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Interactive comment on "Landscape evolution models using the stream power incision model show unrealistic behavior when *m/n* equals 0.5" *by* Jeffrey S. Kwang and Gary Parker

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We thank the reviewer taking the time to review our paper. The comments will be very helpful to us for improving our manuscript.

Reviewer 2: *"This paper presents a call to arms, urging landscape evolution modelers who use the stream power incision model (SPIM) to "move on to more sophisticated models", which better represent the physical mechanisms responsible for river erosion of bedrock, such as abrasion by sediment."*

This is indeed a one of the main motivations for our manuscript. Thank you for recognizing the point.

Reviewer 2: *"The argument rests primarily on the finding of scale invariant solutions when the SPIM exponent ratio m/n = 0.5, for the case where the commonly-used hillslope "diffusion" term is omitted."*

While we believe that the scale invariant case is the most interesting and unexpected of our results, we think that our analysis of the slope and elevation singularities at the ridge and of the scaling when $m/n \neq 0.5$ are also important for our argument. In particular, relief increases with scale for $m/n < 0.5$, but decreases with scale for $m/n > 0.5$. We will make sure we emphasize its important in a revised version of the manuscript.

Reviewer 2: *"While I am sympathetic to the stated goals of this work, I worry that, ironically, this paper may have the opposite impact by focusing so narrowly on a rather anecdotal result."*

The horizontal scale invariance for $m/n = 0.5$ is indeed glaring. As documented immediately below, we suggest that this choice is not anecdotal, but instead reflects common usage in the landscape community. We need, however, to emphasize more clearly that our focus is not narrow, but covers the entire range of values of m/n. Repeating text above, relief is scale-invariant for $m/n = 0.5$, relief increases with scale for $m/n < 0.5$, but decreases with scale for $m/n > 0.5$. We can think of nothing about the morphodynamics of natural systems that would dictate such behavior.

Reviewer 2: *"The model behavior described here will rarely occur in model studies* *because modelers typically use other m/n ratios, or hillslope diffusion terms, minimum hillslope lengths or other model components that avoid this result."*

We would like to suggest otherwise. Firstly, many modelers have indeed used the value m/n = 0.5, either as the sole value or as an option. Some notable examples are Willett et al. 2014 (use $m/n = 0.5$ and $hm/n = 0.5$), the FASTSCAPE MODEL (e.g. Braun and Willett 2013, use $m/n = 0.5$ and $hm/n = 0.5$), and LANDLAB (e.g. Hobley et al. 2017, use $m/n = 0.5$). While the values of both m and n can be altered in LANDLAB, $m/n = 0.5$ is set as the default. Our argument is when modelers have little information on what the m/n ratio should be, their default value is 0.5, the value that leads to scale invariance. We provide a table of papers in which a value of m/n equal or close to 0.5 has been used.

Secondly, neither the inclusion of a hillslope diffusion term nor the use of a minimum hillslope length rectifies the scale invariance problem associated with $m/n = 0.5$ in the larger sense. We refer back to middle three panels of Figure 2b of our manuscript. Shown therein are steady-state landscapes for $m/n = 0.5$, with horizontal scale $L_{2D} =$ 22.4 km, 224 km and 2240 km. We assume for illustration that the "fine scale" length (diffusion or hillslope length) is 2 km. It follows that unrealistic scale invariance prevails over lengths corresponding to 91.1% of the smallest basin, 99.1% of the medium basin, and 99.9% of the largest basin. SPIM forces the landscape to behave like the bellows of an accordion; pushing scale down jacks up ALL the slopes when $m/n = 0.5$.

We emphasize our belief that it is important to study the behavior of SPIM itself without the use of other sub-models (e.g. hillslope diffusion). We further argue that insight into the fundamental behavior of SPIM will be valuable when choosing e.g. a bedrock abrasion-incision model for implementation within a landscape evolution model.

Reviewer 2: *"I agree with the suggestions of the first reviewer for how this work could be extended in constructive ways."*

The second reviewer also agrees with the first reviewer's suggestions for improving our work. As we said in the response to the first reviewer, we will add examples of conditions where SPIM leads to incorrect interpretations, and apply our scaling analysis to show that more sophisticated models do not suffer from horizontal scale invariance. Please look at our written response to the first review.

Reviewer 2: *"For example, can scale analysis be used to identify when the SPIM may lead to incorrect interpretations, or test the validity of divergent model outcomes, such as the findings of Egholm et al. (2013) who directly compared the SPIM with a bedload abrasion incision model?"*

Thank you for citing the Egholm et al. 2013 paper; this paper will be cited within our modified manuscript. This paper clearly features a problem that requires a model that is more sophisticated than SPIM. We appreciate the direct comparisons between SPIM and a bedload abrasion incision model. Egholm et al. [2013] uses many sub-models (e.g. hillslope diffusion, isostasy, landslides, etc.). Our paper, however, is focused on how SPIM captures incision in a 2D landscape model. Our paper will be improved by adding a comparison between the way in which relief structure created with a) SPIM and b) a bedrock-abrasion incision model scale with horizontal length. Thank you for your insightful comments, and we hope our proposed additions will satisfy your concerns regarding our paper.

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Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/esurf-2017-15/esurf-2017-15-AC2-supplement.pdf

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