For clarity, reviewer comments and author responses are numbered throughout as R1C1, R1R1, R2C1, R2R1, etc., where “R1” and “R2” refer to Reviewer 1 and Reviewer 2, “C” refers to the reviewer comment, and “R” refers to our response. We use these labels to cross-reference related points where the two reviewers or our responses raise overlapping issues.

[R1C1]: *First, this manuscript is not to be treated as an effort in hydrologic modelling per se, because the developed framework is based on simplified approach to flow generation and routing. Each node is connected to the network, thus no distinction between surface/slope runoff and channelized runoff is made. Flow generation is controlled via power-law based recession (Eq. 8), precipitation to evaporation ratio, and very basic assumptions about soil properties (Eq. 14-15).*

[R1R1] We thank the reviewer for this useful framing comment. We agree that the manuscript should not be interpreted as presenting a fully calibrated catchment hydrology model or an operational rainfall–runoff model for a specific basin. The framework deliberately simplifies flow generation and routing in order to isolate the interaction between network topology, dynamic forcing, in-channel storage, and residence time.

However, we would still characterise the work as hydrological modelling in a mechanistic and theoretical sense, since the model explicitly simulates discharge, storage, flow disturbance, and residence times across a river network. The simplifications noted by the reviewer are intentional and are consistent with the aim of developing a controlled proof-of-concept framework rather than resolving all hillslope, groundwater, floodplain, and channel-exchange processes.

We will also revise the manuscript to make this scope clearer, in line with our response to R2C2. In particular, we will state more explicitly that effective precipitation is used as the forcing term, that non-channel runoff pathways are not explicitly resolved, and that the framework is intended to isolate residence-time dynamics in fragmented networks rather than provide a complete catchment hydrology model.

[R1C2] *The manuscript is overall consistently written and easy to follow, and without in depth review of the underlying equations I have no major comments to make concerning the work done. However, certain minor comments remain, and I am unsure as to what degree these might lead to minor or moderate revision*

[R1R2] We thank the reviewer for their careful reading of the manuscript and for the positive reaction to the paper. We are also grateful for the detailed technical comments, which will help us improve the clarity of the model setup, parameter definitions, formatting, and explanation of key assumptions. We try to address each of these comments below.

[R1C3] *First, I need a model setup displayed on the figures to be explained more in detail. E.g., a plate from Fig. 1 and forward has 100 x 100 = 10000 nodes (from caption to Fig. 1), has the total domain area of 400 km2 , hence li = 200 m (from caption to Fig. 3), this is only understood from figure captions, not the main text*

[R1R3] We thank the reviewer for this comment. We agree that the numerical domain and spatial discretisation were not described clearly enough in the main text. In the revised manuscript, we will introduce the model setup explicitly in Section 2 before presenting the figures or numerical experiments. This includes the 100 × 100 grid, the total of 10,000 nodes, the 400 km^2 domain area, the resulting 200 m grid spacing, and the definition of segment length for cardinal and diagonal flow connections.

[R1C4] *Certain model parameters are fixed by setup, and this comes unclear early in the MS, the fixed values are scattered across the MS text and mostly in captions; Table 1 comes certainly too late in the text, and might appear way earlier.*

[R1R4] We agree with the reviewer. Several fixed setup values and default parameters were introduced too late or were distributed across captions, which made the model setup less transparent than intended. We will therefore revise the manuscript so that the default parameters are introduced earlier and more systematically. This will also address the related point raised by Reviewer 2 (R2C5) regarding the placement of Table 1.

[R1C5] *For Eq. 5, the assumption of a2 + b2 + c2 = 1 is not met neither in the caption to Fig. 3 (0.26 + 0.32 + 0.34 = 0.92) nor in Table 1 (0.26 + 0.32 + 0.32 = 0.9). The second constraint on hydraulic geometry equations, a1b1c1 = 1, is not explicitly mentioned while it also has to, but in fact is met (2 x 0.4 x 1.25 = 1). It is unclear how that could affect the accuracy of the presented results. Also, the MS provides no explanation to how these hydraulic geometry exponent values were assigned, or I missed that altogether. Indeed, the classic Leopold & Maddock (1953) set is [0.5, 0.4, 0.1] but otherwise these could vary significantly.*

[R1R5] We thank the reviewer for identifying this issue. We agree that the hydraulic-geometry parameterisation requires clearer explanation and that the submitted manuscript contains inconsistencies in the reported exponent values. In the revised manuscript we will carefully recheck the hydraulic-geometry coefficients and exponents in Eq. 5, Figure 3, and Table 1, and correct these so that the parameterisation is internally consistent. We will also check the corresponding code values to determine whether this is only a reporting inconsistency in the manuscript or whether any simulation inputs require correction; if any model quantities or results are affected, these will be recomputed.

We will clarify that the proportionality constants are intended to satisfy a1 b1 c1 = 1, and that the exponent values should be consistent with the volumetric relation Q_i = w_i d_i v_i. We will revise the text so that this closure is stated correctly and so that the values used for width, depth, and velocity are unambiguous.

We will also clarify how the hydraulic-geometry relations enter the model calculations. Discharge is initially prescribed from contributing area, and channel width, depth, and velocity are then assigned using hydraulic-geometry relations. Width and depth set channel storage and therefore directly affect the residence-time calculation. Velocity is part of the hydraulic-geometry closure and initial hydraulic description, but in the current implementation it is not used as an independent dynamic state variable in the storage update. Therefore, if the inconsistency is confined to the reported velocity exponent, we do not expect it to alter the main residence-time results or conclusions; nevertheless, we will verify this explicitly during revision.

We will also add a brief explanation of the chosen outlet slope, eta_outlet = 0.005, noting that it sets the overall slope scale of the synthetic network and therefore influences the absolute ponded volumes and residence times.

[R1C6] *This may be totally legit, but I do not grasp why the model has to redefine Qi at each time step via Eq. 8 while it is intuitive to define Qi(t) = Qj (t-1), assuming that peak discharge moves exactly one segment per unit time regardless of v which can also be considered fixed. Ultimately, does the model really need Eq. 8 and, probably, Eq. 7? Node volume change equals difference between discharges at moment t, then node volume at (t+1) equals sum of discharges in upstream connected nodes at moment t (L115-116), then d can* *be derived either from Eq. 7 or Eq. 5 through corresponding hydraulic geometry coefficients, and Eq. 8 is thus expletive.*

[R1R6] We thank the reviewer for this comment. We agree that the role of Eqs. 7–8 was not explained clearly enough in the original manuscript and will revise the text to clarify this point. The model is not intended to route a discharge peak by translating (Q_j(t-1)) one segment downstream per timestep. This would impose an artificial routing speed determined by the grid spacing and timestep, and would effectively assume that discharge is passed through the network without being modified by local storage. That approach would be appropriate for a simplified kinematic routing scheme, but it is not suitable for the present problem, where the central quantity of interest is residence time and its modification by local storage.

In our formulation, each node is treated as a local storage–discharge element. The volume at a node is updated from the balance between upstream inflow, local effective runoff, and downstream outflow. Eq. 7 then converts the updated node volume into flow depth, and Eq. 8 uses the hydraulic-geometry relation as a local rating-curve closure to compute the resulting discharge. These equations are therefore required to couple volume, depth, and outflow dynamically. They allow changes in storage, including those produced by flow disturbances, to feed back onto discharge and residence time.

The suggested expression (Q_i(t)=Q_j(t-1)) would remove this storage–discharge feedback and would make discharge propagation depend primarily on the chosen timestep rather than on local hydraulic state. It would also make it difficult to represent the attenuation, ponding, and residence-time changes that are central to the study.

[R1C7] *The presentation of flow disturbances in the model is overall clear, but certain questions arise that require clarification: (1) does the model always assume that the dam is set at the node outlet (so I assume from Fig. 7), so that lmax = li in all nodes? (2) from L194, the dam has width and height; how these are set up, and how their extreme values are defined? (2) from general considerations, the dam will have limit on wmax equal node width, and on hmax, defined as a slope-length product such as hmax = l x Δh (backwater limited to a single node condition met), is this correct for the model in question? (3) if the former is true, does this automatically imply that hmax will depend on whether the connection is cardinal or diagonal, since their lengths differ?*

[R1R7] We thank the reviewer for these helpful clarification questions. We note that the single-node ponding constraint and dam-location feasibility are already discussed in the Limitations section and Supplementary Information, but we agree that the key assumptions should be stated earlier where the flow-disturbance model is first introduced. In the current formulation, the dam is represented as a local flow-control structure within a node, with ponding constrained to the geometry of that node. The maximum ponded length is calculated from local dam height and slope, and dam placement is only admissible where this length does not exceed the segment length. This condition is already explored in Supplementary Section S2, where feasible dam locations are shown for different values of the dam-height multiplier ζ.

We will revise the flow-disturbance section to make these assumptions more explicit. Specifically, we can clarify how dam height and width are prescribed relative to local channel geometry, how the single-node ponding constraint limits admissible dam locations, and how segment length differs for cardinal and diagonal connections. We can also better cross-reference the Limitations section and Supplementary Section S2 so that the reader can more easily see how these geometric constraints impact the feasibility of dam placement.

[R1C8] *The MS text says of dams and weirs (L193), considering them altogether along the MS text. Does the MS calculation approach fully apply to weirs, since unlike dams, weirs maintain certain discharge that is head-dependent; will this equally apply to notched weirs?*

[R1R8] We thank the reviewer for this helpful clarification. We agree that dams and weirs should not be treated as hydraulically identical structures in practice. In the present manuscript, the way we implemented flow disturbance should be understood as a generalised local flow-control structure that increases storage and modifies the local head–discharge relation, rather than as a complete hydraulic representation of every possible dam or weir design.

That said, the framework is deliberately modular. A standard weir equation, including formulations for broad-crested, sharp-crested, or notched weirs, could be substituted directly at the disturbed node by replacing the current generalised discharge relation with the appropriate head–discharge equation. Indeed, there is considerable overlap in the physical and mathematical behaviour of standard weir equations and our formulation by design, since both express discharge as a nonlinear function of upstream head, geometry, and empirical coefficients. However, we have used a more generalised expression here to avoid restricting the proof-of-concept model to a particular class of engineered weir constants or crest geometries. We will revise the manuscript to clarify this distinction.

[R1C9] *Divergence of OCN-derived hydraulic geometry from the real-world properties owing to heterogeneity of basin properties is an inevitable feature (Ibbitt, 1997, J. Hydrol. 196), do the authors have any hypotheses on how this divergence could affect the study outcomes, i.e., in terms of residence time and amplification ratios?*

[R1R9] We thank the reviewer for this useful comment. We agree that real catchments inevitably contain heterogeneity in hydraulic geometry, lithology, confinement, roughness, floodplain connectivity, and channel modification history that absolutely cannot be fully represented by an idealised OCN-derived network. This is a known feature of synthetic network approaches and is part of the reason why we do not present the model as a calibrated representation of a particular catchment.

However, the purpose of using an OCN is not to reproduce every local hydraulic property of a real river. Rather, OCNs provide a physically grounded synthetic network that captures the first-order scaling structure of river systems, including the relationships among contributing area, length, slope, discharge, and network topology. These are the critical flow-defining properties for the theoretical question addressed here.

Our expectation is that divergence between OCN-derived hydraulic geometry and real-channel properties would affect the absolute magnitude and precise location of residence-time amplification. For example, local variations in slope, width, depth, roughness, lateral storage, or confinement would shift where amplification is strongest and may increase or decrease its magnitude. However, this would not remove the importance of the coupled dynamic behaviour that we identify in the paper, namely that local flow disturbances can produce amplified residence times through the interaction of network geometry, storage, discharge, slope, and obstruction geometry, even where outlet discharge remains relatively insensitive. We will revise the manuscript to clarify this point in the limitations section.

[R1C10] *There is an issue with equations numeration, the Eq. 5 has gone unnumbered, though it is referenced as such in caption to Fig. 3, thus the following numeration breaks, also certain in-text formulations, such as the one for κ in L172, could be also set up separately as equations.*

[R1R10] We thank the reviewer for pointing this out. We will check and correct the numbering throughout the manuscript.

[R1C11] *Hack's exponent – can its value in the Table 1 be shown to equal 0.635 since it is scale-invariant?*

[R1R11] We thank the reviewer for pointing this out, we will update the table accordingly.

[R1C12] *Overall, the manuscript presents an attempt to relate OCN approach to a simplified runoff & flow routing model with precipitation in order to evaluate the effects of flow disruption in the immobile bed setting. The study is generally accurately set up, but several crucial parameter values, i.e., hydraulic geometry exponents or outlet slope, are set up a priori without clear justification, and their effect on general conclusions, i.e., on residence time, remains unexplored. Certain assumptions, i.e., on derivation of Qi(t) from di(t) rather than Qj(t-1), are counter-intuitive. These are several issues concerning the definition of a flow disruption, and the derivation does not explain the effect of connection direction (cardinal vs diagonal) on extreme parameter values and thus, on the limits of flow disruption effects on residence time.*

[R1R12] We thank the reviewer for this careful summary and for the thorough technical review of the manuscript. We agree that the points raised here correspond to the detailed comments addressed above, particularly regarding parameter definition and justification, hydraulic-geometry assumptions, the role of outlet slope, the storage–discharge closure used to compute (Q_i(t)), and the geometric constraints on flow disturbances.

As described in the preceding responses, we will revise the manuscript to make the model setup and parameter choices clearer, corrected the equation numbering and hydraulic-geometry presentation, clarified the role of Eqs. 7–8 as a storage–depth–discharge closure rather than a timestep-based routing rule, and expanded the explanation of dam geometry, admissible dam placement, and the effect of cardinal versus diagonal segment lengths. We will also improve cross-referencing to the Supplementary Information, where relevant sensitivity and feasibility analyses are presented. We certainly appreciate the reviewer's constructive comments, which will help improve the transparency and readability of the work.

[R2C1]: *Hydrological models are indispensable tools for streamflow simulation under changing climatic conditions. This study focuses on modeling the dynamic behavior of water residence time by integrating the Optimal Channel Network (OCN) theory and accounting for time-varying precipitation–runoff processes*

[R2R1] We thank the reviewer for taking time to review our manuscript and for recognising the central aim of the study, which is to investigate dynamic residence-time behaviour by coupling Optimal Channel Network theory with time-varying precipitation–runoff processes. We address the reviewer's specific comments in detail below.

[R2C2]: *The manuscript is overly theoretical and lacks real-world case studies to validate the practical applicability of its theoretical findings. All theoretical results are established on certain predefined assumptions. Therefore, the factors affecting the validity and robustness* *of the proposed theoretical outcomes should be quantitatively demonstrated and verified using observational or real-world dataset.*

[R2R2]: We thank the reviewer for this comment, but we respectfully disagree that testing against an observational real-world dataset is necessary, or the most appropriate validation framework, for the specific contribution of this manuscript. The paper does not seek to present a calibrated model of a particular catchment, but to develop and analyse a mechanistic proof-of-concept framework for residence-time dynamics in fragmented river networks.

The approach is not untested theory. Rather, the framework links several well-established and observationally grounded components, such as: Optimal Channel Networks, hydraulic geometry, Budyko-style water balance reasoning, stochastic runoff/recession theory, and geomorphic scaling relationships. These foundations are cited throughout the manuscript. The novel contribution here is the coupling of these elements into a controlled framework for testing how dynamic forcing and in-channel disturbance alter network residence times.

The use of OCNs is central to this aim. OCNs provide synthetic but physically grounded river networks in which topology, contributing area, slope, discharge, hydraulic geometry, forcing, and disturbance location are known. This enables the effects of climate forcing and in-channel structures to be tested explicitly and independently. A real catchment would not provide the same controlled framework, because boundary conditions, groundwater exchange, floodplain storage, modification history, and measurement uncertainty would all necessarily be entangled. Observational application to real fragmented catchments is therefore an important next step, but it addresses a different question from the one posed here.

Within this framework, the validity and robustness of the results are addressed quantitatively in several ways. The main manuscript compares steady and periodic runoff forcing, undisturbed and disturbed networks, and single-dam and multi-dam configurations. The Supplementary Information further examines network pathways, dam-location constraints, precipitation, potential evapotranspiration, dam discharge parameters, and numerical timestep. These analyses show that the reported behaviour is not a special consequence of a single arbitrary parameter choice.

We will revise the manuscript to make this scope clearer. We will also state explicitly that the analytical scaling argument is a consistency check using reduced analytical theory on the numerical OCN results, asking whether the same residence-time amplification behaviour can be recovered from simpler hydraulic and network-scaling relationships without running the full model. We will also strengthen the links between the main text and the Supplementary Information so that the existing uncertainty and sensitivity analyses are clearer.

This point is also linked to R1C1/R1R1 and R1C9/R1R9. Reviewer 1 explicitly recognises that the manuscript should not be interpreted as an operational catchment model, but as a simplified framework designed to isolate particular hydrological mechanisms. In R1R1 we also clarify this scope, and in R1R9 we also demonstrate why OCN-derived hydraulic geometry is used as a physically grounded scaling baseline rather than as a complete representation of real-channel heterogeneity. These points reinforce the point that observational application to a specific catchment is an important future step, but not the appropriate validation criterion for the theoretical contribution made here.

[R2C3]: *Excessive mathematical derivations are distributed throughout the manuscript. In standard research papers, most mathematical formulations and derivations are concentrated in the Methods section. However, this study presents relevant mathematical content in a fragmented and scattered manner. The authors are advised to explicitly justify such an unconventional layout. Alternatively, all mathematical derivations should be systematically organized and unified within the Methods section to improve readability and structural consistency.*

[R2R3]: We thank the reviewer for this comment. We agree that the manuscript can be improved by clearer signposting, but we do not agree that the mathematical derivations are excessive for the type of contribution being made. This is a theoretical and mechanistic modelling paper, and the derivations are the primary means by which the model is constructed, analysed, and interpreted. In this context, mathematical development is not additional material beyond the analysis, it is the analysis.

Such papers are very common in hydrology and geomorphology where new theoretical links or scaling relationships are proposed, many of which are cited in our paper and follow very similar formats. The manuscript follows this tradition by first constructing a coupled and controlled OCN-based numerical framework and then deriving a reduced scaling argument to explain the residence time amplification observed in the model. The analytical derivation is therefore not simply a methodological detail that can be separated from the results, but a central contribution of the paper.

We will certainly nevertheless revise the manuscript to make the structure clearer. In particular, we can more explicitly distinguish between: (i) equations required to define the numerical model; (ii) equations used to compute residence time in the numerical experiments; and (iii) the final analytical scaling derivation used to interpret the numerical results.

This also relates to R1C6/R1R6. Reviewer 1 specifically asks why Eqs. 7–8 are needed, and our response explains that these equations provide the storage–depth–discharge closure required for the model. This illustrates why the mathematical formulation cannot simply be removed or treated as incidental, the equations define the coupled dynamics that generate the residence-time behaviour analysed in the manuscript. We will nevertheless improve the signposting and organisation of the mathematical sections, including the equation-numbering issue noted in R1C10/R1R10.

[R2C4]: *Given the critical role of residence time in hydrological and ecological systems, the practical implications of the theoretical findings for real-world hydrological issues remain unclear. For instance, prolonged residence time is generally associated with deteriorated water quality in lakes and reservoirs. The authors are strongly recommended to incorporate real-world case applications to elaborate the practicability and application value of the proposed method.*

[R2R4]: We thank the reviewer for this comment. We agree that the practical implications of residence-time change are important and note that these implications are already discussed in detail in the manuscript, particularly in relation to nutrient uptake, sedimentation, microbial activity, contaminant and pathogen degradation, habitat transitions, and biogeochemical hotspots. We will revise the manuscript to make these points more prominent and to link them more explicitly to the numerical results.

We also respectfully note that prolonged residence time should not be interpreted as necessarily causing deteriorated water quality. Its effect is process dependent. In some cases, increased residence time may increase risks associated with warming, oxygen depletion, algal growth, or pollutant persistence. In other cases, it may enhance attenuation, settling, degradation, uptake, or pathogen die-off. The practical importance of residence time therefore lies in its role as a control on the opportunity for hydrological, ecological, and biogeochemical processing, rather than in implying a single direction of water-quality response, which we try to highlight in the manuscript.

The model results are practically relevant because they show that local flow obstructions can substantially alter internal residence-time structure while producing little or no detectable change in overall catchment outlet discharge. This has direct implications for fragmented river networks, where low-head weirs, small impoundments, and natural structures such as beaver dams may create residence-time hotspots that would be missed by more outlet focused hydrological data analysis or model.

This response can also be read alongside R1C8/R1R8 and R1C9/R1R9. In R1R8 we clarify how the framework could be adapted for specific real-world flow-control structures, including broad-crested, sharp-crested, or notched weirs, by substituting the appropriate head–discharge equation at the disturbed node. In R1R9 we explain how real-channel heterogeneity would be expected to modify the magnitude and location of residence-time amplification. Together, these points clarify the practical route from the present proof-of-concept framework toward future real-catchment applications, without requiring the current manuscript to become a site-specific calibration study.

[R2C5]: *The overall structure of the manuscript requires further optimization. For example, Table 1 (List of model variables, descriptions, and default values) is currently placed in Section 5 (Limitations). Considering the fundamental importance of this table for model interpretation, it should be relocated to Section 2 (Model description) for reasonable structural arrangement.*

[R2R5]: We thank the reviewer for this comment. We agree that Table 1 provides important information for model interpretation and that its placement should support readability. We note that conventions differ between journals and article types, with some preferring these near the beginning of the model formulation, while others place them after the main text to avoid interrupting the flow. This also addresses the related concern raised by Reviewer 1 in R1C4/R1R4, where the reviewer notes that fixed setup values and default parameters were introduced too late or mainly in figure captions.