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Interactive comment on "Scaling and similarity of a stream-power incision and linear diffusion landscape evolution model" *by* Nikos Theodoratos et al.

Anonymous Referee #2

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This paper contains a clever and thorough elaboration on dimensional analysis of modeled hillslope-channel interactions. I enjoyed reading it, and I expect it will be influential. The key advance is the recasting of a governing equation for landscape evolution due to uniform rock uplift, linearly slope-dependent soil creep, and stream power-dependent channel incision in a dimensionless form with no parameters, which allows the rescaling of a single solution (for a given set of initial and boundary conditions) to any set of dimensional parameters. I have a few comments for the authors to consider as they revise their manuscript, but I recommend publication without re-review.

The authors might consider commenting on the limits of rescaling the linear diffusion

term to very steep slopes.

What about channel width? Presumably channels in this model are assumed to be "sub-grid-scale"; how is that taken into account in the governing equation or the numerical scheme?

Section 4.1.1: I suspect that this framework could quantify the independent controls on elevation contour shapes and vertical relief noted by Howard (1997), Tucker and Bras (1998), and Perron et al. (2008). Perhaps worth discussing.

P16 L28-29; Fig. 9: Absent any tectonic deformation that creates a shifting topographic divide, it is a geometric fact that a drainage divide migrates if and only if erosion rates differ across the divide, and that the sign of the difference in erosion rates determines the direction of drainage divide migration. I wouldn't characterize this as a research finding.

Section 4.2.2: The use of the flow path length (which depends on topography) as the horizontal length scale in the spatially variable Peclet number seems to run counter to the spirit of the non-dimensionalization that is the paper's centerpiece, in which the authors avoided using any length scales that are topographic outcomes of the model evolution. I understand the authors' reason for doing the calculation this way (Fig. 11), but it does make it difficult to use this definition of the Peclet number to predict model outcomes.

The summary and conclusions section is a bit long.

The inclusion of the dimensional analysis for the more general case of 2m != n is nice. Although it is understandably relegated to an appendix, this analysis considerably broadens the applicability of the paper.

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