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; Bogaard & Greco, 2015; Braun, 2018; Gabet, 2000; Gabet & Mudd, 2010; Perron, 2017). 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 & 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., 2008; Perron et al., 2009):

$$\text{Pe} = \frac{k_d\, l^{2m+1}}{k_{hl}} \quad (4)$$

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 & 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 & Wiersma, 1973), (ii) soil creep produced by cyclical wetting-drying, shrink–swell, and freeze–thaw strains (Anderson & Anderson, 2010), (iii) bioturbation by burrowing animals and tree throw that mix and move regolith (Gabet et al., 2003; Roering et al., 2010), 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 & 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 & 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.

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.

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., 2008; Perron et al., 2009; 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 ($Pe < \infty$). 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.

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