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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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We thank the reviewer for their careful criticism of the text and inciteful comments that have helped us to clarify and improve the manuscript. We give detailed responses to specific comments below.

p2 L28: what does "steady state form of a landscape" mean here? You've just convinced me it doesn't exist in these settings...this is a bit more clear that you mean something like a flux steady state after reading the rest of the paper, but it seems that there is no steady landscape form except in the vertical-contacts case.

We changed the wording in this sentence, and added an additional sentence to clarify that we are talking about a flux steady state rather than topographic steady state.

p3 L19 A change in process (e.g. away from stream-power erosion) under steep conditions breaks this relationship, as noted above on L10 or so. This is discussed to some extent but could bear more emphasis. These boundaries are the very places where erosion processes are changing. For example, some of the same authors have published on how blocky debris from strong lithologies locally alters the erosion by streams in these settings. The change to effectively a transport-limited system may necessitate at least a change in the exponents, if not the form, of the erosion law. It is clear from the later discussion that the authors appreciate this; it would be useful at this point perhaps to point out that the formulation in Eq. 3 is effectively a reference case, deviations from which may reflect the process variability present in any particular landscape.

We agree with the reviewer on this point and have added a couple of sentences to make this assumption explicit.

p4 L15 What is considered "subhorizontal" here? How close to horizontal can the contact be before this singularity becomes important? It is rare in nature (but common in LEMs) to have a perfectly uniform, mathematically horizontal dip over a significant distance. I suggest adding an extra set of lines (or two) to Fig. 3 with some dip cases close to horizontal, perhaps 5 and 10 dip, in addition to the vertical and pure horizontal cases.

Subhorizontal is defined on Lines 1-2 of page 4. It is whenever rock dip is small compared to channel slope. Therefore, the cases shown actually span a wide range of possible contact and channel slopes. There is not a simple way that we can think of to show specific other choices of dip angle. We have modified the main text and figure caption to make it clearer that these two limits are not explicitly a function of rock dip, but rather a comparison between rock dip and channel slope.

p4 L18 "solely a function of erodibility." In this framework. I would argue that process variation is critical here. There is certainly field support for a retreat

rate that is independent of slope but a function of drainage area in relevant landscapes, a la Crosby Whipple 2006 (cited) and Berlin Anderson 2007 JGR (not cited but quite relevant). But another way to view this singularity is that perhaps n=1 works well away from contacts in sub-horizontal rocks but the stream power erosion law itself is not a good model in these situations. As noted, this is also where numerical inaccuracies may become very important in LEMs. I appreciate the authors pointing out where numerical models may diverge from reality when considering this continuity framework.

We agree. For n = 1 the horizontal retreat rate is a function of erodibility AND drainage area and independent of slope. This is a direct consequence of stream power erosion law. (In chi space, for n = 1 the horizontal retreat rate is a function of erodibility and independent of slope AND recharge area.) We have corrected the text to include drainage area as a factor influencing retreat rate. We also agree that the singularity precludes validity of the stream power erosion law in these situations because it causes the predicted slopes to not be small enough. We have also added a couple of lines of discussion concerning the cited field work and implications for the n=1 case.

p6 L13 "time-averaged incision rate through both rock types: : :" This needs some clarification. Do you mean vertical incision rate in both rocks is identical to the uplift rate? That doesn't seem quite right. Averaged over what time period? p6 L17-18 "continuity state is a type of flux steady state" Here this is presented as if it follows from the above analysis, but it was stated on line 13 above that the analysis is based on assuming flux steady state. It reads as being a circular argument, but perhaps the phrasing just needs some clarification.

This section was not very clear and did appear circular. We have edited it to make it clearer. From the results of the simulations, and specifically the fact that the landscape is periodic in chi space, you can argue that the system must be in a flux steady state. Using this conclusion, we can derive full profiles. Finally, that the whole story holds together is further confirmed by the fact that we can match the simulated profiles using

the equation derived from flux steady state.

p7 L27 "two cycles through the rock layers" not clear what this means - what cycles? The perturbation has traversed two sets of contacts?

Not exactly - it means the knickpoint caused by the perturbation has travelled so far upstream that two sets of contacts now separate it from the downstream end of the channel that is being perturbed. The number of contacts it traversed on its way (if any) depends on the ratio between horizontal retreat rates and knickpoint celerity. As a side note, knickpoints pass from one lithology to another unobstructed. They get damped through formation of stretch zones as in Royden Perron 2013 and through interfering with one another. We have edited the text to try to clarify this point.

p7 L30 how does layer thickness affect this result? Presumably it affects the distances across which a profile is developed in each rock type. A common geological scenario is thinner layers of hard rock between thick layers of soft rock. Will thin layers of hard rock slow down knickpoints for less time than thick ones, reducing the damping lengthscale? The analytical expressions and 1D modeling here stick to equal thicknesses of each type. I suspect the general result is the same, but pointing out the effect would be useful, and how to account for it in the framework described on p7. I see this issue is addressed to some extent in the 2D model setup, but its effect is not then discussed, and the 200 and 300 m alternating thicknesses are similar enough that I wouldn't expect a big impact. What about 100 m of weak rock alternating with 10 m strong-rock interbeds?

Only the thickness of the stronger layer influences this length scale. This results because the problem is asymmetric with respect to the two rocks. The strong rock knickpoints are always slower. The time for the weak knickpoint to catch up depends on only three things: 1) how big of a head start the strong knickpoint has, 2) the velocity of the weak knickpoint, 3) the velocity of the strong knickpoint. The velocities of the two knickpoints are independent of layer thickness. The head start of the strong knickpoint is only dependent on the thickness of the strong rock. Therefore, the thinner the strong rock layer, the quicker the knickpoints should decay.

We agree that it would be interesting to simulate some cases with thin, hard layers. We will begin simulating such cases for possible inclusion within the manuscript.

p10 L4-5 It's pretty hard to call the reach corresponding to a caprock waterfall a "channel", especially once flow is detached from the face. I think eSurf gives you the space to elaborate a bit more on how processes might commonly change in these settings (see my notes above) and how in general one would incorporate this into the continuity framework (without detailed exploration of such a case).

We agree that processes dramatically change in this setting. Our speculation in the manuscript is that stream power erosion, specifically in subhorizontal rocks with $n<1$ is one possible mechanism to drive the system toward the caprock waterfall state. Once the system reaches this state, stream power erosion has certainly broken down. We are considering how we might further elaborate on these ideas and will include more detail in our final reviewer response.

Minor notes p2 L17: responce -> response Fig 7 caption is missing punctuation at the end.

These typos were corrected.

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