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Earth Surface Dynamics Discussions

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

Anonymous Referee #1

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This manuscript describes the formulation of a fluvial erosion model based on the principle of continuity, which as described, results in a type of flux steady state. It explores the dynamics of the commonly used stream-power erosion law within this framework, and arrives at a useful new reference case for fluvial analysis in the face of strong contrasts in rock erodibility. This is an important and common geological situation that has until recently received little attention compared to uniform-erodibility cases, perhaps because of its intrinsic complexity. This work contains a thought-provoking analysis that provides a framework for addressing this complexity.

I find the manuscript to be fairly mature and close to ready for publication. I have no major concerns about the formulation or the interpretations, but below point out a few places where further discussion may be warranted. There are a few scattered typos, etc., which I list last.

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.

p3 L12 "numerical models are more likely to maintain continuity. . ." this is a good point.

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.

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.

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 $\sim$ 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.

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.

p7 This analysis of the damping behavior is very interesting with strong field implications.

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

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?

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).

Minor notes

p2 L17: responce -> response

Fig 7 caption is missing punctuation at the end.

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