Earth Surf. Dynam. Discuss.,
doi:10.5194/esurf-2017-15-RC1, 2017

Earth Surface Dynamics
Discussions
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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

Anonymous Referee #1

Received and published: 11 May 2017

This paper shows that if the equation

del z / del t = U – K * Aˆm * Sˆn

is made dimensionless, the parameters form a non-dimensional group that includes Lˆ(2m-n), where L is a horizontal length. Thus, if 2m = n, the dimensionless governing equation is independent of L, and topographic surfaces that satisfy that equation do not depend on horizontal length. The authors demonstrate this result numerically by solving the dimensional equation on identical grids with the same initial surface but different delta x and delta y and obtaining horizontally identical solutions. They also point out that near drainage divides, where A is small, the second term on the RHS becomes small, and elevations in the solution become large. They interpret these

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Discussion paper

results as evidence that the stream-power term is an inadequate model of bedrock channel incision.

The singularity at A = 0 is well known. 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. 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.

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. 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). 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 mˆ2 domain can be stretched so that it is identical to the corresponding landscape for a 100 kmˆ2 domain"? The authors also do not consider channel width, another potential source of scale dependence.

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. My impression is that most researchers who use the stream power model understand that it is potentially relevant and potentially useful only at scales where erosional channels form, and that the form of the model written in terms of drainage area and slope is a convenient geometric simplification that is useful only under certain conditions. 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. 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.

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. 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.

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