily precipitation, reference evapotranspiration, and network-mean SM time series. The classification followed the direction and persistence of the hydroclimatic trajectory: wet periods represented sustained high-SM conditions; drying-down began when SM entered a persistent seasonal decline from those wet conditions; dry periods represented sustained low-SM conditions; and wetting-up began when SM entered a persistent recovery from dry conditions. Individual rainfall-induced SM increases or short dry excursions were not by themselves used to define a transition. No fixed minimum duration or algorithmic rate-of-change criterion was imposed. Rather, the transition dates were assigned from the combined evolution of SM, precipitation, and atmospheric water demand. This allowed the classification to follow year-specific hydroclimatic trajectories instead of forcing transitions at individual percentile crossings. For example, the initial autumn 2023 wetting-up was followed by renewed drying because subsequent precipitation did not sustain soil-water recovery.

Because SM contributed to the delineation, differences in mean SM among the resulting periods are not independent of the classification itself and will therefore be used descriptively to characterise the hydroclimatic states rather than as independent evidence supporting their differentiation. The storm-flow/non-flow comparisons conducted within each hydrological period address a different question, whether event characteristics and SM responses differ between event classes under broadly comparable seasonal conditions, but we will explicitly acknowledge that the period stratification itself was partly informed by SM.

To make this procedure fully transparent, the revised Methods will state explicitly that the classification was manual and rule-guided rather than algorithmic, and will describe the role of

the 25th and 75th SM percentiles and the joint interpretation of precipitation, reference evapotranspiration, and network-mean SM. The Supplement will provide the exact start and end dates assigned to every hydrological period, while the daily hydroclimatic time series used for the classification will remain visible in the corresponding figure. This will allow the exact classification used in the study to be reproduced in subsequent analyses and will make the basis for each transition auditable, while acknowledging that the boundary identification itself involved manual interpretation rather than a deterministic algorithm.

Specific comment 4 – L237–243

Please report the number of runoff and non-runoff events per season in Table 2 and state which specific statistical tests were applied to each metric.

Authors response

We thank the reviewer for this suggestion. The revised event catalogue comprises 32 storm-flow and 154 non-flow events: 17 and 64 during the wet period, 5 and 23 during drying-down, 1 and 33 during the dry period, and 9 and 34 during wetting-up, respectively. We have adopted the term “storm-flow event” instead of “flood event” throughout the revised manuscript because it more precisely describes the measurable outlet-flow response observed in this small microcatchment and avoids the broader connotations of inundation commonly associated with “flood”. This is a terminological change only and does not affect the event classification.

These event counts are now reported directly in Table 2 for the complete study period and for each hydrological period. The revised Table 2 is provided above in our response to General Comment 2.

Table 2 now also identifies the specific statistical test applied to each comparison. As clarified in the Methods, Student’s two-sample *t*-test was used when both groups satisfied normality and variances were homogeneous, Welch’s two-sample *t*-test when both groups satisfied normality but variances were heterogeneous, and the Mann–Whitney U test when either group departed from normality. No inferential comparison was conducted for the dry period because only one storm-flow event occurred.

Specific comment 5 – Table 1 caption

“ration” should be corrected to “ratio.”

Authors response

We thank the reviewer for identifying this typographical error. “Ration” will be corrected to “ratio”.

Specific comment 6 – Table 2, dry season

The runoff-producing event has a duration of 1.0 h, IP_{max15} of 119.2 mm h⁻¹, and total precipitation of 49 mm. This appears to be an exceptional storm. A brief description, including an estimated return period based on the record beginning in 1957 if such an estimate is defensible, would be valuable.

Authors response

We agree that the 3 September 2024 storm was exceptional and that additional context is useful. We do not, however, consider a return-period estimate based on the precipitation record beginning in 1957 to be defensible for the characteristic of interest. That long-term reference series is available at daily resolution, whereas the exceptional feature of this event was its sub-hourly intensity ($IP_{\max 15} = 119.2 \text{ mm h}^{-1}$). A return period derived from daily precipitation totals would therefore describe a different temporal scale.

We instead examined the available sub-daily precipitation record near the microcatchment from October 2013 onwards. The 3 September 2024 event delivered 49.0 mm in approximately one hour, including 29.8 mm in one 15-min interval ($IP_{\max 15} = 119.2 \text{ mm h}^{-1}$). This was the largest 15-min precipitation accumulation in the October 2013–December 2024 sub-daily record. In addition, 47.6 mm fell within two consecutive 15-min intervals, representing the largest 30-min accumulation in that record and substantially exceeding the next-largest 30-min accumulation of 31.6 mm on 28 September 2015.

Given the limited length of the sub-daily record, we consider these observed rankings more defensible than fitting an extreme-value distribution or assigning a formal return period. We therefore do not report a return period for this event, but will provide this empirical context in the Results and state explicitly that the available data do not support a robust return-period estimate at the relevant sub-hourly timescale.

Specific comment 7 – Figure 5

Consider adding marginal distributions or a fitted logistic separation rather than relying on scatter plots alone. With 155 non-flow and 31 runoff events, overplotting may obscure the underlying density structure.

Authors response

We agree that the unequal numbers of storm-flow and non-flow events (154 and 32, respectively, in the revised catalogue) can obscure the underlying distributional structure when shown as scatter plots alone. To address this, we revised Fig. 5 by adding point transparency to reduce overplotting and by including class-specific marginal kernel-density curves for the soil-moisture variable at the right side of each panel. These density curves are normalised separately for non-flow and storm-flow events and help visualise the degree of overlap and separation between the two event classes. This revised version will replace the submitted Fig. 5 in the manuscript.

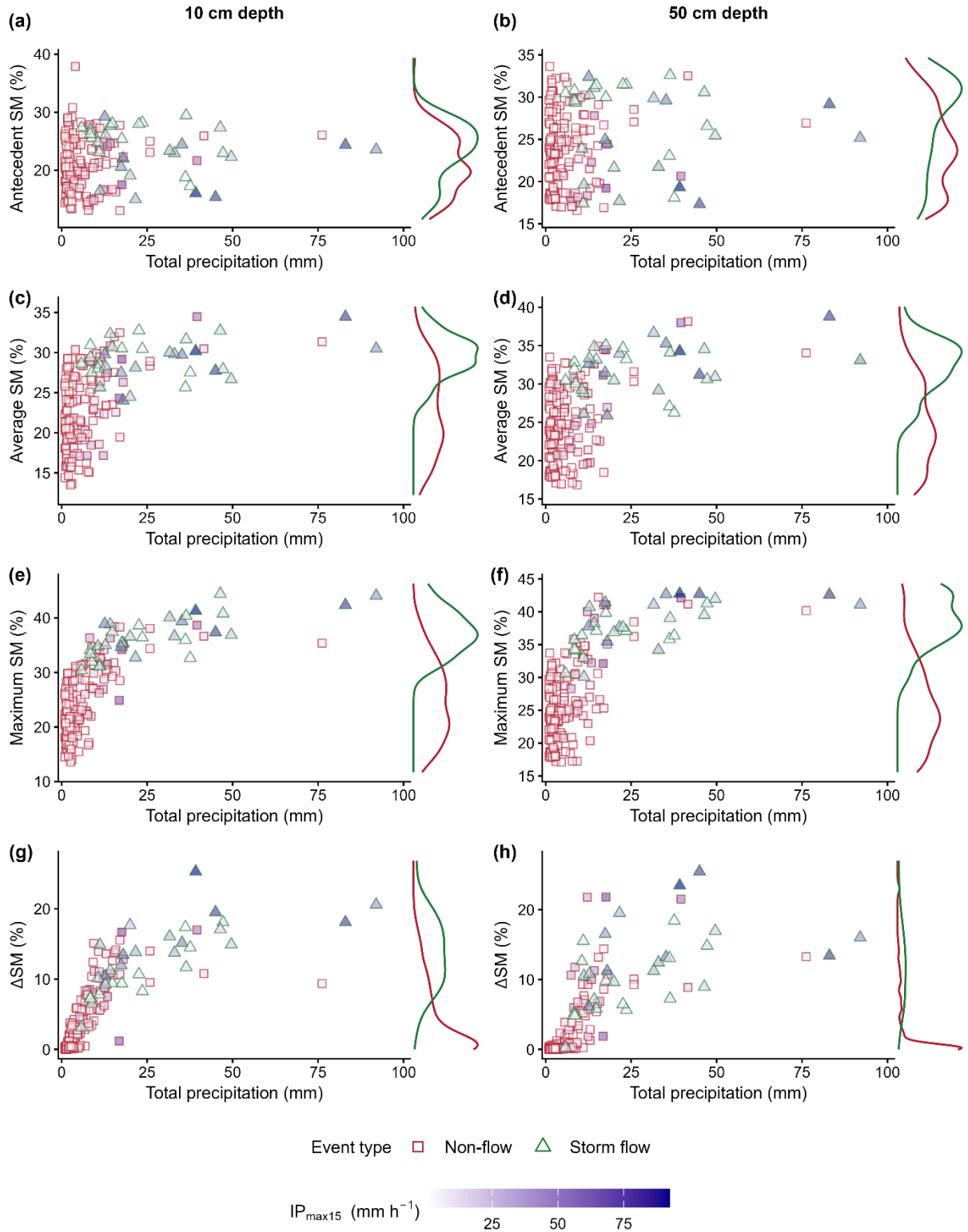


Fig. 5. Event-scale relationships between total precipitation and soil-moisture (SM) metrics for storm-flow and non-flow events during the complete study period. Antecedent SM at (a) 10 cm and (b) 50 cm depth, average SM at (c) 10 cm and (d) 50 cm depth, maximum SM at (e) 10 cm and (f) 50 cm depth, and Δ SM at (g) 10 cm and (h) 50 cm depth. SM metrics at each depth represent the mean across all sensors installed at the corresponding depth. Class-specific kernel-density curves shown to the right of each panel summarise the marginal distribution of the corresponding SM metric for non-flow and storm-flow events and are normalised separately within each event class.

We also considered the reviewer's suggestion of adding fitted logistic separation. However, we do not consider this appropriate for panels showing average, maximum, and Δ SM, because

these are within-event response metrics rather than independent antecedent predictors of storm-flow occurrence. Adding logistic fits to these descriptive panels could therefore blur the distinction between exploratory visualisation and the formal occurrence analysis. The probability of storm-flow occurrence is instead evaluated separately using precipitation characteristics and antecedent SM as candidate predictors. We will retain Fig. 5 as a descriptive figure and present the logistic modelling results in the dedicated analysis section.

Specific comment 8 – Figures 6–7

The reported thresholds (32.5%, 42.5–45%, and 13–17.5%) appear to have been identified visually. If they are to be described as thresholds, they should ideally be estimated formally, for example by minimizing classification error and reporting uncertainty bounds. Otherwise, they may be more appropriately described as approximate envelopes.

Authors response

We agree that the submitted values were visually inferred and should not have been described as formal thresholds. We will therefore remove the threshold terminology from the revised manuscript and will not estimate or report classification thresholds from these figures.

Because the SM probes were used without site-specific calibration, we recalculated the analyses underlying Figs. 6-7 using the sensor-specific relative-wetness framework described in our response to General Comment 3 and prepared revised versions of both figures. The previously reported absolute SM values will therefore be removed. The revised Fig. 6 shows the maximum relative-wetness percentile attained during each event, whereas the revised Fig. 7 shows Δ relative wetness in percentile points.

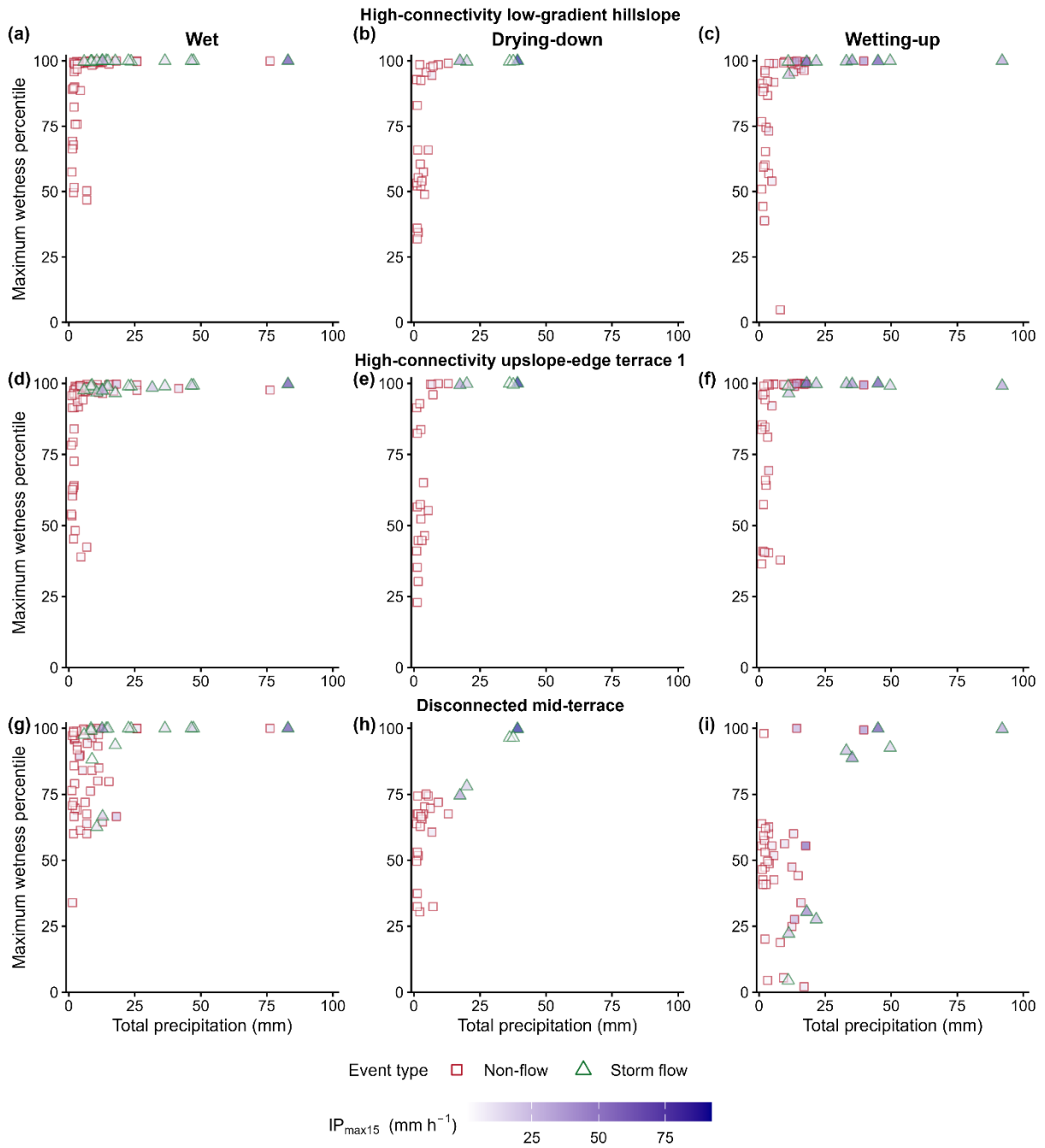


Fig. 6. Event-scale relationships between total precipitation and maximum relative wetness for storm-flow and non-flow events at High-Connectivity Low-Gradient Hillslope during (a) wet, (b) drying-down, and (c) wetting-up; at High-Connectivity Upslope-Edge Terrace 1 during (d) wet, (e) drying-down, and (f) wetting-up; and at Disconnected Mid-Terrace during (g) wet, (h) drying-down, and (i) wetting-up.

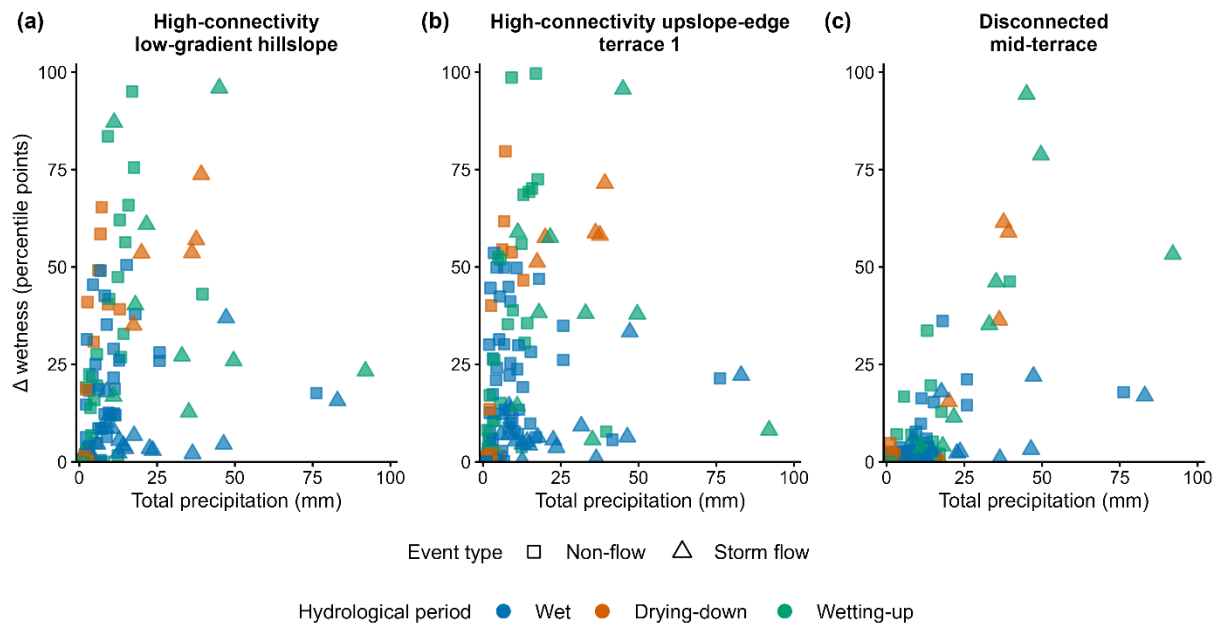


Fig. 7. Event-scale relationships between total precipitation and Δ relative wetness for storm-flow and non-flow events at (a) High-Connectivity Low-Gradient Hillslope, (b) High-Connectivity Upslope-Edge Terrace 1, and (c) Disconnected Mid-Terrace across wet, drying-down, and wetting-up periods.

Both figures will be interpreted descriptively. Fig. 6 shows how storm-flow and non-flow events are distributed within each sensor's empirical wetness range, while Fig. 7 shows the magnitude and seasonal variability of event-scale local wetting. We do not undertake a separate classification-threshold analysis because storm-flow occurrence is evaluated independently using precipitation characteristics and antecedent SM as candidate predictors. The revised Figs. 6-7 will replace the submitted versions in the manuscript.

Specific comment 9 — Section 3.3.1

Three of the eleven sensors were selected to represent the connectivity classes. Please state the selection criteria explicitly and demonstrate, in the Supplement, that the non-selected sensors show comparable behaviour. Equivalent figures for at least one additional site would be more informative than tables alone.

Authors response

We agree that the rationale for highlighting three of the eleven SM sensors should be stated more explicitly. The three positions were selected after examining the event-scale response patterns across the monitoring network. We retained three sensors installed at the same depth (10 cm) that captured contrasting observed responses while also spanning contrasting IC-derived structural settings within the hillslope-terrace system: High-Connectivity Low-Gradient Hillslope, High-Connectivity Upslope-Edge Terrace 1, and Disconnected Mid-Terrace. Keeping measurement depth constant avoided introducing vertical differences in SM dynamics into this spatial comparison. The selection was therefore intended to provide illustrative examples of contrasting spatial responses rather than statistically selected representatives of discrete connectivity classes, and we will clarify this in the revised manuscript.

To show directly how these three positions compare with the non-selected sensors, we prepared equivalent descriptive plots for the complete monitoring network using the same event metrics shown in Figs. 6-7. Fig. SC9-1 shows total precipitation against maximum relative wetness for all 11 sensors, whereas Fig. SC9-2 shows total precipitation against Δ relative wetness.

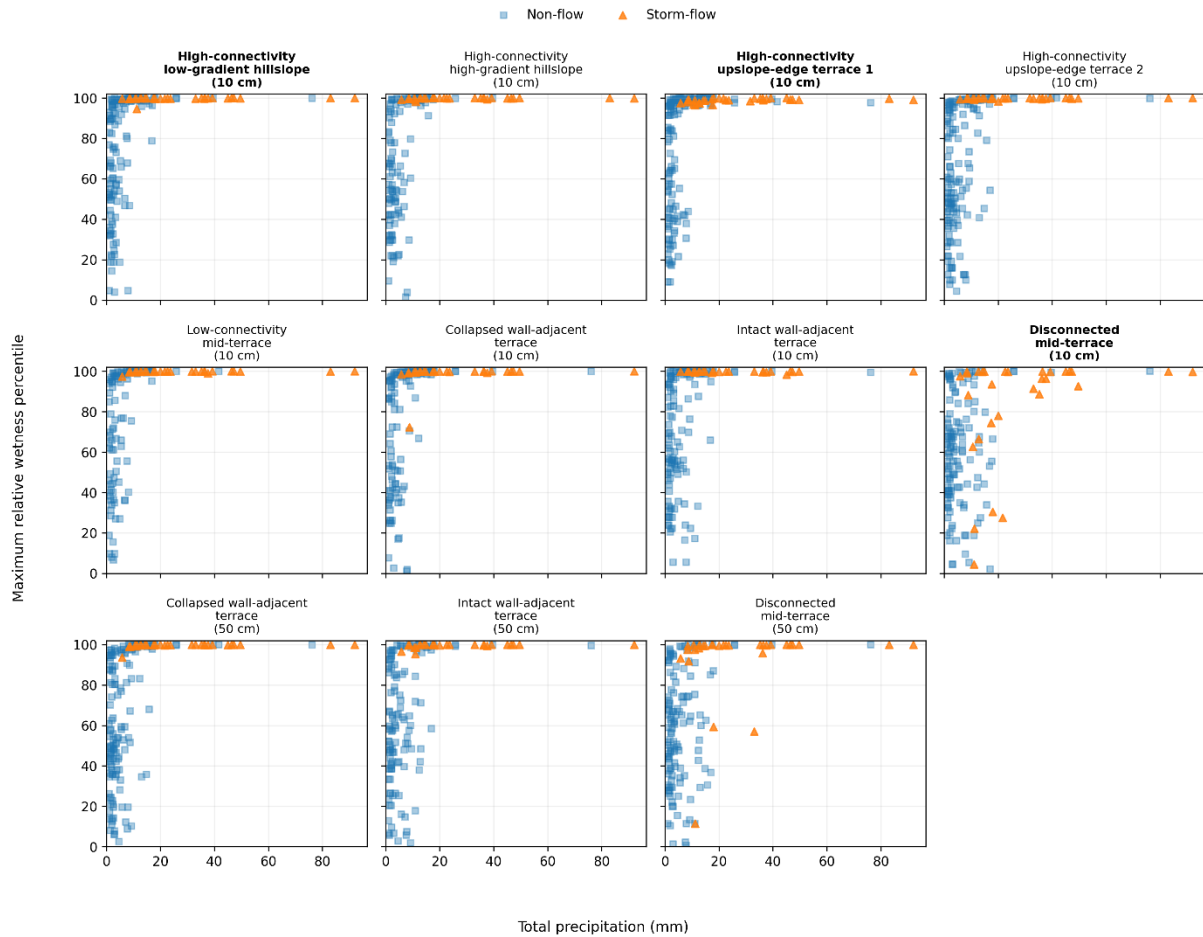


Fig. SC9-1. Event-scale relationships between total precipitation and maximum relative wetness for storm-flow and non-flow events across all 11 SM sensors. Bold labels indicate the three monitoring positions retained in the main-text figures.

The full-network comparison shows that the pattern illustrated by the selected positions is not restricted to those sensors. At most monitoring positions, storm-flow events are concentrated toward the upper part of the corresponding sensor-specific wetness distribution, although the amount of overlap with non-flow events differs among positions. The two selected high-connectivity positions therefore illustrate a recurring response observed elsewhere in the network, whereas Disconnected Mid-Terrace captures a contrasting response in which storm-flow events occurred across a broader range of local relative-wetness conditions.

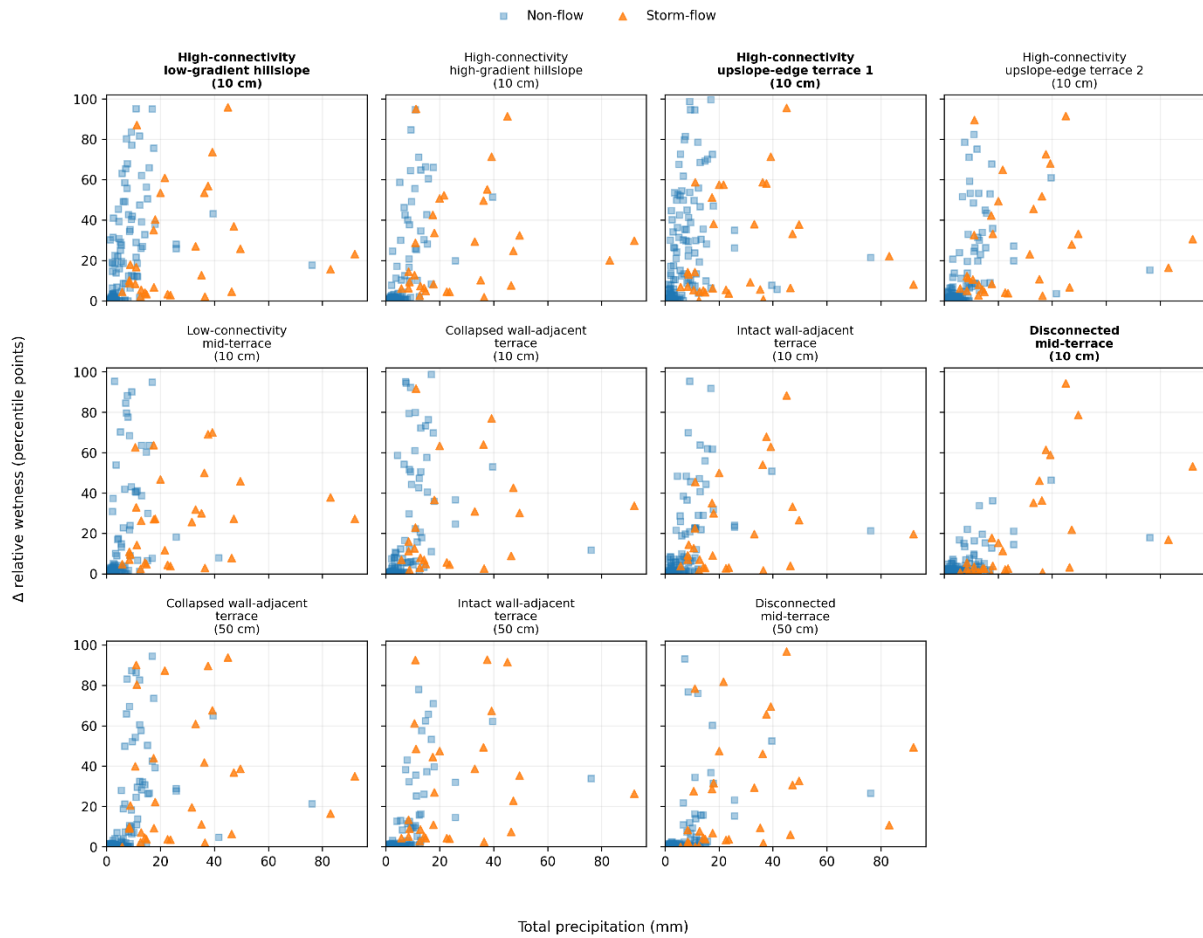


Fig. SC9-2. Event-scale relationships between total precipitation and Δ relative wetness for storm-flow and non-flow events across all 11 SM sensors. Bold labels indicate the three monitoring positions retained in the main-text figures.

The full-network comparison for Δ relative wetness shows greater heterogeneity among monitoring positions (Fig. SC9-2). The magnitude of local event-scale wetting and the degree of separation between storm-flow and non-flow events vary across the network. We will therefore retain Δ relative wetness as a descriptive measure of local event-scale wetting and avoid treating it as a spatially uniform indicator of storm-flow occurrence.

As an additional check on the spatial contrast represented by the three selected positions, we also examined the timing between detectable local SM response and outlet-discharge onset across all 11 sensors, as developed in response to General Comment 4 (Fig. SC9-3). This provides a complementary temporal perspective on the full monitoring network.

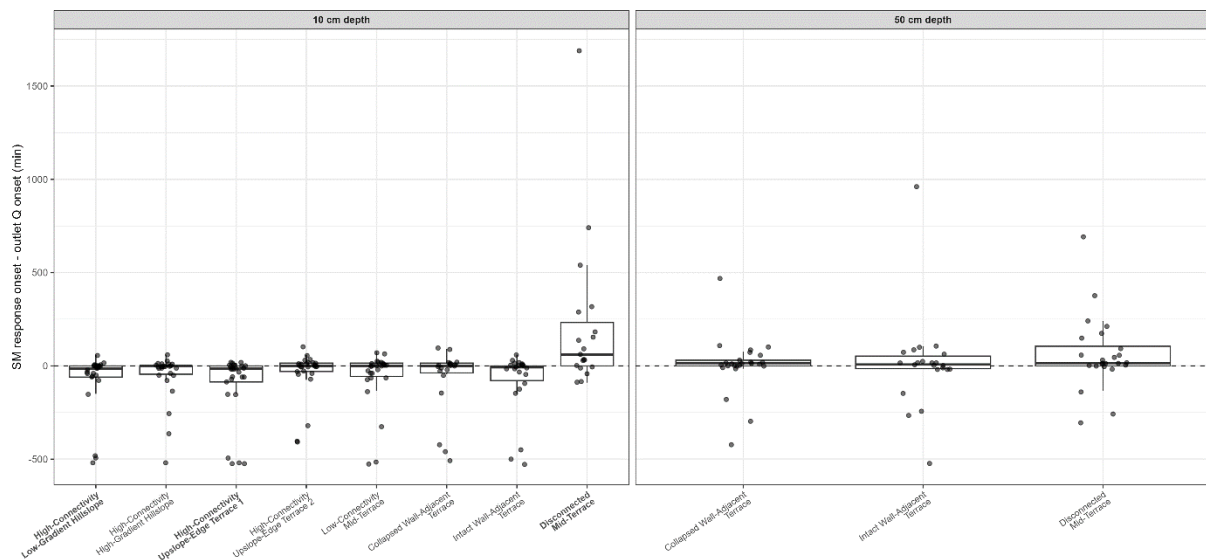


Fig. SC9-3. Event-scale lag between detectable local SM response onset and outlet-discharge onset across all 11 SM sensors. Negative values indicate that the local SM response preceded outlet-discharge onset, zero indicates occurrence within the same 15-min interval, and positive values indicate that outlet discharge preceded the detectable local SM response. Bold labels indicate the three monitoring positions retained in the main-text figures.

The timing comparison further shows that the three highlighted positions span contrasting response behaviours within the network. Detectable SM responses at High-Connectivity Low-Gradient Hillslope and High-Connectivity Upslope-Edge Terrace 1 generally preceded or occurred close to outlet-discharge onset, whereas responses at Disconnected Mid-Terrace were generally later and more variable. Because this timing assessment was developed during the revision in response to General Comment 4, we use it as complementary support for the spatial contrast represented by the three positions rather than as a criterion used in their original selection.

We will therefore retain the three 10-cm positions in main-text Figs. 6–7 as illustrative examples of contrasting spatial responses. The revised Methods and Results will state explicitly how these positions were selected, and the three full-network figures will be included in the Supplement, allowing readers to assess directly both the recurring patterns and the spatial variability across all non-selected sensors.

Specific comment 10 – L509–511

“closed to saturation” should be corrected to “close to saturation.”

Authors response

We thank the reviewer for identifying this wording error. “Closed to saturation” will be corrected to “close to saturation”.

Specific comment 11 – L360

Please revise the construction “being ΔSM more relevant” for idiomatic English.

Authors response

We agree that the construction is not idiomatic. Because the surrounding interpretation is also being revised under the sensor-specific relative-wetness framework, we will rewrite the sentence rather than make a purely grammatical substitution. The revised wording will describe the observed storm-flow/non-flow differences without treating Δ wetness as an independent control.

Specific comment 12 – L476–478

The sentence beginning “Ultimately, R generation by the dynamic activation of connectivity...” appears to be missing its principal verb.

Authors response

We agree that the sentence is grammatically incomplete. The passage will be rewritten as part of the broader revision of the connectivity interpretation, and the formulation based on “dynamic activation of connectivity” will be removed rather than simply repaired grammatically.

Specific comment 13 – Author contributions

“Wainwrigth” should be corrected to “Wainwright.”

Authors response

We thank the reviewer for identifying this typographical error. “Wainwrigth” will be corrected to “Wainwright” in the Author contributions section.

Specific comment 14 – Estrany et al. (2019) versus Estrany et al. (2019a)

Citations are inconsistent between the text (L102, L128, and L195) and the reference list, which appears to contain only one 2019 entry.

Authors response

We thank the reviewer for identifying this inconsistency. Only one Estrany et al. (2019) reference is included in the manuscript, and therefore the “2019a” designation is unnecessary. The citations will be standardised to “Estrany et al. (2019)” throughout the manuscript.

Specific comment 15 – Data availability

“Available on request” may not meet current expectations for a data-driven EGU/HESS paper. Given the value of this dataset, I strongly encourage the authors to deposit the event catalogue and 15-minute time series in a repository with a DOI at the revision stage. This would also substantially increase the long-term value and citability of the study.

Authors response

We thank the reviewer for this suggestion and agree that the data availability statement should be strengthened. At the revision stage, we will deposit a complete event-scale dataset in a public repository with a DOI. This dataset will contain the full catalogue of the 186 precipitation events

analysed in the study together with all event-level variables used in the manuscript and revised analyses, including precipitation characteristics, storm-flow/non-flow classification, hydrological period, soil-moisture metrics, and the corresponding derived event-scale variables.

We will not deposit the complete continuous 15-min time series at this stage because these records form part of a broader ongoing research dataset that is currently being used in additional unpublished analyses. However, all event-scale data required to support the analyses and conclusions presented in this manuscript will be made openly available through the repository.

The Data Availability section will be revised accordingly, replacing the current “available on request” statement with the repository DOI and a description of the deposited event-scale dataset.

Specific comment 16 – Throughout

The frequent use of single-letter abbreviations (R, P, Q, and SM) in running prose makes some sentences unnecessarily difficult to parse. At minimum, I suggest avoiding “R” as an abbreviation for runoff in prose, particularly because it may be confused with the statistical software mentioned in the Methods.

Authors response

We agree that the frequent use of single-letter abbreviations reduces readability in running prose. We will therefore remove the abbreviations R, P, and Q from the running text throughout the manuscript, replacing them with “runoff”, “precipitation”, and “discharge” or “outlet discharge”, respectively, according to context. The abbreviation SM will be retained after its first definition as “soil moisture”, because it is used consistently throughout the manuscript and does not present the same ambiguity as the single-letter abbreviations.

Single-letter symbols may still be retained where appropriate in figure axes and table headings, provided that they are clearly defined. The manuscript will be checked systematically to ensure consistent terminology throughout.

Concluding remarks

Overall, I believe this manuscript has the potential to make a valuable contribution. Its principal strength is the unique multi-year, event-resolved dataset, particularly the inclusion of non-runoff events that are rarely retained in comparable studies.

At present, however, the manuscript uses causal and probabilistic language that the analysis does not yet fully support. The revision path is relatively clear: (1) fit an event-occurrence model and test whether soil moisture metrics add information beyond rainfall forcing; (2) directly address the dependence on the November 2021 event cluster; (3) use normalized or calibrated soil moisture thresholds across sites; (4) add lag or hysteresis analysis to substantiate the coupling claims; and (5) adjust the wildfire framing to match the analyses performed.

Most of the revisions suggested above are analytical rather than editorial, but they build directly on an already valuable dataset and should substantially strengthen the manuscript. With these changes, the paper could make a strong contribution to the connectivity literature

and provide a useful benchmark dataset for connectivity-aware modelling. I would be pleased to review a revised version.

Authors response

We thank the reviewer for this constructive synthesis and for outlining a clear revision path. We have addressed the principal issues raised in the review through: (i) a probabilistic storm-flow-occurrence analysis using precipitation characteristics and antecedent SM as candidate predictors, while treating maximum SM and Δ SM separately as within-event responses; (ii) a sensitivity analysis excluding the exceptional November 2021 sequence; (iii) reanalysis of the spatial soil-water observations using sensor-specific relative wetness; (iv) complementary wetness-state and onset-lag analyses to strengthen the observational basis for hydrological coupling; and (v) revision of the wildfire framing so that fire history is treated as environmental context rather than as an explanatory factor tested by the present study. The corresponding Methods, Results, figures, and interpretation will be incorporated into the revised manuscript as described above.

These revisions lead to a more constrained interpretation of the dataset. Precipitation forcing, particularly total precipitation, emerges as the most robust general predictor of storm-flow occurrence. Antecedent wetness provides additional information in the complete record, but this support is strongly influenced by the exceptionally wet November 2021 sequence and does not persist when that sequence is excluded. Maximum SM and Δ SM are therefore interpreted as within-event soil-water responses rather than as independent predictors or controls of storm-flow occurrence. This is consistent with the revised occurrence analysis, in which total precipitation remained the most consistently supported predictor, whereas the additional contribution of antecedent SM was concentrated in the exceptional November 2021 sequence.

The interpretation of hydrological coupling has likewise been narrowed to what can be supported directly by the observations. Coupling is evaluated from the correspondence between local wetness states and outlet-flow state, together with the relative timing of detectable local SM response and outlet-discharge onset, without treating these relationships as evidence of a specific internal flow pathway or direct source-to-outlet connection.

We appreciate the reviewer's careful assessment and the constructive direction provided throughout the review. The additional analyses and associated revisions substantially strengthen the quantitative basis of the study while placing clearer limits on the interpretation of soil-moisture controls, hydrological coupling, and wildfire context.

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