Review of "Bedrock River Erosion through Dipping Layered Rocks: Quantifying Erodibility through Kinematic Wave Speed" by Mitchell and Yanites (esurf)

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General comments

Mitchell and Yanites present a method to explore the impact of uplifting layers of different rock strength on landscape evolution. They use a semi-analytical 1D stream power model to investigate how these differences in rock strength translate to fluvial morphology, the spatial distribution of erosion rates and the K vs n parameters, by expressing the migration of the contacts with a kinematic wave equation. They also apply this method to a real landscape to undertake the difficult exercise of constraining the values of erodibility K for a layered landscape. Overall, the paper nicely outlines how dynamic differential lithology evolving in 3D can significantly and sometimes counter-intuitively affect landscape, with for example rivers on weaker rocks being steeper than on harder rocks because of a combination of factors (dipping strata, n, ...). I suggest this manuscript is an excellent candidate for publication in Esurf: (i) the topic is very relevant for the geomorphological community, (ii) the work builds on previous studies (Forte et al., 2018, Perne et al., 2017, Darling et al., 2020, Royden and Perron, 2013) while bringing novelties and (iii) the methods/results support the discussions/conclusions. I have a number of minor points I suggest need a bit of work before publication; most of them are methodological points in order to ensure the robustness of the analysis.

Specific comments

First, k_{sn} and χ are key metrics for this manuscript. They are calculated with $\theta = 0.5$, like a lot of other studies. However, this value is based on Whipple and Tucker (1999), which suggested their range of 0.35 - 0.6 based on a number of assumptions about hydraulic conditions, spatial variations in erosional processes or uplift/erodibility conditions, and limited number of empirical observations. I suggest that in the case of this study, it is not clear whether these assumptions can be made: for example, different rock types can commonly induce a difference in erosional processes. Recent studies (e.g. Perron and Royden, 2013 or Mudd et al., 2018) demonstrate that 0.5 is not ubiquitous. Then, even within the restricted range of $0.35 < \theta < 0.6$, k_{sn} may vary significantly and in a non-linearly (see Mudd et al., 2018 or this preprint https://doi.org/10.1002/essoar.10505724.1). This can have a strong impact on k_{sn}, especially when extracted from real landscapes. I am not suggesting the whole study needs to consider varying θ, but a sensitivity analysis on a specific case might be good to at least show that it does not significantly affect the end results. This is especially true as different values of n are utilised which would mean, as $\theta = m/n$ within the stream power referential, that the area exponent adjusts itself to n?

Then, a more minor point is the calculation of C_H involving binning of area data which can be sensitive to the number of bins utilised (see Perron and Royden (2013) or Mudd et al.(2018) figure 2). Along the same lines of comments, I would suggest to add a quick sensitivity analysis on the latter to ensure the consistency of the method.

To finish about the methodological points, I would recommend to add few details (briefly and potentially in the supplemental) about the topographic analysis: which algorithms/methods have been used to calculate slope, drainage area and k_{sn} as these can impact the final data (see Wobus et al. (2006), Mudd et al. (2014,2018) or Gailleton et al (2019) for a few examples - Note that I am not necessarily suggesting the addition of these references).

In term of the manuscript structure and readability, I echo the comment from Reviewer #1: the manuscript is quite long. I do not necessarily mind, but I see how a reasonable shortening or restructuring could benefit interested readers. For example, I find the introduction rather long and it could be split (for example with a "Motivations" subsection starting on line 40). At smaller scales, the paragraph starting on line 68 is relatively long while Figure 2 already says a lot of it in a clear way. The paragraph starting on l. 97 is quite dense and could be shortened and clarified to concisely state the manuscripts aims. Subsections 2.2 and 2.3 could also be summarised while migrating some details to the supplemental materials. The results sections have some repetitions between the description of the figures in the text, their captions and the figure itself.

I noticed different terms used for referring to k_{sn} (e.g. stream steepness, channel steepness, river steepness). I suggest the manuscript would be clearer if this were homogenised. I believe the right term would be *normalised channel steepness* as the metric is k_{sn} and not the not-normalised to a θ_{ref} k_s.

I have a small concern about the field site utilised. The area is quite arid and displays a lot of plateaus. Although the aridity is a nice feature which helps detect the contact between formations, I wonder if these factors complicate (i) the calculation of drainage area and (ii) the implicit link between area and discharge required by k_{sn} (e.g. Flint (1974))? Maybe this has been studied for the area.

Finally I would raise an opening point. The limitations of this approach are clearly stated in the discussion: the model assumes a detachment-limited law, given erodibility contrasts between layers and discrete values of n. I believe the study would really benefit from a subsection in the discussion exploring the impact of more variability in these parameters. In particular, I am thinking about the impact of regularity in the results: would the relationship be that clear with "randomly" varying layer thickness and/or K? how fast would the signal get obscured by randomise variations? I acknowledge that I do not know how much work this last point might require and do not consider it as a major missing point, but rather as a valuable potential addition to the contribution.

line-by-line comments

l. 17: I don't mind the term "stream steepness"; however I would recommend using "channel steepness" as a more common alternative.

l. 30: I would replace "and" by "or" as one could suggest stream power has been used in many other situations (all the chi-related works expressed in the stream-power referential following Perron and Royden (2013) as one example among many).

l. 46: the work of Lavarini et al. (2019, https://doi.org/10.1029/2018JF004610) also is a nice example of the consequences differential lithology can have on detrital analysis, beyond the sole difference in erosion rates.

l. 52-56: I apologise for the inelegant suggestion, but I think this preprint (https://doi.org/10.1002/essoar.10505201.1) would be a relevant reference for the use of channel steepness to explore lithological variations and their implications in landscapes evolution (it is in the final stages of the peer-review process and I hope will be in an accepted form for the authors' revisions).

l. 65: Briefly add a couple of words to detail the k_{sn} extraction method (i.e. from S–A, $dz/d\chi$ regression or else).

Figure 1: The unit of k_{sn} is $m^{2\theta_{ref}}$ (where $\theta_{ref} = m/n$ within the stream power law referential), also the value of θ_{ref} needs to be reported.

l. 75: Alternatively, studies using steepness to unravel landscape evolution could also misinterpret variations in channel steepness due to lithologic variations as erosion contrasts (e.g. knickzones) due to base level falls. The different set ups in Figure 2 could lead to different type of misinterpretations.

l. 97: "Here" ? Do the authors mean "in this contribution"?

Figure 2: Nice figure!

l. 124: "nonzero" should be 'non-zero"

l. 130: "upwind", do you mean you are calculating dz/dx in the upstream direction or with an explicit Euler scheme? Calculating slope in the upstream direction could have some numerical consequences (see Campfort and Govers (2015), https://doi.org/10.1002/2014JF003376).

l. 136: see my main comment about θ.

l. 164: I don't find it confusing, it makes sense to me!

l. 180: Does χ_{sp} vary within the slope patch? I guess it does not matter here.

l. 201: *ka* refers to relative time (10 ka = 10,000 years ago), I would suggest to stick with *kyrs* and *yrs* for the whole paper.

l. 202: This is a rather short time step. Any particular reason?

l. 291: Just to make sure, steeper reaches = higher k_{sn} or higher S?

l. 294: It is also important to state that the non-linearity of the relationship increases with $|n - 1|$

l. 300: I feel like it could be stated more clearly that $n = 1$ is not numerically stable/representable with this equation.

l. 308: Why would C_{HW} and C_{HS} be equal?

l. 330: replace "you" by "one" or "we".

l. 345: It also assumes constant erodibility and layer thickness for each rock type?

l. 452: It is not clear why these specific values of n are used.

Figure 9: The figure is difficult to read, especially the legends. Again, I wonder how sensitive the data is to the way A is binned.

Figure 10: the scatter plots are quite dense and difficult to read. Maybe smaller points, or unfilled symbols or another type of visualisation would make

it clearer?

l. 780: Generally accurate for numerically “perfect” data, I suggest it is important to note this.

Figures S1, S2, S3 and S4: These figures are very difficult to read, I would really recommend to rethink their style. I am not sure one can extract relevant information from them.