Dear Editor and Reviewers,

We thank the Editor and the three anonymous reviewers for their time and for providing such thorough and constructive feedback on our manuscript. We sincerely appreciate the effort involved in a detailed review, and their insightful comments have been instrumental in helping us significantly improve the paper.

The reviewers highlighted several key areas where the manuscript could be strengthened. The major themes of the feedback included the need to: (1) more thoroughly describe the deficiencies and assumptions of the stream power-diffusion model, (2) provide a more comprehensive mechanistic description of the observed transient slope change reversal, and (3) better contextualize our findings using non-dimensional frameworks.

In response to these valuable suggestions, we have undertaken a substantial revision of the manuscript. We have restructured parts of the Results and Discussion, added a dedicated "Model Limitations" section, and expanded our introduction and methodological descriptions to clarify the rationale and context of our simplified approach.

We have carefully addressed every point raised by the reviewers. Below, we provide a detailed, point-by-point response (in green) to each comment, outlining the specific changes (in blue) made in the revised manuscript. We have also attached a version of the manuscript with tracked changes for your convenience. We believe the revisions have greatly improved the clarity, rigor, and overall contribution of our work.

Sincerely,

Yinbing Zhu (on behalf of all co-authors)

Response to comments from Anonymous Referee #1

The manuscript is well-organized, clearly written, and presents a rigorous investigation into how landscapes respond to tectonic and climatic forcing, with a focus on slope evolution at river headwaters. Using the Badlands landscape evolution model, the authors demonstrate that changes in rainfall rate, unlike uplift rate changes, can induce transient slope change reversals near drainage divides, a process strongly modulated by hillslope diffusion. This insight offers a novel perspective on differentiating between climatic and tectonic controls in geomorphic systems. The experimental design is systematic, the interpretation is robust, and the study is presented in fluent and technically precise language. The work makes a valuable contribution to Earth surface process research and is suitable for publication with minor revisions. To further improve the clarity and scientific strength of the paper, the following suggestions are offered:

1. The authors argue that the transient slope change reversals are driven by differential erosion rates between the divide and adjacent areas. It is recommended to elaborate on why changes in rainfall rate lead to this contrast, while changes in uplift do not. What are the respective roles of hillslope diffusion and river incision in generating or amplifying these differences? A more detailed mechanistic discussion would strengthen the internal logic of the manuscript.

Thank you for this excellent suggestion. We agree that a more detailed mechanistic discussion was needed to strengthen the manuscript's logic. To address this, we have significantly expanded the discussion in the new **Section 4.1 Mechanism of transient slope change reversal**. The revised text now elaborates on why rainfall and uplift changes produce different responses by detailing the temporal mismatch between river incision and hillslope diffusion at the headwaters.

We have added the following text to Section 4.1:

The transient slope change reversal arises from a lag between two characteristic timescales: the hillslope response time and the channel incision response time. Following a change in rainfall, the river incision rate adjusts almost instantaneously. However, hillslope response lags behind channel response (Clubb et al., 2019). Hillslope diffusion, which controls sediment transport from divides to channels, is driven primarily by slope and remains initially unchanged. Near drainage divides, the river incision is weak and hillslope diffusion dominates (Dietrich et al., 2003). The temporal mismatch creates the observed imbalance at the headwaters. For instance, following a decrease in rainfall rate, sediment continues to diffuse toward the channel at pre-disturbance rates, but the ability of the channel to transport sediment is reduced due to lower discharge (Mitchell, 2020; Montgomery et al., 2000). This imbalance causes the rate of sediment supply from hillslopes at the headwaters to exceed the rate of sediment removal by rivers, reducing channel slope temporarily and causing a transient slope change reversal. As the channel adjusts and the erosion wave migrates upstream, this reversal gradually disappears.

In contrast, a change in uplift rate uniformly raises the entire landscape without immediately affecting the efficiency of diffusion and incision. Because both the divide and its adjacent areas experience similar erosion conditions under constant discharge, no transient slope reversal occurs.

Notably, a lower Pe amplifies the imbalance between sediment supply from hillslopes and removal by rivers. This enlarges the zone where divide erosion rates differ from downstream areas. Therefore, the transient slope change reversal persists over a longer channel segment and for a longer duration, as observed in model M3 (Fig. 6 c2 and c3). In contrast, increasing Pe enhances river incision, which reduces the relative influence of diffusion. This leads to a shorter channel segment experiencing transient slope change reversal and a shorter duration of the transient response in model M4 (Fig. 6 d2 and d3).

2. Since the authors mention that slope-area and chi analysis can help identify transient slope change reversals, it is recommended to include representative plots from the numerical models. These visualizations would help illustrate how such plots reveal the geomorphic response to climatic or tectonic perturbations.

Thank you for this valuable suggestion. We have added a figure and the following text to Section 4.2:

In our models, a decrease in rainfall rate produces a localized flattening at high χ (headwaters), directly reflecting the transient slope-change reversal (Fig. 9). By contrast, in uplift-driven transients the χ–elevation profile bows downward at low χ, while the high-χ (headwater) segment remains straight and is simply translated upward. However, χ–elevation analysis has limitations: it requires a steady-state baseline profile to distinguish different types of disturbances. χ–elevation is therefore best used in concert with additional information, such as independent erosion-rate measurements, to robustly identify and attribute transient slope-change reversals.

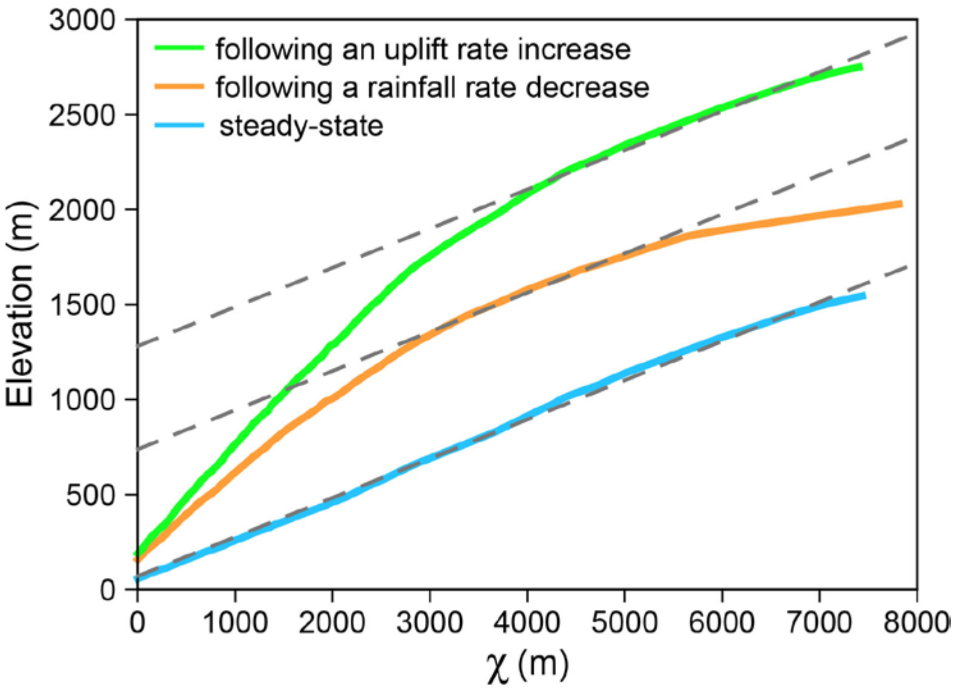

Figure 9. χ–elevation profiles of trunk streams in model M3 under three conditions: following an uplift rate increase (green), following a rainfall rate decrease (orange), and steady-state

(light blue). The three gray dashed lines are parallel reference trends. χ–elevation profiles are calculated using a reference concavity index (θ_{ref}) of 0.4.

3. The manuscript suggests that erosion rate patterns near drainage divides, measurable through cosmogenic nuclides, may help distinguish between climatic and tectonic forcing. Including a brief geological example would help bridge the model findings with field-based applications and strengthen the practical relevance of the study.

Thank you for this great suggestion. We agree that a geological example would strengthen the practical relevance of our findings. Obtaining erosion patterns near drainage divides requires cosmogenic nuclide measurements at high spatial resolution across the divide zone. However, current studies focus on basin-averaged erosion rates with sparse sampling that cannot capture the spatial variation in erosion rates near divides that our model predicts. Our literature review confirmed that existing datasets lack the necessary spatial resolution to test our model predictions. This represents an important research gap that our study helps to identify. Perhaps, our paper will prompt experts to test our model.

4. In the methodology section, it is recommended that the authors clearly list all model parameters used, including the values of m and n in the stream power incision equation. Additionally, the initial condition is described as a uniform elevation of 10 meters; however, the landscape at the end of the first stage appears asymmetric. If any initial perturbation or topographic noise was introduced to generate this asymmetry, it should be explicitly stated and justified in the methods section.

Thank you for this valuable suggestion. We have added a sentence stating that $m = 0.5$ and $n = 1.0$ (Line 117). We started with a perfectly flat 40 km × 80 km grid at 10 m elevation (400 m × 400 m cells). Badlands converts that grid into a triangular irregular network (TIN), which is asymmetric. Because erosion and diffusive transport in Badlands operate along those triangle edges (and depend on their length and slope), the tiny asymmetry of the TIN grows over time, yielding the slightly asymmetric topography at the end of Stage 1. We have verified that the asymmetry does not influence the conclusion.

5. Technical Corrections

Figure 4(a): The labels for 25 Ma and 26 Ma are reversed in the figure legend.

Thank you for catching this. We have corrected the time labels in the figure legend of Figure 4(a).

Response to comments from Anonymous Referee #2

The manuscript is well organized, and the topic is attractive. In the Introduction section, the authors made a good summary of the work of their predecessors, and then put forward their own new understanding through the method of numerical simulation. The method is reliable. I read the manuscript with great interest as it is full of important knowledge in the field of drainage system evolution. However, after careful reading and consideration, I think there are two significant issues which need to be addressed before the possible publication of this manuscript.

Major concerns:

(1) Why the mean elevation increase when the hillslope diffusion increase? This is counterintuitive. In Line 134, the authors also consider it as an astonishing phenomenon. However, the authors did not present the explanation. The topographic differences affect the migration time of the knickpoints from low to high. For example, the adjustment time of river channel in Figure 5a1 is ~2 Ma, while that in Figure 5c1 is ~4 Ma, which are the conclusion of this manuscript. Moreover, from Figure 5a1 and c2, we can also see the difference in the channel slope, which is consistent with the difference in elevation. When the area and erosion coefficient remain basically unchanged, the slope differs by three times, but the erosion rate remains the same? This is not follow the rule of the stream power model ($E=KA^mS^n$). In Lines 234 and 236, the authors use the phrases of “wider divide” and “narrower divide”. Therefore, a possible explanation is that the hillslope area is not included in the drainage area. However, the divide should be a curve without width. Therefore, I suggest that the authors check the setting conditions of the numerical simulation or give a reasonable explanation for this surprising phenomenon.

Thank you for drawing attention to this point. To address this issue, we have calculated and added the “drainage density” (total channel length per unit drainage area) to Fig. 3 in the results. Drainage density decreases systematically as the hillslope diffusion coefficient increases. The runoff and erosion efficiency decreases as the drainage density decreases, resulting in an increase of landscape elevation.

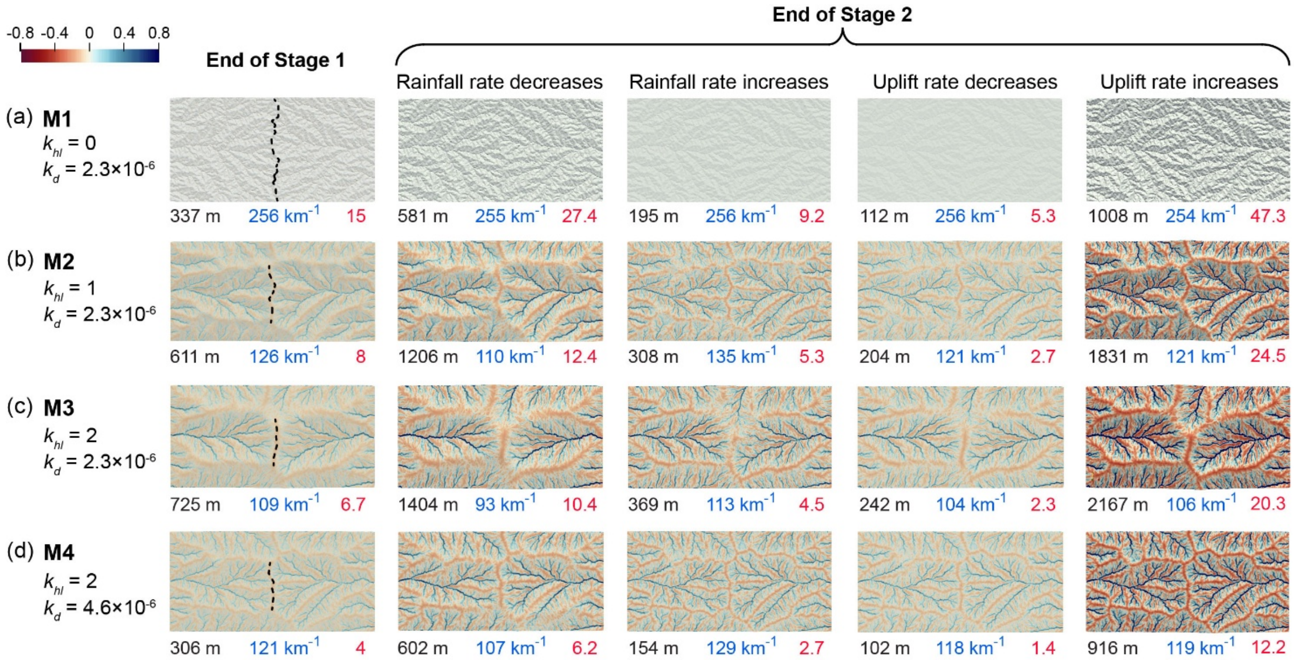

Figure 3. Hillshade maps showing erosion and deposition rates resulting from hillslope diffusion at the end of Stage 1 and the end of Stage 2 for models M1 (a), M2 (b), M3 (c), and M4 (d). Each model differs in hillslope diffusion coefficients (k_{hl}) and erodibility values (k_d). Blue areas indicate deposition, while red areas represent erosion. Color bar values indicate depositional (positive) and erosional (negative) rates (mm/yr). Numbers below each map display the mean elevation (black), drainage density (blue), and roughness (red). Dashed lines on maps at the end of Stage 1 denote the divides. The divides in Stage 2 are similar to those in Stage 1 and are not marked in this stage.

We have made some revision to Section 3.1. Before presenting the results of mean landscape elevation, drainage density, and surface roughness, we have added the following text:

To quantitatively compare landscape responses across our experiments, we compute three metrics: mean landscape elevation, drainage density, and surface roughness. Mean landscape elevation serves as an integrated measure of the overall erosional state of the landscape, representing the cumulative effect of tectonic uplift, channel incision, and hillslope processes on topographic development. Drainage density, defined as the ratio of total channel length to drainage basin area (Strahler, 1964), serves as a proxy for channel spacing and quantifies the degree of landscape dissection and runoff efficiency (Perron et al., 2009; Perron et al., 2008; Tassew et al., 2021). This metric provides insight into the spatial organization of the drainage network and its capacity to evacuate sediment and water from the landscape. Surface roughness quantifies the local topographic variability resulting from the competing effects of processes that create and destroy relief (Doane et al., 2024). We calculate roughness as the difference between the maximum and minimum elevation values within a defined neighborhood surrounding each central pixel using the ‘roughness’ algorithm of GDAL in QGIS (Wilson et al., 2007).

We have changed *“Impact on patterns of drainage networks”* to *“Impact on drainage networks and density”,* and added the following text to the end of this section:

However, when the erodibility remains constant, the drainage density decreases systematically with increasing diffusion coefficient in the order M1 > M2 > M3. This decrease in drainage density indicates wider valley spacing and reduced network tightness under stronger hillslope diffusion. M3 and M4 share the same hillslope diffusion coefficient, but the larger erodibility of M4 yields a higher drainage density than M3.

In addition, we have added the explanation to the end of Section 3.1:

Stronger diffusion smooths local slopes and reduces river incision rates under a constant uplift rate, while also widening valley spacing and lowering drainage density. Together, these effects have resulted in reduced drainage efficiency in some areas where the uplift rate exceeds the erosion rate, resulting in a higher mean elevation.

Finally, we agree with your suggestion that a divide should be a curve without width. Therefore, we have avoided using the phrases “wider divide” and “narrower divide” in the discussion.

(2) The“transient slope change reversal”is an interesting phenomenon, and also the highlight of this manuscript. However, it is not difficult to understand that this phenomenon occurs in the numerical simulation under the set conditions. As the hillslope diffusion is not changed, the erosional difference between the river channel and the hillslope area will appear, when the rainfall changes. The erosion rate changes when the rainfall changes, while the hillslope erosion rate keeps. Then the“transient slope change reversal”phenomenon appears. Therefore, I admit that this phenomenon will occur in the numerical simulation of this study. However, in reality, the hillslope diffusion should be affected by the rainfall (Braun, 2018 GR). Therefore, Whether this phenomenon has practical significance has not been tested yet. At least the authors need to have some description on this in the Discussion section.

Thank you for this insightful comment. We agree that the hillslope diffusion should be affected by the rainfall in nature. We have added a new section “**4.3 Model limitations**” to the discussion, acknowledging that the diffusion coefficient likely is not constant in response to climate change.

Section 4.3 is as follows:

In this study, we aim to explore the first-order impact of hillslope diffusion and river incision on landscape and consider a landscape evolving under the action of hillslope diffusion and river incision only. While the linear diffusion model is a common starting point, we acknowledge that it does not capture nonlinear processes, such as those driven by shallow landslides, which can become significant on steeper slopes (e.g., Jiménez-Hornero et al., 2005; Martin, 2000; Roering et al., 1999). Furthermore, our model does not account for potential feedback between climate and the diffusion coefficient itself. In natural settings, the hillslope diffusion coefficient can vary with climatic conditions via processes such as frost-crack weathering, and near-surface processes such as soil saturation, and root growth (Andersen et al., 2015; Gabet and Mudd, 2010; Gabet, 2000; Bogaard and Greco, 2015; Perron, 2017; Braun, 2018). Considering this feedback could introduce additional complexity. For instance, an increase in rainfall rate could increase the hillslope diffusion coefficient through higher soil moisture (Perron, 2017), potentially amplifying the transient slope change reversal. Conversely, a decrease in rainfall rate could decrease the hillslope diffusion coefficient and dampen the reversal. Future work could explore the parameter space where these feedbacks become significant.

In addition, our use of a detachment-limited stream power model simplifies the complexities of sediment flux. The “transient slope change reversal” we observe is fundamentally a result of a disequilibrium between hillslope sediment supply and the channel's transport capacity following a change in rainfall. A more complex model incorporating sediment transport dynamics (a “transport-limited” or “mixed” model) would likely modulate the magnitude and duration of this reversal.

Other suggestions:
Figure 4: The blue profile should be 25 Ma, and the red profile should be 26 Ma.

Thank you for catching this. We have corrected the time labels in the figure legend of Figure 4(a).

Lines 192-199: This paragraph is exactly the same as the previous paragraph. Therefore, one of the paragraphs needs to be deleted.

Thank you for catching this. We have deleted a repetitive paragraph.

Response to comments from Anonymous Referee #3

Summary

In this contribution the authors aim to examine how topography responds to changes in uplift versus climate using the streampower+diffusion landscape evolution model. They specifically focus on how diffusion modulates the response of channel profiles to changes in rainfall rate in the streampower model. They find a nonmonotonic response of channel slope near ridges when precipitation is increased or decreased, which doesn't appear in the absence of diffusion, or when the uplift rate is changed. They propose that this occurs because diffusion-driven erosion is unaffected by change in precipitation, inducing a local change in the balance of advection versus diffusion processes near channel heads. They suggest that this could be a diagnostic feature of response to changes in climate.

While this is an interesting signature that I don't think has been described before, the authors have missed a few important points that call into question the usefulness of the feature they have described. First, and most importantly, their method relies on the idea that the diffusion term is insensitive to changes in climate. This seems highly unlikely, given that slope stability is sensitive to hydrological processes (Bogaard & Greco, 2016), and other biophysical processes that drive soil production and creep are almost certainly climate-sensitive (Andersen et al., 2015; Gabet, 2000; Gabet & Mudd, 2010). While theories that clearly link hillslope processes to climate are still needed, it is generally accepted that both soil production and the diffusion coefficient increase with mean annual precipitation (Perron, 2017).

Thank you for this insightful comment. We agree that our model's assumption of a climate-insensitive diffusion coefficient is a critical simplification. In this study, we intentionally hold the diffusion coefficient constant to isolate how a given level of diffusive efficiency modulates the transient channel slope response to step changes in uplift or rainfall rate. This simplification allows us to identify the baseline “transient slope change reversal” mechanism, which could be obscured by more complex, competing feedbacks. We have added a new section “**4.3 Model limitations**” to the discussion, acknowledging that the diffusion coefficient likely is not constant in response to climate change.

Section 4.3 is as follows:

In this study, we aim to explore the first-order impact of hillslope diffusion and river incision on landscape and consider a landscape evolving under the action of hillslope diffusion and river incision only. While the linear diffusion model is a common starting point, we acknowledge that it does not capture nonlinear processes, such as those driven by shallow landslides, which can become significant on steeper slopes (e.g., Jiménez-Hornero et al., 2005; Martin, 2000; Roering et al., 1999). Furthermore, our model does not account for potential feedback between climate and the diffusion coefficient itself.

In natural settings, the hillslope diffusion coefficient can vary with climatic conditions via processes such as frost-crack weathering, and near-surface processes such as soil saturation, and root growth (Andersen et al., 2015; Gabet and Mudd, 2010; Gabet, 2000; Bogaard and Greco, 2015; Perron, 2017; Braun, 2018). Considering this feedback could introduce additional complexity. For instance, an increase in rainfall rate could increase the hillslope diffusion coefficient through higher soil moisture (Perron, 2017), potentially amplifying the transient slope change reversal. Conversely, a decrease in rainfall rate could decrease the hillslope diffusion coefficient and dampen the reversal. Future work could explore the parameter space where these feedbacks become significant.

In addition, our use of a detachment-limited stream power model simplifies the complexities of sediment flux. The “transient slope change reversal” we observe is fundamentally a result of a disequilibrium between hillslope sediment supply and the channel's transport capacity following a change in rainfall. A more complex model incorporating sediment transport dynamics (a “transport-limited” or “mixed” model) would likely modulate the magnitude and duration of this reversal.

A second related issue is distilling the effects of climate change down to a linear increase in average precipitation. Climate change is manifested in changes to the not just the mean, but also the distribution of event magnitudes and the phase (snow, rain) of precipitation due to changing temperature, which will be especially important in mountainous settings such as those considered (Meira Neto et al., 2020). Settings respond to precipitation changes differently depending on the dominant runoff generation mechanisms (Uhlenbrook et al., 2005), which are further modulated by erosion thresholds (DiBiase & Whipple, 2011). Such thresholds are especially important in headwaters, where the authors report their slope effect. Furthermore, geomorphic models that consider vegetation response to climate change suggest that the erosion response to precipitation change could even be reversed due to dynamic feedbacks with vegetation cover and evapotranspiration (Yetemen et al., 2019). None of these processes are mentioned in the present paper.

Thank you for this detailed and constructive feedback. You have raised several excellent points about the real-world complexities of climate change that our simplified model does not capture.

Based on this valuable feedback, we agree that using the term "Climate" was too broad for our study's scope. We have revised the title to be more precise: “**Rainfall and Tectonic Forcing Lead to Contrasting Headwater Slope Evolutions**”. As you have noted, our study simplifies the climate forcing to a change in the mean rainfall rate. This simplified approach allows us to isolate the outcome of fundamental processes that would be obscured in more complex and realistic scenarios.

In addition, we have added the following text after the introduction of Equation 1 in Section 1:

Eq. (1) simplifies the impact of climate on erosion. However, real landscapes respond to climate change through shifts not only in mean P but also in (i) the distribution of storm magnitudes, (ii) the phase of precipitation (snow vs. rain) that controls the timing of snowmelt runoff (Meira Neto et al., 2020), and (iii) the dominant runoff-generation mechanism (Uhlenbrook et al., 2005). Moreover, incision in channels is often controlled by erosion thresholds (Dibiase and Whipple, 2011) and may be further moderated by vegetation–evapotranspiration feedbacks (Yetemen et al., 2019). While these factors are critical for site-specific predictions, Eq. (1) is used here to isolate the first-order impact of a change in fluvial erosion efficiency on landscape form, providing a baseline for understanding these more complex interactions.

While I'm unsure that the streampower+diffusion model is the right tool to answer questions of climate sensitivity, I understand the tendency to stick with it in the name of interpretable simplicity. One of the reasons to stick with this model is because of its well-developed nondimensional forms (Bonetti et al., 2020; Litwin et al., 2025; Perron et al., 2008; Theodoratos et al., 2018), which provide clear methods for understanding fundamental process competition. The authors run into the problem of non-uniqueness in process competition when they change the streampower coefficient and diffusion coefficient but maintain their ratio. However, they do not provide any explanation of the fundamental scaling between the two, which is well understood (e.g., Perron et al., 2008).

Thank you for emphasizing the value of established nondimensional forms. We have clarified our methodology by explicitly computing the dimensionless Pe number, a well-established measure of the relative dominance of advective (fluvial) vs. diffusive processes (Perron et al., 2008, 2009). While varying streampower coefficient (k_d) and diffusion coefficient (k_{hl}) while holding Pe constant could theoretically produce similar steady-state forms, we emphasize that our analysis focuses on transient dynamics, where the absolute values of k_d and k_{hl} (not just their ratio) could influence response times. By adopting the dimensionless framework, we align our work with fundamental scaling theory (e.g., Perron et al., 2008) while retaining the interpretability of our results.

We have added the following text to Section 2:

For each model, we compute the dimensionless parameter Pe to combine two a priori independent parameters (the diffusion coefficient k_{hl} and the erodibility k_d) into a single dimensionless measure of process competition (Perron et al., 2009; Perron et al., 2008):

$$\mathrm{Pe} = \frac{k_d\, l^{2m+1}}{k_{hl}} \tag{4}$$

Pe is analogous to a Péclet number, which is the ratio of a diffusion timescale to an advection timescale (Perron et al., 2008). Low Pe values indicate diffusion-dominated systems, while high values indicate advection-dominated systems. We take the characteristic horizontal length scale l to be 40 km, representative of the real landscape. Based on our parameter values, model M3 has the lowest Pe, indicating that diffusion is more dominant in this model than in the others. Furthermore, models M2 and M4 share the same Pe because their parameters for k_{hl} and k_d were both doubled in M4 relative to M2, keeping their ratio constant.

Table 1. Diffusion coefficient and erodibility of four models

Model	Diffusion coefficient k_{hl} (m^2/yr)	Erodibility k_d (1/yr)	Pe
M1	0	2.3×10^{-6}	∞ (no diffusion)
M2	1	2.3×10^{-6}	3680
M3	2	2.3×10^{-6}	1840
M4	2	4.6×10^{-6}	3680

Overall, I think this paper needs substantial work to become a valuable contribution. My main recommendation would be to engage with models that link changes in climatic, hydrological, and geomorphic processes in some more realistic level of detail. If not, they could describe the deficiencies of the streampower+diffusion model and provide a more comprehensive description of the effect they observe, using available nondimensional frameworks and acknowledging that the diffusion coefficient likely is not constant in response to climate change.

Thank you for this insightful comment and great suggestion. We have made clear in the revised manuscript that as in many other modelling-based research, our intention is not to simulate nature in all its complexity, but to understand the role of individual processes. According to your advice, we have added a new section “**4.3 Model limitations**” in the discussion to describe the deficiencies of the streampower+diffusion model and acknowledged that the diffusion coefficient likely is not constant in response to climate change. In addition, we have used the nondimensional Pe number to provide a better description of our results.

Line-by-line comments

1. Transient slope change reversals are not yet defined.

Thank you for this comment. We have defined transient slope change reversals in Section 3.2.2 as follows:

In contrast, a decrease in rainfall rate triggers a "*transient slope change reversal*", a phenomenon we define as a non-monotonic adjustment where the headwater channel slope initially changes in the opposite direction of its final steady state.

2. Needs more description of how changes in climate actually yield changes in runoff production. Q=PA is a little too simple.

Thank you for this valuable comment. We have added the following text following the introduction of Equation 1 in Section 1:

Eq. (1) simplifies the impact of climate on erosion. However, real landscapes respond to climate change through shifts not only in mean *P* but also in (i) the distribution of storm magnitudes, (ii) the phase of precipitation (snow vs. rain) that controls the timing of snowmelt runoff (Meira Neto et al., 2020), and (iii) the dominant runoff-generation mechanism (Uhlenbrook et al., 2005). Moreover, incision in channels is often controlled by erosion thresholds (Dibiase and Whipple, 2011) and may be further moderated by vegetation–evapotranspiration feedbacks (Yetemen et al., 2019). While these factors are critical for site-specific predictions, Eq. (1) is used here to isolate the first-order impact of a change in fluvial erosion efficiency on landscape form, providing a baseline for understanding these more complex interactions.

48-49. Needs better description of what causes diffusion processes, how they might be linked to climate as well.

Thank you for pointing it out. We have expanded the relevant paragraph in Section 1. The revised text now provides a more detailed description of the physical processes represented by the diffusion coefficient, such as soil creep, rain splash, and bioturbation. Furthermore, it now explicitly describes how these processes are linked to climate through factors like temperature, moisture, and vegetation cover.

We have added the following text in Section 1:

Hillslope diffusion is the result of a combination of multiple near-surface processes: (i) rainsplash and sheet-flow creep driven by raindrop impact and overland flow (Guy et al., 1987; Meyer et al., 1975;

Young and Wiersma, 1973), (ii) soil creep produced by cyclical wetting-drying, shrink–swell, and freeze–thaw strains (Anderson and Anderson, 2010), (iii) bioturbation by burrowing animals and tree throw that mix and move regolith (Roering et al., 2010; Gabet et al., 2003), and (iv) small shallow landslides that act diffusively when averaged over long timescales (Martin, 2000).

Climate controls the relative efficiency of these mechanisms. Mean annual precipitation and storm magnitudes regulate rainsplash fluxes and influence vegetation density, which in turn affects soil creep (Istanbulluoglu and Bras, 2006). Freeze–thaw frequency, governed by temperature and moisture, dictates the rate of frost creep and solifluction in high-altitude or high-latitude settings (Hales and Roering, 2007).

62-63. Part of the reason this hasn't been explored is because we don't have adequate theory describing how the diffusion coefficient changes with climate, although it almost certainly does.

Thank you for this very insightful comment. We agree completely. To incorporate this important context into our paper, we have added a sentence to our Introduction that acknowledges this theoretical gap as a reason for the problem being under-studied. We believe this strengthens the rationale for our work.

The sentence is as follows:

This knowledge gap exists in part because there is not yet a comprehensive theory describing how the hillslope diffusion coefficient changes with climate.

84-86. Needs citation.

Thank you. We have added the citation (Line 104).

91-94. Maybe just describe the processes that are relevant. No marine? Source to sink?

Thank you for this great suggestion. We have deleted irrelevant processes (Line 109).

Table 1. Those are really large values of the diffusion coefficient! Usually, find values on the order of 0.001-0.01 m2/yr using hilltop curvature and erosion rates. The sensitivity to the value used is dependent on the grid size, and there are already well-established nondimensional forms that can help describe this (Bonetti et al., 2020; Litwin et al., 2025). It might be useful to consider those.

1. Again, this would be evident if you used established nondimensionalizations.

Thank you for this comment. We agree that the absolute values for the diffusion coefficient used in our models are larger than those typically measured in the field. We have used the established Pe number from Perron et al. (2008). The Pe values used in our models range from 1840 to 3680 for models M2-M4, which are comparable to those explored in the foundational work of Perron et al. (2008). This suggests that our choices of diffusion coefficients are consistent with the widely accepted ranges of Pe and are appropriate for our study context.

2. You have not explained why topographic roughness is a useful or interesting metric, or how you are calculating it.

Thank you for this comment. Topographic roughness is a crucial metric because it quantifies the local variability in surface relief caused by incision and diffusion processes, which are central to landscape evolution. Understanding roughness helps us investigate how the landscape responds to perturbations like changes in uplift or rainfall and provides insight into the spatial variability across different models. We calculate roughness using the GDAL 'roughness' algorithm in QGIS (Wilson et al., 2007), which measures the difference between the maximum and minimum elevation values within a defined neighborhood around each central pixel.

We have added the following text in Section 3.1:

To quantitatively compare landscape responses across our experiments, we compute three metrics: mean landscape elevation, drainage density, and surface roughness. Mean landscape elevation serves as an integrated measure of the overall erosional state of the landscape, representing the cumulative effect of tectonic uplift, channel incision, and hillslope processes on topographic development. Drainage density, defined as the ratio of total channel length to drainage basin area (Strahler, 1964), serves as a proxy for channel spacing and quantifies the degree of landscape dissection and runoff efficiency (Perron et al., 2009; Perron et al., 2008; Tassew et al., 2021). This metric provides insight into the spatial organization of the drainage network and its capacity to evacuate sediment and water from the landscape. Surface roughness quantifies the local topographic variability resulting from the competing effects of processes that create and destroy relief (Doane et al., 2024). We calculate roughness as the difference between the maximum and minimum elevation values within a defined neighborhood surrounding each central pixel using the ‘roughness’ algorithm of GDAL in QGIS (Wilson et al., 2007).

3.2 “rivers’ channel”

Fig. 4 Here it sounds like the effect of diffusion is unimportant, but in subsequent figures, it clearly is important. You could just explain Figure 4 as the kind of null case.

Thank you for this great suggestion. We have divided Section 3.2 into these two subsections: **3.2.1 Null-case control (Model M1, $k_{hl} = 0$)** and **3.2.2 Diffusion-enabled models (M2–M4)** to present the results more clearly and logically.

1. “Monotonously”
2. Needs to be more specific.

Thank you for this comment. We have revised the text and replaced “Monotonously” with “monotonically” in Section 3.2.1. The revised text is as follows:

Notably, within 1-2 Myrs of the change in rainfall or uplift rates, the channel elevation at the headwaters changes, but the slope remains nearly constant (Fig. 5 a1-3 and Fig. 6 a1-3). As the erosion wave approaches the headwaters, the channel slope increases or decreases monotonically and eventually stabilizes.

190-191. Not all channels experience this effect? Is there a threshold where it starts to occur?

Thank you for this comment. Transient reversal occurs in all channels. The extent of reversal becomes larger when Pe is lower. No threshold exists.

We have revised the relevant text in Section 3.2.2:

We do not find a distinct threshold for the initiation of the transient slope change reversal; rather, it is present whenever hillslope diffusion is active ($\mathrm{Pe} < \infty$). The primary control on the reversal is its magnitude and persistence, which vary continuously with Pe. Our results show that landscapes with lower Pe values, where hillslope diffusion is more dominant relative to channel incision, exhibit more pronounced and persistent reversals. For example, model M3, which has the lowest Pe, shows a reversal that persists longer and extends over a longer channel segment compared to other models (Fig. 6 c1-3).

192-199. Just a copy of the previous text.

Thank you for catching this. We have deleted a repetitive paragraph.

References

Andersen, J. L., Egholm, D. L., Knudsen, M. F., Jansen, J. D., and Nielsen, S. B.: The periglacial engine of mountain erosion – Part 1: Rates of frost cracking and frost creep, Earth Surface Dynamics, 3, 447-462, https://doi.org/10.5194/esurf-3-447-2015, 2015.

Anderson, R. S. and Anderson, S. P.: Geomorphology: the mechanics and chemistry of landscapes, Cambridge University Press, 2010.

Bogaard, T. A. and Greco, R.: Landslide hydrology: from hydrology to pore pressure, WIREs Water, 3, 439-459, https://doi.org/10.1002/wat2.1126, 2015.

Braun, J.: A review of numerical modeling studies of passive margin escarpments leading to a new analytical expression for the rate of escarpment migration velocity, Gondwana Research, 53, 209-224, https://doi.org/10.1016/j.gr.2017.04.012, 2018.

Clubb, F. J., Mudd, S. M., Hurst, M. D., and Grieve, S. W. D.: Differences in channel and hillslope geometry record a migrating uplift wave at the Mendocino triple junction, California, USA, Geology, 48, 184-188, https://doi.org/10.1130/g46939.1, 2019.

DiBiase, R. A. and Whipple, K. X.: The influence of erosion thresholds and runoff variability on the relationships among topography, climate, and erosion rate, Journal of Geophysical Research, 116, https://doi.org/10.1029/2011jf002095, 2011.

Dietrich, W. E., Bellugi, D. G., Sklar, L. S., Stock, J. D., Heimsath, A. M., and Roering, J. J.: Geomorphic Transport Laws for Predicting Landscape form and Dynamics, in: Prediction in Geomorphology, 103-132, https://doi.org/https://doi.org/10.1029/135GM09, 2003.

Doane, T. H., Gearon, J. H., Martin, H. K., Yanites, B. J., and Edmonds, D. A.: Topographic Roughness as an Emergent Property of Geomorphic Processes and Events, AGU Advances, 5, https://doi.org/10.1029/2024av001264, 2024.

Gabet, E. J.: Gopher bioturbation: field evidence for non-linear hillslope diffusion, Earth Surface Processes and Landforms, 25, 1419-1428, 2000.

Gabet, E. J. and Mudd, S. M.: Bedrock erosion by root fracture and tree throw: A coupled biogeomorphic model to explore the humped soil production function and the persistence of hillslope soils, Journal of Geophysical Research: Earth Surface, 115, https://doi.org/10.1029/2009jf001526, 2010.

Gabet, E. J., Reichman, O. J., and Seabloom, E. W.: The Effects of Bioturbation on Soil Processes and Sediment Transport, Annual Review of Earth and Planetary Sciences, 31, 249-273, https://doi.org/10.1146/annurev.earth.31.100901.141314, 2003.

Guy, B., Dickinson, W., and Rudra, R.: The roles of rainfall and runoff in the sediment transport capacity of interrill flow, Transactions of the ASAE, 30, 1378-1386, 1987.

Hales, T. C. and Roering, J. J.: Climatic controls on frost cracking and implications for the evolution of bedrock landscapes, Journal of Geophysical Research: Earth Surface, 112, https://doi.org/10.1029/2006jf000616, 2007.

Istanbulluoglu, E. and Bras, R. L.: On the dynamics of soil moisture, vegetation, and erosion: Implications of climate variability and change, Water Resources Research, 42, https://doi.org/10.1029/2005wr004113, 2006.

Jiménez-Hornero, F. J., Laguna, A., and Giráldez, J. V.: Evaluation of linear and nonlinear sediment transport equations using hillslope morphology, Catena, 64, 272-280, https://doi.org/10.1016/j.catena.2005.09.001, 2005.

Martin, Y.: Modelling hillslope evolution: linear and nonlinear transport relations, Geomorphology, 34, 1-21, 2000.

Meira Neto, A. A., Niu, G.-Y., Roy, T., Tyler, S., and Troch, P. A.: Interactions between snow cover and evaporation lead to higher sensitivity of streamflow to temperature, Communications Earth & Environment, 1, https://doi.org/10.1038/s43247-020-00056-9, 2020.

Meyer, L. D., Foster, G. R., and Römkens, M. J. M.: Source of soil eroded by water from upland slopes, in: Present and prospective technology for predicting sediment yields and sources, USDA-ARS, U.S. Gov. Print. Office, Washington, DC, 177-189, 1975.

Mitchell, S. B.: Sediment transport and Marine Protected Areas, in: Marine Protected Areas, 587-598, https://doi.org/10.1016/b978-0-08-102698-4.00030-7, 2020.

Montgomery, D. R., Zabowski, D., Ugolini, F. C., Hallberg, R. O., and Spaltenstein, H.: 8 - Soils, Watershed Processes, and Marine Sediments, in: International Geophysics, edited by: Jacobson, M. C., Charlson, R. J., Rodhe, H., and Orians, G. H., Academic Press, 159-iv, https://doi.org/https://doi.org/10.1016/S0074-6142(00)80114-X, 2000.

Perron, J. T.: Climate and the Pace of Erosional Landscape Evolution, Annual Review of Earth and Planetary Sciences, 45, 561-591, https://doi.org/10.1146/annurev-earth-060614-105405, 2017.

Perron, J. T., Dietrich, W. E., and Kirchner, J. W.: Controls on the spacing of first-order valleys, Journal of Geophysical Research, 113, https://doi.org/10.1029/2007jf000977, 2008.

Perron, J. T., Kirchner, J. W., and Dietrich, W. E.: Formation of evenly spaced ridges and valleys, Nature, 460, 502-505, https://doi.org/10.1038/nature08174, 2009.

Roering, J. J., Kirchner, J. W., and Dietrich, W. E.: Evidence for nonlinear, diffusive sediment transport on hillslopes and implications for landscape morphology, Water Resources Research, 35, 853-870, https://doi.org/10.1029/1998wr900090, 1999.

Roering, J. J., Marshall, J., Booth, A. M., Mort, M., and Jin, Q.: Evidence for biotic controls on topography and soil production, Earth and Planetary Science Letters, 298, 183-190, https://doi.org/10.1016/j.epsl.2010.07.040, 2010.

Strahler, A. N.: Quantitative geomorphology of drainage basin and channel networks, Handbook of applied hydrology, 1964.

Tassew, B. G., Belete, M. A., and Miegel, K.: Assessment and analysis of morphometric characteristics of Lake Tana sub-basin, Upper Blue Nile Basin, Ethiopia, International Journal of River Basin Management, 21, 195-209, https://doi.org/10.1080/15715124.2021.1938091, 2021.

Uhlenbrook, S., Didszun, J., and Leibundgut, C.: Runoff generation processes on hillslopes and their susceptibility to global change, Global Change and Mountain Regions: An Overview of Current Knowledge, 297-307, 2005.

Wilson, M. F. J., O’Connell, B., Brown, C., Guinan, J. C., and Grehan, A. J.: Multiscale Terrain Analysis of Multibeam Bathymetry Data for Habitat Mapping on the Continental Slope, Marine Geodesy, 30, 3-35, https://doi.org/10.1080/01490410701295962, 2007.

Yetemen, O., Saco, P. M., and Istanbulluoglu, E.: Ecohydrology controls the geomorphic response to climate change, Geophysical Research Letters, 46, 8852-8861, 2019.

Young, R. A. and Wiersma, J.: The role of rainfall impact in soil detachment and transport, Water Resources Research, 9, 1629-1636, 1973.