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Earth Surface Dynamics Discussions

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

Jeffrey S. Kwang and Gary Parker

jeffskwang@gmail.com

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We thank the reviewer for their thoughtful comments and suggestions. From the review's introduction, we believe the reviewer has a good understanding of our main results and our motivation for writing this paper.

Reviewer 1: *"The singularity at A = 0 is well known."*

There are two issues here: elevation singularity and slope singularity at the ridges.

We agree that it is readily seen that there is a slope singularity at the ridge at steady state when drainage area goes to zero. However, without integrating the conservation equation at steady state, the existence of an elevation singularity at the ridges for $m/n \geq 0.5$, and its absence for $m/n < 0.5$ cannot be easily deduced. This is especially true in the 2D model, which has no analytical solution. Within the literature, there has been little discussion specifically oriented to the singular behavior of SPIM in regard to slope at the ridge. To our knowledge, the presence or absence of the elevation singularity (according to the value of m/n) in 2D models has never been shown in the literature.

Reviewer 1: *"It is a part of the solution that exists on paper but is never realized in nature because other mechanisms dominate erosion near drainage divides, where drainage area is small."*

We agree with the comment. But our goal is not to understand how SPIM performs in conjunction with other mechanisms, but rather to see how SPIM itself performs. For this reason, we did not include hillslope diffusion in our model. There are two additional issues. 1: When the using a grid size in the 2D model that is larger than the hillslope length scale, hillslope diffusion has little to no effect on the landscapes relief. 2: When the basin is sufficiently large compared to, e.g. the scale of hillslope diffusion, unrealistic horizontal scale invariance prevails at all scales larger than that of hillslope diffusion when $m/n = 0.5$.

Reviewer 1: *"The authors already seem to consider this a secondary point – they don't mention it in the abstract – so removing it would not change the paper much."*

We do not agree with this statement; the singular behavior is an integral part of this paper. It is our belief that the singular behavior is an important for demonstrating some of the pitfalls of modeling landscapes with SPIM. The reviewer makes good points here, and we thus propose to expand our discussion on the singularity and focus on explaining its importance.

Reviewer 1: *"The special mathematical case for 2m=n is interesting, and the thorough analysis presented in the paper could form an important part of a more general study of scaling in landscape evolution models. However, I am not convinced that a paper that presents only this result can stand on its own."*

Our paper does indeed focus on the scale invariant case, as we believe it is the most interesting and surprising part of the analysis, and corresponds to the most commonly used value of m/n. However, looking at Figure 2, we not only present the scale invariant case of $m/n = 0.5$, but also $m/n = 0.4$ and $m/n = 0.6$. We need to emphasize the following point in our revised text. 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 1: *"The demonstrated scale invariance occurs in a model from which terms that impart scale dependence have been omitted. One such term is the diffusion term in equation 2 (which should be positive)."*

Thank you for pointing out the mistake in our equation; it has been fixed. We have purposely omitted the hillslope diffusion terms in order to study the behavior of SPIM itself. The use of hillslope diffusion resolves the problem of horizontal scale invariance for $m/n = 0.5$ only at the finest scales.

Reviewer 1: *"The authors argue that hillslope diffusion "operates only at small scales". Sure, but might that not contradict the conclusion that "the steady-state landscape for a 1 m^2 domain can be stretched so that it is identical to the corresponding landscape for a 100 km^2 domain?"*

No contradiction, just hard to get an acronym into the abstract. Here is a proposed rewriting. "Landscape evolution models often utilize the stream power incision model (SPIM) to simulate river incision. That is, the steady-state landscape predicted using SPIM alone for a 1 m^2 horizontal domain can be stretched so that it is identical to the corresponding landscape for a 100 km^2 domain."

Reviewer 1: *"The authors also do not consider channel width, another potential source of scale dependence."*

Width can indeed provide a source of scale dependence. We will point this out in a revised text. But the purpose of our paper is to study a 2D implementation of SPIM in the context of landscape evolution. SPIM does not predict channel width, and the addition of hillslope diffusion or hillslope length does not change this.

Reviewer 1: *"I appreciate what the authors are trying to do: discovering flaws in widely used models is one way that science advances. But they seem to construe their discovery as evidence that the entire community is asleep at the wheel, and I don't think that is true."*

We thank the reviewer for grasping the central point of our paper. The comment *"But they seem to construe their discovery as evidence that the entire community is asleep at the wheel, and I don't think that is true"* is more sociological than scientific. We believe that our results stand on their own, and that the science speaks for itself without editorializing. It does not make sense, however, to point out the scale invariance issue when $m/n = 0.5$ without also pointing out that the use of this value is ubiquitous in the literature. (See table at the end of this response).

Reviewer 1: *"The fact that this simplification gives rise to scale invariance with a particular combination of parameters is indeed an odd quirk – one that is probably worthy of a cautionary tale – but it doesn't mean that the underlying arguments for relating incision rate to drainage area and slope are fundamentally flawed."*

We do not agree that our central result is an odd quirk. It is built into the fabric of SPIM. We repeat. 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 1: *"The version of the stream power model presented in this paper certainly has substantial limitations, and discussions of its shortcomings – as well as proposed improvements – abound in the literature."*

Yes, but... we have not found a single instance in the literature where the scale invariance associated with $m/n = 0.5$ has been recognized.

Reviewer 1: *"I see two ways in which the authors could potentially use their analysis to contribute to those discussions. First, perhaps they could show more clearly how scale-invariant models would lead researchers to draw incorrect conclusions about drainage basins, even if those researchers are aware of the limitations of the stream power model as a process law."*

Thank you for the suggestions. We agree that including these suggestions in our manuscript will greatly improve the impact and discussion of our manuscript. We hope to include examples of where the stream power incision model can lead to incorrect conclusions. Our first example of how SPIM can lead to incorrect conclusion is in its use to predict landscape relief. Whipple and Tucker [1999] show in a 1D model that SPIM can be used to predict landscape relief given the location of the channel head, X_c. In 2D models, the corresponding variable would be a critical area threshold, A_c, where above this threshold fluvial processes dominate (e.g. Montgomery and Dietrich 1988). We believe that our work on scaling relationships and ridge singularities can show that predicting relief using a 2D SPIM-based model cannot be reliable without a good understanding of what physical processes set the scale in landscapes (i.e. hillslope length/channel head location). Because the singularities affect the channel profile/relief most strongly in the headwaters, the fluvial relief of the landscape is sensitive to the choice of hillslope length. In determining the relief of the landscape, it could be that its value is more sensitive to the choice of hillslope length instead of the horizontal length of the basin (as predicted by SPIM). In addition, we believe our results also have implications for recent work on drainage basin reorganization and the stability of drainage divides. Most of the literature uses the χ methodology with SPIM to predict locations and stability of drainage divides. The stability of a drainage divide is taken to depend on the values of χ on either side of the drainage divide. χ is evaluated at a threshold value (X_c or A_c) from the ridge, and like η, χ varies sensitively due to the singular behavior near the ridge. We believe that our analysis on the ridge singularities in both the 1D and 2D model can help elucidate the uncertainty in the prediction of stable drainage divide locations.

Reviewer 1: *"Second, they might consider whether the particular shortcomings they document offer any insights into how a better model of river incision could be* *constructed – and whether any of the proposed improvements to the stream power law avoid these problems."*

In the literature, there have been many proposals for landscape evolution models that incorporate bedrock incision based on abrasion from saltating sediment particles. For example, Gasparini et al. 2007 propose a generalized saltaton-abrasion model. The steady state slope is given by the following equation $S = aA^{1-b}\left(1 - cA^{-0.5}\right)^{-1}$ (Note: We replace the actual parameters with simplified bulk parameters). If we non-dimensionalize this equation in the same manner as our manuscript, we get $\hat{S} = \frac{ag}{v^2}\hat{A}^{1-b}L^{3-2b}\left(1 - c\hat{A}^{-0.5}L^{-1}\right)^{-1}$. Just by inspection of this equation, we can see that it is impossible to make elevation invariant to horizontal length scale L. In addition, the exponent, b, formulation is made from empirical laws, where it is unlikely the term $3 - 2b \leq 0$. We will expand this discussion in a revised version of the manuscript to show that some bedrock incision models do not necessarily experience scale invariance.

We hope that additions such as the ones stated above will help our manuscript contribute to the discussion of the strengths and weakness of SPIM, and help motivate improved prediction of landscape evolution.

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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-AC1-supplement.pdf

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