General Comments:

This manuscript builds on previous work (Reitano et al., 2020) to explore the evolution of drainages in response varying precipitation rates and regional slopes using analog experiments. The manuscript is well-structured, and poses an analog model configuration that can be used to investigate landscape evolution in future studies. Furthermore, the authors rely on well-established channel profile analyses to quantify the relationship between analog models and natural settings.

This work presents a good contribution to the field geomorphology, especially as we try to link models with nature. However, I have two overarching critiques of the manuscript that should be addressed by the authors. First, there appears a lack of clarity in some of the methodology. Some metrics are given and discussed without an explanation of how they were derived (see notes given in the Specific Comments section). Explaining these methods in further detail will help give robustness to the manuscript results and interpretations. Second, I think there is a disconnect between the model setup and its relationship to tectonic orogens. These models do not impose uplift rates, but instead start with a background slope; essentially simulating an orogen that rapidly grew and stopped, with erosion occurring only after the tectonic phase. The authors rightly state that they're models fall more in line with passive margin settings, but also imply the background slopes are proxy for tectonics. I think this needs some further justification.

Specific Comments:

Line 15 – 16: Authors argue for the need to adequately model tectonic settings, but their setup simulates low uplift rates / passive margins. This motivation seems slightly skewed.

Line 48: Here and elsewhere in the paper, 'boundary condition' seems to be used interchangeably with model forcing; these are not the same. Rainfall rate and imposed slope should be described only as model forcings (and even then, the imposed slope is more of an initial condition).

Line 63: Authors don't change nozzle flow rate to test precipitation rates, but instead change the number of nozzles. This should be elaborated on – what configurations were used and what tests conducted to ensure uniform rainfall?

Line 84: Some studies (e.g., Harel et al., 2016) suggest n isn't necessarily positive. Although this is described later in the manuscript, the use of 'positive' here is misleading.

Line 88: What is 'steady-state' here? With no uplift rate imposed, it would imply $E = 0$; how does this fit into k_s and θ? U also implies base-level lowering, which can justify the use of Eqs. (4) – (6), but I think some elaboration is needed.

Line 97 – 101: Although Supplementary Information describes the extraction of eroded volumes and incision, there's a fair bit of methodology that is lacking and should be explained (see comments below). Also, I have looked through the data repository, although much of the data is present, the 'ad hoc' Matlab scripts for analysis appears missing.

Lines 109 – 111: I'd recommend different abbreviations for maximum surface slope and surface slope mode. MSS and SSM are difficult to keep track of, maybe use SS_{Max} and SS_{Mode} to help keep these terms clear. Furthermore, these are two metrics that need clarified – How are these metrics calculated? Are there regions of the DEM that were ignored for MSS & SSM?

Fig. 1a-b: Going along with my comment above, I don't understand how box plots are being created for MSS and SSM. Max and mode would imply a single value for each model time step, not a distribution. Are these taken from the final time step through a moving window (similar to MLR/LRM)? Are these the values of the 10 representative time steps (and if so, does it make sense to have these since these values are evolving towards one value that may not overlap with the median implied by the plots)?

Furthermore, how much of the trend in Fig. 1a is simply a product of the initial condition, as opposed to evolution of the model? If these values were normalized by the original slope, would these trends still exist?

Line 117: Is there a reason that Figs. S2/S3 are kept in the supplement? Fig. S2 is the main observational item for how the models evolve, and provides the foundational data that Figs 1-4 are built, shouldn't it be in the main text?

Line 120: Again, some clarity is needed in the methodology (either in main text or supplement). How are channels being determined? Is there an imposed drainage area threshold? Many of these models appear to have channel widths that are multiple pixel sizes, how is that being accounted for with TopoToolbox? Are k_s and θ determined by power-law regression between local slope and area?

Line 124: Similar to before, I recommend using different abbreviations for maximum local relief and local relief mode.

Line 186: What is V_n normalized by? What is its normalized range and units? Normalization implies that it should be unitless, which would make Ae dimensional. Or is V_n the value used to normalize V? How is incision rate calculated here – is it the mean or max rate over the entire domain?

Line 194: This statement (and Fig. 2.c-d) suggest these trends may be better represented with a log-scale on the y-axis?

Lines 195 – 197 and Fig. 2d: I question the use of plotting S_e/R against Ae. Since Ae contains S_e/R, it would be expected that these values relate. Would it be more beneficial to plot S_e/R against IV_n/V to more clearly show the relationship between forcing and response?

Lines 201 – 203 and labels of Fig. 2d: This description and figure labels seem at odds with the data and earlier discussion. Topography of mod2070 shows incised channels (Fig. S2), yet Fig. 2d's characterization is of a no-channel landscape. The authors qualify this as incision being broadly distributed; however, earlier they highlighted mod2070 as an example of high regional slopes being able to overcome high precipitation rates and have enough potential energy for channelization (lines 160 – 162). I don't see how mod2070 can be used as evidence for both the trade-off between slope and precipitation to form channels, and having erosion too-broadly distributed for channels to be relevant?

Lines 211 – 213: Given the concerns above, I don't see how Ae in its current form provides enough information to be used a threshold parameter or some predicting factor. In its current formulation, evaluating Ae requires an analog model to already be ran, so it would be apparent whether channels develop. S_e/R alone seems a better predictor than Ae.

Line 242 and Fig. 4: I assume these k_{sn} values were all normalized by the same concavity index? This should be stated somewhere (and that value that was used).

Technical Corrections:

Line 119: I think a small typo – 'the final stage'.

Line 134: I think magnitude is missing from the stated value – should it be 1.4 $x10^6$ mm^3?

Lines 195, 198, 204, 207: Fig. 1d or Fig. 2d?