

Final authors response to Referee 2

Hydrological spatial coupling within an abandoned Mediterranean terraced microcatchment

MS No. egusphere-2026-2711

General comments

This paper investigates the conditions under which runoff (R) reaches the outlet of a small abandoned Mediterranean terraced micro catchment. Precipitation (P), soil moisture (SM) and outlet discharge (Q) were monitored continuously over four years (2021 to 2024), with SM recorded at paired depths in positions selected to represent contrasting hillslope and terrace settings. Events with and without outlet flow are compared through P and SM metrics, disaggregated by season, and the results are interpreted in terms of the spatial organisation of hydrological coupling and connectivity within the micro catchment. The study rests on a sustained field effort in a demanding setting, and the resulting record is, in my view, its principal asset. Several elements deserve particular recognition. The decision to treat events without outlet flow as informative rather than as an absence of data is conceptually sound and well argued (lines 68 to 71): the observation that ignoring such events may bias the identification of controlling factors and limit the interpretation of state dependent responses is an interesting contribution, and it is worth emphasising further. Among the results presented, those concerning the seasonal differentiation of the controls on outlet response are the ones most directly supported by the observations. Antecedent SM discriminates between flow and non-flow events only during the wet season, and storm duration shows the same seasonal restriction (lines 336 to 338), while metrics describing wetting during the event itself (maximum SM and the change in SM) separate the two event types consistently across all seasons (lines 486 to 493). These findings are well grounded in the data, and I believe they point to where the scientific weight of this manuscript lies. Figure 5 conveys core aspects of the experiment effectively, and the clearer separation obtained for the change in SM at the deeper sensors is an interesting and potentially important detail.

I think the central difficulty concerns the distance between what was measured and what is concluded. Hydrological coupling and hydrological connectivity are the organising concepts of the paper: they appear in the title, structure the abstract, frame the objectives, are addressed in the discussion, and carry the conclusions. However, neither is measured or at least indirectly quantified. The instrumentation consists of vertical SM profiles at discrete points (with some horizontal setting that I do not fully understand), together with P and Q at the outlet. There are no observations of surface R at the scale of individual terraces or hillslope segments (facets), no measurements at any internal point of the drainage network, no monitoring of channels, tracks or wall openings, and no direct evidence of lateral flow, whether at the surface or in the subsurface.

Point vertical profiles are informative about local wetting, but they carry little information about the horizontal transfer of water between positions, which is precisely what coupling and connectivity are meant to describe. Statements such as the dynamic activation of connectivity (lines 476 to 479), the prolongation of functional connectivity (line 450), or the demonstration that R occurrence emerges from the spatial organisation of hydrological coupling (line 606) therefore rest on inference rather than on evidence. The Sediment Connectivity Index is

introduced as the one quantitative descriptor of structural connectivity, yet it is presented only by reference, its values are never clearly shown, and it is not carried into any subsequent analysis.

A related concern is terminological. The manuscript employs hydrological coupling, hydrological connectivity, structural connectivity, functional connectivity, spatial hydrological coupling and, in the conclusions only, subsurface connectivity. The relationships among these terms are not established, and several appear to overlap substantially. None is given an operational definition, that is, a definition expressed in terms of a quantity that could be measured or computed from the data presented. The consequence is felt most acutely in the abstract, where expressions such as partially coupled conditions, well connected positions, disconnected mid terrace areas and connectivity aware approaches are used before any of them has been introduced. The abstract may therefore be difficult to interpret on a first reading. I believe that simplifying this vocabulary, ideally to a single concept with an explicit metric attached, would improve the paper considerably and would make the interpretation of the results much easier to follow.

The evidential base for the central comparison is thinner than the manuscript acknowledges. Over four years, R at the outlet was rare: 2023 yielded no R at all and suffered data loss, 2022 was dominated by essentially a single large event, and much of the total is attributable to the anomalously wet November 2021. Establishing a robust contrast between flow and non-flow events, and characterising the thresholds that separate them, is difficult with so few positive cases. Two further methodological issues compound this. First, the water level sensor was replaced in January 2023 with no reported overlap period, so no relationship between the two instruments was established. The 2021 and 2024 hydrographs behave very differently despite comparable cumulative P, and the runoff response in 2024 corresponds to the precipitation record more consistently than in 2021, where the accumulated curve shows an abrupt step without a clear counterpart in precipitation. On the evidence presented, the possibility that part of this difference is instrumental cannot be excluded. Second, the SM sensors appear to have been used with factory calibration and without site specific verification. Under those conditions the record supports relative comparisons and relative thresholds, but it does not support the identification of absolute SM thresholds. Consistency of sensor type and installation protocol across locations, as argued in lines 197 to 198, secures comparability among positions; it does not secure accuracy in absolute terms. The statistical strategy would also benefit from fuller reporting: the normality and variance tests invoked in lines 240 to 241 are never reported, nor is the false discovery rate correction, so it is not possible to verify which procedure was applied in each comparison.

Two further points concern the representativeness of the monitored positions and the terminology used for events. The instrumented locations are clustered in the southern portion of the catchment, so that most of the catchment area lacks observations. The manuscript acknowledges this only in part, and only in the Discussion (lines 613 to 620), by referring to the unmonitored western sector. This makes it difficult to sustain the claim that these positions capture structural variability across the hillslope and terrace system as a whole, or that they support inferences about connectivity at catchment scale. Separately, a comment on terminology.

The manuscript refers throughout to flood events. This usage is consistent with the formal definition of flood in the International Glossary of Hydrology (WMO/UNESCO, 2011), which admits a relatively high flow measured by stage or discharge. In practice, however, the term is widely associated with inundation processes, including riverine flooding, flash flooding and urban flooding, and a reader may therefore expect something other than outlet discharge in a micro catchment of this size. Storm flow events, or alternatively runoff generating events, might convey the intended meaning with less ambiguity, with non-flow events retained as the complementary category. This is offered as a suggestion for clarity and is left to the authors' judgement.

Some aspects of the presentation would benefit from further attention. Lettering in essentially all figures is too small to be read comfortably; enlarging it would be particularly valuable in this manuscript, since the figures carry much of the burden of conveying the experimental design, and they are well placed to do so. Figure 1 and Figure 2 overlap somewhat in content, and the reader would be helped by a figure devoted specifically to the experimental design. Photographs of the instrumented positions, together with a scheme illustrating what is meant by disconnected, mid terrace and high connectivity settings, would be a valuable addition in this respect. The Discussion is long, and some material is revisited in more than one place, for instance in the overlap between Sections 4.1.1 and 4.2.3 on evapotranspiration and vegetation, and in the restatement at lines 510 to 512 of arguments made earlier. The authors may wish to consider a nested organisation, temporal scale followed by spatial scale, and to explore whether some discursive passages could be conveyed through tables or schematic summaries; either change would make this section easier to follow.

Finally, in my view, there are passages whose scope reaches somewhat further than the observations reported here can support. The land management discussion (lines 613 to 620) addresses terrace abandonment, degradation, afforestation and wildfire. These are important themes, but none of them was measured or tested in this study, and the discussion of them necessarily draws on the wider literature rather than on the results presented. The acknowledgement at line 623 that vegetation effects were not explicitly assessed is welcome, although the difficulty remains, since the surrounding text still attributes hydrological consequences to processes that were not observed. One option would be to set these themes aside; another would be to retain them within a clearly labelled section on directions for future research, where their status would be unambiguous. The manuscript would also benefit from a consolidated statement of its limitations, covering the small number of R events, the missing 2023 record, the sensor replacement without a reported overlap period, the calibration status of the SM probes, and the fact that observations are confined to a small number of points clustered in the southern part of the catchment, so that most of sectors and connectivity structures in the catchment area are unmonitored.

In summary, this is a carefully instrumented study containing several results that are genuinely supported by the data, particularly the seasonal differentiation of the controls on outlet response and the treatment of non-flow events as informative. Those results are, however, embedded in a conceptual framework that the measurements cannot sustain, and the conclusions are correspondingly overextended.

Authors response

We thank the reviewer for this detailed and constructive assessment. We agree with the central concern that the submitted manuscript did not distinguish clearly enough between directly observed relationships, predictive relationships, and mechanistic interpretation. In particular, hydrological coupling was not defined operationally enough, and several passages interpreted soil-water observations at individual monitoring positions too directly, in terms of internal water-transfer pathways. The revision will therefore tighten both the analytical framework and the terminology, as detailed in the responses below.

We distinguish between direct observation of internal water-transfer pathways, which the present monitoring design does not provide, and an operational assessment of hydrological coupling based on observable local-to-outlet relationships. As part of the revision, hydrological coupling is now evaluated through two complementary observations: (i) correspondence between sensor-specific relative-wetness states and the presence or absence of measurable outlet discharge within precipitation-event response windows, and (ii) the relative timing between detectable local soil-water response and outlet-discharge onset. Hydrological coupling will therefore refer specifically to the event-scale relationship between soil-water dynamics at an individual monitoring position and the integrated outlet response.

These observations do not demonstrate direct transfer of water from a monitored position to the outlet and do not identify the physical pathway involved. They do, however, provide an observational basis for evaluating how local soil-water behaviour corresponds to the integrated outlet response and how that relationship varies among monitored positions.

The revised manuscript will also separate this spatial component from the analysis of storm-flow occurrence. Storm-flow occurrence is the binary outlet outcome and is evaluated probabilistically using precipitation characteristics and antecedent SM as candidate predictors. Maximum SM and Δ SM are treated separately as within-event soil-water responses rather than as independent predictors of storm-flow occurrence.

Structural connectivity will likewise be treated separately as the comparatively stable terrain and surface configuration represented by the Index of Connectivity (IC). The connectivity-based monitoring-position names will be retained, but their meaning will be defined explicitly as referring to IC-derived structural connectivity rather than observed event-scale hydrological behaviour. Functional connectivity will not be presented as a directly measured variable, and mechanistic interpretations involving specific internal flow pathways will be treated as hypotheses consistent with the observations rather than as demonstrated processes.

We also agree that the seasonal and event-scale storm-flow results deserve greater prominence. The revised manuscript will therefore be organised around two complementary questions: what controls the occurrence of measurable outlet storm flow, and how does the relationship between local soil-water response and the integrated outlet response vary among monitored positions? The methodological and interpretative limitations identified by the reviewer are addressed individually in the recommendations and specific comments below.

Considering the above, I would suggest the following:

- 1. Define the central concepts operationally. Reduce the number of overlapping terms, and for each retained term provide a definition expressed as a measurable or computable*

quantity. If the Sediment Connectivity Index is to serve as the descriptor of structural connectivity, introduce it earlier and with a proper description, present its values, and use it explicitly in the analysis. If it is not used analytically, its role should be reconsidered.

Authors response

We agree that the conceptual framework should be simplified and that the terms retained in the manuscript should have clearly differentiated roles. In the revised manuscript, hydrological coupling will be the principal operational term for the event-scale relationship evaluated from our monitoring data, whereas hydrological connectivity, structural connectivity, and functional connectivity will be used more specifically within the broader conceptual framework.

Structural connectivity will refer to the comparatively stable terrain and surface configuration represented by the IC. We consider the IC appropriate for characterising structural connectivity in this terraced environment because it describes the spatial organisation of landscape elements according to contributing area, slope, downslope routing, and surface impedance. These attributes are particularly relevant in abandoned terraced landscapes, where terrace benches and walls, breaks in slope, vegetation, and surface roughness reorganise potential downslope pathways. The use of the IC for characterising spatial connectivity in this same Mediterranean terraced landscape is also supported by previous site-specific work (Calsamiglia et al., 2017, 2018; Estrany et al., 2019). As clarified in our response to Referee 1, we use this literature to support the structural interpretation of the IC, not to imply that the IC itself measures event-scale functional hydrological connectivity.

We have also reconsidered explicitly the role of the IC in response to the reviewer's recommendation. The IC was calculated before installation of the SM monitoring network and was used to identify contrasting structural settings within the hillslope–terrace system and to inform where the monitoring locations were established. It was not used subsequently as an explanatory variable in the event-scale hydrological analyses, and we do not consider it appropriate to introduce it retrospectively as a predictor of storm-flow occurrence because the study was not designed to test such a relationship. Storm-flow occurrence is an integrated outlet response, whereas the IC characterises the structural setting of individual monitored positions.

For the same reason, we do not propose to introduce a separate numerical comparison of IC values among monitoring positions. The monitoring-position classification was not based on formal numerical IC thresholds, and the exact raster value at each position is not used as an analytical variable. Instead, the revised study-area figure will show the spatial distribution of the IC together with the monitoring locations, allowing the structural setting of each position to be seen directly. The connectivity-based monitoring-position names will be retained, but terms such as “high-connectivity”, “low-connectivity”, and “disconnected” will be defined explicitly as referring to IC-derived structural connectivity of the terrain and surface-routing configuration, rather than to observed event-scale hydrological behaviour. Hydrological behaviour will therefore be evaluated from the soil-water and outlet-discharge observations rather than inferred from the structural labels.

Hydrological coupling will be treated separately as the event-scale relationship between soil-water dynamics at an individual monitoring position and the integrated outlet response. As part of the revision, including complementary analyses developed following the comments of

Referee 1, this relationship is now evaluated through two observable dimensions: (i) correspondence between sensor-specific relative-wetness states and outlet-flow state within precipitation-event response windows, and (ii) the timing of detectable local SM response relative to outlet-discharge onset. These observations quantify whether and when local hydrological responses correspond with the microcatchment outlet response, but they do not demonstrate direct water transfer between an individual monitoring position and the outlet or identify the physical pathway involved.

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. Hydrological coupling therefore provides observational information relevant to the functional component of hydrological connectivity without being treated as equivalent to a directly measured functional flow pathway. We will also remove “hydrological spatial coupling” as a separate technical term and describe differences among monitoring positions instead as spatial variation in hydrological coupling.

The revised Introduction and Methods will therefore establish these definitions before the concepts are used interpretatively, explain the geomorphometric basis and role of the IC more clearly, and define hydrological coupling through the wetness-state and response-timing observations used in the analysis. Functional connectivity will not be presented as a measured quantity, and terminology such as “subsurface connectivity” or “dynamic activation of connectivity” will be removed or reformulated where it implies direct observation of internal pathways. The manuscript title will also be revised to “Spatial variability in hydrological coupling within an abandoned Mediterranean terraced microcatchment”, consistent with this simplified terminology.

2. State explicit working hypotheses. The abstract and the Introduction do not currently present testable hypotheses, and their absence is felt throughout the Discussion, which is organised around themes rather than around propositions being tested.

Authors response

We agree that stating the scientific expectations explicitly would strengthen the alignment between the study objectives, analyses, and interpretation. The submitted manuscript was structured around explicit objectives rather than formal hypotheses, and we do not consider a thematic Discussion inherently problematic. Nevertheless, formulating the corresponding working hypotheses will make the relationships being evaluated more transparent.

The first hypothesis will address temporal and event-scale controls on storm-flow occurrence. We hypothesise that storm-flow occurrence varies with precipitation forcing and antecedent soil-water state, and that the associations with these factors vary among hydrological periods. We further expect storm-flow and non-flow events to differ in the soil-water states attained during the event and in the magnitude of event-scale wetting. As part of the analytical revisions developed in response to Referee 1, antecedent wetness will be treated as a pre-event predictor, whereas maximum wetness and Δ wetness will be treated as within-event responses rather than independent predictors.

The second hypothesis will address spatial variation in hydrological coupling. We expect the relationship between local soil-water responses and the integrated outlet response to differ

among monitored positions, expressed through differences in wetness-state correspondence and in the timing of detectable local soil-water response relative to outlet-discharge onset. This formulation does not imply direct measurement of water transfer or internal pathways.

The two working hypotheses will be stated at the end of the Introduction. We do not propose to reproduce them as formal hypothesis statements in the Abstract. Instead, the Abstract will summarise the corresponding study questions and relationships evaluated. The Discussion will retain a thematic organisation, because this allows the temporal/event-scale and spatial findings to be integrated rather than treated as isolated tests, but its structure will be clarified so that these two components map explicitly onto the corresponding working hypotheses. The Conclusions will likewise be aligned with these two components of the study.

3. *Realign the claims with the evidence. Where coupling, connectivity or lateral flow activation are invoked, either present the supporting measurement or reformulate the statement as a hypothesis consistent with the observations. I would suggest concentrating the scientific contribution on the seasonal and event scale results that the data do support.*

Authors response

We agree that the strength of the claims should be matched more closely to what is directly supported by the observations. The submitted manuscript sometimes used terms such as “activation of connectivity”, “functional connectivity”, or lateral-flow language in ways that implied direct observation of internal water-transfer pathways.

We will therefore distinguish explicitly among directly observed relationships, the operational assessment of hydrological coupling, and mechanistic interpretation. The directly supported contribution comprises the seasonal and event-scale controls on storm-flow occurrence, the relative roles of precipitation forcing and antecedent wetness, the within-event soil-water response, and spatial variation in the correspondence and timing between local soil-water responses and the integrated outlet response.

Hydrological coupling will be retained only in the operational sense defined in response to Recommendation 1: an event-scale local-to-outlet relationship evaluated through wetness-state correspondence and response timing. These analyses provide an observational basis for evaluating spatial variation in hydrological coupling, but they do not demonstrate direct source-to-outlet water transfer or identify the physical pathways involved.

We agree that statements invoking lateral subsurface flow, fill-and-spill behaviour, saturation-excess runoff, infiltration-excess runoff, or activation of internal flow pathways should not be presented as demonstrated processes. Where these interpretations remain useful in the Discussion, they will be reformulated explicitly as mechanistic hypotheses consistent with the observations and with the relevant literature. Unsupported pathway language will be removed elsewhere in the manuscript where it implies direct observation of processes that were not measured.

We agree that the seasonal and event-scale results represent an important component of the scientific contribution and will ensure that they are presented clearly and prominently. However, following the additional analyses developed during the revision in response to Referee 1, we do not consider it necessary to reduce the role of the spatial hydrological-coupling

component in the manuscript. The revised wetness-state correspondence and response-timing analyses provide an explicit observational basis for evaluating spatial variation in hydrological coupling among the monitored positions, while maintaining clear limits on what can be inferred about internal water-transfer pathways. We will therefore retain the overall framing of the manuscript around both temporal/event-scale controls on storm-flow occurrence and spatial variation in hydrological coupling, rather than treating the latter as a secondary component.

Claims throughout the manuscript will be revised accordingly so that directly observed relationships, operational hydrological-coupling analyses, and mechanistic hypotheses are clearly separated.

4. *Address the data limitations explicitly. Report any comparison between the two water level sensors, or state clearly that no overlap exists and discuss the consequences for the homogeneity of the series. Clarify the calibration status of the SM sensors and, if no site-specific calibration was performed, reframe all thresholds as relative. Report the outcomes of the normality, variance and multiple comparison procedures.*

Authors response

We agree that these aspects of the measurement and statistical framework should be made explicit. Their implications differ, however, as the absence of overlap between outlet sensors constrains direct assessment of instrument equivalence, whereas the SM calibration issue primarily constrains comparisons of absolute volumetric thresholds among individual monitoring positions. The statistical issue concerns transparency of the test-selection and multiple-comparison procedures. These points are addressed in greater detail in SC-M11, SC-M12, and SC-M15 below.

For the outlet stage record, there was no overlap period between the CS451-L and OTT PLS sensors because the former was replaced following an unexpected malfunction rather than through a planned transition. Consequently, no direct cross-calibration or simultaneous comparison between the two instruments was possible, and this will be stated explicitly as a limitation. Both instruments were nevertheless installed at the same outlet measurement point, referenced to the same dry-flume zero, and stage was converted to discharge using the same rating relationship for the fixed H-flume geometry. Low-amplitude coherent storm-flow responses were detected with both instruments, although, in the absence of overlapping measurements, we cannot demonstrate that their practical detection limits were identical. Representative low-amplitude hydrographs from each sensor period are presented in our response to SC-M12 below.

For the SM measurements, factory calibration without site-specific calibration does not justify interpreting absolute volumetric values as directly comparable hydrological thresholds among monitoring positions with contrasting soil properties. We will therefore remove the submitted position-specific absolute SM thresholds rather than replace them with formal “relative thresholds”. The treatment of SM now differs according to the analytical question. In the broad event-scale comparison presented in Table 2, volumetric SM is retained after aggregation by depth across the sensor network, because these analyses compare storm-flow and non-flow events at the network/depth level rather than defining position-specific absolute thresholds. For the spatial analyses comparing individual monitoring positions, each sensor record is instead transformed to a sensor-specific relative-wetness distribution, so that the corresponding metrics

describe the position of each observation within that sensor's own observed wetness distribution. These position-specific analyses are therefore expressed as relative-wetness percentiles and percentile-point changes in Supplementary Tables S1–S3 and in the revised spatial figures.

We have also revised and rerun the statistical comparisons. Normality is assessed separately for storm-flow and non-flow events using Shapiro–Wilk tests, and homogeneity of variance using Levene's test. Student's two-sample t -test is used when both groups satisfy normality and variances are homogeneous, Welch's two-sample t -test when both groups satisfy normality but variances are heterogeneous, and the Mann–Whitney U test when either group departs from normality. No inferential comparison is conducted when either event class contains fewer than three observations. Consequently, no test is performed for the dry period, which contains only one storm-flow event.

The multiple-comparison procedure has also been made explicit. For Table 2, p -values are adjusted using the Benjamini–Hochberg false-discovery-rate procedure separately for the overall analysis and for each hydrological period across the 11 event-scale metrics. The revised table identifies the statistical test and BH-adjusted p -value for each comparison. For the sensor-specific spatial analyses in Supplementary Tables S1–S3, the correction is applied separately within each hydrological period across the 44 combinations of four relative-wetness metrics and 11 monitoring positions. The corresponding Shapiro–Wilk and Levene diagnostics, selected tests, raw p -values, and adjusted p -values will be reported in the Supplementary Material so that the test-selection and multiple-comparison procedures can be verified directly.

The Methods, Results, and limitations discussion will be revised accordingly so that the consequences of the lack of sensor overlap, the specific scope of the SM calibration constraint, and the complete statistical decision procedure are distinguished clearly rather than treated as equivalent limitations.

5. *Consider an alternative designation for the events analysed. The use of flood events is consistent with the definition of flood in the International Glossary of Hydrology (WMO/UNESCO, 2011), so this is a suggestion for clarity rather than a correction: since the term is widely associated in practice with inundation processes, “storm flow events” or “runoff generating events” might convey the intended meaning with less ambiguity, with non-flow events retained as the complementary category.*

Authors response

We thank the reviewer for this suggestion. We agree that “storm-flow events” describe the observed outlet response more clearly and avoid the broader connotations commonly associated with “flood events”. We will therefore replace “flood events” with “storm-flow events” throughout the manuscript and retain “non-flow events” for the complementary event class.

We will also apply this terminology consistently in the text, tables, figures, and captions. “Runoff” will be retained when referring to the hydrological process, runoff volume, runoff coefficient, or runoff-generation mechanisms, rather than as the designation of the event class.

6. *Restructure the Discussion, removing duplication and perhaps adopting a nested temporal and spatial organisation, and remove or clearly delimit the passages on*

vegetation, abandonment, afforestation and wildfire that are not supported by the observations.

Authors response

We agree that the Discussion can be substantially streamlined. In the submitted manuscript, temporal, event-scale, and spatial interpretations are partly repeated across Sects. 4.1 and 4.2. In particular, spatial differences in soil-water response are introduced in Sect. 4.1.3 and then revisited in greater detail in Sect. 4.2, while seasonal influences on these spatial relationships are discussed again in Sect. 4.2.3.

We will therefore reorganise the Discussion into a clearer temporal-to-spatial sequence. The first part will focus on seasonal and event-scale controls on storm-flow occurrence, including precipitation forcing, antecedent soil-water status, and within-event wetting responses. The second part will focus on spatial variation in local-to-outlet hydrological coupling, using the revised wetness-state correspondence and onset-lag analyses. The spatial material currently divided between Sects. 4.1.3 and 4.2 will therefore be consolidated into a single coherent section rather than discussed repeatedly at different points in the Discussion. This organisation will also align the Discussion more clearly with the two working hypotheses introduced in response to Recommendation 2.

We also agree that vegetation should not be invoked as a demonstrated control where it was not measured analytically. Statements attributing spatial or seasonal hydrological behaviour to canopy cover, rooting patterns, interception, or plant water use will therefore be removed or, where useful, retained only as clearly identified hypotheses consistent with the literature. Atmospheric water demand will be treated separately because reference evapotranspiration was estimated from the monitored meteorological variables and is directly relevant to the observed seasonal evolution of SM.

Agricultural abandonment, afforestation, terrace degradation, and wildfire will be retained where they are needed to describe the environmental and land-use context of the study, but they will not be presented as explanatory factors tested by the present dataset. In particular, wildfire will be treated as environmental history rather than as a causal control evaluated in this study. Broader land-use implications will be reduced and clearly separated from interpretation of the measured hydrological responses.

These revisions will shorten the Discussion, remove duplication, and clarify the distinction between directly supported temporal and spatial findings, measured environmental context, and broader mechanistic or land-use hypotheses.

7. Rework the figures. Increase lettering size throughout, merge or differentiate Figure 1 and Figure 2, add a dedicated figure of the experimental design, add photographs of the instrumented positions, and add a scheme clarifying the connectivity settings referred to in the text.

Authors response

We agree that the figures should be reworked to improve both readability and the presentation of the experimental design. We will increase lettering, axis-label, legend, and annotation sizes throughout the manuscript.

We agree that Figs. 1 and 2 should have more clearly differentiated functions, although we do not consider it necessary to merge them. Fig. 1 will be retained as the study-area and structural-context figure. The current wildfire panel will be replaced by a panel showing the IC together with the monitoring locations, while the current photographic panels will be removed. This will allow the IC to be presented explicitly in its revised role as a terrain-based descriptor of structural connectivity and of the setting used to inform monitoring-location selection, rather than as a measure of event-scale hydrological coupling.

Fig. 2 will be redesigned as a dedicated experimental-design figure based on photographs of the monitored hillslope-terrace sector. It will include an annotated overview photograph showing all eight monitoring locations at their approximate physical positions, together with enlarged photographs of the three monitoring positions highlighted in the main text to illustrate their contrasting geomorphic settings. The overview and caption will also clarify the relationship between the eight monitoring locations and the eleven soil-water sensors, including the locations instrumented at both 10 and 50 cm depth.

We do not propose to add a separate schematic assigning the monitoring positions to hydrological connectivity classes. Consistent with our response to Recommendation 1, the existing connectivity-based monitoring-position names will be retained because they describe the IC-derived structural setting used in the original monitoring design. Terms such as “high-connectivity”, “low-connectivity”, and “disconnected” will therefore be defined explicitly as structural classifications derived from the IC, not as observations of event-scale hydrological or functional connectivity. Together, the IC panel in Fig. 1 and the annotated experimental-design photograph in Fig. 2 will make the basis and physical meaning of these classifications explicit without introducing an additional schematic.

Detailed cartographic information that is useful for documenting the exact monitoring design but not essential to the main narrative may be transferred to the Supplement.

Specific comments

Abstract

SC-A1 – L22-23: in the statement that legacy features such as stone walls modify storage and flow pathways, the verb may understate the role you attribute to these features elsewhere. Walls appear to do more than modify storage: they determine, modulate, define or configure it, and they impose the geometry of the flow pathways. A stronger verb might better convey the active control you describe.

Authors response

We agree that the sentence should describe the structural role of terrace walls more explicitly. However, we would avoid stronger causal terms such as “determine” or “impose”, because the present study does not directly quantify the extent to which individual walls control storage or specific water-transfer pathways. We will instead revise the wording to describe legacy terrace structures such as dry-stone walls as configuring local water storage and potential flow-routing pathways. This conveys their structural role while remaining consistent with the distinction between terrain-based structural connectivity and event-scale hydrological coupling. The Abstract will therefore not imply that their effect on hydrological coupling or particular flow pathways was demonstrated directly by the present observations.

SC-A2 – L24: after the word “repellence” the construction becomes somewhat ambiguous. You might consider replacing the comma with a subordinating link, for example: “[...] and water repellence, hence lowering infiltration capacity, enhancing R generation [...]”. In the same sentence you refer to reorganising connectivity during a post fire window. What do you mean by connectivity here, and how is it defined? This is the first appearance of a term that carries much of the argument.

Authors response

We agree that the original sentence is both syntactically awkward and conceptually too compressed. Rather than making a local grammatical correction, we will substantially revise this part of the Abstract.

Consistent with the broader revision of the wildfire framing, the generic post-fire sequence involving vegetation loss, water repellence, reduced infiltration capacity, enhanced runoff generation, and “reorganisation” of connectivity will be removed from the Abstract because these processes were not evaluated directly in the present study. The 2013 wildfire will instead be retained, where relevant, as part of the environmental history of the study site.

This revision will also resolve the premature introduction of connectivity terminology. Structural connectivity and hydrological coupling will only be introduced where they can be defined explicitly and linked to their respective roles in the study.

SC-A3 – L28: you introduce Q and use R as a separate quantity throughout. What exactly distinguishes the two in this study? R is not measured anywhere as a flux in its own right, and it is not defined, for example, as surface R generated within a plot. Short definitions, even in the abstract, would be welcome.

Authors response

We agree that the distinction between runoff and discharge was not sufficiently clear in the submitted manuscript. Outlet discharge is the quantity measured directly at the H-flume, whereas runoff depth is derived by integrating outlet discharge over the relevant event, season, or year, and normalising the resulting volume by the microcatchment area. Runoff is therefore not an independently measured internal flux in this study and does not refer to surface runoff measured at a plot or internal catchment location.

Rather than adding potentially cumbersome symbol definitions to the Abstract, we will simplify the terminology. The revised Abstract will refer explicitly to outlet discharge, storm-flow occurrence, and storm-flow/non-flow events, without introducing R and Q as separate abbreviated quantities. This removes the ambiguity between the two quantities without requiring separate single-letter definitions in the Abstract. The Methods will define the quantitative relationship between measured outlet discharge, derived runoff depth, and the runoff coefficient.

We will apply the same distinction throughout the manuscript. “Storm-flow occurrence” or “outlet response” will be used for the binary event classification, “outlet discharge” for the measured flow rate, and “runoff depth” for discharge integrated through time and normalised by microcatchment area. References to runoff generation will be retained only where the hydrological process itself is being discussed and will not imply that runoff was measured independently at internal locations.

SC-A4 – L32: what are partially coupled conditions? The expression appears before any definition of coupling has been given.

Authors response

We agree. “Partially coupled conditions” was not operationally defined and implies a categorical coupling state for which no explicit criterion was established. We will therefore remove this terminology.

Moreover, the result to which this expression was attached will itself be revised following the new storm-flow-occurrence analysis. Rather than relating the predictive contribution of antecedent wetness to an undefined degree of coupling, the revised Abstract will state that precipitation forcing, particularly total precipitation, was the most consistently supported predictor of storm-flow occurrence, whereas the additional information provided by antecedent wetness was strongly influenced by the exceptionally wet November 2021 sequence. Hydrological coupling, where retained in the Abstract, will refer separately to the observed correspondence between soil-water states at monitored positions and outlet discharge and to their relative response timing, rather than to qualitative categories such as partially coupled, coupled or uncoupled conditions.

SC-A5 – L33: in the reference to weak coupling between local SM and outlet Q , what does local SM denote? The SM at one sensor, at one position, an average across positions within a plot or similar land units?

Authors response

We agree that “local SM” is ambiguous. In the submitted Abstract, it referred to the soil-water measurements from individual monitoring positions that were compared with outlet discharge, rather than to an average across sensors or landscape units.

However, this sentence will be revised more substantially because the new wetness-state and onset-lag analyses provide a more explicit basis for evaluating the local-to-outlet relationships than the qualitative description of “weak coupling” used in the submitted Abstract. We will therefore replace ambiguous references to “local SM” with expressions such as “sensor-specific soil-water state at an individual monitoring position” or “soil-water response at an individual monitoring position”, and will describe the observed spatial differences directly in terms of wetness-state correspondence with outlet discharge and relative response timing.

SC-A6 – L34: similarly, what is meant by well-connected positions and by disconnected mid-terrace areas? A reader encountering these expressions for the first time may find lines 34 to 35 difficult to follow.

Authors response

We agree that these expressions are difficult to interpret when encountered for the first time in the Abstract. In the submitted manuscript, terms such as “well-connected” and “disconnected” referred to the IC-derived structural setting of the monitoring positions, rather than to event-scale hydrological behaviour observed from the soil-water and outlet-discharge measurements. This distinction was not clear in the Abstract.

We will therefore remove these connectivity-based descriptions from the revised Abstract, where their meaning cannot be introduced adequately without unnecessary methodological

detail. The connectivity-based monitoring-position names will nevertheless be retained elsewhere in the manuscript, consistent with our response to Recommendation 1, and will be defined explicitly at first use as referring to terrain-based structural connectivity represented by the IC, not to observed event-scale hydrological behaviour.

The surrounding result statement in the Abstract will also be reformulated. Rather than describing particular positions as “well-connected” or “disconnected”, or characterising terrace behaviour as “storage-controlled”, the revised Abstract will report the directly observed spatial differences in wetness-state correspondence with outlet discharge and in the timing of detectable soil-water responses relative to outlet-discharge onset. Mechanistic explanations involving storage or specific flow pathways will be reserved for the Discussion and presented, where appropriate, as hypotheses consistent with the observations.

The IC will therefore remain a terrain-based descriptor of structural connectivity and of the monitoring-design context, but it will not be interpreted as an event-scale hydrological-connectivity class.

SC-A7 – L35: you state that R emerges from the interaction between P forcing, evolving SM and spatially variable structural connectivity. What of Q in this formulation? The relationship between R and Q remains unclear at this point.

Authors response

We agree that the distinction between runoff and outlet discharge is unclear in this formulation. As described in response to SC-A3, we will remove the use of R and Q as separate abbreviated quantities from the revised Abstract and instead refer explicitly to storm-flow occurrence and outlet discharge. The event outcome evaluated in the study is the presence or absence of measurable outlet discharge, while runoff depth is subsequently derived from the outlet-discharge record and is not a separately measured internal flux.

We will also revise the substantive interpretation of this sentence. The submitted wording implied that storm-flow occurrence was demonstrated to emerge from an interaction among precipitation forcing, evolving soil-water conditions, and structural connectivity. The revised analyses support a more cautious interpretation. Precipitation forcing, particularly total precipitation, was the most consistently supported predictor of storm-flow occurrence. Antecedent wetness provided additional information in the complete record, but this support was strongly influenced by the exceptionally wet November 2021 sequence. The spatial component will be addressed separately through observed differences in the correspondence and timing between soil-water responses at individual monitoring positions and outlet discharge.

Structural connectivity will likewise be treated separately as a terrain-based property of the monitored setting rather than as a causal control demonstrated by the event-occurrence analysis.

SC-A8 – L36: the call for connectivity-aware, event-scale approaches builds on terminology that has not yet been introduced, which compounds the difficulty.

Authors response

We agree that “connectivity-aware” is too compressed and insufficiently defined in the Abstract. We will therefore remove this terminology.

The concluding statement of the revised Abstract will instead synthesise the relationships directly supported by the study, emphasising precipitation forcing as the most consistently supported predictor of storm-flow occurrence, the more conditional contribution of antecedent soil-water state, the within-event soil-water responses, and the observed spatial variation in local-to-outlet hydrological correspondence and response timing. Structural connectivity and hydrological coupling will be defined explicitly in the main text rather than condensed into an undefined “connectivity-aware” label in the Abstract.

SC-A9: General point on the abstract: no working hypotheses are stated. Given that the paper is framed around the conditions under which coupling is or is not activated, explicit hypotheses would help the reader considerably. It would also help to introduce connectivity by relating it to flow paths or water pathways before the term is used analytically.

Authors response

We agree that the scientific expectations underlying the study should be clearer. As described in response to Recommendation 2, two explicit working hypotheses will be added at the end of the Introduction. We do not propose to reproduce these as formal hypothesis statements in the Abstract. Instead, the revised Abstract will state more clearly the two questions addressed by the study: first, how precipitation forcing, antecedent soil-water state, and within-event soil-water responses relate to storm-flow occurrence across hydrological periods; and second, how the relationship between soil-water responses at individual monitoring positions and the integrated outlet response varies spatially.

We also agree that connectivity terminology should not be used before its meaning is clear. The revised Abstract will therefore avoid introducing an undefined general concept of connectivity. The structural context will first be described through terrain and terrace features that configure potential flow-routing pathways. The spatial hydrological component will then be described through the correspondence between soil-water states at individual monitoring positions and outlet discharge, together with the relative timing between local soil-water response and outlet-discharge onset. Where the term hydrological coupling is retained, it will refer specifically to this operationally evaluated local-to-outlet relationship rather than to direct observation of internal water-transfer pathways.

We will therefore not frame the study in terms of coupling simply being “activated” or “not activated”. The revised analysis treats hydrological coupling as a spatially variable observational relationship rather than as a predefined binary internal state.

Introduction

SC-II – L64: where you refer to the transient filling of soil storage, I think “soil storage capacity” would be the more precise expression.

Authors response

We thank the reviewer for this suggestion. We agree that the original wording can be made more precise, but we do not consider “soil storage capacity” to be the appropriate replacement in this context. Storage capacity refers to the maximum amount of water that can be stored, whereas the sentence refers to the dynamic filling of soil-water storage during an event. We will therefore revise the expression to “transient filling of soil-water storage”.

SC-I2 — L68-70: you argue that focusing on flow events risks overlooking the hydrological relevance of non-flow events, and that non-flow events inform the conditions under which spatial coupling fails to develop. Relevance in what sense? The point would repay being made explicit because, in my view, this is one of the stronger arguments in the paper.

Authors response

We agree that the hydrological relevance of non-flow events should be stated more explicitly. In the submitted Introduction, their role was described mainly in terms of conditions under which coupling “fails to develop”. We now consider this formulation too strong, because the absence of measurable storm flow at the outlet does not by itself demonstrate that internal hydrological connections failed to develop.

Their principal analytical value is that they provide the complementary cases needed to identify which combinations of precipitation forcing and soil-water conditions are associated with the presence versus absence of measurable storm flow at the microcatchment outlet. If only storm-flow events are considered, conditions observed during those events may appear important simply because there is no comparison with events in which similar conditions occurred without an outlet response. Retaining non-flow events therefore allows the overlap and separation between the two event classes to be quantified and provides the basis for the probabilistic analysis of storm-flow occurrence using both observed outcomes, rather than conditioning the analysis of events that already produced flow.

Non-flow events are also important for the wetness-state component of hydrological coupling, because they allow us to determine whether soil-water states observed at individual monitoring positions during periods of measurable outlet discharge also occur when outlet discharge is absent. This comparison helps distinguish local wetting from local-to-outlet hydrological correspondence and provides one of the two observational dimensions used to evaluate hydrological coupling, together with relative response timing.

We will revise this part of the Introduction accordingly, giving greater prominence to non-flow events as the reference condition required to evaluate both storm-flow occurrence and hydrological coupling, while removing the implication that they simply represent conditions in which coupling “fails to develop”.

SC-I3 — L70-71: the observation that ignoring these events may bias the identification of controlling factors and limit the interpretation of state dependent responses is well made, and I would encourage you to give it greater prominence.

Authors response

We agree that this point deserves greater prominence. In the submitted Introduction, the potential bias introduced by considering only events with measurable outlet flow was stated briefly, although it provides an important rationale for the event-based design of the study.

We will therefore strengthen this argument in the revised Introduction. Specifically, we will make explicit that restricting the analysis to storm-flow events conditions the dataset on a positive outlet response and removes the contrasting cases needed to determine whether particular precipitation or antecedent soil-water conditions are specifically associated with storm-flow occurrence or are also common during events without measurable outlet flow. Including non-flow events consequently provides the reference condition required to evaluate

candidate predictors of storm-flow occurrence and to identify state-dependent differences in event response.

This rationale will be given greater prominence in the Introduction and linked directly to the unified analysis of storm-flow and non-flow events, rather than being framed primarily in terms of whether coupling “fails to develop”.

SC-I4 – L73-74: you state that addressing this limitation is essential for advancing a probabilistic understanding of R occurrence. Why probabilistic specifically, and on what basis? An example or an indication of the intended application would clarify the ambition.

Authors response

We agree that the use of “probabilistic” was insufficiently justified in the submitted Introduction. The term was intended to distinguish the likelihood of storm-flow occurrence under different event conditions from a deterministic threshold interpretation, but the submitted manuscript did not include an explicit event-probability model.

This limitation is now addressed by a new binomial logistic analysis. Storm-flow occurrence is treated as a binary event outcome, and its probability is modelled as a function of precipitation characteristics and antecedent soil-water state. The joint inclusion of storm-flow and non-flow events therefore provides the two outcomes required to quantify how the likelihood of a measurable outlet response changes across the observed range of event conditions.

We will revise the Introduction so that the term “probabilistic” is used only in this explicit sense. Rather than presenting it as a general ambition, we will link it directly to estimating the probability of storm-flow occurrence and to evaluating whether antecedent soil-water state provides information beyond precipitation forcing. We will not imply that the resulting models constitute an operational forecasting tool. Their purpose is to quantify event-occurrence relationships within the observed dataset.

SC-I5 – L75-79: hydrological connectivity is presented as a useful framework, and structural and functional connectivity are then distinguished, although connectivity itself is not defined in objective, measurable terms. The relationship between hydrological connectivity and hydrological coupling is also left open: as used here they appear closely related, if not equivalent. It may be worth considering whether both are needed, and whether the set of terms could be simplified. Whichever terms are retained would benefit from being defined by reference to a metric that can be evaluated with the data available.

Authors response

We agree that the submitted Introduction introduced several closely related terms without sufficiently distinguishing their conceptual and operational roles. In particular, hydrological connectivity, structural connectivity, functional connectivity, and hydrological coupling were introduced in close succession, making it unclear whether they represented distinct concepts or partly overlapping terminology.

We will therefore simplify and reorganise this conceptual framework. Hydrological connectivity will be retained as the broader literature concept describing water-mediated linkage among spatially separated parts of a catchment. Within this broader framework, structural connectivity will refer to the comparatively stable terrain and surface configuration

that conditions the potential for hydrological linkage, whereas functional connectivity will refer to the dynamic, event-dependent expression of that linkage under particular hydrological conditions.

Structural connectivity will be characterised using the IC, which provides a terrain-based descriptor of the structural setting of the monitoring positions. The IC will therefore be treated as a metric of structural connectivity, but not as evidence that water transfer actually occurred between a monitoring position and the outlet during a particular event.

Hydrological coupling will be retained as the principal operational term for the local-to-outlet relationship that can be evaluated directly from our monitoring data. It will refer to the correspondence between soil-water dynamics at an individual monitoring position and the integrated outlet response, assessed through two complementary observable dimensions: (i) wetness-state correspondence with outlet-flow state, evaluated by comparing sensor-specific relative-wetness distributions during periods with and without measurable outlet discharge within precipitation-event response windows; and (ii) response timing, quantified as the lag between detectable local soil-water response and outlet-discharge onset. These two dimensions will be evaluated separately rather than combined into an arbitrary single coupling index.

We consider hydrological coupling to provide an observationally constrained expression relevant to the functional component of hydrological connectivity. Greater wetness-state correspondence and closer response timing indicate stronger observed local-to-outlet coupling and are therefore consistent with stronger functional hydrological linkage. However, they do not demonstrate continuous water transfer between an individual monitoring position and the outlet, identify the physical pathway through which such transfer occurs, or constitute a direct measurement of functional connectivity itself.

The revised manuscript will therefore use hydrological coupling 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”. Hydrological connectivity will provide the broader conceptual framework, structural connectivity will be represented explicitly through the IC, and functional connectivity will describe the dynamic event-dependent linkage to which the observed hydrological coupling is relevant, without being presented as a directly measured variable.

SC-I6 – L92-93: I would suggest “[...] and induced significant and time dependent changes [...]”, rather than “[...] and induced significant but time dependent changes [...]”. Note also that infiltration capacity is itself one of the soil properties listed, so the enumeration is not strictly parallel.

Authors response

We agree that “and” is more appropriate than “but” in this context and that the original enumeration is not strictly parallel, because infiltration capacity is itself a soil property. We will therefore revise the sentence to refer to significant and time-dependent changes in vegetation cover and soil properties, including infiltration capacity.

SC-I7 – L96-98: the statement that the interaction of legacy effects with structural features and with seasonally varying SM demands further investigation is well founded, and I agree with it.

Authors response

We thank the reviewer for this positive assessment.

Study area and methods

SC-M1 – L129-137: the description of the different land units would benefit greatly from photographs of each, and from a schematic representation of the whole system. As it stands the reader must reconstruct the setting from text alone.

Authors response

We agree that the physical arrangement of the monitored hillslope-terrace system is difficult to reconstruct from text alone. As described in response to Recommendation 7, we will therefore revise Figs. 1-2 to provide a clearer visual representation of the monitored land units and their spatial organisation.

In particular, Fig. 2 will be redesigned as a dedicated experimental-design figure based on photographs of the monitored hillslope-terrace sector. It will include an annotated overview photograph showing all eight monitoring locations at their approximate physical positions, together with enlarged photographs of the three monitoring positions highlighted in the main text, which represent contrasting geomorphic and structural settings. The figure and caption will also distinguish clearly between the eight monitoring locations and the 11 SM sensors, including the locations instrumented at both 10 and 50 cm depth.

Rather than adding a separate photograph for every monitoring position and an additional schematic, the annotated overview will provide the whole-system representation requested by the reviewer, while the enlarged photographs will provide more detailed visual examples of the contrasting monitored settings. In parallel, Fig. 1 will show the broader study-area and structural context, including the IC and the monitoring locations.

Together, the revised Figs. 1 and 2 will allow the reader to identify the monitored hillslope, terrace-edge, terrace-interior, and wall-associated settings and understand their spatial relationship without having to reconstruct the system from text alone. The existing connectivity-based monitoring-position names will be retained and defined explicitly as descriptors of IC-derived structural connectivity, consistent with our response to Recommendation 1.

SC-M2 – L136-137: you describe terraces as exerting a persistent ecogeomorphological and hydrological control on surface R, sediment redistribution and SM patterns, despite variable preservation. What is meant by preservation in this context, and what is the variability being referred to? The sense of the term is not entirely clear here.

Authors response

We agree that the meaning of “variable preservation” was not sufficiently clear. We were referring to spatial differences in the present structural condition of the abandoned terrace structures, particularly the dry-stone walls, including both well-preserved and partially or fully collapsed sections. We will make this explicit in the revised manuscript and replace the ambiguous expression “variable preservation” with a more precise description of terrace structural condition.

SC-M3 – L140: the statement that these soils exhibit poorly developed profiles may not be readily meaningful to readers outside soil science. A short explanation in terms of observable or measurable attributes would help.

Authors response

We agree that “poorly developed profiles” is too general and may not be readily interpretable outside soil science. We will replace this expression with a more explicit description of the soil characteristics, namely their shallow profiles with limited horizon development over carbonate bedrock, high stone content, and low water-holding capacity.

SC-M4 – L143-145: you report that terraced soils have higher carbonate concentrations and alkalinity but lower organic matter, cation exchange capacity and biological activity than hillslope soils. Why is this so? Some explanation of the process would be valuable.

Authors response

We agree that some interpretive context for these soil contrasts would be useful. However, the processes responsible for them were not tested in the present hydrological study, so we will avoid introducing a detailed mechanistic explanation that extends beyond the evidence available here.

The reported differences derive from previous soil studies in the same study area and are interpreted primarily in the context of persistent land-use legacies associated with former cultivation and incomplete soil recovery after abandonment. Despite several decades of abandonment, the terraced soils retain higher carbonate contents and alkalinity together with lower organic matter, cation exchange capacity, and biological activity than the adjacent non-terraced hillslope soils.

We will therefore revise the passage to provide this land-use and post-abandonment context, while making clear that the present study reports these properties as characteristics of the contrasting soil settings rather than testing the processes responsible for their development.

SC-M5 – L152: you state that terraces support denser and more continuous vegetation. How is this reconciled with the lower organic matter content reported at lines 143 to 145? The two statements appear to be in tension, and a brief clarification would resolve the apparent contradiction.

Authors response

We agree that this distinction should be made explicit, although the two observations are not inherently contradictory because they refer to different ecosystem components. The “organic matter” referred to at lines 143-145 is soil organic matter content, measured from mineral soil samples after removal of the surface litter, rather than the amount of litter or organic material accumulated above the soil surface.

The statement at line 152, by contrast, describes the current aboveground vegetation structure around the monitored hillslope–terrace sector, with terraced areas supporting denser and more continuous vegetation than the adjacent non-terraced hillslopes. Greater present-day vegetation cover therefore does not necessarily imply a greater concentration of organic matter in the sampled mineral soil, because aboveground vegetation structure and mineral-soil organic

matter represent different ecosystem compartments and integrate present and historical controls over different timescales.

The lower soil organic matter reported for terraced soil has instead been interpreted as part of the persistent legacy of former agricultural use and incomplete post-abandonment soil recovery, which can remain detectable despite substantial vegetation recovery. We will revise the text to make this distinction between current aboveground vegetation cover and soil organic matter content in the mineral topsoil explicit and thereby clarify why the two observations can coexist.

SC-M6 — Figure 1: the lettering throughout is small and not easy to read; Figure 1(b) is particularly demanding, since the panels are small and the labels smaller still. In Figure 1(c) there are marks that are not explained in the caption. In Figure 1(d) you identify the AEMET reference meteorological station, although your own experimental meteorological station does not appear anywhere in the figure, and its location is important. In the caption to Figure 1(b) the meteorological station is again referred to without specifying which one is meant: the AEMET reference station or your own. Showing both, and distinguishing them explicitly, would remove the ambiguity.

Authors response

We thank the reviewer for highlighting that the distinction between the two meteorological stations was not sufficiently clear in the submitted figure and caption. We would like to clarify that both stations were already represented in the submitted Fig. 1, as panel (b) showed the external B118 AEMET reference meteorological station, whereas the blue triangle in panels (c, d, f) identified our experimental meteorological station within the So na Vidala microcatchment. However, we agree that the figure and caption did not distinguish these two stations clearly enough.

As described in our response to Recommendation 7, Fig. 1 will be substantially redesigned as the study-area and structural-context figure. In the revised figure, the B118 AEMET reference station and the So na Vidala experimental meteorological station will be shown and labelled separately and unambiguously, and the caption will state explicitly which station is represented in each relevant panel. Symbols associated with the SM monitoring locations, experimental meteorological station, and outlet gauging station will likewise be identified explicitly rather than requiring interpretation from another panel.

The redesign will also address the readability issues noted by the reviewer by increasing lettering and symbol sizes and simplifying the panel composition where necessary, particularly for the information currently concentrated in panel (b). Together with the redesigned Fig. 2 described in Recommendation 7, this will provide a clearer distinction between the broader study-area context and the experimental monitoring setup.

SC-M7 — L180: you report a total of eleven SM sensors, whereas Figure 2 shows eight probe locations. It would help to reconcile the two.

Authors response

We thank the reviewer for identifying this ambiguity. The monitoring network comprised 11 SM sensors distributed across eight monitoring locations. In the submitted Fig. 2, the eight symbols represented monitoring locations rather than individual sensors. Five locations were

instrumented with a single sensor at 10 cm depth, whereas three locations contained paired sensors at 10 and 50 cm depths, resulting in 11 sensors across the eight locations.

As described in our responses to Recommendation 7 and SC-M1, Fig. 2 will be redesigned to present the experimental setup more clearly. The revised Methods, Fig. 2 and its caption will explicitly distinguish the eight monitoring locations from the 11 individual sensors and identify which locations were instrumented at both 10 and 50 cm depth.

SC-M8 – L181-182: you state that pairs of sensors were installed horizontally at 10 and 50 cm depth in order to capture vertical variability. What does installed horizontally mean in this context? Depth cannot be interpreted horizontally, and the description is difficult to follow. A diagram of the installation would resolve this. The same wording recurs at lines 188 to 189.

Authors response

We agree that the installation description should be clearer. However, probe orientation and measurement depth refer to different aspects of the installation and are not contradictory: “horizontal” or “vertical” describes the orientation of the probe rods, whereas 10 and 50 cm describe their nominal measurement depths below the soil surface.

At the three monitoring locations instrumented at two depths, the probe rods were oriented horizontally within the soil profile at nominal depths of 10 and 50 cm below the soil surface. At the five locations monitored only at 10 cm, the probe rods were oriented vertically, with the sensor installed to represent the nominal 10 cm depth.

We will revise both occurrences in the Methods so that this distinction is explicit and clarify that the paired installations were intended to characterise soil-water dynamics at two vertical levels. We do not consider a separate installation diagram necessary once the orientation and depth are described unambiguously in the Methods. As described in our responses to Recommendation 7 and SC-M1, the redesigned Fig. 2 and its caption will distinguish the three paired-depth locations from the five locations monitored only at 10 cm.

SC-M9 – L193-196: you state that the monitoring locations were selected to capture structural variability within the hillslope and terrace system and the associated contrasts in structural hydrological connectivity. Two difficulties arise. First, the monitored positions are a small number of points clustered in the southern part of the catchment, so that the greater part of the catchment area (of which the western sector is one portion) carries no observations at all; these positions are therefore unlikely to represent the catchment as a whole. Second, and more fundamentally, these are vertical SM profiles at points. They provide no information on horizontal transfer of water, whether at the surface or in the subsurface, and no surface R, channel flow or flow along tracks is measured anywhere within the catchment. On what basis, then, can hydrological connectivity or coupling be assessed from these observations? If you consider that they can, the justification would be worth setting out explicitly in the manuscript.

Authors response

We agree with both points raised by the reviewer. First, the eight monitoring locations should not be presented as spatially representative of the entire 4.7-ha microcatchment. They constitute targeted observations within the instrumented hillslope-terrace sector and were selected to sample contrasting geomorphic and IC-derived structural settings within that sector, rather than

to represent the full spatial variability of the microcatchment. We will revise the Methods and Discussion to make this limitation explicit and avoid extrapolating the behaviour of these locations to unmonitored parts of the microcatchment.

Second, the soil-water measurements at individual monitoring positions, including the paired-depth observations at three locations, do not directly quantify horizontal water transfer between locations, either at the surface or in the subsurface. Nor does the monitoring network identify internal flow pathways or determine which parts of the microcatchment supplied water to the outlet during individual events. We will therefore not interpret these measurements as direct observations of hydrological or functional connectivity. Structural connectivity will instead be treated separately as the terrain and surface configuration represented by the IC.

Hydrological coupling will be retained as the operational term for the local-to-outlet relationship that can be evaluated from the available observations. Its observational basis is provided by two complementary dimensions developed in response to Referee 1: (i) wetness-state correspondence with outlet-flow state, evaluated by comparing sensor-specific relative-wetness distributions during periods with and without measurable outlet discharge within precipitation-event response windows; and (ii) response timing, quantified as the lag between detectable soil-water response at an individual monitoring position and outlet-discharge onset. These analyses quantify whether and when local soil-water responses correspond with the integrated microcatchment outlet response.

They therefore allow spatial variation in hydrological coupling among the monitored positions to be evaluated, but they do not demonstrate horizontal water transfer, identify specific internal flow pathways, establish direct source-to-outlet connections, or delineate the area effectively connected to the outlet. Hydrological coupling will consequently be interpreted as an observationally constrained expression relevant to the functional component of hydrological connectivity, rather than as a direct measurement of functional connectivity itself. We will state both this distinction and the limited spatial representativeness of the monitoring network explicitly in the revised manuscript.

SC-M10 – L194-196: the Sediment Connectivity Index is introduced here by citation alone, without description. Given that it is the only quantitative descriptor of structural connectivity in the paper, it would benefit from being introduced earlier and explained. It would also help to state what it is used for, since it does not appear to enter any subsequent analysis.

Authors response

We agree that the role of the IC was insufficiently explained in the submitted manuscript. The IC was calculated before installation of the SM monitoring network and was used to identify contrasting terrain-based structural settings within the instrumented hillslope-terrace sector and to inform where the monitoring locations were established. Its principal role was therefore in the design of the monitoring network, rather than as an explanatory variable in the subsequent event-scale hydrological analyses.

In the revised manuscript, we will introduce the IC earlier, before describing the monitoring-position selection, and explain its geomorphometric basis and what it represents. The index integrates terrain and surface characteristics relevant to potential structural linkage, including contributing area, slope, downslope routing, and surface impedance. In this study it is therefore

used to characterise the comparatively stable structural configuration of the monitored sector rather than event-dependent hydrological behaviour.

Its role in the manuscript will therefore be limited to documenting the terrain-based structural context used to design the monitoring network and characterising the settings represented by the monitored locations. The network was not designed as a statistically representative sampling of a continuous IC gradient, and the IC will therefore not be used retrospectively as an event-scale predictor of storm-flow occurrence or hydrological coupling. Hydrological behaviour at the monitored positions will instead be evaluated from the soil-water and outlet-discharge observations.

SC-M11 – L197-198: you state that the setup is appropriate for identifying SM thresholds associated with R generation. If the sensors were used with factory calibration and were not verified in the field (local soil types), the record would capture relative variability of SM, and substantial bias may be present in absolute terms. On that basis I would hesitate to describe the arrangement as appropriate for identifying absolute thresholds. Relative thresholds, understood as levels of the measured signal rather than as actual SM values, would be a defensible formulation, and making the distinction explicit would strengthen this passage. The argument that thresholds are derived consistently across locations using the same sensor type and installation protocol establishes comparability among positions. It does not, however, establish that the values correspond to actual SM, and so does not underwrite absolute thresholds.

Authors response

We thank the reviewer for this clarification and agree that the use of factory calibration without site-specific calibration does not support interpretation of the measurements as directly comparable absolute volumetric soil-water contents across monitoring positions with contrasting soil properties. We have therefore revised the spatial comparisons of individual monitoring positions so that they are expressed in relative rather than absolute terms.

Specifically, the available 2021-2024 record from each sensor was transformed to a sensor-specific empirical percentile distribution. Event-scale antecedent, average, maximum, and Δ relative wetness were then recalculated from these percentile series. The resulting values describe the relative wetness state of each monitoring position within the distribution observed by the corresponding sensor, rather than assuming that the same factory-calibrated volumetric value represents an equivalent soil-water state across contrasting locations. Δ relative wetness is correspondingly expressed in percentile points.

This transformation is used for comparisons among individual monitoring positions. The broader event-scale comparison between storm-flow and non-flow events in Table 2 retains volumetric SM metrics aggregated across sensors at each depth, but these values are not interpreted as position-specific thresholds or as evidence of direct absolute comparability among individual sensors.

We will revise the Methods accordingly and remove the statement that the monitoring setup is appropriate for identifying absolute SM thresholds. We will also avoid simply replacing these with “relative thresholds”, because no formal threshold is estimated. The Results and Discussion will instead describe sensor-specific relative-wetness conditions and event-scale wetting responses for the spatial analyses.

SC-M12 — L204: you report that the sensor was replaced in January 2023. Was there an overlap period during which both instruments operated? Since the series combines measurements from the earlier probe and from the new pressure transducer, a relationship between the two would be valuable, and if no such comparison is available this would be worth stating as a limitation.

Authors response

We thank the reviewer for raising this point. There was no overlap period between the two pressure transducers because the CS451-L was replaced following an unexpected malfunction rather than as part of a planned sensor transition. Consequently, no direct cross-calibration or simultaneous comparison between the two instruments was possible, and we will state this explicitly as a limitation.

The malfunction was identified before any storm-flow event occurred during the affected interval. Both instruments were installed at the same outlet measurement point, referenced to the same dry-flume zero, and stage was converted to outlet discharge throughout the monitoring period using the same rating relationship for the fixed H-flume geometry. We will clarify these aspects in the Methods.

To document the practical continuity of low-flow detection across the two sensor periods, we examined the lowest-amplitude confirmed storm-flow responses with complete quantitative discharge records for each instrument. The 21 November 2021 event, recorded with the CS451-L, had a peak discharge of 0.057 L s^{-1} , whereas the 5 January 2024 event, recorded with the OTT PLS, had a peak discharge of 0.086 L s^{-1} . Both produced coherent event-related hydrograph responses despite their small magnitude (Fig. SC-M12-1). These observations show that low-amplitude outlet responses were detectable with both instruments, but they are not a substitute for direct cross-calibration and do not demonstrate identical practical detection limits.

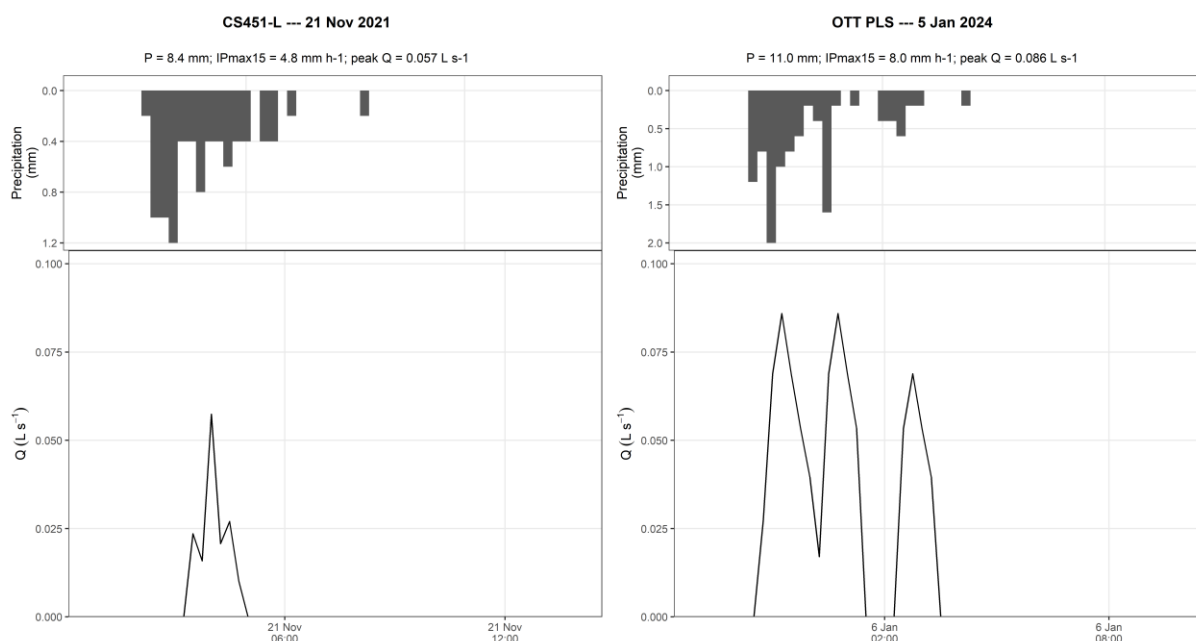


Fig. SC-M12-1. Examples of low-amplitude confirmed storm-flow responses recorded before and after replacement of the outlet stage sensor in January 2023. The 21 November 2021 event was recorded with the CS451-L and the 5 January 2024 event with the OTT PLS. These events were the lowest-peak-discharge confirmed storm-flow events with complete

quantitative discharge records and no measurable outlet discharge during the preceding hour within their respective sensor periods.

Because no overlap period was available, we will therefore avoid implying that equivalent sensor performance can be demonstrated directly across the replacement and will identify this explicitly as a limitation.

SC-M13 – Figure 2: this figure overlaps substantially with Figure 1, and the two might either be merged or differentiated more clearly. In Figure 2(c), what do the orange lines or delineations represent? The lettering is again very small. The caption refers to disconnected, mid terrace and high connectivity settings, although these are not schematised anywhere, so the reader is left without a clear sense of what they denote. A dedicated diagram would help considerably.

Authors response

We agree that the submitted Figs. 1-2 overlap substantially and that their purposes should be differentiated more clearly. As described in our responses to Recommendation 7 and SC-M1, we will therefore substantially redesign both figures rather than make incremental changes to the submitted Fig. 2.

In the submitted Fig. 2, panel (a) provided the microcatchment-scale IC context, panel (b) located the monitored headwater terrace sector, and panels (c-e) showed progressively enlarged views of groups of monitoring locations. The orange linear features visible in panel (c) are not separate mapped delineations, they correspond to relatively high IC values within the continuous IC raster and are represented by the same colour scale as the remainder of the map.

In the revised manuscript, Fig. 1 will provide the study-area and terrain-based structural context, including the spatial distribution of the IC and the monitoring locations, whereas Fig. 2 will be redesigned specifically to communicate the experimental setup. Fig. 2 will use an annotated overview photograph of the monitored hillslope-terrace sector to show all eight monitoring locations at their approximate physical positions, together with enlarged photographs of the three positions highlighted in the main analysis. The figure and caption will also distinguish the eight monitoring locations from the 11 individual sensors and identify the locations instrumented at both 10 and 50 cm depth.

The connectivity-based monitoring-position names will be retained, but terms such as “high-connectivity”, “low-connectivity”, and “disconnected” will be defined explicitly as referring to the IC-derived structural setting rather than to observed event-scale hydrological behaviour. Their spatial meaning will therefore be visible from the revised Fig. 1, while the annotated overview in Fig. 2 will show their actual geomorphic setting.

Rather than adding a separate schematic, the combination of the IC-based structural-context map in Fig. 1 and the annotated photographic overview in Fig. 2 will provide the whole-system representation requested by the reviewer. We will also increase lettering, annotation, and symbol sizes throughout the redesigned figures to improve readability.

SC-M14 – L72 and throughout: the use of flood event is consistent with the definition of flood given in the International Glossary of Hydrology (WMO/UNESCO, 2011). I would nonetheless invite you to consider an alternative, since in practice the term is generally associated with

inundation processes, whereas what is observed here is outlet Q in a micro catchment. Storm flow event, runoff generating event or flow producing event might reduce the potential for ambiguity, with non-flow event retained for the complementary case. This is offered simply as a suggestion for your consideration.

Authors response

We thank the reviewer for this suggestion. As noted in our response to Recommendation 5, we agree that “storm-flow event” more clearly describes the observed event class in this microcatchment and avoids the broader connotations commonly associated with “flood event”. We will therefore replace “flood event” with “storm-flow event” throughout the manuscript, while retaining “non-flow event” for the complementary category.

Consistent with the terminology defined elsewhere in the revision, storm-flow occurrence will refer to the binary presence or absence of measurable outlet discharge, whereas “runoff” will be retained for derived runoff depth or coefficient and, where appropriate, for discussion of the hydrological process itself.

SC-M15 – L240-241: you state that t tests or Mann Whitney U tests were applied depending on the results of the preliminary checks. You might consider adding Welch t tests for cases that satisfy normality but not homogeneity of variance, which would complete the strategy. More substantively, the results of the Shapiro Wilk and Levene tests are not reported, nor is the false discovery rate correction, so it is not possible to verify which procedure was applied in each comparison or how multiplicity was handled.

Authors response

We thank the reviewer for this suggestion and agree that the original statistical description did not provide sufficient information to verify either the test-selection procedure or the treatment of multiple comparisons. We therefore revised and reran the event-scale statistical analyses.

For each comparison, normality was assessed separately for storm-flow and non-flow events using Shapiro-Wilk tests, and homogeneity of variance was evaluated using Levene’s test. When both groups satisfied the normality criterion, Student’s two-sample t -test was used when variances were homogeneous and Welch’s two-sample t -test when variances were heterogeneous. Mann-Whitney U tests were used when either group departed from normality. No inferential test was conducted when either event class contained fewer than three observations. Consequently, no comparison was performed for the dry period, which contained only one storm-flow event.

We also made the multiple-testing procedure explicit. P -values were adjusted using the Benjamini-Hochberg false-discovery-rate procedure, separately for the complete study-period analysis and for each hydrological-period analysis across the 11 event-scale metrics compared in Table 2. Statistical significance was evaluated at adjusted $p < 0.05$.

The revised Table 2 now identifies the statistical test used and the BH-adjusted p -value for every comparison. The corresponding Shapiro-Wilk and Levene results, together with the selected test and raw and adjusted p -values, will be reported in Supplementary Table S5, allowing the complete test-selection procedure and multiple-comparison correction to be verified directly.

The same test-selection framework and Benjamini–Hochberg correction were also applied to the sensor-specific relative-wetness comparisons reported in Supplementary Tables S1–S3, which likewise identify the selected test and BH-adjusted p -value for each comparison.

The revised Table 2 is provided below to show explicitly how the selected statistical test and BH-adjusted p -value are now reported for each comparison.

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 $\pm$ 14.4	6.3 $\pm$ 8.5	MWU	<0.001
	Wet ($n = 17/64$)	13.4 $\pm$ 9.5	8.2 $\pm$ 7.4	MWU	0.013
	Drying-down ($n = 5/23$)	17.4 $\pm$ 20.9	4.7 $\pm$ 4.6	MWU	0.087
	Dry ($n = 1/33$)	1.0	3.2 $\pm$ 2.9	-	-
	Wetting-up ($n = 9/34$)	16.4 $\pm$ 19.1	6.9 $\pm$ 13.7	MWU	0.089
Total P (mm)	Study period	28.8 $\pm$ 20.7	6.5 $\pm$ 8.5	MWU	<0.001
	Wet	23.8 $\pm$ 20.1	8.1 $\pm$ 11.2	MWU	<0.001
	Drying-down	30.1 $\pm$ 10.5	3.7 $\pm$ 3.1	MWU	0.001
	Dry	49.0	4.6 $\pm$ 3.8	-	-
	Wetting-up	35.2 $\pm$ 25.4	7.5 $\pm$ 7.9	MWU	<0.001
IP _{max15} (mm h ⁻¹)	Study period	28.1 $\pm$ 28.2	7.2 $\pm$ 9.0	MWU	<0.001
	Wet	17.3 $\pm$ 18.3	5.8 $\pm$ 4.8	MWU	<0.001
	Drying-down	32.6 $\pm$ 36.4	3.3 $\pm$ 2.4	MWU	0.004
	Dry	119.2	9.4 $\pm$ 11.0	-	-
	Wetting-up	35.8 $\pm$ 19.8	10.5 $\pm$ 13.4	MWU	<0.001
Antecedent SM 10 cm (%)	Study period	23.5 $\pm$ 4.3	21.3 $\pm$ 4.5	MWU	0.012
	Wet	26.5 $\pm$ 1.9	24.4 $\pm$ 3.4	MWU	0.009
	Drying-down	18.3 $\pm$ 1.7	19.7 $\pm$ 2.1	MWU	0.131
	Dry	-	15.7 $\pm$ 1.4	-	-
	Wetting-up	20.6 $\pm$ 3.8	21.7 $\pm$ 4.2	MWU	0.472
Antecedent SM 50 cm (%)	Study period	26.3 $\pm$ 5.1	23.9 $\pm$ 4.4	MWU	0.010
	Wet	30.1 $\pm$ 2.1	27.3 $\pm$ 3.1	MWU	<0.001
	Drying-down	21.4 $\pm$ 2.8	22.9 $\pm$ 2.8	Student t	0.306
	Dry	-	18.5 $\pm$ 1.2	-	-
	Wetting-up	22.1 $\pm$ 4.4	23.1 $\pm$ 3.7	Student t	0.488
Average SM 10 cm (%)	Study period	29.1 $\pm$ 2.4	22.9 $\pm$ 4.8	MWU	<0.001
	Wet	30.3 $\pm$ 2.0	26.0 $\pm$ 3.4	MWU	<0.001
	Drying-down	27.1 $\pm$ 2.2	20.0 $\pm$ 2.1	MWU	0.002
	Dry	-	17.1 $\pm$ 2.3	-	-
	Wetting-up	27.9 $\pm$ 2.1	23.9 $\pm$ 4.1	Student t	0.013

Average SM 50 cm (%)	Study period	31.9 ± 3.2	24.9 ± 4.9	MWU	<0.001
	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
Maximum SM 10 cm (%)	Study period	36.8 ± 3.7	25.1 ± 6.2	MWU	<0.001
	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
Maximum SM 50 cm (%)	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
	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
ΔSM 10 cm (%)	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
	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
ΔSM 50 cm (%)	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
	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.

SC-M16 – L246-247: you restrict the analysis to timestamps at which Q was observed at the outlet. What motivated this choice? The evolution of SM prior to the response is precisely what would inform the analysis of antecedent conditions, and its exclusion appears to set aside information central to the stated objectives.

Authors response

We agree that restricting the submitted analysis to timestamps with ongoing outlet discharge excluded information relevant to the development of the soil-water response before outlet-discharge onset. The original restriction was intended to characterise the soil-water states occurring while measurable outlet discharge was present, but we agree that this alone provides an incomplete basis for evaluating local-to-outlet hydrological relationships.

We have therefore expanded the analysis in two complementary directions. First, although antecedent SM was already included in the submitted event-scale comparisons, the revised storm-flow-occurrence analysis now treats it explicitly as a pre-event predictor, distinct from average, maximum, and ΔSM , which are within-event response metrics. Second, the hydrological-coupling analysis is no longer restricted to timestamps with measurable outlet discharge. Sensor-specific relative-wetness distributions are now compared between periods without and with measurable outlet discharge within the 186 precipitation-event response windows, and an event-scale onset-lag analysis quantifies the timing of detectable soil-water response at each monitoring position relative to outlet-discharge onset.

The wetness-state comparison illustrates why inclusion of periods without measurable outlet discharge is informative. At High-Connectivity Low-Gradient Hillslope, 8.0 % of $Q = 0$ observations reached or exceeded the median relative wetness observed during $Q > 0$. The corresponding proportions were 14.6 % at High-Connectivity Upslope-Edge Terrace 1 and 15.2 % at Disconnected Mid-Terrace. Thus, very wet local conditions were not exclusive to periods with measurable outlet discharge, while the degree of overlap differed among monitoring positions (Fig. SC-M16-1).

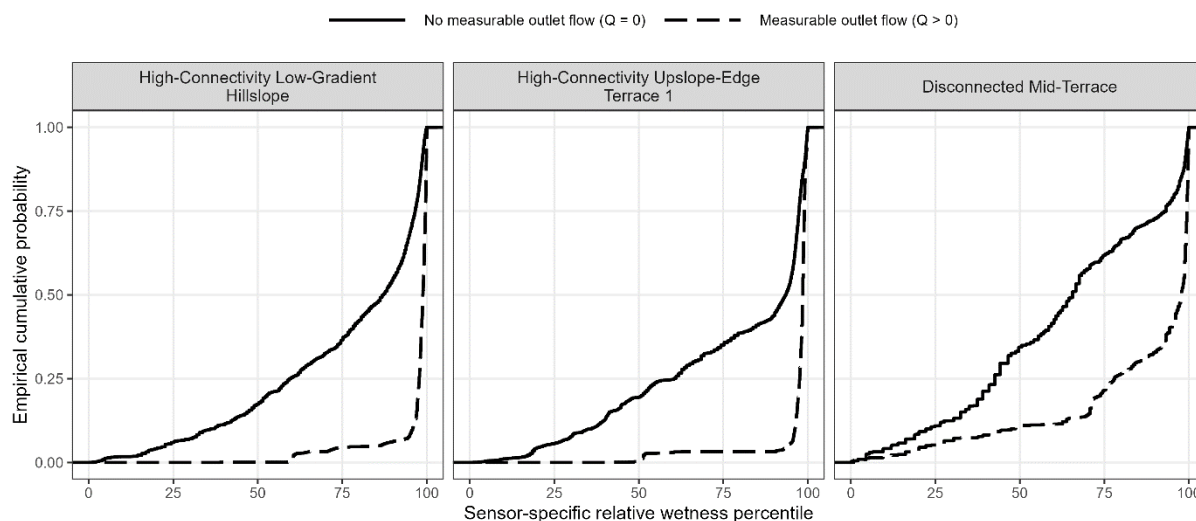


Fig. SC-M16-1. Empirical cumulative distributions of sensor-specific relative wetness during periods without ($Q = 0$) and with ($Q > 0$) measurable outlet discharge within precipitation-event response windows at the three illustrative 10-cm monitoring positions.

The timing analysis further incorporates soil-water evolution relative to the beginning of the outlet response. Considering all valid event-level lags available at each position, median detectable soil-water response occurred 15 min before outlet-discharge onset at both High-Connectivity Low-Gradient Hillslope and High-Connectivity Upslope-Edge Terrace 1, whereas it occurred 60 min after outlet-discharge onset at Disconnected Mid-Terrace. For the 19 events with valid onset lags at all three positions, onset lags differed significantly among positions (Friedman test, $\chi^2(2) = 33.30$, $p < 0.001$). Pairwise paired Wilcoxon signed-rank tests with Holm correction showed significantly later responses at Disconnected Mid-Terrace than at both High-Connectivity Low-Gradient Hillslope and High-Connectivity Upslope-Edge Terrace 1 (Holm-adjusted $p < 0.001$ in both cases), while the two high-connectivity positions also differed significantly (Holm-adjusted $p = 0.013$). 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 in the sensitivity analysis (Fig. SC-M16-2).

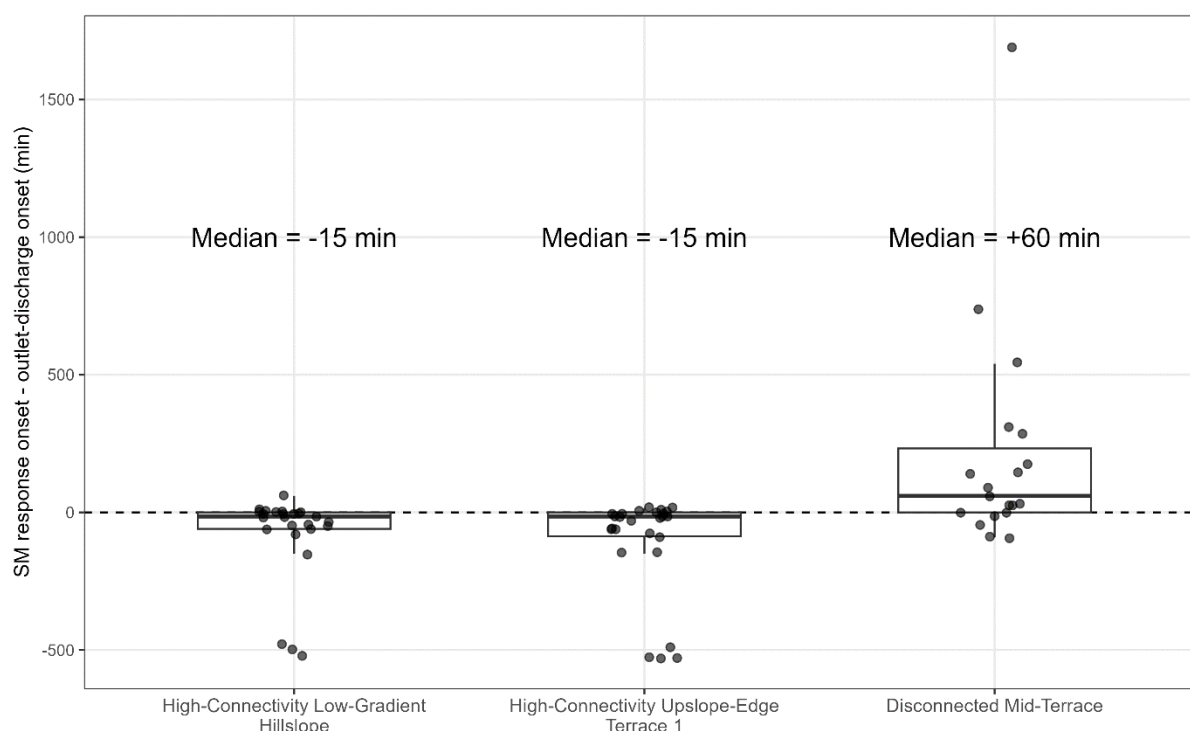


Fig. SC-M16-2. Event-scale lag between detectable local soil-moisture response and outlet-discharge onset at the three illustrative 10-cm monitoring positions. Negative values indicate that detectable local SM response preceded outlet-discharge onset, whereas positive values indicate that outlet discharge preceded detectable local SM response. All valid event-level lags are shown.

Taken together, these analyses replace the submitted reliance on contemporaneous soil-water conditions during ongoing outlet discharge with an assessment based on both wetness-state correspondence and relative response timing. They quantify whether and when soil-water responses at individual monitoring positions correspond with the integrated outlet response, without implying direct water transfer from a monitoring position to the outlet or identifying the internal flow pathway.

The revised Methods and Results will therefore distinguish explicitly between antecedent soil-water conditions, within-event soil-water responses, wetness-state correspondence with outlet-flow state, and response timing relative to outlet-discharge onset.

SC-M17 – L248: you refer to the seasonal evolution of SM conditions locally associated with ongoing Q. Q at any instant reflects processes and conditions that occurred earlier in time and elsewhere in the catchment, so a strictly contemporaneous association may not capture the relationship of interest.

Authors response

We agree that a strictly contemporaneous comparison between soil-water conditions at an individual monitoring position and outlet discharge does not fully capture possible temporal offsets between local and outlet responses. The submitted comparison provided a useful description of the soil-water states occurring while measurable outlet discharge was present,

but outlet discharge integrates hydrological responses occurring across the microcatchment area and these responses need not be synchronous with soil-water changes at a particular monitoring position. The revised analysis therefore extends this approach by explicitly accounting for relative response timing.

As described in our response to SC-M16, the revised analysis therefore no longer relies solely on contemporaneous SM-discharge associations. In addition to comparing relative-wetness states during periods with and without measurable outlet discharge, we introduced an event-scale onset-lag analysis that explicitly quantifies whether detectable soil-water response at each monitoring position precedes, coincides with, or follows outlet-discharge onset. This directly accommodates the temporal displacement highlighted by the reviewer.

The revised analysis therefore evaluates hydrological coupling through wetness-state correspondence and relative response timing, rather than assuming synchronous local and outlet behaviour.

We will consequently revise the submitted wording referring to the “timing and seasonal evolution of SM conditions locally associated with ongoing outlet Q”. The revised Methods and Results will instead describe the wetness-state and onset-lag analyses explicitly and avoid interpreting contemporaneous SM-discharge observations alone as evidence of local-to-outlet hydrological coupling.

Results

SC-R1 – L257-258: the total of 186 P events is restated here, having already been given in Section 2.4. Several such repetitions occur through the manuscript and could usefully be trimmed.

Authors response

We agree that unnecessary repetition should be avoided. We consider the event counts useful at the beginning of the Results because they define the analytical sample for the analyses that follow. We will therefore retain them in Sect. 3.1.1, where the revised Results will report the final catalogue of 186 precipitation events, comprising 32 storm-flow events and 154 non-flow events, while distinguishing that complete discharge records are available for 31 of the storm-flow events.

To avoid duplicating the same information, we will streamline Sect. 2.4 so that it focuses on the criteria used to identify and classify events and on the treatment of incomplete discharge records, rather than repeating the final event counts. We will also review the remainder of the manuscript for similar unnecessary repetitions.

SC-R2 – L261: you report a contribution of 72.9 % of total R. Please give the corresponding depth in millimetres and clarify whether total R refers to that year or to the whole study period.

Authors response

We agree that this should be stated more explicitly. The 72.9 % refers to the proportion of the total quantified runoff over the complete 2021–2024 study period, not to the 2021 annual total. This corresponds to approximately 5.23 mm of runoff during November 2021, out of 7.18 mm

quantified over the study period. We will report both the runoff depth and the reference period explicitly in the revised Results.

SC-R3 – L262: you indicate that the station shown in Figure 1(b) is the AEMET station. Where, then, is your own experimental station located, and could it be displayed as well? This information is important for interpreting the P record.

Authors response

As clarified in our response to SC-M6, both meteorological stations were already represented in the submitted Fig. 1. Panel (b) shows the external B118-AEMET reference station, whereas the blue triangle within the microcatchment in panels (c-e) represents our experimental meteorological station at the headwaters of So na Vidala.

We will revise Fig. 1 and its caption so that the two stations are identified explicitly and cannot be confused.

SC-R4 – L264: where you refer to data loss at the gauging station, do you mean the Q gauging station? A brief clarification would help.

Authors response

We agree that this should be specified more clearly. The data loss concerned the water-level record at the H-flume gauging station at the microcatchment outlet. Although a storm-flow response was confirmed from the available telemetry, loss of the complete local record prevented reconstruction of the discharge hydrograph and quantification of the event runoff. We will clarify this in the revised manuscript.

SC-R5 – L265: for the figure of 8.2 % of total R, please clarify whether this refers to the entire measurement period. In the same line, “a R coefficient” should read “an R coefficient”, and I believe you mean an annual R coefficient of 0.1 %.

Authors response

We agree and will clarify that the 8.2 % value refers to the proportion of total quantified runoff during the 2021–2024 monitoring period, and that 0.1 % is the annual runoff coefficient for 2024. Consistent with the revised terminology used throughout the manuscript, we will refer explicitly to the “runoff coefficient” rather than to an “R coefficient”.

SC-R6 – Figure 3(a): the column headed “Study period” for the R coefficient might be labelled “Annual mean for the study period”, since it is an average. No uncertainty is reported for the R coefficient; uncertainties can be estimated for percentages and would be informative here. The 2023 column would also benefit from a note indicating the data loss for that year. Finally, in the rows where you refer to average SM at 10 and 50 cm, “Catchment-average SM” would distinguish it more clearly from the local values discussed elsewhere.

Authors response

We thank the reviewer for these suggestions. We will revise Fig. 3(a) to clarify the calculation of the study-period runoff coefficient and the treatment of the 2023 data loss.

First, the runoff coefficient shown for the study period is not the arithmetic mean of the annual runoff coefficients. It was calculated from aggregated runoff and precipitation over the

monitoring period, excluding the 27 August 2023 event because runoff could not be quantified for that event. We will therefore retain it as a study-period runoff coefficient rather than relabelling it as an annual mean, and we will make the calculation basis explicit in the figure caption.

Regarding uncertainty, we prefer not to report an uncertainty estimate for this coefficient because formal measurement uncertainties for precipitation and runoff depth were not propagated in the present study. A standard deviation of annual runoff coefficients would represent interannual variability rather than uncertainty in the aggregated study-period coefficient and would therefore describe a different quantity.

Second, the 2023 runoff depth and runoff coefficient will no longer be shown as zero. As clarified in SC-R4, a storm-flow response occurred during the 27 August 2023 event, but loss of the complete outlet record prevents its runoff volume from being quantified. The 2023 entries will therefore be indicated as “not quantifiable”, with this limitation stated in the figure caption.

Finally, we agree that the SM rows should be distinguished more clearly from the position-specific values discussed elsewhere. However, because the monitoring network is not spatially representative of the entire microcatchment, we will not use “catchment-average soil moisture”. Instead, these rows will be labelled “network-mean soil moisture”, indicating that they represent averages across the monitored sensors at each respective depth.

SC-R7 — Figure 3: lettering and numerals in panels (a), (b), (c) and (d) are very small and hard to read. Figure 3(c) and Figure 3(f): cumulative P in 2021 and 2024 is similar in amount and comparable in the shape of its distribution, yet the R response differs drastically. The R response in 2024 corresponds to the P record more consistently than in 2021, where the accumulated curve shows an abrupt step without a clear counterpart in P. Given that the water level sensor was replaced between these years and that no overlap period is reported, an instrumental origin for part of this contrast is difficult to exclude on the evidence presented. Some indication of the homogeneity of the combined series would help before the difference is interpreted hydrologically.

Authors response

We thank the reviewer for raising this point. We will ensure that the lettering and numerical labels remain clearly legible at the final publication dimensions of the revised figure.

We would, however, clarify the interpretation of the cumulative curves in panels (c) and (f). The annual precipitation totals in 2021 and 2024 were not equivalent: 725.8 mm was recorded in 2021 compared with 562.2 mm in 2024. Moreover, the annual curves are expressed as cumulative percentages of each year’s total, so similarity in their shape reflects the temporal distribution of precipitation within each year rather than similarity in absolute precipitation amount.

The abrupt increase in cumulative runoff during 2021 coincides with the exceptionally wet conditions of November 2021. This month received 299.8 mm of precipitation and accounted for 72.9 % of the total quantified runoff over the 2021-2024 study period. The disproportionate runoff response therefore occurred during a period of unusually persistent wetness. The revised event-scale analyses likewise indicate that precipitation forcing remains the general control on

storm-flow occurrence, while antecedent wetness becomes comparatively informative under the exceptional conditions represented by November 2021.

We agree that the absence of an overlap period between the CS451-L and the replacement pressure transducer means that identical performance of the two instruments cannot be demonstrated directly, and this limitation will be stated explicitly. However, the available observations do not suggest that the contrasting 2021 and 2024 runoff responses were primarily caused by the sensor replacement. Both instruments were installed at the same outlet measurement point, referenced to the same zero-flow level of the H-flume, and the same stage-discharge relationship for the fixed H-flume geometry was applied throughout the monitoring period. In addition, 2022 was still monitored with the original CS451-L and nevertheless produced very little runoff, whereas low-amplitude but coherent storm-flow responses were detected after installation of the OTT PLS. As shown in our response to SC-M12, confirmed storm-flow responses with peak discharges as low as 0.057 L s^{-1} were detected with the CS451-L and 0.086 L s^{-1} with the OTT PLS. These observations show that small outlet responses were detectable with both instruments, although the absence of an overlap period prevents direct comparison of their practical detection limits.

SC-R8 — Related to the above: 2023 records no R at all and 2022 is dominated by essentially a single event. With so few R events across the four years, establishing a robust contrast between flow and non-flow events, which is the central aim of the paper, becomes difficult. Stating this limitation openly, and considering its consequences for the conclusions, would strengthen the paper.

Authors response

We agree that the temporal distribution of storm-flow events needs to be considered explicitly when interpreting the results. However, we do not regard the concentration of storm-flow events within particular periods as a deficiency of the dataset in itself. In this Mediterranean microcatchment, storm-flow occurrence is highly episodic and concentrated under particular hydrological states, so clustering of responses during wetter periods is part of the catchment behaviour that the monitoring record is intended to capture. The relevant limitation concerns the extent to which statistical relationships derived from a temporally clustered sample can be generalised across the full range of hydrological conditions.

We would also like to clarify the apparent absence of runoff in 2023. The submitted Fig. 3 showed annual runoff as 0 mm because runoff volume could not be quantified for that year. However, a measurable outlet response occurred on 27 August 2023. The complete local discharge record was subsequently lost, so its runoff volume cannot be reconstructed. The revised figure will therefore indicate the 2023 runoff depth and runoff coefficient as “not quantifiable” rather than zero. This event is retained in analyses based on storm-flow occurrence but excluded from analyses requiring a complete quantitative discharge record.

The audited event catalogue contains 186 precipitation events, comprising 32 storm-flow and 154 non-flow events. These storm-flow events are not evenly distributed among hydrological periods: 17 occurred during the wet period, 5 during drying-down, 1 during the dry period, and 9 during wetting-up. This uneven distribution is hydrologically informative, but it also means that period-specific inference is necessarily stronger for some hydrological states than for

others. In particular, no inferential comparison is made for the dry period because only one storm-flow event occurred.

The strongest temporal concentration occurs in November 2021, which contained 15 precipitation events, including 10 storm-flow events. We consider this sequence scientifically informative rather than anomalous or invalid, because it represents an exceptionally persistent wet state that genuinely occurred during the monitoring period. Nevertheless, its disproportionate contribution to the complete storm-flow sample could influence relationships involving antecedent soil-water conditions.

We therefore repeated the probabilistic storm-flow-occurrence analysis after excluding all events beginning in November 2021, not to remove this period from the primary analysis, but as a sensitivity test of which relationships remain supported outside the exceptional wet sequence. After exclusion, 22 storm-flow events remain. The dominant precipitation signal is retained: the top-ranked model at both depths contains total precipitation and IPmax15. In contrast, antecedent SM is no longer retained in the top-ranked models and its inclusion does not improve cross-validated discrimination.

This sensitivity analysis therefore refines, rather than invalidates, the interpretation of the complete record. Precipitation forcing, particularly total precipitation, remains the most robust general predictor of storm-flow occurrence, whereas the additional contribution of antecedent wetness is strongly dependent on the exceptionally wet conditions represented by November 2021. We will therefore avoid generalising the antecedent-SM relationship across all hydrological conditions while retaining November 2021 as an integral and hydrologically meaningful part of the primary dataset.

The revised manuscript will make the temporal concentration of storm-flow events explicit and distinguish clearly between an intrinsic feature of the observed hydrological regime and the inferential constraints that arise from its uneven representation across hydrological states. The Discussion will temper period-specific interpretations where storm-flow sample sizes are small, particularly for the dry period, and the Conclusions will distinguish relationships that remain robust across the observed record from those whose support is concentrated under exceptionally wet conditions.

SC-R9 – L278: “high atmospheric demand” would read more precisely as “high atmospheric water demand”.

Authors response

We agree and will replace “high atmospheric demand” with “high atmospheric water demand”.

SC-R10 – L287-290: you report that daily reference evapotranspiration peaked during the drying down and dry seasons and was lowest during wet periods. Since reference evapotranspiration is computed from meteorological variables through a fixed formulation, this is the behaviour expected from a well measured meteorological series. Stating this explicitly would present the result as a consistency check rather than as a finding.

Authors response

We agree that the seasonal pattern in reference evapotranspiration is consistent with the expected seasonal evolution of the meteorological variables from which reference

evapotranspiration is calculated. However, we do not consider this to make the result merely a consistency check. Reference evapotranspiration is a derived hydrometeorological variable that integrates the observed meteorological forcing during the study period, and its seasonal magnitude and timing remain relevant results for characterising atmospheric water demand and interpreting the concurrent soil-moisture dynamics. We will therefore retain the seasonal ET₀ pattern in the Results, while avoiding presenting it as an unexpected or independent process-level finding.

SC-R11 – L287-290: there appears to be a causality issue in the statement that total P was highest during wet periods and lowest during dry periods. If seasons are delimited on the basis of measured SM levels, then high P produces the high SM that defines the wet period, so the relationship as written inverts the direction of the reasoning.

Authors response

We agree that the relationship between the hydrological-period classification and the variables used to characterise those periods should be made explicit. However, we do not consider the statement that precipitation totals differed among hydrological periods to constitute an inversion of causal reasoning. The submitted Results report a descriptive contrast among the identified periods rather than a causal relationship between precipitation and the period classification.

We would also like to clarify that the seasonal delineation was not based on soil moisture alone or on a strict soil-moisture threshold. The four hydrological periods were identified from the joint temporal evolution of soil moisture, precipitation, and reference evapotranspiration. The 25th and 75th percentiles of network-mean soil moisture were used as guides for relatively dry and wet soil-water states, respectively, rather than as automatic boundaries. The resulting periods therefore represent contrasting hydroclimatic states arising from the seasonal balance among water inputs, atmospheric water demand, and soil-water storage.

We nevertheless agree that, because precipitation contributes to the delineation and evolution of these hydroclimatic states, differences in precipitation totals among the resulting periods should be interpreted descriptively rather than as an independent test of their controls. We will retain the precipitation, reference-evapotranspiration, and soil-moisture contrasts in the Results because they characterise the hydroclimatic conditions represented by each period, but we will ensure that they are not presented as evidence of causal relationships among the variables.

SC-R12 – L290-292: the result that R occurrence was strongly seasonal, with the highest totals and coefficients in wet and wetting up seasons and minimal R in drying down and dry seasons, is as expected but nonetheless worth reporting, and it is soundly supported.

Authors response

We thank the reviewer for this positive assessment. We will retain this result in the revised manuscript.

SC-R13 – L311 and L317-318: in my view, this is one of the most interesting passages in the paper. The finding that events with and without outlet flow differ significantly in all P and SM metrics, yet overlap in antecedent SM at both depths, so that initial wetness alone does not determine outlet response, is a valuable result. The greater separation obtained for average

SM, maximum SM and the change in SM is equally interesting. My reservation concerns the interpretation rather than the result: these are point observations of vertical wetting, and they do not in themselves demonstrate the activation of coupling or the transmission of water to the outlet. It would help either to justify the representativeness of the monitored positions explicitly, or to frame the interpretation as consistent with, rather than demonstrative of, the proposed mechanism.

Authors response

We agree with the reviewer's distinction between the observed event-type differences and the interpretation originally attached to them. The overlap in antecedent SM between storm-flow and non-flow events indicates that antecedent wetness alone does not provide a deterministic separation between events with and without measurable outlet response. Conversely, the greater separation observed for average SM, maximum SM, and Δ SM shows that storm-flow events are associated with different soil-water states attained during the event and with greater event-scale wetting.

The revised probabilistic storm-flow-occurrence analysis provides a complementary basis for interpreting these contrasts. In that analysis, antecedent SM is treated explicitly as a pre-event predictor together with precipitation characteristics, whereas average SM, maximum SM, and Δ SM are not included as predictors because they develop during the precipitation event. The results identify precipitation forcing, particularly total precipitation, as the more robust general predictor of storm-flow occurrence, while the additional predictive contribution of antecedent SM is concentrated in the exceptionally wet November 2021 sequence.

However, we agree that the observed differences in within-event soil-water metrics should not themselves be interpreted as demonstrating the activation of hydrological coupling or the transmission of water from the monitored positions to the outlet. Average SM, maximum SM, and Δ SM describe within-event soil-water responses associated with the two event classes. They do not identify the pathway by which water reaches the outlet or demonstrate source-to-outlet transfer.

Regarding representativeness, the monitored positions were not intended to provide a spatially representative sample of the entire microcatchment. They constitute targeted observations within the instrumented hillslope-terrace sector, selected to sample contrasting structural settings. We will make this spatial scope explicit and will not extrapolate the behaviour of individual monitoring positions to the entire microcatchment.

We will therefore revise the interpretation of this Results passage so that the storm-flow/non-flow contrasts are presented as evidence of differences in antecedent and within-event soil-water conditions, with the probabilistic analysis used separately to evaluate the predictors of storm-flow occurrence, without using these contrasts alone to infer activation of hydrological coupling or water transmission. Hydrological coupling will instead be evaluated through the analyses specifically developed for that purpose: the correspondence between local relative-wetness states and outlet-flow state, and the relative timing between detectable soil-water response at individual monitoring positions and outlet-discharge onset. These analyses quantify local-to-outlet hydrological correspondence, while mechanistic interpretations concerning the actual transfer pathway will be framed as hypotheses consistent with, rather than demonstrated by, the observations.

SC-R14 — Figure 5: the lettering is small, but the figure itself is very effective and summarises the core of the experiment well. It is notable that the change in SM discriminates more clearly at the deeper sensors than at the shallow ones, which is an interesting result and worth highlighting in the text. In the caption you note that symbol colour represents maximum 15-minute P intensity (IP_{max15}) for each event; this appears to be applied only to events with outlet flow. Extending it to non-flow events would be informative and would make the comparison more complete.

Authors response

We thank the reviewer for this assessment of Fig. 5. We have increased the lettering and revised the figure to improve both readability and the representation of the storm-flow and non-flow event distributions.

Regarding the suggestion that ΔSM discriminates more clearly between event types at 50 cm than at 10 cm, we reassessed this pattern using the revised Fig. 5, which now includes class-specific marginal distributions. These distributions do not support a clearly stronger separation at 50 cm. Storm-flow and non-flow events show marked differences in ΔSM at both depths, but the degree of separation appears broadly comparable, with substantial within-class variability at both 10 and 50 cm. We therefore do not consider the visual pattern in the submitted figure sufficient evidence for a systematic depth-dependent difference in the degree of separation between event classes.

This interpretation is also preferable given the monitoring design: measurements at 10 cm are available across all eight monitoring locations, whereas measurements at 50 cm are available only at the three paired-depth locations. A direct comparison of separation between depths would therefore involve different spatial samples. We will consequently report the clear storm-flow/non-flow differences in ΔSM at both depths without attributing greater discriminatory capacity to the deeper measurements.

Regarding IP_{max15}, we would like to clarify that this variable was already represented for both storm-flow and non-flow events in the submitted Fig. 5. The original caption stated that symbol colour represented IP_{max15} for each precipitation event. No substantive change was therefore required in this respect.

The revised Fig. 5 is shown below and will replace the submitted version in the manuscript.

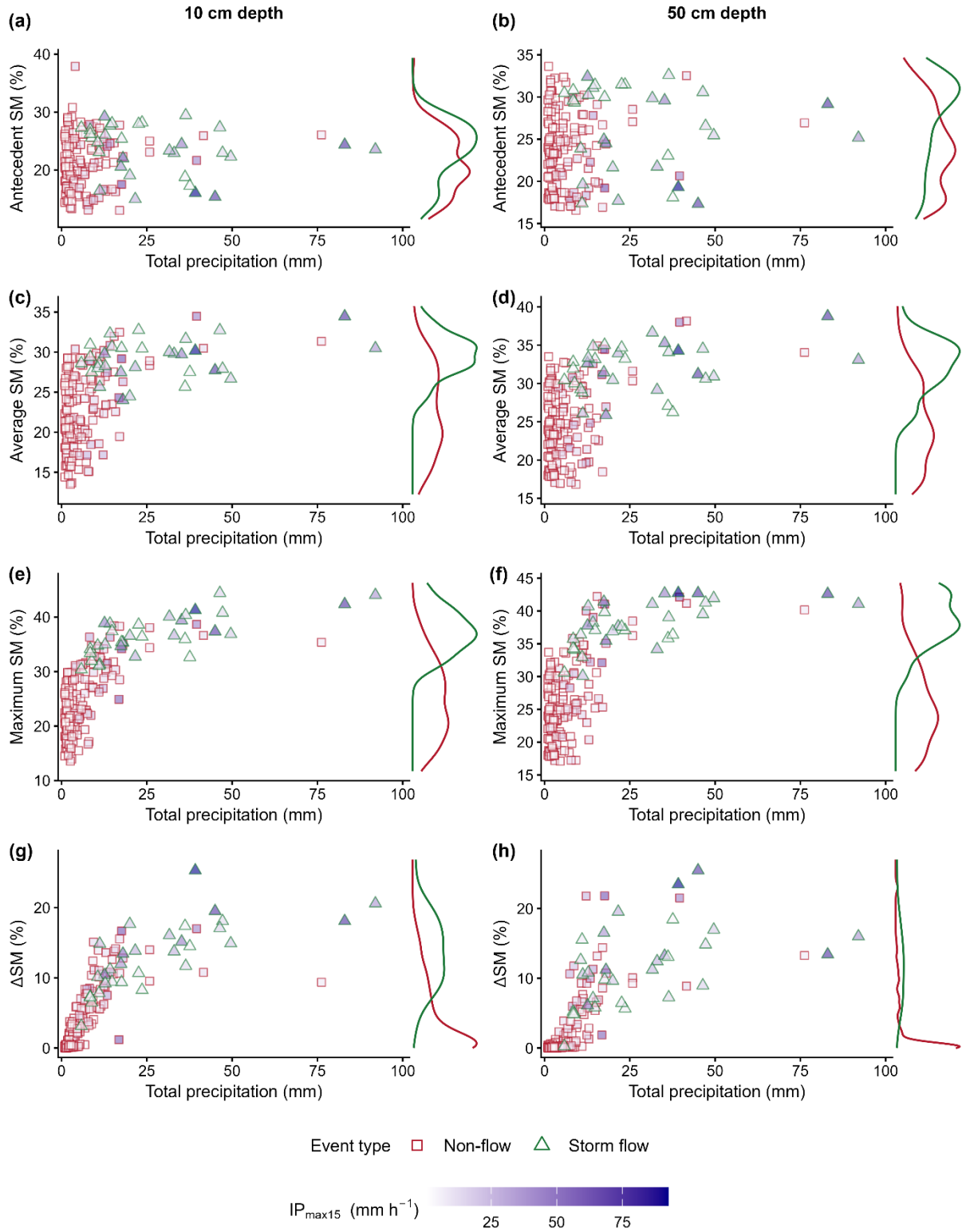


Fig. 5. Event-scale relationships between total precipitation and soil-moisture (SM) metrics for storm-flow and non-flow events during the complete study period. Antecedent SM at (a) 10 cm and (b) 50 cm depth, average SM at (c) 10 cm and (d) 50 cm depth, maximum SM at (e) 10 cm and (f) 50 cm depth, and 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.

SC-R15 – L336-338: this result is among those most directly supported by the observations. Antecedent SM discriminates between event types only during the wet season, while storm duration shows the same seasonal restriction. This is the kind of finding that the observational design is well placed to support, and I would encourage you to give it a more central place in the scope of the paper, in preference to claims about coupling that the measurements cannot readily underwrite.

One way to retain the concept of coupling on a firm evidential footing would be to define it as a binary metric at catchment scale: whether or not R is aggregated and routed, and reaches the outlet as Q . That is directly observable with your data, and it would allow the seasonal analysis to be framed in terms of coupling without requiring inferences about internal flow pathways that were not measured.

Authors response

We agree that the seasonal differentiation of the factors associated with storm-flow occurrence is among the results most directly supported by the observational design and should receive greater prominence in the revised manuscript. The revised analyses retain this seasonal result while refining its interpretation.

Storm duration remains significantly greater for storm-flow than non-flow events only during the wet period after application of the revised statistical procedure and false-discovery-rate correction. Antecedent SM likewise differs significantly between event classes during the wet period but not during drying-down or wetting-up. However, the probabilistic storm-flow-occurrence analysis provides an important additional qualification. When the exceptionally wet November 2021 sequence is excluded, antecedent SM is no longer retained in the top-ranked occurrence models and does not improve cross-validated discrimination, whereas precipitation forcing remains strongly supported. We will therefore give greater prominence to the seasonal variation in event-scale relationships while avoiding the broader inference that antecedent wetness is generally important throughout wet conditions. Precipitation forcing, particularly total precipitation, remains the more robust general predictor of storm-flow occurrence.

Consistent with this, the revised scope, Results, Discussion, and Conclusions will place greater emphasis on the seasonal and event-scale relationships associated with storm-flow occurrence. This does not require removing the spatial hydrological-coupling component, but it does require separating it clearly from claims about internal water-transfer mechanisms that were not directly observed.

We also appreciate the reviewer's suggestion to define coupling as a binary catchment-scale metric based on whether a precipitation event produces measurable outlet discharge. We do not adopt this definition because that binary outcome is already explicitly represented in the study as storm-flow occurrence. Re-labelling storm-flow occurrence as "coupling" would make the two concepts synonymous and would not describe how soil-water dynamics at different monitored positions relate to the integrated outlet response. The presence of measurable outlet discharge demonstrates that the catchment produced an integrated storm-flow response, but it does not by itself quantify spatial variation in local-to-outlet relationships.

In the revised manuscript, hydrological coupling will therefore retain a distinct operational meaning: the observed event-scale correspondence between soil-water dynamics at an individual monitoring position and the integrated outlet response. It will be evaluated through

two complementary dimensions: (i) correspondence between relative-wetness states at individual monitoring positions and the presence or absence of measurable outlet discharge within event-response windows, and (ii) the relative timing between detectable local soil-water response and outlet-discharge onset. This preserves the distinction between storm-flow occurrence as the binary catchment-scale outcome and hydrological coupling as the spatially variable local-to-outlet relationship, while avoiding any claim that internal water-transfer pathways were directly measured.

SC-R16 – L339: you might consider opening the sentence with a phrase such as “Over the observation period, only two flood events [...]”.

Authors response

We agree that the small number of storm-flow events during the dry period should be stated explicitly. Following the revision and audit of the event catalogue, however, the dry period contains only one storm-flow event rather than two. We will therefore revise the sentence accordingly and make clear that no inferential comparison between event classes is possible for this period.

SC-R17 – L339-340: for the two events described here, it would be worth examining whether the response is consistent with infiltration-excess (Hortonian) overland flow, and stating the evidence either way.

Authors response

We agree that the possible runoff-generation mechanism associated with the dry-period response is worth considering. Following the revised event classification, only one storm-flow event remains within the dry period: the 3 September 2024 event, which delivered approximately 49 mm of precipitation in about one hour and had the highest $IP_{\max 15}$ observed during the study period (119.2 mm h^{-1}).

The exceptionally high rainfall intensity makes infiltration-excess overland flow a plausible mechanism for this event. However, rainfall intensity alone is not sufficient to diagnose Hortonian runoff. We do not have measurements of infiltration capacity, internal surface runoff, or flow pathways. In addition, for this event, concurrent soil-water observations are unavailable. The available observations therefore do not allow us to determine whether rainfall intensity exceeded local infiltration capacity or to distinguish infiltration-excess runoff from other possible runoff-generation mechanisms.

We will revise the text accordingly, noting that the event characteristics are compatible with infiltration-excess runoff while making clear that the mechanism cannot be established from the available measurements.

SC-R18 – L342: I would write “based on the consistent discrimination” rather than “based on their consistent discrimination”. In the same line, you retain maximum SM and ΔSM : what led you to set average SM aside, given that at lines 317 to 318 it was also reported to separate the two event types?

Authors response

We thank the reviewer for this comment. Rather than simply replacing “their” with “the”, we will revise the sentence because the rationale for retaining maximum SM and Δ SM also requires clarification.

The selection was not based solely on which metrics discriminated between storm-flow and non-flow events. Average SM also showed clear differences between the two event classes and remains included in the statistical comparisons. For the spatial analysis, however, maximum relative wetness and Δ relative wetness were retained because they describe two complementary and more readily interpretable aspects of the local event response. Maximum relative wetness represents the highest wetness state attained at a monitoring position during the event, expressed relative to that sensor’s own observed wetness distribution. Δ relative wetness represents the magnitude of event-scale wetting from the wetness state at event onset to the maximum wetness attained during the event.

Average relative wetness, by contrast, integrates conditions over the entire event window and therefore reflects a combination of the initial wetness state, the magnitude and timing of the soil-water response, and the persistence of elevated wetness during the event. Although informative for comparing storm-flow and non-flow events, it does not isolate either the state attained or the magnitude of event-scale wetting as directly as maximum relative wetness and Δ relative wetness. Including it in the spatial figures would therefore provide partly overlapping information rather than an additional distinct dimension of the local response.

We will revise the corresponding text to make this rationale explicit, while retaining average relative wetness in the statistical comparisons.

Discussion

SC-D1 – L435: where you refer to “internal storage”, perhaps “subsurface storage”, “ground storage” or “soil storage” would each be more specific.

Authors response

We thank the reviewer for this comment. We agree that “internal storage” is unnecessarily vague in this context and will replace it with the more precise term “soil storage”.

SC-D2 – L441-443: you attribute the likelihood of outlet response to an internal state shaped by SM, P and atmospheric [water] demand. The evidence presented does not seem to support atmospheric demand as a control on R occurrence. November is consistently among the months of lowest reference evapotranspiration, and November 2021 was anomalously wet and contributed disproportionately to total R. Atmospheric demand varies on a seasonal timescale, whereas R generation here responds to individual storms and, in particular, to their intensity: over the few days that separate antecedent wetting from an intense event, atmospheric demand does not change materially. That a small number of high magnitude storms dominates the R record itself suggests that atmospheric demand is not a determining factor at the event scale. Restricting its role to the seasonal conditioning of SM, would make this passage more robust.

Authors response

We agree that atmospheric water demand should not be presented as an event-scale control on storm-flow occurrence. In the submitted Discussion, precipitation, soil moisture, and atmospheric water demand were grouped together as components of the internal state

controlling outlet response, which overstates the role directly supported for atmospheric water demand.

In the revised manuscript, atmospheric water demand will instead be discussed as a seasonal conditioning factor contributing to soil-water depletion and the evolution of antecedent wetness between precipitation events. Event-scale storm-flow occurrence will be interpreted primarily in relation to precipitation forcing and antecedent soil-water state. We agree with the reviewer's broader interpretation in this respect, although we do not infer from the November 2021 sequence alone that atmospheric water demand is unimportant at event scale, nor do our revised analyses identify rainfall intensity as the dominant precipitation control. Consistent with the revised probabilistic storm-flow-occurrence analysis, precipitation forcing, particularly total precipitation, provides the more robust general predictor of storm-flow occurrence, while the additional contribution of antecedent SM is strongly influenced by the exceptionally wet November 2021. Reference evapotranspiration is not included as an event-scale predictor in these models, and we will revise the Discussion accordingly.

SC-D3 – L450: what is meant by prolonging functional connectivity? Is the sense one of delay, or of continuity in time? The difficulty is compounded by the fact that functional connectivity is not measured; the index mentioned earlier is not presented with data or figures, so there is no quantity to which prolongation could refer.

Authors response

We agree that the expression “prolonging functional connectivity” is not sufficiently supported by the measurements. The intended meaning was greater temporal persistence or continuity, rather than a delay in the onset of connectivity. However, functional connectivity was not directly measured, so this temporal interpretation cannot be quantified from the available observations.

We also clarify that the IC introduced earlier characterises the comparatively stable structural configuration of the terrain and surface-routing potential. It is not a measure of event-scale functional connectivity and therefore cannot provide a quantity to which “prolongation” could refer.

We will therefore remove the statement that deeper soil-water storage sustained lateral flow or prolonged functional connectivity. Depth-related differences will instead be described only in terms of the observed soil-water dynamics, without inferring persistence of lateral flow or internal flow pathways. Where such processes are discussed, they will be presented only as possible mechanisms consistent with the observations rather than as measured responses.

SC-D4 – L466-469: the statements that R occurrence reflects the seasonal reorganisation of connectivity, and that structural connectivity defines the potential flow pathways, come close to the definitions that are missing earlier. They would be considerably more useful in the Introduction, expressed in terms of objective metrics, and accompanied by a statement of how structural connectivity relates to hydrological coupling.

Authors response

We agree that the conceptual and operational definitions should appear before they are used interpretatively in the Discussion. As described in our response to SC-I5, the revised

Introduction will distinguish explicitly between the broader concept of hydrological connectivity, structural connectivity, and hydrological coupling. Structural connectivity will be represented by the terrain-based IC, which characterises the comparatively stable spatial configuration relevant to potential surface routing. Hydrological coupling will be defined operationally as the event-scale correspondence between soil-water dynamics at individual monitoring positions and the integrated outlet response, evaluated through (i) correspondence between local relative-wetness states and the presence or absence of measurable outlet discharge and (ii) the timing of detectable local soil-water response relative to outlet-discharge onset.

The revised Introduction will also state explicitly how these concepts are related. The IC-derived structural setting provides the spatial context within which event-scale hydrological responses occur, whereas hydrological coupling describes the observed, dynamic local-to-outlet correspondence. Structural connectivity therefore may condition hydrological coupling, but the IC does not itself demonstrate that a monitoring position is functionally connected to the outlet or identify the pathway through which water is transferred.

We will also remove wording implying that storm-flow occurrence itself reflects a measured “seasonal reorganisation of connectivity”. Likewise, we will not retain the statement that structural connectivity “defines” potential flow pathways, because the IC characterises potential routing configuration rather than directly identifying flow pathways. Seasonal changes will instead be described in terms of storm-flow occurrence, soil-water state, and the observed local-to-outlet correspondence. The Discussion will then use these terms consistently with the operational definitions introduced earlier.

SC-D5 — L476-479: this passage is not entirely clear to me and might benefit from reformulation. Beyond the wording, the substantive question is how a dynamic activation of connectivity can be concluded when connectivity is neither measured nor modelled in this study. What is the quantity, where is it observed, and by what method is it evaluated?

Authors response

We agree that the submitted wording about the “dynamic activation of connectivity” was not linked to an objectively evaluated quantity and therefore overstated the evidence. We will remove this formulation and will not interpret the observations as demonstrating activation of functional connectivity or of a particular internal flow pathway.

In the revised manuscript, we instead evaluate hydrological coupling operationally as the event-scale correspondence between soil-water dynamics at individual monitoring positions and the integrated response measured at the microcatchment outlet. Two complementary quantities are used. First, wetness-state correspondence is evaluated from the sensor-specific relative-wetness distributions observed with and without measurable outlet discharge within precipitation-event response windows, with their overlap quantified descriptively. Second, response timing is evaluated for storm-flow events as the lag between detectable local soil-water response at each monitoring position and outlet-discharge onset. The latter is identified from the 15-min soil-moisture series relative to the preceding local baseline, with sensitivity to the response criterion also evaluated.

Thus, the quantities are observed at the individual soil-moisture monitoring positions and at the outlet, and are evaluated through wetness-state overlap and event-scale onset lag, respectively.

These analyses quantify spatial variation in local-to-outlet hydrological correspondence, but they do not demonstrate direct water transfer from a monitoring position to the outlet or identify the physical pathway involved.

The Discussion will be reformulated on this basis.

SC-D6 – L486: you report that total P and IP_{max15} remained consistently associated with R occurrence. Why these two variables in particular, and what does this imply about the underlying process? The association with short duration intensity would be consistent with an infiltration excess (Hortonian) mechanism, whereas the emphasis elsewhere on progressive wetting and storage replenishment points towards a saturation excess (Dunne) mechanism. Clarifying which is operating, and on what evidence, would strengthen the paper considerably.

Authors response

We agree that the relative roles of precipitation amount and intensity, and their mechanistic implications, require clearer interpretation. In the revised probabilistic storm-flow-occurrence analysis, total precipitation and IP_{max15} were evaluated together with storm duration and antecedent SM rather than selected a priori as the relevant precipitation variables. Total precipitation emerged as the most robust predictor of storm-flow occurrence, with essentially complete model support across both depths. IP_{max15} provided additional predictive information, but its support was substantially weaker than that of total precipitation. We therefore do not interpret precipitation amount and intensity as equally important controls.

We also agree that these associations are relevant to runoff-generation mechanisms, but we do not think they allow a unique distinction between infiltration-excess and saturation-excess runoff. High short-duration rainfall intensity is compatible with infiltration-excess runoff where rainfall intensity exceeds local infiltration capacity, and this is particularly plausible for the exceptional high-intensity dry-period event discussed above. However, infiltration capacity was not measured, so the association with IP_{max15} does not itself demonstrate a Hortonian mechanism.

Conversely, the strong association with total precipitation is compatible with the need for sufficient water input to overcome catchment storage deficits, while the contribution of antecedent wetness in the complete-record analysis is consistent with greater storm-flow occurrence under wetter conditions. However, the sensitivity analysis shows that the additional predictive contribution of antecedent SM is strongly influenced by the exceptionally wet November 2021 sequence. These patterns are therefore compatible with storage-dependent or saturation-related responses, but they do not demonstrate a saturation-excess mechanism.

The two mechanisms should consequently not be treated as mutually exclusive alternatives that can be diagnosed from the present analysis. Their relative importance may vary among events and hydrological conditions. Because infiltration capacity, spatial saturation extent, internal surface runoff, lateral fluxes, and flow pathways were not measured, the available observations cannot distinguish uniquely among infiltration-excess, saturation-excess, fill-and-spill, or combinations of these mechanisms. We will revise the Discussion accordingly, separating the observed predictors of storm-flow occurrence from the runoff-generation mechanisms that could plausibly produce those patterns.

SC-D7 — L486-493: this passage contains genuinely valuable findings and is worth foregrounding. In summary: total IP_{max15} minute intensity are associated with R occurrence across all seasons, identifying them as the primary triggers; maximum SM and ΔSM separate the two event types consistently throughout the year, indicating that both the peak wetness attained and the magnitude of wetting during the event are controls that operate year round; whereas antecedent SM discriminates only in the wet season, when soils approach saturation and, as you put it, relatively small P inputs could trigger outlet flow. Outside that season its predictive value declines and the response depends more strongly on P magnitude. I believe this is a coherent and well supported account, and it does not depend on the connectivity framework.

Authors response

We agree that the temporal and event-scale contrasts highlighted in this passage are among the most directly supported results and should receive greater prominence in the revised manuscript. However, several elements of the interpretation proposed here require qualification in light of the revised analyses.

Total precipitation and IP_{max15} are both significantly higher for storm-flow than non-flow events during the wet, drying-down, and wetting-up periods; no inferential comparison is possible for the dry period because only one storm-flow event occurred. In the revised probabilistic storm-flow-occurrence analysis, however, total precipitation emerges as the most robust general predictor, whereas IP_{max15} provides additional but weaker predictive information. We will therefore avoid describing the two variables jointly as equivalent “primary triggers”.

Maximum SM and ΔSM likewise differ between storm-flow and non-flow events across all hydrological periods for which inferential comparison is possible. However, because both metrics develop during the precipitation event, we do not interpret them as independent controls or predictors of storm-flow occurrence. They instead describe complementary aspects of the within-event soil-water response: the wetness state attained and the magnitude of event-scale wetting, respectively. In the revised sensor-specific spatial analysis, maximum relative wetness shows the more spatially consistent storm-flow/non-flow contrast, whereas Δ relative wetness is more strongly seasonally variable.

Antecedent SM differs between event classes in the wet period but not during drying-down or wetting-up. We will retain this seasonal contrast, while avoiding the stronger inference that wet-period soils were necessarily approaching saturation. The probabilistic sensitivity analysis further shows that the additional predictive contribution of antecedent SM is strongly influenced by the exceptionally wet November 2021 sequence, so we do not interpret antecedent wetness as a generally important predictor across all wet conditions.

We will therefore foreground the event-scale and seasonal results, as suggested by the reviewer, while distinguishing clearly between precipitation predictors, antecedent state, and within-event soil-water responses rather than treating them as equivalent controls. These findings can be interpreted independently of the spatial coupling framework, but they do not replace the revised hydrological-coupling analysis, which addresses a separate question: how local soil-water responses at different monitoring positions correspond in state and timing with the integrated outlet response.

SC-D8 — L498: “lateral surface flow activation” would be preferable to “lateral flow activation”, given that no evidence of subsurface lateral flow is presented. It is worth noting in any case that no lateral flux of either kind is measured directly in this study.

Authors response

We agree with the reviewer’s underlying concern that no lateral water flux, either at the surface or in the subsurface, was measured directly. We therefore do not consider that replacing “lateral flow activation” with “lateral surface flow activation” would be justified, because the available observations do not identify either pathway.

We will instead remove the statement that specific soil-water conditions are required to activate lateral flow. Where lateral redistribution is discussed, it will be framed explicitly as a possible mechanism consistent with the observed patterns rather than as a measured process.

SC-D9 — L500-502: the association between large changes in SM and R occurrence during drying down and wetting up periods is interpreted as a fill and spill mechanism. This interpretation is plausible, and it would be worth developing further, including its relationship to saturation-excess (Dunne) generation, and indicating what evidence would be required to distinguish it from the alternatives.

Authors response

We agree that fill-and-spill behaviour is a plausible process hypothesis for some of the observed event-scale wetting responses, but large changes in soil moisture alone do not provide specific evidence that fill-and-spill occurred. The available measurements do not identify the internal pathway by which stored water may have become connected to the outlet.

We will therefore discuss fill-and-spill as a possible storage-threshold mechanism rather than as the demonstrated explanation for the observed storm-flow/non-flow contrasts. In this framework, local storage deficits would first be replenished and subsequent inputs could promote downslope transfer once sufficient storage had been attained. This mechanism is not necessarily distinct from saturation-excess runoff: fill-and-spill can involve the development and connection of locally saturated or near-saturated areas, whereas saturation-excess overland flow specifically requires that the relevant source area becomes saturated and additional water is routed at or near the surface. Neither condition was measured directly in this study.

We will also distinguish these interpretations from infiltration-excess runoff, which would require evidence that rainfall intensity exceeded local infiltration capacity. Discriminating among these mechanisms would require observations of the spatial and temporal development of saturation and hydraulic gradients, for example through soil-water-pressure or piezometric measurements, together with measurements of infiltration capacity and internal surface or lateral water fluxes. Tracers could provide additional evidence on source areas and flow pathways.

The revised Discussion will therefore present fill-and-spill, saturation-excess, and infiltration-excess as plausible and potentially overlapping process interpretations, while making clear that the present observations do not uniquely identify any of them.

SC-D10 – L504-505: the statement that deep soil wetting is necessary to activate lateral flow does not appear to be supported by measurement. ΔSM is a subsurface quantity, and unless conditions approach saturation, which would be an extreme case here, its relevance to the activation of lateral flow is not established by the data presented.

Authors response

We agree. The statement that deep soil wetting is necessary to activate lateral flow is not supported by the measurements. The 50-cm soil-water response describes local soil-water change at that monitored depth and does not demonstrate either saturation or lateral water transfer.

We will remove this causal statement. Deeper soil-water changes will instead be interpreted as evidence of event-scale wetting at the monitored depth, while any possible relationship with subsurface water redistribution or lateral transfer will be presented only as a hypothesis requiring direct process measurements.

SC-D11 – L510-512: this material restates arguments made earlier in the Discussion. More generally, Section 4.2 is demanding to follow, and the Discussion as a whole revisits the same results and interpretations in more than one place. A nested organisation, treating the temporal scale and then the spatial scale, might be more efficient. Conveying some of the discursive text through tables or schematic representations would also help the reader considerably, given the length of this section.

Authors response

We agree that the current Discussion contains repetition and that the distinction between temporal and spatial scales should be made clearer. As described in our response to Recommendation 6, the Discussion will therefore be substantially reorganised and shortened.

The revised structure will proceed first from seasonal and event-scale relationships associated with storm-flow occurrence to spatial variation in local-to-outlet hydrological coupling, evaluated through the revised wetness-state correspondence and response-timing analyses. Material currently repeated between Sects. 4.1 and 4.2, including discussion of seasonal soil-water behaviour, spatial contrasts, and connectivity, will be consolidated so that each result is interpreted principally in one place. Mechanistic interpretations will be clearly separated from the directly observed relationships and treated more cautiously.

We will also substantially shorten the broader implications material so that the central observational results remain prominent. We have considered the suggestion of transferring some of the Discussion to an additional table or schematic summary. However, because the principal temporal and spatial results are already presented in the revised tables and figures, we consider that adding a further summary element could introduce additional duplication. We will therefore first address the readability issue through restructuring, consolidation, and shortening of the text rather than adding another summary table or schematic.

SC-D12 – L599-601: the treatment of vegetation and its influence on SM depletion and recharge is hard to follow here, and it overlaps substantially with the evapotranspiration material in Section 4.1.1. It is not entirely clear how Section 4.2.3 differs in scope from Section 4.1.1, and the two might usefully be consolidated.

Authors response

We agree that the current treatment of vegetation and atmospheric water demand in Sect. 4.2.3 overlaps with material already discussed in Sect. 4.1.1, and that this contributes to an unclear distinction between the scopes of the two sections.

As part of the broader restructuring of the Discussion, this duplicated seasonal material will be consolidated. Seasonal soil-water dynamics and atmospheric water demand will be discussed once within the temporal and event-scale section, whereas the subsequent spatial section will focus specifically on variation in local-to-outlet hydrological coupling among monitored positions.

Because vegetation was not analysed explicitly in this study, we will also avoid attributing observed differences in soil-water dynamics, runoff pathways, or hydrological coupling directly to vegetation. Where vegetation is retained, it will be used only as broader contextual interpretation supported by the literature, or identified explicitly as a hypothesis requiring further investigation.

SC-D13 – L606: the statement that this study demonstrates that R occurrence in terraced Mediterranean systems emerges from the spatial organisation of hydrological coupling is, in my assessment, considerably stronger than the evidence allows. Given the small number of R events, the limitations of spatial sampling, and the absence of any direct measurement of coupling, a more cautious formulation would be better matched to the data.

Authors response

We agree that the submitted statement is stronger than the evidence supports and will remove it. In particular, the present observations do not justify the general claim that storm-flow occurrence in terraced Mediterranean systems emerges from the spatial organisation of hydrological coupling.

The revised synthesis will distinguish two results that are supported by the observations. First, storm-flow occurrence is strongly episodic and is associated primarily with precipitation forcing, particularly total precipitation. The additional predictive contribution of antecedent SM is concentrated in the exceptionally wet November 2021 sequence rather than being generally supported across all monitored conditions. Second, hydrological coupling is evaluated operationally among the monitored positions through wetness-state correspondence with outlet-flow state and the relative timing of detectable local soil-water response and outlet-discharge onset. These analyses show spatial variation in local-to-outlet correspondence among the monitored positions.

We therefore distinguish between an operational assessment of hydrological coupling and direct measurement of internal water transfer. The latter was not available in this study. The limited number and uneven temporal distribution of storm-flow events, together with the targeted spatial coverage of the monitoring network, also constrain the extent to which these spatial relationships can be generalised beyond the monitored positions.

Accordingly, the revised Discussion will not claim that the spatial organisation of hydrological coupling controls storm-flow occurrence across the complete microcatchment or more broadly

across terraced Mediterranean systems, nor will it infer the internal transfer mechanisms responsible.

SC-D14 – L613-620: the discussion of terrace abandonment, degradation, afforestation and wildfire, and of their effects on the balance between storage and transmission, addresses important themes, although none of them was measured or tested here, so the discussion necessarily draws on the wider literature rather than on the results presented. There are no data in this study against which these effects could be contrasted or tested.

Authors response

We agree with the reviewer's main point that the hydrological effects of terrace abandonment and degradation, afforestation, and wildfire were not isolated or tested in the present analyses. These factors form part of the documented environmental history and structural context of the study site, but the monitoring design does not provide contrasts from which their individual effects on water storage, transmission, storm-flow occurrence, or hydrological coupling can be quantified.

We will therefore substantially reduce this part of the Discussion. Where these factors are retained, they will be used only to contextualise the study landscape or to identify broader hypotheses supported by previous literature, rather than to explain the observed hydrological responses in the present dataset. Statements attributing the observed response directly to these factors will be removed.

In particular, the speculative statement that the unmonitored western sector likely contributes disproportionately to rapid hydrological response or connectivity will be removed, because no observations from that sector are available to support such an inference.

SC-D15 – L623: the acknowledgement that vegetation effects were not explicitly assessed is welcome, although the difficulty identified above remains, since the surrounding text still attributes hydrological consequences to processes that were not observed. One option would be to set these themes aside; another would be to retain them within a clearly labelled section on directions for future research, where their status would be unambiguous.

Authors response

We agree that acknowledging that vegetation effects were not explicitly assessed is not sufficient if the surrounding text subsequently attributes the observed hydrological behaviour to vegetation. Although vegetation characteristics form part of the study-site description, the present analyses do not test their effects on soil-water dynamics, runoff generation, or hydrological coupling.

We will therefore remove vegetation as an explanatory factor for the observed spatial hydrological patterns. Where vegetation is retained in the Discussion, it will be presented only as broader ecological context or as a potential influence identified from previous literature, not as a mechanism demonstrated by the present dataset. Potential effects of interception, evapotranspiration, rooting, and vegetation structure will instead be framed explicitly as hypotheses for future investigation.

SC-D16 – L624: the reference to the persistence of functional connectivity across hillslopes is presented as a finding, although functional connectivity is neither defined in objective terms nor measured or computed anywhere in the manuscript.

Authors response

We agree that the submitted statement about the persistence of functional connectivity across hillslopes overstates what was measured. Functional connectivity was invoked conceptually in the submitted manuscript but was not operationalised as a measured or computed variable, and the available observations do not support an inference about the persistence of internal hydrological pathways. We will therefore remove this formulation.

Consistent with the revised terminology, functional connectivity will be retained only as a conceptual, event-dependent component of the broader hydrological-connectivity framework and will not be presented as a measured result of this study. Spatial relationships between the monitored positions and the outlet will instead be described in terms of hydrological coupling, operationally evaluated through wetness-state correspondence with outlet-flow state and the timing of detectable local SM response relative to outlet-discharge onset.

These observations provide information relevant to the functional component of hydrological connectivity, but they do not demonstrate the persistence, continuity, or physical pathway of internal water transfer.

Conclusions

SC-C1 – L643-647: you conclude that R occurrence was strongly modulated by seasonal variations in SM, which controlled the extent of functional hydrological coupling across the micro catchment. Following your own definition of coupling, was this quantity measured in any way? If the intention is to refer to the spatial index mentioned in the methods, where is that analysis presented? As written, the conclusion asserts a result for which no supporting analysis appears in the paper.

Authors response

We agree that the submitted conclusion is not supported in this form. No catchment-wide quantity representing the “extent of functional hydrological coupling” was measured or computed. The IC is not the quantity referred to in this sentence: it characterises the comparatively stable terrain and surface configuration associated with structural connectivity and will not be used to infer event-scale functional connectivity.

The Conclusions will therefore be rewritten to separate the temporal and spatial findings. For storm-flow occurrence, total precipitation will be identified as the most robust general predictor, with IPmax15 providing additional information, while the additional predictive contribution of antecedent SM will be identified as being concentrated in the exceptionally wet November 2021 sequence. Maximum SM and Δ SM will be treated separately as within-event soil-water responses: storm-flow events attained higher maximum SM and larger event-scale wetting than non-flow events during the wet, drying-down, and wetting-up periods, without interpreting these metrics as independent pre-event predictors or causal controls.

Separately, hydrological coupling will refer to the event-scale relationship between soil-water dynamics at individual monitored positions and the integrated outlet response, operationally evaluated through wetness-state correspondence with outlet-flow state and the timing of detectable local SM response relative to outlet-discharge onset. These analyses allow us to describe spatial variation in hydrological coupling among the monitored positions, but they do not quantify the spatial extent of functional connectivity across the microcatchment.

We will therefore remove the conclusion that seasonal soil-water variation “controlled the extent of functional hydrological coupling across the microcatchment”.

SC-C2 – L650: subsurface connectivity appears here, so far as I can determine, for the first and only time in the manuscript, and it appears directly in the Conclusions. What is the definition of this term, and by what means was it verified?

Authors response

We agree. “Subsurface connectivity” appears only in the submitted Conclusions and was neither operationally defined nor directly measured in this study. It should therefore not be presented as a finding, and the term will be removed.

The corresponding conclusion will instead describe ΔSM simply as a within-event soil-water response metric representing the magnitude of event-scale wetting. Its association with storm-flow occurrence will not be interpreted as evidence for the activation of subsurface connectivity, lateral subsurface flow, or any specific internal water-transfer pathway.

SC-C3 – L652-656: the contrasts described among hillslope, high-connectivity terrace and disconnected mid-terrace positions are interesting, and the observation that similar event scale responses may arise from different underlying mechanisms is a valuable one. The difficulty is that the R generating mechanisms themselves are not clearly established by the evidence presented, so the attribution of the contrasts to distinct mechanisms remains a hypothesis rather than a conclusion.

Authors response

We agree that the observed contrasts among monitoring positions are more directly supported than the mechanistic explanations originally attached to them.

The revised Conclusions will retain the observed spatial differences among monitoring positions, described in terms of wetness-state correspondence with outlet-flow state and the timing of detectable local SM response relative to outlet-discharge onset. The existing connectivity-based position names will be retained, but terms such as “high-connectivity” and “disconnected” will be defined explicitly as referring to IC-derived structural connectivity rather than to the observed strength of hydrological coupling.

We will remove from the Conclusions the stronger interpretation that the contrasting responses demonstrate distinct runoff-generation mechanisms. Potential explanations involving differences in storage, lateral redistribution, infiltration-excess runoff, saturation-excess runoff, or fill-and-spill behaviour will instead be discussed as mechanistic hypotheses consistent with the observations, not as demonstrated causes of the spatial contrasts.

Technical corrections

Below I recommend technical and typographical corrections to this manuscript, and some wording suggestions. In each case the wording of the manuscript is given in quotation marks, followed by the proposed change.

***SC-T1 – L22-23:** "legacy features such as stone walls modify storage and flow pathways". Consider replacing "modify" with "determine", "define" or "configure".*

Authors response

As described in our response to SC-A1, we agree that “modify” understates the structural role of terrace walls. We will therefore revise the wording to describe legacy terrace structures such as dry-stone walls as configuring local water storage and potential flow-routing pathways. Consistent with the revised distinction between structural connectivity and hydrological coupling, we will avoid implying that their effect on event-scale hydrological coupling was directly demonstrated by the present observations.

***SC-T2 – L24:** "and water repellence, and lower infiltration capacity, enhancing R generation". Consider: "and water repellence, hence lowering infiltration capacity, enhancing R generation".*

Authors response

As described in our response to SC-A2, this sentence will be substantially rewritten rather than corrected only syntactically. The generic sequence linking wildfire, water repellence, reduced infiltration capacity, enhanced runoff generation, and reorganisation of connectivity will be removed from the Abstract because these processes were not evaluated directly in the present study.

***SC-T3 – L64:** "the transient filling of soil storage". Consider: "the transient filling of soil storage capacity".*

Authors response

As stated in response to SC-I1, we agree that the original wording can be made more precise, but we do not consider “soil storage capacity” to be the appropriate replacement in this context. Storage capacity refers to the maximum amount of water that can be stored, whereas the sentence refers to the dynamic filling of soil-water storage during an event. We will therefore revise the expression to “transient filling of soil-water storage”.

***SC-T4 – L92-93:** "and induce significant but time dependent changes in vegetation cover, soil properties, and infiltration capacity". Consider: "and induced significant and time dependent changes in vegetation cover, soil properties, and infiltration capacity".*

Authors response

As described in response to SC-I6, we agree that “and” is more appropriate than “but” and that the original enumeration is not strictly parallel because infiltration capacity is itself a soil property. We will therefore revise the sentence to refer to significant and time-dependent changes in vegetation cover and soil properties, including infiltration capacity. The verb tense will be adjusted to the revised sentence structure.

SC-T5 – L265: "with a R coefficient of 0.1 %". Should read "with an R coefficient of 0.1 %", and consider "with an annual R coefficient of 0.1 %".

Authors response

As described in response to SC-R5, we will identify 0.1 % explicitly as the annual runoff coefficient for 2024. Because the revised manuscript will avoid the abbreviation “R coefficient”, the specific “a/an R coefficient” construction will no longer occur.

SC-T6 – L278: "high atmospheric demand". Consider: "high atmospheric water demand".

Authors response

We agree and, as stated in response to SC-R9, will use “high atmospheric water demand”.

SC-T7 – L339: consider opening the sentence with "Over the observation period, only two...", so that the small number of cases is stated plainly.

Authors response

As noted in response to SC-R16, the revised event catalogue contains only one storm-flow event during the dry period. The sentence will therefore be rewritten to state this explicitly rather than referring to two events.

SC-T8 – L342: "based on their consistent discrimination". Consider: "based on the consistent discrimination".

Authors response

As explained in response to SC-R18, the sentence will be rewritten more substantially because maximum relative wetness and Δ relative wetness are no longer selected simply on the basis of “consistent discrimination”. Their complementary roles as measures of the wetness state attained during the event and the magnitude of event-scale wetting, respectively, will instead be stated explicitly.

SC-T9 – L435: "where internal storage imposes". Consider: "where subsurface storage imposes".

Authors response

We agree that “internal storage” should be replaced by a more specific term. As stated in response to SC-D1, we will use “soil storage” in this context.

SC-T10 – L498: "critical SM conditions required for lateral flow activation". Consider: "critical SM conditions required for lateral surface flow activation".

Authors response

We agree that no lateral flux was measured directly. For this reason, rather than replacing “lateral flow activation” with “lateral surface flow activation”, we will remove the unsupported mechanistic statement altogether. Where lateral redistribution is discussed, it will be presented only as a possible mechanism consistent with the observations.

SC-T11 – Throughout: "Average SM at 10 and 50 cm". Where a catchment wide value is intended, consider "Catchment average SM", to distinguish it from the local values discussed elsewhere.

Authors response

We agree that these values should be distinguished more clearly from measurements at individual monitoring locations. However, because the monitoring network does not provide spatially representative coverage of the complete microcatchment, we consider “catchment-average soil moisture” potentially misleading. Consistent with our response to SC-R6, we will instead use “network-mean soil moisture” at the corresponding depth, indicating that these values represent averages across the monitored sensors rather than catchment-wide estimates.

SC-T12 – Figure 3(a): the column headed "study period" for the R coefficient. Consider "annual mean for the study period", since it is an average.

Authors response

As described in response to SC-R6, the runoff coefficient reported for the study period is not an arithmetic mean of the annual runoff coefficients, but an aggregated study-period coefficient. We will therefore retain the designation “study-period runoff coefficient” rather than relabelling it as an annual mean, and we will make its calculation basis explicit in the figure caption. The revised figure will also indicate that the 2023 runoff depth and runoff coefficient are not quantifiable because of the incomplete outlet-discharge record, rather than treating them as zero.

SC-T13 – All figures: please consider increasing the size of lettering, axis labels and legends throughout, as several panels are not easy to read at the size at which they are reproduced.

Authors response

We thank the reviewer for this suggestion. Several figures will be substantially redesigned as part of the revision, and we will review all graphical elements at their final publication dimensions to ensure that lettering, axis labels, symbols, and legends remain clearly legible. Sizes will be adjusted where necessary rather than applying a uniform enlargement to all panels.

SC-T14 – Figure 1(c): the marks shown in the panel are not explained in the caption.

Authors response

As clarified in response to SC-M6, the marks in panel (c) correspond to the monitoring symbols also identified elsewhere in Fig. 1, including the soil-moisture monitoring positions, experimental meteorological station, and outlet gauging station. The revised Fig. 1 and caption will identify these elements explicitly.

SC-T15 – Figure 2(c): the orange lines or delineations shown in the panel are not explained in the caption.

Authors response

As clarified in response to SC-M13, the orange linear features in the submitted panel (c) are not separate mapped delineations, but correspond to relatively high IC values within the continuous IC raster and are represented by the same colour scale as the remainder of the map.

Because Fig. 2 will be redesigned as an experimental-design figure, this ambiguity will not persist in the revised main figure. The revised Fig. 1 will retain the IC as part of the study-area and structural-context information, while the more detailed IC mapping currently shown in Fig. 2 may be retained in the Supplementary Material to document the structural setting and original monitoring design.

References

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