Dear Dr. Langston, dear Dr. Tucker,

I am excited to see this work come out and I am very much looking forward to seeing the paper published. Below, I am noting a few questions and comments that arose during my readthrough and I hope that some of these might be helpful in the revision process.

In summary, there are three major points that I thought could be clarified (and that I address in more detail in the line comments below). First, the treatment of sediment in the model is only mentioned relatively briefly at the very end of the discussion section. In order to better appreciate the modeling result, I suggest that it may be helpful to lay out the role of sediment in the model (and in particular how it "becomes" bedrock when deposited) early in the section about the numerical implementation.

Second, as marked below, I suggest to introduce the concept of channel mobility earlier and maybe comment on the difference between mobility within loose sediment (which, in my limited knowledge, much of the literature that uses the term 'channel mobility; is concerned with), and the mobility linked to movement within a pure bedrock landscape as it is defined in this paper. This distinction may also clarify the discussion about the importance of channel mobility on p 17. I comment more extensively on this below.

Third, I would suggest a more extensive discussion of the implications of using the stream power model without a treatment of sediment tools and cover. The lack of the cover effect is mentioned in a sentence on P 19 L11 but in the list of limitation that follows and in the following discussion I did not find a clear mention about the possible limitations that the absence of tools may introduce. An appreciation of the implications of the stream-power model are particularly important before model results are compared with other studies such as those by Hartshorn et al., 2001 and Fuller et al., 2016. The effects of tools and cover seem an integral part of the interpretations of the observations that were made by these cited studies (and in other studies such as Hancock and Anderson 2002 etc.). Therefore, without a discussion about the role of tools and cover, the link between the present stream power model and other studies (that is made early in the manuscript) appears problematic. Moreover, tools and cover may affect lateral and vertical erosion in different ways. For example, an increase of lateral erosion rates because of a change in the amount of sediment that is deflected toward a channel wall (Fuller et al., 2016), may or may not be accompanied by changes in vertical erosion rates. Therefore, the response of the system to a change in water or sediment flux may be more complex than predicted by the model. In short, I can imagine that a more expansive discussion of this limit may be useful. In particular, these complications should probably be mentioned before the model is compared to results from other studies.

Line comments

P.2 L.7 – Nitpicky comment: it may or may not be worth noting that cessation of incision and cutting of straths has also been observed in harder lithologies such as quartzites or granites – These straths are narrow and they don't contradict the statement that sizes of strath terraces and rates of strath cutting seem strongly linked to bedrock strengths, but the way the sentence is phrased now, it could be understood that straths never form in stronger lithologies: See for example:

- Pratt-Sitaula, B., D. W. Burbank, A. Heimsath, and T. Ojha (2004), Landscape disequilibrium on 1000–10,000 year scales Marsyandi River, Nepal, central Himalaya, *Geomorphology*, *58*(1–4), 223-241

Or for an example of rapid widening in unweathered granite

- Anton, L., A. E. Mather, M. Stokes, A. Munoz-Martin, and G. De Vicente (2015), Exceptional river gorge formation from unexceptional floods, *Nature Communications*, *6*.

Somewhere in the setup and introduction (for example somewhere in the paragraph starting P.2 L.11), it might be worth mentioning published models that consider the control of valley wall-height on widening rates e.g.:

- Malatesta, L. C., J. P. Prancevic, and J.-P. Avouac (2016), Autogenic entrenchment patterns and terraces due to coupling with lateral erosion in incising alluvial channels, *J. Geophys. Res*

P.5 L.7. Setting $W=k_wQ^{1/2}$ may be common knowledge but I wonder if it is worth citing the original works that this scaling is based on.

P.5 L.16. I like the idea of looking at the centripetal force. I was wondering at this stage what happens to straight segments of rivers. The way straight segments are treated is layed out later: They are treated as having a range of radii of curvature. However, there is also evidence for erosion in perfectly straight channels (e.g. Fuller et al., 2016). Maybe a quick note of the limits and possible alternatives of this model formulation could be made here or in the discussion? At the latest, this should probably be mentioned when the model results are compared to results from Fuller et al., 2016.

P.6 L. 12 It was a little unclear to me what the word "which" refers to here – either equation 12 or the variable K_l.

P.6 L. 16ff

As far as I understand, the result that K_l/K_v scales with $Q^{1/2}$ or $R^{1/2}$ is derived within the framework of stream power models. I feel that the link to the field studies is a little misleading in this context. The changes in the ratio of lateral to vertical erosion rates between high and low flows measured in the Liwu river (Hartshorn et al., 2001) have been interpreted to be due to changes in the distribution of sediment in the flow (interpretation by Hartshorn et al., 2001) or to variable shielding of the bed (Turowski et al., 2008, ESPL). Because the importance of tools and cover for lateral and vertical erosion are not considered in the stream power model presented here, whereas the end result may be similar between model and field study (high discharge = high lateral erosion), the processes are likely different. Therefore, a comparison between field study and model without a more extensive discussion might be misunderstood. In turn, the increased sinuosity with storminess found by Stark et al., 2010 was interpreted by the authors as an expression of the importance for hillslope mass wasting in controlling lateral erosion. This interpretation may or may not be true, but it again, complicates the link of the model to this field examples.

Later (P18 L10), there is a similar issue with the comparison of the model to the study by Fuller et al., 2016. – see comment further down.

Generally, I think it is valuable to discuss whether the model behavior is observed in nature but I think it necessitates a more detailed discussion of the limits of the stream power model before the comparison can be made.

P.7 L.15 I wonder, if, before detailing the way lateral and vertical erosion is calculated, it would be worth detailing one entire timestep and the order in which equations are solved. In particular, at this point of the manuscript, I was unsure how streams migrate. As far as I understand, at the beginning of a timestep, flow is routed across a topography via a D8 algorithm, then the lateral and vertical erosion is calculated, the topography updated and the flow rerouted through the landscape. Could this maybe be briefly laid out step by step? Or as a flow chart figure?

Even more importantly, at this point in the manuscript (and up to the very last paragraph of the discussion) it was unclear to me how sediment was treated. This is important to appreciate many of the features of the model (channel migration and channel mobility in particular) Questions that would be good to clarify are: Is deposited material added to the topography of a cell? What happens to a cell that is partly sediment and partly bedrock? Is the difference in erodibility considered or does deposited sediment "become" bedrock? Detailing the treatment of sediment could probably be intertwined with the walkthrough of one model timestep.

Section 3.1: I was a little confused by the (as I understood it) differentiation between resistant lithologies for which the slumped material has to be eroded (therefore bank height is important) and weak lithologies for which all material is swept away after a slump happens (therefore bank erosion is not important). The way it is described in the text is that the material is "transported away". This formulation seems ambiguous to me. Is the material added to the sediment flux Qs or does the material "vanish" in the model. I believe the later is meant. If the material "vanishes", I was wondering where such a model would be applicable in nature. I had thought that even for loose, non-resistant sediment, there should be a bank height control and that transport capacity is important.

As in aside, the importance of wall height, even in loose sediment seems to be implied by the later mentioned study by Bufe et al., 2016. Here, we demonstrated that in loose sediment, the width of valleys across an uplift is a function of the uplift rate (controlling the growth of valley walls) and the channel mobility (controlling the frequency at which a river revisits a given point in the valley). The area of valley that is cut across a fold reaches some equilibrium value that can be maintained and that is flanked by steep, high walls. One interpretation of this finding is that the equilibrium valley area that is actively "maintained" by the river is limited by the bank heights that the stream has to rework as it moves across the valley. For example, when a river moves from point x to point y and back to x, the bank height that has grown at point x during the time the river traveled across the valley depends on the channel mobility. The slower the migration rate of the river, the higher the walls that it encounters at x once the river returns. The observed equilibrium valley area therefore seems to imply that the wall-height and the capacity of the river to transport the material of the walls is important even in loose sediment.

Section 4.1.1: It may be clearer to introduce the concept of channel mobility and the way it is defined in this study either earlier in the paper or at the beginning of this section. At the moment, the channel mobility is defined at the beginning of the second paragraph of the section. As far as I know, the term channel mobility has mostly been used in the framework of alluvial rivers. I am guessing that the processes that limit the mobility of channels in loose sediment and in cohesive bedrock are partly different. Therefore, it would be helpful to clearly make a link here to the treatment of deposition of sediment in the model and to emphasize that in this model, any lateral movement involves bedrock erosion.

P.9 L.15 Because the treatment of deposition was not clear to me, at this point, I found it hard to wrap my head around how α affects channel mobility. Is it purely the effect of sediment deposition creating topography and therefore causing channel to switch more frequently? Or does sediment deposition also create an alluvial surface across which channel can migrate rapidly? After reading the end of the manuscript it became clearer that sediment, when deposited, "becomes" bedrock. Therefore, I am guessing the reason that increased sediment flux creates more mobile channels is only because sediment deposition creates "topography" that moves channels? I could imagine, that such questions could be avoided if the treatment of sediment is explained earlier.

P.10 L. 6. I had to read the sentence a few times until I understood. Possibly rephrase to write it as "That is, the maximum possible extent of x positions occupied by the channel is equal to lambda, but the actual […]".

P.10 L. 26: Here, the increase of channel mobility with alpha is discussed. I was just wondering if the decrease of channel mobility at alpha=2.0 is relevant to mention? At least that it may constitute an outlier?

P.11 L.18. The expression "The […] models take […] 10 ky to respond to lateral erosion" was a little unclear to me. What constitute a "response to lateral erosion"? Is it the time lag between the onset of the lateral erosion after the spin-up and the corresponding appearance of a signature in the topography? In which case, is there some characteristic that was used to define when the topography was thought to show a response? I am sorry if I misunderstood this…

P. 12 L. 20-21 Typos in this section “runs are easily”, “processes due to their low relief”, “has recently been shaped”

P. 12 L. 20-21: Again, I am sorry if I am misunderstanding but I would be interested in some expansion of the thoughts behind why the widest valleys occur in models with low channel mobility. I am unsure what is meant by “hillslope processes” in this context. I don’t think any hillslope processes have been introduced in the model or in the introduction and theory sections of the paper. This word makes me think of landslides, hillslope creep or gullying – none of these processes are in the model I believe and I am not sure I understand what is meant here.

P. 13 L. 20-22: The sequence of incision followed by lateral erosion in the TB models versus simultaneous incision and lateral erosion in the UC models would be nice to see in a figure. It is not clear from Fig. 6. In Figure 8, one panel is missing for the TB and the UC models respectively (the panel for 120 ky, just before lateral erosion starts in the TB model) to appreciate that sequence. Maybe it is possible to add one more panel and to refer to Fig. 8 at this point already? The same added panels would be nice to have at the end of this section (P. 14 L.12-19).

P14 L24-26: I did not understand the explanation for why there is no lag time. Is the argument: 1) Lateral erosion rate is increased more than incision rate and 2) the bank height is not important in the UC models -> Therefore any increase in lateral erosion rate translates directly into a widening rate?

P14 L24-26 typo: “two times” or “two time steps”?

P.16 L. 12. I might be missing something obvious, in which case ignore the comment, but I am unsure of how to distinguish valley width formed via valley infilling or via lateral erosion from the curves of Fig. 11c…

P17 L19-26 I am glad to see a discussion about channel mobility and I was thinking about whether this discussion could benefit from a few clarifications and some restructuring. Therefore, I briefly come back to the definition of channel mobility that I mentioned above. The cited studies (Wickert et al., 2013 and Bufe et al., 2016) define channel mobility in the context of the fluvial reworking of an aggrading (or steady) alluvial surface. In my mind, this “alluvial channel mobility” is not exactly equivalent to the rate at which actively uplifting valley walls are eroded – and therefore to the definition of channel mobility in this study. I totally agree that one can define a channel mobility in the context of the cumulative migration metric that was used in this paper. Such a metric can be calculated and defined independently of any regard for whether rivers are migrating across a valley, across an alluvial fan, or whether rivers are eroding valley walls. However, I am unsure that there is an a-priori reason to directly, and without further explanation, use the same terminology for migration rate of alluvial channels within an alluvial valley and the lateral erosion rate of valley walls, and therefore valley width. There are of course reasons to make the link between channel mobility and valley wall erosion. Hancock and Anderson 2002 hypothesized the importance of the frequency of the contacts between the river and valley walls. Malatesta et al, 2016, and this study, demonstrate a potential importance of valley wall height. As mentioned above, if wall height is important, then channel migration rates across the active valley and the uplift rate should control valley width. This was demonstrated by Bufe et al., 2016 at least for loosely consolidated valley walls. In short, there seem to be links between the “classic, alluvial” channel mobility and the lateral erosion of valley walls but I think the link merits expanding upon before the term “channel mobility” is interchangeably in both contexts.

Section 5.2: Maybe here, the comparison with Hartshorn et al., 2001 and Stark et al., 2010 can be made. However, the limits of not considering tools and cover in the models might have to be discussed in more detail before that.

P.18 L.11. This paragraph discusses the model setup that links lateral erosion to channel curvature. I think it is worth noting in this context that Fuller et al., 2016 documented lateral erosion in a straight channel. As noted by the authors, the deflection of sediment (tools) toward the walls seemed to control lateral erosion in these experiments, thereby documenting the importance of tools. Because the model (and this paragraph in the paper) discusses the importance of channel curvature and because the significance of the absence of tools in the model has not been discussed, maybe the comparison with the Fuller et al., 2016 study can be moved and/or expanded upon?

P18 L 14 Typo: “has come into equilibrium”?

P18 L23. Maybe worth discussing

- Anton, L., A. E. Mather, M. Stokes, A. Munoz-Martin, and G. De Vicente (2015), Exceptional river gorge formation from unexceptional floods, *Nature Communications*, *6*.

This study documents knickpoint retreat and subsequent widening (maybe comparable to the TB models?) in hard bedrock.

P18 L 33 Typo: “stream power to carry”?

P19 L1-2 As mentioned before, it would be worth to discuss the treatment of sediment in more detail, and earlier in my opinion.

<u>Figure comments</u>

Figs. 2-3: It might help to spell out the abbreviations “UC”, “TB”, and “spin” in the figure legend. There should be enough space. If not, it would be useful to have the definitions in the caption. It could also be helpful to add the other variable (K or alpha) to the boxes on top of the figures. For example “high K, moderate alpha”, “high alpha, moderate K” etc.

Figure 4: The term “spinup model” could be used in panel ‘a’ to more easily relate these models to the previous figure. Also, I would tend to try not having text and grid overlap. Finally, the axis labels for x and y axes may be useful

Fig. 5; the c-axis (slope legend) needs a label and maybe x and y axes could use labels, even though it is fairly obvious what they are

Fig. 6. The last sentence in the caption reads as if there was only waterflux from 100-150 ky – I am guessing “increased drainage area” or “increased waterflux” is meant?

Fig. 9: Maybe you can add the type of model to the title of panels a and b – as well as give the actual number of K instead (or in addition to) “low” and “medium” K.

Fig. 11: Should the y axis label not be “difference in valley width”?

I hope these comments are clear enough and may be of some use. I am looking forward to seeing the study published!

Sincerely,

Aaron Bufe