Response to Reviewers: "Hillslope diffusion and channel steepness in landscape evolution models"

We would like to thank both reviewers for their insightful comments, which we have addressed as described below. There were four primary recommendations from the reviewers: (1) to better contextualize available fluvial models that account for sediment transport and the bed cover effect; (2) focus on the novelty of the scaling analysis rather than the steepening itself; (3) clarify the choice of SPACE parameters to achieve detachment limited conditions; and (4) explain the behavior of SPACE+Diffusion sediment thickness under mixed bedrock-alluvial conditions.

We have addressed the first request by introducing the sediment-aware models in Section 1, and discussing other ways bedrock rivers adjust, e.g., channel width in Section 4.2. Second, we have changed the text to ensure it is clear that the steepening effect with diffusion is already known, but the scaling is new. We refer more directly to the original demonstration of this effect by Howard (1994) in Section 1, and in the discussion. We still believe that this effect is underappreciated by many users of SPD models, so we did not change the text to the extent of assuming readers already know exactly why this occurs.

We would like to thank Charlie for his commentary on the third point, which was raised by the other reviewer. As Charlie addressed, there was some confusion (on our part as well!) about the different meaning of setting the fraction of fine sediment Ff=1 and setting the effective settling velocity V=0 in the SPACE model. Ff=1 means that "when bedrock is eroded by fluvial processes, the sediment produced from bedrock erosion effectively disappears," while V=0 effectively prevents entrained sediment from redepositing. We experimented with these two possibilities when SPACE was coupled with hillslope diffusion. While they suggest slightly different mechanisms by which a system can be detachment limited, we found that the steady-steady state results are extremely similar, both in terms of equilibrium channel steepness and sediment thickness. We decided to switch to using V=0 for detachment limited, since it has a more intuitive meaning. We clarified the text in Section 4.2 to ensure we did not misrepresent the meaning of parameters in SPACE.

The fourth point concerned whether the increase in steepness caused by adding diffusion to SPACE in the mixed bedrock-alluvial cases (Figure 7f,h) was the result of a numerical error. We believe it is not. We now present a derivation for SPACE+Diffusion steepness based on assumptions about how hillslope sediment flux affects the local sediment thickness (Appendix A, Eq. A5-A9), and show that the solution predicts the modelled steepness almost exactly (Figure 7f,h). Additional discussion of this can be found in the response to the question by Reviewer 1 below.

Reviewer 1

This review is of "Hillslope diffusion and channel steepness in landscape evolution models" by Litwin et al. In general I think it is a clearly written and elegant analysis of idealized models that the authors connect to real landscape applications and point out the challenges and limitations in doing so.

My main suggestion for improvement is to cite more landscape evolution modeling papers early on that have included sediment in different ways. If a reader was not very familiar with landscape evolution modeling done in the last 20+ years, they would be led to believe (until getting to the discussion section at least) that nobody had ever thought to incorporate effects of sediment coming off of hillslopes and into channels on channel incision. Cite the SPACE paper early on, and other works that have used it, and maybe Whipple and Tucker (2002), and many various other papers that have explored mixed bedrock alluvial models. I also suggest changing the intro to better explain why this approach of using what is an idealized, reduced complexity model (SPM, or SPD) is useful—to me anyway, the benefit is the simplicity, that it can be calibrated to real landscapes to infer parameters (and the paper nicely shows that parameter values can be misleading), and to not use a more complex model than is needed for a given task, while acknowledging that more complex equations do already exist.
Thank you for your support and helpful comments. We have restructured our introduction of landscape evolution models, putting more up front in the introduction, and expanding what we cover. It is of course not exhaustive, but should provide some context for readers in terms of the history and state-of-the-art for sediment-aware landscape evolution models. We have also added text to the introduction to convey the value of the SPD model, as you suggested.

Line Comments

Line 54: add comma before S(x)
Done

103-104: Explain why these particular values were used. Why this particular ratio of diffusion to advection? Think of it from the point of view of someone not super familiar with the equations, and try to explain a little more.
Thank you for this suggestion. We have added the following text:

This corresponds to an active landscape with efficient erosion process rates. Typical K values used in SPLD models are 10^{-8}--10^{-5} $m^{(n-1)}$ (Ruetenik et al., 2023) and D values have been reported $4.4x10^{-4}$--$3.6x10^{-2}$ (Fernandes & Dietrich, 1997). The ratio m/n is commonly 0.4--0.6, generally with n>= 1 (Harel et al., 2016). In streampower-based modelling studies, m=0.5 and n=1 is commonly used as a baseline configuration (e.g. default in Landlab, Hobley et al., 2017). As we will show later, individual parameter values in the SPLD model are less important for behavior than combinations that make up the model characteristic scales (Litwin et al., 2022; Theodoratos et al., 2018).

145: "effective uplift rate" is an interesting idea. Is it really correct, directly comparable? I don't think so but am not sure. Do you think of it as a conceptual idea or an actual way to evaluate channel steepness? Maybe at steady state. So if that's the case, shouldn't it be possible to "correct" for any best-fit bias in steepness (and K) parameters just by somehow adjusting the uplift rate? If the authors want to push effective uplift rate as a thing, I suggest explaining it more in the discussion, and clarifying it a little more here as well.
We have reconsidered coining the term "effective uplift" to minimize confusion with any real concept that could be quantified outside of an SPD model. Instead, we say what we say:

Figure 4a shows this for part of the watershed highlighted in Figure 1, revealing that the deposition in valley bottoms locally increases the uplift rate 'experienced' by the channels by a factor of two, because the deposited sediment is not distinguished from bedrock.

This is essentially what we meant by effective uplift. We also found that the term was not necessary to derive equivalence between the steepness deviation and diffusion relative to uplift (Formerly in Appendix B, now Equation 14), so we removed the term there as well.

197: Either in main text or in another appendix, I think you should give the Ganti et al. (2012) nonlinear diffusion equation. Its helpful to see equations.
Thank you for this suggestion. In Section 3.2, we now show the critical slope model (Equation 22) and the approximation by Ganti et al. (2012) (Equation 23).

204-207: This description of cover effects being important in channels seems very incomplete, because basically no other landscape evolution models with cover have been cited or described (yet). Covering them in a paragraph in the intro would address this, or maybe here.
Thank you for your suggestion. We have added a paragraph to the introduction that introduces models available that treat sediment and bed cover effects. This should better contextualize SPACE before we arrive at the discussion of it in Section 4.2.

231-234, 241-244: Again, this is the content I think should be mentioned earlier.
We have significantly modified this section in light of an update to the introduction. We removed the first paragraph, which was an insubstantial introduction to these landscape evolution models that do account for sediment, as it is now already introduced in the introduction. In the second paragraph of Section 4.2, rather than introduce SPACE singularly, we introduce it in the context of other models available:

An alternate approach is to represent, in some fashion, the erosion and deposition of sediment independently from bedrock (e.g., Beaumont et al., 1992; Shobe et al., 2017; Tucker et al., 2001; Yuan et al., 2019). Ideally, the simulation should be able to smoothly transition between bedrock and alluvial behavior based on the availability of sediment. Here we use one such model, Stream Power with Alluvium Conservation and Entrainment (SPACE, Shobe et al., 2017) coupled with hillslope diffusion, and contrast the sensitivity of channel steepness to hillslope processes under different configurations.

253-255: While I think the use of the SPACE model in this way is technically correct, I think it should be explained more here, because its an odd usage of it. I'm fairly familiar with the SPACE equations and it took me a while to think through why it worked the way the authors show. The sediment is run with a high settling velocity (parameter given in the appendix), and low sediment erodibility, so its conceptually representing coarse sediment that acts like bedrock, and that deposits at some step in how the SPACE model is solved. So it includes a cover effect from this sediment. But then when it is entrained it effectively disappears. I think I'm right to say that the model could have just as easily been written to be numerically solved (with a different ordering of steps within a timestep) so that this wouldn't work: If the model first checked Ff, and if Ff=1, then all the sediment would immediately be treated as washload that could not deposit, and would be equivalent to having a settling velocity of zero. I think; I could be wrong. My suggestion would be to explain a little more that this is an unusual combination of parameter the authors are using to enable the model to do something it wasn't exactly designed for, though probably works.

Please see the discussion at the beginning of our response. We may have led to some of this confusion, as we misunderstood aspects of Ff as well. We now use V=0 rather than Ff=1 for the detachment-limited cases.

262, 265, 269: I know you say (if I understand correctly) that you have an analytical solution for the SPACE model steepness? Give the equations, either here or in the SPACE appendix. The reason is that you've already talked about analytical solutions for the SP model, and just calling them analytical solutions here is confusing. In other words its not clear to me that the authors are now using an analytical solution that includes the SPACE sediment. Or maybe they're using a solution that includes hillslope diffusion instead? I note that Guryan et al. (2024) recently presented SPACE model steady state steepness analytical solutions; others may have as well.
Thank you for this question. We have done two things to clarify the text. First, in the SPACE steady-state solutions appendix, we have replaced the slope-area relationships with the equivalent solutions for channel steepness. These are obtained by isolating the coefficient on area in the slope-area relationships we originally presented. This should make it clearer how we calculated the analytical solution values referenced in Section 4.2 and shown in labels on Figure 7. Second, we reference back to the equation in the appendix for the particular analytical solution used to obtain a value.

274-275: This result has me confused. I understand with the SP vs SPD model why the diffusion of what is effectively bedrock deposition into the channel increases steepness even at steady state erosion. But in the SPACE model, sediment from over the whole pixel area (and all of the upstream pixels) should be accounted for regardless of whether the hillslopes are diffusing or assumed to lower at exactly the same rate as the channel. I realize the authors try to explain this, but I still don't quite understand why it would be different. It makes me wonder if it's a numerical method or assumption or rounding error in SPACE rather than a real effect. At steady state incision across a landscape, shouldn't the amount of sediment coming into each model node in SPACE be the same regardless of hillslope diffusion? Any explanation would be helpful.
We understand that there is something counterintuitive about this, but we are confident it is a feature of the SPACE+Diffusion model rather than a numerical artifact. Hillslope diffusion constitutes an additional flux of sediment moving between cells, where the total change depends on curvature. In areas of negative curvature, there is still an additional net outflux of sediment due to hillslope diffusion, and concave areas still a net influx to balance. As a result, SPACE+Diffusion produces systematic patterns of sediment thickness across the landscape at steady state, greater in valley bottoms than on ridges. For large drainage areas, the total flux out still should be U*A, but the additional local influx is enough to tip the scale on the equilibrium thickness, and as a result, the channel slope.

We have tried to clarify this in the text, and now support it with a new solution for channel steepness of SPACE+Diffusion (Appendix A, Equation A9), based on the principles we describe above. This solution matches the modelled steepness very closely, suggesting that it is a reasonable conceptualization of the model behavior.

In Appendix A we also note the limitations of this solution:
Note that this solution holds in the case where the only sediment production is from fluvial erosion of bedrock (applied everywhere in the landscape), and that hillslope sediment transport is independent of regolith thickness. In the code provided with this paper, we

include the option to produce transportable hillslope regolith with an exponential production function, but do not present results of that configuration as that is out of scope for this study.

286-287: The authors could consider mentioning Guryan et al. (2024), who looked at how alluvial cover changes steepness and effective erodibilities by comparing the SPACE model to the SP model, in their case also considering layered rocks.
Thank you for this reference. We have added it to the end of the earlier paragraph, supporting our observation that steepness is less sensitive to other factors (hillslope length in our case, lithology in Guryan et al. (2024):

Similarly, when Guryan et al. (2024) compared the steepness of SP and SPACE model channels eroding through layered stratigraphy, they found that accounting for sediment diminished the difference in steepness between hard and soft lithologies.

290-293: if bringing up tools effects, the authors could also mention other channel feedbacks such as width adjustments, which also mediate tools and cover effects.
Thank you for your suggestion. We have added the following text to that paragraph:

Furthermore, bedrock rivers exhibit co-adjustment of width, steepness, and sediment cover, which also mediates the effectiveness of sediment tools (e.g., Johnson & Whipple, 2010; Lavé & Avouac, 2001; Yanites et al., 2010). Dynamic bedrock channel width adjustment has been implemented for channel profiles (Yanites, 2018) but remains challenging in two-dimensional channel-hillslope models.

333-334: This is just a stylistic suggestion, but I prefer present-tense, so "We show" rather than showed, even for the conclusions, since its pretty common for people to read the conclusions right after the abstract.
We made this change in the conclusion, and also in several other places in the text that used the past tense unnecessarily.

351: I think "limited applicability in the field" is too strong, since the level of complexity of a model depends on what you want to use it for. The same arguments could be applied to the SPD model. Just be a little more nuanced in the criticism.
Thank you for your suggestion. We have tried to be a bit more nuanced, but still make our point: the predicted scaling between channel steepness and diffusion is likely not to appear in field data, as we find in our small field test. We are not trying to say that SPD models are useless, but that we don't expect this feature of them to be supported by field data:

As a result, the predicted scaling between hillslope diffusivity and channel steepness is likely not supported by field data in general.

Reviewer 2

Review of Litwin et al.: Hillslope diffusion and channel steepness in landscape evolution models

The authors analyze a widely used landscape evolution model in an effort to better understand the previously identified steepening of channels due to delivery of hillslope-derived sediment. They show the relative importance of this effect across the model parameter space (Fig. 2), they come up with a way to predict the magnitude of the effect based on model parameters (appendix B), and then show how the effect diminishes when a model is used that incorporates some additional physical realism (the SPACE model in this case). Finally, they show three field examples illustrating the steepening effect and showing that it is not captured by the simple stream power/diffusion model.

This is a well-written and useful paper. Geomorphologists, and landscape evolution modelers in particular, have known anecdotally about the key effect the authors describe (diffusion steepening channels) for a long time, and I think it's fair to say that we pretty much already knew the cause (the hillslope-derived sediment "becomes" bedrock when it hits the channel; it is not hard to intuit why this is bad) in an informal sense. However, to my knowledge, the current paper is the first systematic investigation of the diffusion-induced-steepening phenomenon. For that reason, and for the elegant way in which the authors derive predictions for the effect as a function of model parameters, this paper will be useful to the community.

My suggested changes are fairly minor. Before getting into those, though, I want to follow up on the perceptive comments by the other reviewer about some of the SPACE model results. Their questions are good ones and, since I have spent a lot of time building SPACE and then trying to understand its behavior, maybe I can be helpful.

Thank you for your support, and your help in clarifying some points regarding SPACE. Please see our specific responses below.

Reviewer 1's points about SPACE:

Their comment on line 253-255: I understand what the reviewer is thinking here, but I think there may be confusion about what the F_f parameter means. F_f only applies to the entrainment of bedrock (in the SPACE Python code it is defined as the "fraction of permanently suspendable fines in bedrock"). So the idea that "when [sediment] is entrained it effectively disappears" when F_f = 1 isn't true, regardless of when in the model solution entrainment is calculated. It would be accurate to say that "when bedrock is eroded by fluvial processes, the sediment produced from bedrock erosion effectively disappears" when F_f = 1, but that doesn't affect the fluvial response to sediment diffusing in from the hillslopes. My understanding is that SPACE+LD in this configuration is acting basically the way the authors want—they are showing that SPACE can replicate the sediment-driven-steepening effect, and showing the effect with a model that explicitly simulates both bedrock and alluvium gives us some insight into the mechanisms behind the effect. I do think the authors could revise the text a bit to prevent confusion: for example in line 273, F_f = 0 is equated with conserving sediment mass. I see what the authors mean, but this is likely to confuse readers. Really F_f is about whether or not you conserve mass during the transition from fluvially eroded bedrock to sediment. Once rock has become sediment, SPACE conserves all sediment mass. So I can see where the confusion arose. My advice is just to lay out more clearly what F_f actually is/does before you start diving into the results of the SPACE work: sed mass is always conserved, but we can simulate detachment-limited fluvial erosion as in Fig. 7A-D by saying that the bedrock becomes wash load when eroded. That's where F_f comes in.

Thank you for this clarification. As we describe at the beginning, we decided to use V=0 rather than Ff=1.0 because we think it will be clearer to readers, and appears to produce nearly identical steady-state results.

Their comment on line 274-275: Reviewer 1 has a point here; I also find this behavior surprising. The key question I'd like to see this discussion address is: Why does adding diffusion increase the equilibrium sediment thickness in the channel? If Qs=UA is by definition satisfied at steady state, it feels like the SPACE analytical solution should already

be accounting for the cover effect of all sediment passing through a given point on the channel network. My wild guess would be that this is not the result of an approximation within SPACE itself but a result of the coupling with LD. I'm sure the authors have put a lot of thought into this and I am only speculating; I would just ask, as reviewer 1 did, that they lay out the explanation in a few more steps so it's clear to readers.
We agree that the change in steepness is a consequence of coupling with hillslope diffusion. Please see our discussion in response to the first reviewer, and our derivation of steepness for SPACE+Diffusion in Appendix A (Equation A5-A9). We hope that this addresses the confusion.

General points on the manuscript:
My main suggestions, which should not be terribly hard to address, are that 1) the paper could be better situated within the existing literature both old and newer, 2) that the text could be expanded (by pulling appendices A and B into the main text) and possibly reorganized to better lead readers through key derivations and results, and 3) that there should be a little more focus earlier in the paper on the physical aspects of the problem rather than just the mathematical aspects.
Thank you for your helpful comments, we describe how we have addressed them below.

- This criticism could probably be leveled at any geomorphology paper published in the last 30 years: the current draft understates the extent to which a key concept was already recognized by Alan Howard. True, the authors do cite the 1994 paper in which Howard shows (his Fig. 5A), using basically the SPLD equation, the importance of the diffusion term in setting channel slope. But the current draft repeatedly presents identifying the existence of this effect as one of its central results, which I think also does the current paper a disservice because it does so much more! To me, the key value the authors are adding is in the *following* sentence about their impressive ability to predict the magnitude of this effect from model parameters, and in their careful analysis of why/when/how this occurs. Along these lines, some changes to the writing would help clarify what's known and what's new:
  We have taken your advice to try to shift the emphasis away from the observation that diffusion affects steepness, and toward the systematic exploration of this, and new ability to predict this effect from model parameters.

  - change the wording in lines 38-41 to focus your justification for the paper: it's not just that this effect hasn't received much attention. It's that we know there's this issue, but we don't have a way to quantitatively predict its magnitude. You're solving that, which is great! Along with that, line 42 could become much more specific. You are doing some really cool stuff here, but your contributions are not really about recognizing that this effect exists so much as they are about quantifying what controls it.
    Thank you for this suggestion. We have added text to this paragraph that lays out why the channel steepening happens, and focus the research gap on how it is not understood systematically. We now write:

    "Despite early demonstration of this SPD model feature, it has not been systematically described, nor is there currently a way to predict SPD channel slope from model parameters."

- in the abstract, I think it would be fairer to say that we already knew about the diffusion effect on slope and that you are making the contribution of figuring out when and how much it affects modeled outcomes.
  Thank you for the suggestion, we have changed the abstract in line with the rest of the text:

  In such models, channels are known to be steeper than the detachment-limited SP model implies due to deposition in valleys from diffusive hillslope sediment transport, but this effect has not been systematically described. Our results show that diffusion has a substantial effect on channel steepness index in SPD models across a large parameter space. We use a scaling analysis to show that the effect is predictable from model parameters when diffusion is linear.

- In 3.1 (line ~102), make sure to cite Howard again and point out that your results are a (very useful and thorough) generalization of the point.
  In this first sentence, we have added a reference to Howard (1994) as you suggest:

  Stream channels extracted from steady-state results of the SPLD model have higher normalized steepness ksn and relief than predicted by the SP model, consistent with the difference in slope observed by Howard (1994).

- I agree with reviewer 1 that the paper could be better situated in the literature from the beginning. Late in the paper the authors point out that there was a long history of modeling that did treat sediment separately from bedrock, but to some extent that was put on the back burner in favor of simpler, more efficient approaches in recent years. I think it would be better to move that point up to section 2. I won't prescribe what should get cited, but there's the series of models out of the Beaumont group, there's CHILD, and then this would also be a place to initially mention SPACE, which is to some extent a descendant of those approaches, so readers know where you're headed later in the paper.
  Thank you for your suggestion. We have added paragraphs to the introduction that better explain how SPACE relates to other fluvial models, and explain why there are benefits both to the models that explicitly treat sediment and simpler models that do not. It's not a comprehensive review by any means, but it should provide some context for readers. We have cited CHILD (Tucker et al., 2001) and one paper from the Beaumont group (Beaumont et al., 1992), among other examples.

- Ideally this will be a paper with implications beyond just a single family of numerical models. To that end, another concept that I think should show up earlier in the paper is the mechanism(s) for the diffusion steepening effect. As the authors note late in the paper, this basically occurs because 1) sediment and bedrock aren't distinguished and 2) sediment mass is conserved on hillslopes but not in channels in SPLD world. This is nicely shown in the paper, but bringing it up earlier and using it to frame the study will help the discussion stay grounded in the physics of Earth's surface, rather than just a quirk of a set of equations.
  We have followed this suggestion, and now introduce this effect in the introduction:

  When a detachment-limited erosion model is used and the erodibility of the hillslope sediment is the same as bedrock or deposited hillslope sediment is not distinguished *bedrock in the first place (e.g., Bonetti et al., 2020; Perron et al., 2008; Theodoratos et al., 2018)apparent hillslope sediment is eroded as if it were bedrock. This increases the channel slope.*

We decided not to get too much further into the details of why, as we feel these details still fit nicely into the discussion.

- Appendices A and B are both short and so is the paper overall. I won't insist on it, but I recommend bringing both appendices into the main text where the appendices are currently referenced so that equations and theoretical constructs don't come out of nowhere for readers not intimately familiar with prior papers in this literature.
  Thank you for your suggestion, we have decided to incorporate these appendices into the text. The former Appendix B now appears in Section 3.2, and no longer refers to "effective uplift." The former Appendix A now appears after this in Section 3.2. Doing this we were also able to eliminate the duplicated definition of lg.

Line comments:

- 5-6: As noted in major comment above: try to be a little fairer about what was already known (this effect exists) and what is new (great quantitative constraints on how/why)
  Thank you for this suggestion. We have significantly reworked the introduction to acknowledge that the effect is known, and be clearer about what we're adding.

- 51/52/throughout: I prefer "SP model" over "SP law," but just decide what you're comfortable with and keep consistency.
  Agreed. We removed remaining references to the "SP law" in favor of "SP model"

- 70: a bit pedantic maybe, but there need not be a connection between adding diffusion and expanding to 2D. You can do one, the other, or both.
  Thank you for pointing this out, we have changed it to say:

  In two dimensions, most of the landscape area is composed of hillslopes, which form due to erosion processes not driven by concentrated water flow. These hillslope processes are most often represented in landscape evolution models with conservation laws in which the sediment flux varies (linearly or nonlinearly) with slope, making them diffusional processes.

- Sec 2.3: Before diving into the tech stuff (Landlab, domain size, BCs, etc) it would be good to have a more general 1-2 sentences just saying what types of simulations you're going to run, and for what purpose. This would be a nice bridge between the theory background and the numerical implementation.
  Thank you for this suggestion. We have added the following text:

  The goal of our main simulations is to understand the systematic differences between steady-state channels in the SPLD model (Equation 8) and those predicted by the SP model. We will run the SPLD model with a range of parameters, extract the large channels from steady-state topography, and use χ analysis to determine their steepness, which we can compare the predicted SP steepness (Equation 3).

- Sec 3.2: see overall comment about pulling text out of appendices A and B.

We decided to follow this suggestion. See above.

- 201-206: I agree with everything written here, but it might make more sense to first discuss the reasons for the steepening effect within the model itself (dubious assumptions about single materials and mass conservation across process domains), and *then* dive into how that does or doesn't square with natural systems. This would involve just reorganizing the order of the discussion.
  We decided to remove this first paragraph, because in retrospect it was not adding much to this section. The references to sediment cover effects already appear in Section 4.3, and Section 4.2 contains a discussion of where local effects of sediment might matter. Without this paragraph, Section 4.1 still covers the physical meaning of the process coupling in SPD models, and provides some field context in terms of sediment retention. We hope this is a satisfying solution!

- 285: In line with the citation to Ott here, in Shobe et al., 2018 (JGR-ES; Fig. 10) we suggested from a modeling exercise that uplift-rate-dependent incision thresholds caused by delivery of hillslope-derived boulders to rivers made channel steepness less sensitive to erosion rate.
  We have added this reference to the paragraph:

  In another modelling study, Shobe et al. (2018) found that the sensitivity of channel steepness to erosion rate is also reduced by uplift-rate-dependent incision thresholds caused by the delivery of hillslope-derived boulders to rivers.

- 295-96: Recommend rewriting this sentence because elsewhere in the paper you are making the very valid argument that the SPLD behavior is basically due to bad/incomplete/simple/whatever assumptions. But here the tone sort of implies that you're talking about a physically real set of relationships. To me, and I think to you as well, this sentence should be more about the fact that we're getting the right answer (hillslope processes affect channel steepness) for the wrong reason (not doing sed/bedrock and mass conservation in our models).
  Thank you for pointing this out. This is the point we were trying to get across, though it was not clear from the text. We have changed it to the following:

  The SPD model can reproduce the field observation that rivers steepen in order to transport hillslope sediment (Finnegan et al., 2017; Johnson et al., 2009; Lai et al., 2021). However, we have shown that the simplifying assumptions of the model result in a scaling between fluvial erodibility, diffusivity and uplift (in l_c or l_g) and channel steepness that is not likely to hold in general. Our demonstration with the SPACE model shows that this relationship is easily weakened or eliminated by differentiating between sediment and bedrock erodibility, or by redeposition of eroded fluvial sediment.

- Figure 8: consider changing linestyle as well as color between ksn and ksnpred; I printed this draft in black and white to review and couldn't tell them apart. Different linestyles would fix that.
  Thank you for catching this. We changed the line style for $k_{sn,pred}$ to dashed, which matches Figure 1.

- 343: indicate that this first result is basically a more systematic confirmation of past work, which allows you to then put the emphasis on the next couple of lines which report really important results from your study.
  Thank you for this suggestion. We have edited the beginning of the conclusion to acknowledge prior work and place our paper in the context:

  Hillslope diffusion is known to increase channel steepness in `streampower plus diffusion' (SPD) landscape evolution models (Howard, 1994). Here we show that this effect occurs systematically across the model parameter space, and demonstrate for the first time that steady-state SPD channel steepness can be predicted from the model parameters when diffusion is linear (the SPLD model).

References

Beaumont, C., Fullsack, P., & Hamilton, J. (1992). Erosional control of active compressional orogens. In K. R. McClay (Ed.), *Thrust Tectonics* (pp. 1–18). Dordrecht: Springer Netherlands. https://doi.org/10.1007/978-94-011-3066-0_1

Bonetti, S., Hooshyar, M., Camporeale, C., & Porporato, A. (2020). Channelization cascade in landscape evolution. *Proceedings of the National Academy of Sciences*, *117*(3), 1375–1382. https://doi.org/10.1073/pnas.1911817117

Fernandes, N. F., & Dietrich, W. E. (1997). Hillslope evolution by diffusive processes: The timescale for equilibrium adjustments. *Water Resources Research*, *33*(6), 1307–1318. https://doi.org/10.1029/97WR00534

Finnegan, N. J., Klier, R. A., Johnstone, S., Pfeiffer, A. M., & Johnson, K. (2017). Field evidence for the control of grain size and sediment supply on steady-state bedrock river channel slopes in a tectonically active setting. *Earth Surface Processes and Landforms*, *42*(14), 2338–2349. https://doi.org/10.1002/esp.4187

Ganti, V., Passalacqua, P., & Foufoula-Georgiou, E. (2012). A sub-grid scale closure for nonlinear hillslope sediment transport models. *Journal of Geophysical Research: Earth Surface*, *117*(F2). https://doi.org/10.1029/2011JF002181

Guryan, G. J., Johnson, J. P. L., & Gasparini, N. M. (2024). Sediment cover modulates landscape erosion patterns and channel steepness in layered rocks: Insights from the SPACE model. *Journal of Geophysical Research: Earth Surface*, *129*(7), e2023JF007509. https://doi.org/10.1029/2023JF007509

Harel, M.-A., Mudd, S. M., & Attal, M. (2016). Global analysis of the stream power law parameters based on worldwide 10Be denudation rates. *Geomorphology*, *268*, 184–196. https://doi.org/10.1016/j.geomorph.2016.05.035

Hobley, D. E. J., Adams, J. M., Nudurupati, S. S., Hutton, E. W. H., Gasparini, N. M., Istanbulluoglu, E., & Tucker, G. E. (2017). Creative computing with Landlab: an open-source toolkit for building, coupling, and exploring two-dimensional numerical models of Earth-surface dynamics. *Earth Surface Dynamics*, *5*(1), 21–46. https://doi.org/10.5194/esurf-5-21-2017

Howard, A. D. (1994). A detachment-limited model of drainage basin evolution. *Water Resources Research*, *30*(7), 2261–2285. https://doi.org/10.1029/94WR00757

Johnson, J. P. L., & Whipple, K. X. (2010). Evaluating the controls of shear stress, sediment supply, alluvial cover, and channel morphology on experimental bedrock incision rate. *Journal of Geophysical Research: Earth Surface*, *115*(F2). https://doi.org/10.1029/2009JF001335

Johnson, J. P. L., Whipple, K. X., Sklar, L. S., & Hanks, T. C. (2009). Transport slopes, sediment cover, and bedrock channel incision in the Henry Mountains, Utah. *Journal of Geophysical Research: Earth Surface*, *114*(F2), 2007JF000862. https://doi.org/10.1029/2007JF000862

Lai, L. S.-H., Roering, J. J., Finnegan, N. J., Dorsey, R. J., & Yen, J.-Y. (2021). Coarse sediment supply sets the slope of bedrock channels in rapidly uplifting terrain: Field and topographic evidence from eastern Taiwan. *Earth Surface Processes and Landforms*, *46*(13), 2671–2689. https://doi.org/10.1002/esp.5200

Lavé, J., & Avouac, J. P. (2001). Fluvial incision and tectonic uplift across the Himalayas of central Nepal. *Journal of Geophysical Research: Solid Earth*, *106*(B11), 26561–26591. https://doi.org/10.1029/2001JB000359

Litwin, D. G., Tucker, G. E., Barnhart, K. R., & Harman, C. J. (2022). Groundwater affects the geomorphic and hydrologic properties of coevolved landscapes. *Journal of Geophysical Research: Earth Surface*, *127*(1), e2021JF006239. https://doi.org/10.1029/2021JF006239

Perron, J. T., Dietrich, W. E., & Kirchner, J. W. (2008). Controls on the spacing of first-order valleys. *Journal of Geophysical Research: Earth Surface*, *113*(4), 1–21. https://doi.org/10.1029/2007JF000977

Ruetenik, G. A., Jansen, J. D., Val, P., & Ylä-Mella, L. (2023). Optimising global landscape evolution models with ^{10}Be. *Earth Surface Dynamics*, *11*(5), 865–880. https://doi.org/10.5194/esurf-11-865-2023

Shobe, C. M., Tucker, G. E., & Barnhart, K. R. (2017). The SPACE 1.0 model: a Landlab component for 2-D calculation of sediment transport, bedrock erosion, and landscape evolution. *Geoscientific Model Development*, *10*(12), 4577–4604. https://doi.org/10.5194/gmd-10-4577-2017

Theodoratos, N., Seybold, H., & Kirchner, J. W. (2018). Scaling and similarity of a stream-power incision and linear diffusion landscape evolution model. *Earth Surface Dynamics*, *6*(3), 779–808. https://doi.org/10.5194/esurf-6-779-2018

Tucker, G., Lancaster, S., Gasparini, N., & Bras, R. (2001). The Channel-Hillslope Integrated Landscape Development Model (CHILD). In *Landscape Erosion and Evolution Modeling*. Boston: Springer US. https://doi.org/10.1007/978-1-4615-0575-4_12

Yanites, B. J. (2018). The dynamics of channel slope, width, and sediment in actively eroding bedrock river systems. *Journal of Geophysical Research: Earth Surface*, *123*(7), 1504–1527. https://doi.org/10.1029/2017JF004405

Yanites, B. J., Tucker, G. E., Mueller, K. J., & Chen, Y. G. (2010). How rivers react to large earthquakes: Evidence from central Taiwan. *Geology*, *38*(7), 639–642. https://doi.org/10.1130/G30883.1

Yuan, X. P., Braun, J., Guerit, L., Rouby, D., & Cordonnier, G. (2019). A new efficient method to solve the stream power law model taking into account sediment deposition. *Journal of Geophysical Research: Earth Surface*, *124*(6), 1346–1365. https://doi.org/10.1029/2018JF004867