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Earth Surface Dynamics Discussions

Interactive comment on "Dimensional analysis of a landscape evolution model with incision threshold" *by* Nikos Theodoratos and James W. Kirchner

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In "Dimensional analysis of a landscape evolution model with incision threshold", the authors extend a previous analysis of the classic advection-diffusion landscape evolution equation with a constant source term to include a threshold for erosion. Though the neither the idea of erosion thresholds nor the advection-diffusion equation themselves are new, I have not seen them combined in this way before. This novel connection, together with the authors very thoughtful and insightful analysis of the equations produces some valuable and widely applicable conclusions. In particular, the fact that introducing a threshold adds a parameter to the nondimensionalized equations that

distinguishes landscapes with different relative threshold magnitudes from one another fundamentally is very interesting.

Overall I find the paper to be well cited, novel, scientifically rigorous and the impact is appropriate for the journal. The authors are clearly knowledgeable of the state of the art, and have placed their work in the correct context. The writing, figures and overall presentation is excellent. One of my few criticisms is that the authors have a tendency to over-explain some concepts, and I think it would be possible to shorten some explanations and derivations. However, the paper is not too long, and I don't think that this is a necessary change.

I have one significant criticism, which is that the authors have used a threshold with a steady-state constant rainfall/discharge, yet have compared it in many ways to thresholds which are derived under the assumption of stochastic forcing. There is very little modern work on erosion thresholds outside of a stochastic forcing context, because without a stochastic forcing, thresholds lead to dramatic, very nonlinear behaviours which are not realistic. I find that it is not difficult to include a simple stochastic forcing, though it would require rerunning the models shown in the paper. I think that the effort required to use stochastic forcing would be rewarded with a much firmer theoretical connection to modern work on incision thresholds and more interest from the community. I have included a document which contains further argument for using a stochastic forcing.

Besides this one major criticism, I would enthusiastically recommend this paper for publication. The authors do include a small section addressing my criticism already, and the novelty of the approach still stands even if the authors do not adopt stochastic forcing. I will not withhold my recommendation for publication contingent on addressing this point. However, I do want to take the opportunity to stress that I feel that including stochastic forcing will increase the significance of the paper, increase how well it fits in with the state of the art, as well as increase its impact, and I very strongly urge the authors to consider redoing the analysis with stochastic forcing, or adding it alongside

the original analysis.

- Eric Deal

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2019-80/esurf-2019-80-RC1-supplement.pdf

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