The authors explore the possibility to introduce dynamic interactions into the optimal channel network (OCN) model so that this once purely geomorphological instrument could be used in exploring the hydrologic effects of flow fragmentation. The authors provide numerous examples for the latter in the Introduction section. The authors begin with constructing an idealized OCN according to the rules consistently described, the probability of network configuration is based on the temperature decay model. The elevation model is then reconstructed from the area-slope relation, then network hydrology is derived from the given topology and the outgoing discharge (Eq. 5, not numbered in the manuscript), hydraulic geometry is obtained from these data, and ultimately a consistently scaled hydrologic network is constructed. Network fragmentation is then introduced, in form of flow-controlling dams, and their effect on residence time in the system is assessed. Finally, since OCN is scale-invariant, this allows derivation of the generalized amplification ratio (presumed Eq. 32?) metrics.

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).

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.

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 km^2, hence l_i = 200 m (from caption to Fig. 3), this is only understood from figure captions, not the main text. 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. 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.

I wonder how this might have affected the MS conclusions. The same counts for the outlet slope of 0.005 that is presented in caption to Fig. 3 but never explicitly discussed in the text. It is clear from Eq. 3 that its value defines slope distribution across the entire network, but its effect on the final network structure and residence time (as affected by flow disruptions). To the authors: please explain and justify all *a priori* model parameter values! If they are crucial to the final research outcomes, do not hesitate to explain how variation in these parameter values might affect the conclusions!

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 $Q_i(t) = Q_j(t\text{-}1)$, 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.

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 $l_{max} = l_i$ 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 h_{max}, defined as a slope-length product such as $h_{max} = l \times \Delta h$ (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 h_{max} will depend on whether the connection is cardinal or diagonal, since their lengths differ?

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?

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?

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.

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

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 $Q_i(t)$ from $d_i(t)$ rather than $Q_j(t-1)$, 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.