Groundwater Hysteresis Increasingly Decouples Flowing Network Length from Streamflow as Snow Shifts to Rain

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Response to Reviewer Comment RC2

We greatly appreciate the reviewers' helpful comments on this manuscript, which we will address in a revision. Our comments are interspersed into the review in blue.

This manuscript presents a substantial and carefully executed investigation of stream-network dynamics and groundwater–surface water interactions under changing climatic conditions. The study combines distributed hydrological modelling with water-quality observations and analyses conducted across multiple temporal scales, representing a considerable modelling and analytical effort. The manuscript is relatively long, although generally well written, and addresses a timely and relevant topic, particularly in the context of ongoing transitions from snow- to rain-dominated hydrological regimes.

Overall, the analyses are thorough and the main findings appear physically plausible and scientifically meaningful. The study provides an extensive exploration of network dynamics and their climatic controls, and the effort invested in model implementation and evaluation is evident throughout the manuscript. The comments below are primarily intended to further clarify aspects of process interpretation and model representation and to help strengthen the Ms.

We thank the reviewer for their interest in our study, and we look forward to addressing the issues raised below.

Major comments

A central contribution of the manuscript is the interpretation of the observed decoupling between flowing network length and discharge in terms of groundwater hysteresis. The modelling results clearly demonstrate the emergence of hysteretic L–Q behaviour. At the same time, the mechanisms described in the Results suggest that this behaviour may arise from differences in how distinct storage components translate into network activation versus discharge generation. For instance, portions of the drainage network may become active without contributing proportionally to outlet discharge, while rapid runoff components may increase discharge without producing equivalent network expansion. This suggests that groundwater hysteresis may operate in combination with activation and connectivity dynamics associated with faster flow pathways. This interpretation is somewhat less explicit in the title, abstract, and introduction, where groundwater hysteresis appears as the primary explanatory mechanism. Clarifying this causal chain would help strengthen the physical interpretation of the results and more clearly distinguish groundwater memory effects from activation–connectivity processes governing network expansion.

We agree that there are multiple interacting sources of spatiotemporal variability that lead to the simulated network dynamics. However, the “rapid runoff components” mentioned by the reviewer are still primarily a groundwater behavior (at least in the model), since these pathways are associated with the rapid increase in transmissivity as the water table becomes shallower (exponential decrease parameter in DHSVM). In fact, these rapid components are implicitly contrasted with slower groundwater pathways, which would imply that the hysteresis caused by groundwater discharge from slowly draining pathways is still the primary explanation for L-Q anomalies in our simulations.

Nevertheless, we will clarify throughout that different groundwater flowpaths can lead to nonlinear activation and connectivity behaviors that ultimately contribute to L-Q dynamics. Even so, these activation behaviors are ultimately related to groundwater hysteresis (again, at least in model-world), since the antecedent water table level controls the rate at which new percolation is transmitted downslope. This relationship will be made more explicit in the manuscript.

A second aspect concerns the use of power-law scaling as a reference framework for interpreting L–Q behaviour. The simulated relationships shown (e.g., Fig. 3) appear to display a tendency toward flattening at higher inputs, suggesting behaviour that may depart from ideal scale-free growth. Because the double-logarithmic representation can partially mask such curvature, presenting the relationship also in natural (non-logarithmic) space could help clarify the interpretation. A more cautious framing of the power law as one possible benchmark rather than an expected scaling relationship may therefore be beneficial. Exploring alternative saturating functional descriptions could further support interpretation of the simulated L–Q dynamics (see also Durighetto et al., 2025). Importantly, the main conclusions of the manuscript do not appear to rely critically on the assumption of power-law scaling.

Yes, we agree that the power law is not valid in the high-flow domain, and this is addressed at the start of Sect. 3.2. In fact, we have limited our analysis to the range of $Q < 2$ mm/d, which is approximately where the L-Q relationship starts to flatten (Fig. 3). However, we also note that the power law is very widely used as the “reference” approximation, including by recent high-profile research in *Science* (Prancevic et al. 2025). Thus, framing the hysteresis as a deviation from the power law puts our study into the context of previous L-Q scaling research and provides a caveat associated with the recent high-profile extrapolation of power law dynamics across thousands of watersheds.

The study represents an impressive modelling effort combining distributed simulations with hydrochemical information and multi-scale analyses. The emergence of L–Q hysteresis within the model appears closely related to the representation of surface–subsurface interactions and to the coupling between surface routing and subsurface storage components. While these processes are clearly important, their implementation can be difficult to visualize from the present description. Providing a schematic representation and/or a more explicit description of exchange mechanisms would help readers better understand how hysteresis emerges within the modelling framework. It may also be useful to clarify which aspects of the inferred mechanisms are directly supported by observations and which primarily emerge from internal model dynamics.

We appreciate that the linkage between physics, model code, and visualized results is not always straightforward. We have already included several schematics related to the model surface-subsurface interactions (e.g., Figs. S1, S2, and 9). As requested by the other reviewer, we will also be adding several equations to the Methods to more explicitly show how the routing equations interact with the stream geometry. Finally, we will clarify that Fig. 9 (in the Discussion) shows several examples of how gaining and losing stream reaches are determined by the water level in the stream channel and dynamic groundwater level, as defined in Fig. S1.

Finally, the manuscript addresses an important question linking stream-network dynamics to hydroclimatic regime shifts. Given the breadth of analyses presented, some streamlining of the manuscript and a clearer separation between model results, mechanistic interpretation, and broader climatic implications could further improve readability and help highlight the novel contribution related to snow-to-rain transitions.

We will attempt to clarify and condense the wording where possible.

In line with the above arguments, the Discussion (which is quite extensive and properly puts this paper in the context of the literature in general) could benefit from a somewhat clearer comparison with previous studies addressing specifically L–Q hysteresis. Several observational and process-based investigations have shown that hysteretic behaviour may emerge from differing activation and connectivity dynamics across events and catchments (e.g., Jensen et al., 2019; Bujak-Ozga et al., 2023; Zanetti et al., 2024, Hydrological Processes). Positioning the mechanism proposed here relative to these interpretations may further reinforce the contribution and clarify its novelty.

We will revisit prior work on L-Q hysteresis to explain how our interpretation may both support and differ from other possible mechanisms.

Minor / inline comments

Line 28: The manuscript refers to headwater streams; however, the study catchment (~30 km^2) may not strictly fall within a typical headwater definition. Clarifying terminology or briefly explaining its relevance for the selected basin would improve consistency. Mentioning the broader global relevance of non-perennial streams could also help contextualize the study.

We have largely removed or clarified this term; in particular, the mentioned line has been changed to “first-order streams” since that is the actual definition used by the cited study.

Line 62: The terms non-monotonic and hysteretic appear at times closely associated. As these concepts describe different behaviours, clarifying terminology and maintaining consistent usage throughout the manuscript would improve interpretability.

We will clarify this wording throughout. We intend to use “non-monotonic” as a broader category of potential deviations from a power law, and “hysteretic” as the specific explanation for the scatter that we predict in our simulations.

Line 71: The statement that groundwater level significantly controls flowing network length may depend on dominant runoff-generation mechanisms. A brief clarification of the conditions under which groundwater exerts primary control on L would be helpful.

We have changed this wording to clarify that our interpretation applies “in regions where GW levels predominantly control L,” instead of claiming that GW controls L everywhere.

Line 74 (see also Line 744): Functional L–Q scaling, hysteresis in the L–Q relationship, and groundwater–discharge hysteresis are not necessarily fully equivalent processes. Clarifying these distinctions throughout the manuscript would improve conceptual clarity.

Agreed—we will attempt to clarify these terms when introduced and used throughout.

Lines 118–119: Zanetti et al. (2024, Hydrological Processes) discuss hydrological processes that potentially generate hysteresis within the water-(im)balance framework and may provide useful context here.

Thank you for pointing out the relevance of this study to this section of our introduction. We have added a sentence to this section discussing the Zanetti et al. study, though we also note that the study did not detect L-Q hysteresis despite variable climate conditions.

Line 120: I think a hierarchical organization within the drainage network does not necessarily imply a unique relationship between flowing length and discharge, but only a one-to-one relation between total flowing length and spatial configuration of the active network.

Thank you for pointing out this subtlety—we agree. We have amended this sentence to more clearly focus on nonstationarity in the spatial organization of stream networks, not just the L-Q relationship.

Line 134: If the selected basin is intended as representative, the research question could potentially be framed more broadly in terms of mountain catchments.

Although we believe our findings should generalize to many other catchments, Sagehen was selected through “convenience sampling” (i.e., where stream network surveys and EC data are already available), so it is not necessarily robust as a statistically representative sample of mountain catchments in general. Thus, we prefer to maintain the relatively limited scope of our current wording.

Lines 178–210: The surface–subsurface coupling description would benefit from a schematic representation or clearer formulation (in the main paper) to facilitate visualization of model processes.

We do include these schematics in Figs. S1-S2, but we will also add clarifying equations to the main text.

Section 2.1.5: I'm surprised by the fact that evaporation from channels might be important.. anyways this point better fits the discussion.

Even though channel evaporation is important for drying very small channels, it is still a relatively minor process. Thus, we believe this single sentence fits contextually into the methods, which is the only place that the channel evaporation is discussed.

Model parametrization: Unless I am missing something, additional information regarding assumptions on riverbed composition, hyporheic exchange processes, interaction depth, hydraulic conductivity, and parameterization of channel–subsurface exchanges would improve transparency/reproducibility.

One of the benefits or limitations (depending on perspective) of our approach is that it does not use any additional subsurface parameterization beyond what was previously required by DHSVM. Thus, there are no parameters or assumptions explicitly related to the hyporheic zone (other than the channel width and depth, which are described in the manuscript). This is described in Sect. 2.1, and it is also clarified in Sect. 2.1.2 and at the start of Sect. 3.1. For example, the hydraulic conductivity of the riverbed is the same as the hydraulic conductivity of the grid cell, and there is no explicit concept of hyporheic exchange other than the net inflow/outflow between surface/groundwater depending on relative water levels (Fig. S1).

Line 359: The use of a power-law formulation is arbitrary: Fig. 3 suggests that alternative functional descriptions could better capture the simulated dynamics.

Yes, but the power law is the accepted current paradigm for L-Q analysis, evidenced by a recent high-profile paper in *Science* (Prancevic et al. 2025) and numerous other L-Q studies. Thus, we are framing our results as a potential limitation or caveat associated with the accepted conceptual model, even though this widely accepted conceptual model is indeed arbitrary.

Line 415: Further clarification of the mechanisms responsible for simulated network disconnections would be informative.

We have added the following explanation:

In our simulations, channel disconnection occurs when the subsurface transmissivity exceeds the total down-valley flow, and channels re-activate on the surface when the total flow exceeds the subsurface capacity, which is consistent with the conceptual model of Godsey and Kirchner (2014).

Line 465: The statement that observed scaling reproduces a power-law relationship is not fully evident from the results; representation of L and Q in natural space may help clarify this point.

We have added the caveat that the power-law behavior is only reproduced within the low- to medium-flow range. We believe that visualization in log-log scale is most effective for this, since in the transformed space a power law is linear.

Line 467: Clarifying whether a maximum active network length is implicitly constrained by the modelling domain (see lines 259–264) would aid interpretation.

We have added a phrase clarifying that the geomorphic channel network is imposed in DHSVM (Fig. S3).

Figure 4: This figure is particularly informative and could benefit from expanded discussion and captioning. Clearer panel identification may also improve readability.

Since the manuscript is already quite long, we have tried to limit our discussion to only the essential points. However, we have still made an effort to interpret each of the 3 examples illustrated by this figure.

Section 3.3: Maybe I'm missing something here but clarifying the exact quantitative role/value of conductivity data within the analysis would strengthen interpretation.

The conductivity (EC) data provide observational evidence supporting a groundwater hysteresis behavior in the study catchment. We have added this clarification at the start of Sect. 3.3.

Lines 745–748: The discussion might more explicitly acknowledge here mechanistic formulations that address hysteresis and non-stationary network dynamics

We have added the caveat that "recent numerical modeling is beginning to account for these considerations (Zanetti et al. 2024)."