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Interactive comment on "Steady state, continuity, and the erosion of layered rocks" *by* Matija Perne et al.

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This manuscript on the influence of horizontally (or close to it) layered rocks and their influence on landscape evolution is very interesting. Some of the results are extremely counter-intuitive, and that always makes for a fun read. The math and modeling seem sound to me, and I'm generally supportive of this paper. The paper is timely, as another paper on a similar topic recently came out – Forte et al., which is cited here. Forte et al., also discussed that steady state is not reached with horizontal layers. Where this paper falls a bit short, in my opinion, is a lack of much discussion and also some lack in details of the modeling. As for the discussion, I thought they might tie in more with the Forte paper at some point, but that never happened. But in general I did not find the discussion to be very deep. As for the modeling, it was not always clear to me why the models

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Discussion paper

were set-up as they were. My general comment is just to give a bit more detail, including around the figures, and some suggestions for this are laid out in my line-by-line comments.

We thank the reviewer for their careful reading of the manuscript, and pointing out a number of items that were unclear or deserved further elaboration. Detailed responses to comments are given below.

Line by line comments: After reading the abstract I'm still not sure what channel continuity means. Notably, the sentence starting on line 5 made no sense to me, and I think that made me stumble through the rest of the abstract. I went back and read it after reading the manuscript and then it made sense to me. I think it was hard for me to envision what retreat in the direction parallel to a contact meant without the schematics, but after seeing the schematics it seems obvious. I don't have a great suggestion for improving this sentence.

We agree that it is difficult to understand without a figure, and are not exactly sure how to make it clearer. In general, we are also considering whether there is a better word than continuity (see Reviewer 3 comments) or an easier way to explain the concept. Any changes regarding this point will be detailed in the final reviewer reply.

The caption in Figure 2 and main text around it confuse me. In A, is the upper layer steeper, or is it simply that the upper layer is overhanging the lower layer, creating an instability? Similarly, in B, isn't the problem that there was a dam created? Equation 2: Is this vertical incision rate?

We have attemped to make this clearer. In case A, the upper layer can become steeper or create an overhang, it depends on the dip of the contact. In case B, the lower does always create a kind of dam (where presumably sediment could accumulate). We have adjusted the text accordingly.

Page 4, first paragraph. I see the math, but this is confusing. A few things. I

wonder if it would be helpful to remind people the relationship between K_w and K_s? As for equation 5 with n<1, the prediction is so counter to my 'gut', that I wonder if some discussion about whether n<1 is realistic, or about whether this counter intuitive relationship has been observed, would be useful. The n=1 case is also difficult for me to wrap my head around. Maybe more discussion is coming later.

It is definitely counter to the common intuition based on prior work. One of the main points of this manuscript is that the assumption behind that prior work actually can break down in subhorizontal rocks. We try to explain this in lines 6-8 of this page. Basically, it results because horizontal retreat rate (or knickpoint celerity) is lower for steeper channels in the case where n<1. This is because, for the same rate of vertical erosion, horizontal retreat rates are less for steeper slopes. In cases where n<1, the increased erosion from the channel becoming steeper isn't sufficient to offset the slope effect. At n=1, these two effects are totally balanced, so slope has no effect on horizontal retreat rate. We have expanded this section to try to make it a bit clearer. We do also discuss later cases where n<1 might be reasonable.

Page 4, line 28: What does it mean that experiments with resolution suggest that the conclusions are not affected by numerics? Does that mean you changed the resolution and ran with different numerical schemes, and got the same answer? Or that your results are not dependent on the resolution for a given implementation of stream power? Please clarify.

Our original statement was a bit too vague. We have edited this to clarify that we ran some higher resolution simulations for some cases that produced the same result.

Page 5, line 6, 7: Is layer thickness thought to vary with uplift? I don't think so. Why do you do this?

We are specifically examining cases where there are many different rock layers, such that the influence of base level perturbations dies out and we can see the continuity equilibrium form. This was just a practical way of generating a similar number of contacts in both the high and low uplift cases (albeit both with parameter ranges that are within the range of natural streams).

Page 5, L 14: What do you mean it holds if ‘slope is replaced with slope‘?

Slope is replaced with “slope in χ-elevation space.” We agree that the repeated word obscures the meaning a bit. We have replaced the second “slope” with “gradient” to remove this repetition.

Figure 4 caption: What is meant by the ‘steady state profile predicted by the theory‘? Just the elev-chi plot for a channel with that erodibility in vertical layers? Or is it the theory that you present in this paper. I’m confused.

We mean the theory presented in this paper. We have edited the caption to say “profile predicted by continuity steady state.”

Page 7, summary in paragraph on line 25: I got a bit lost. I think a bit more description/hand holding for the reader would help. I recognize that lambda* is a way to show how large $\chi_{s,+}$ is. But in the description with respect to figure 6, the damping is described in terms of cycles through rock layers. I don’t understand what this means, or how to get that from the equations. I must be missing something easy. How does $\chi_{s,+}$ related to the depth of the rock layers? How do I know from lambda* how many layers the knickpoint has propagated through?

$\chi_{s,0}$ is the χ length of the strong layer reach near base level at the moment that the weak layer becomes exposed at base level. Consequently, this distance is less than the profile distance spanned by a pair of weak and strong rocks, but is also on the same order of magnitude. The dimensionless damping length scale, $\lambda^* = \lambda/\chi_{s,0}$, therefore provides a rough (conservative) estimate of the number of strong/weak pairs that the knickpoint will pass before significant damping. We have expanded this text to clarify this point.

Figure 7 is difficult for me to interpret. I think I can see the knickpoint that is propagating up in elevation, but I can't really make out the knickpoint that it is 'catching'. Can you tell us how you determined that there was a knickpoint at the red line that was caught? If I look at the dashed line (intermediate time) in C, it does not look like there is any significant change in the chi-elev relationship at the red line, but I think that there is supposed to be a knickpoint there, right? Or at least one close to it that will soon catch up? I only see one knickpoint downstream from there, but maybe I am interpreting incorrectly? Actually, after watching the movies, I may understand this. But I still think it is worthwhile to point out to readers exactly what you are calling knickpoints.

Figure 7 was the best way we could think of to show this statically, though it is much clearer in the animations, as we state in the text. We think that the point of confusion here is that the knickpoints we are talking about are just sudden changes in slope, and can correspond to increases or decreases in slope (depending on n). We have expanded this explanation in the text.

Fastscape runs: It is a bit unsatisfying that the n=2/3, 3/2 runs have channels that extend through 4+ layers of each rock type, but the n=1 run only just barely taps three week layers. I know this is a lot to ask, but it'd be more satisfying to see more of the n=1 profiles, i.e. just make the K values in this run smaller. I'm not adamant about this, as the 2D runs appear to be very similar to the 1D runs.

We will try some cases with smaller K to see if we can replace this figure with one that is more similar to the cases where n is not 1.

In the beginning of Section 4, the authors mention that they include hillslope processes. It seems like this needs a bit more description. How are the different rock types treated with the hillslope model? How do they model hillslopes?

We are using the standard hillslope diffusion approach employed in Fastscape, which does not have the capability to adjust the diffusion coefficient with rock type. We now clarify this in the text.

Discussion and Conclusions: I liked that the authors brought in a real world example. However, this example confused me. I may be wrong, but my impression of Niagara Falls and the Niagara river is that the soft rocks underneath the hard caprock are indeed basically vertical at the waterfalls. But if you move any length downstream the channel is not so steep anymore. I might be wrong as I haven't studied the Niagara River, just visited it. But does the whole length of profile have the 'inverted' relationship (steeper in weak rocks) suggested by Figure 4D, or is it just 'inverted' around the waterfall? This may seem a picky point, but I would guess, as the authors brought up elsewhere, that the processes going on right at the knickpoint are not adequately modeled by stream power. So in some ways this comparison feels a bit odd to me. I felt as though the discussion could be expanded a bit.

If the stream power erosion law continued to hold as the channel steepened, then in theory one would expect the entire channel to remain steep in the weak rocks (for $n<1$)). However, the stream power erosion law breaks down for such steep channels as erosion processes take over that are not well-described by the stream power law. Therefore, we are only speculating that continuity can push the system toward this state (by first making the channel steep in the weak rocks). From a more standard assumption of topographic equilibrium, one would never expect steepening in weak rocks, so it is not clear how you would approach such a state to begin with. There are potentially other explanations, such as non-locality in erosion processes near the contact, that cannot be entirely ruled out. We have expanded this discussion slightly and tried to make it clearer that it is speculative. In the specific case of Niagara Falls, which is one of the most famous of many possible examples, the flattening below the waterfall in part occurs because of the nearby base level imposed by Lake Ontario.

The parameter n turns out to be extremely important in this study. Any thoughts beyond Niagara Falls on how your contribution plays in to the n debate? Have

many studies suggested that n<1? Are there any other landscapes to call upon to illustrate the modeled behavior besides Niagara Falls? I also generally prefer a separate conclusions section. I think it is better for authors because often times the only sections of the paper that get read are the abstract and conclusions. But this is stylistic.

We do not attempt to constrain what realistic values of n should be. There are theoretical arguments that some incision processes will produce n<1 (e.g. Whipple et al. [2000] cited in this work, or Covington et al. [2015], GRL). While there are likely good field sites where a natural experiment could be used to test the ideas developed here, we think that finding and studying such a site is beyond the scope of this manuscript. Our main goal here is to solidify our theoretical understanding of the equilibrium behavior of the stream power erosion law in layered rocks.

We have expanded the discussion and written a separate conclusions section. The expanded discussion focuses on clarifying our ideas concerning caprock waterfalls and on discussing the relationship between our work and that of Forte et al. (2016) more extensively. The lack of a detailed discussion of the Forte et al. paper was an oversight, which partly resulted because we had an initial draft of our manuscript before the Forte et al. paper was published. Citations and brief comments were added after the fact. We now carefully explain the relationship between our work and that of Forte et al.

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