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Interactive comment on "Rivers as linear elements in landform evolution models" *by* Stefan Hergarten

Anonymous Referee #3

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Overview:

This paper addresses an important, and still largely unsolved, question in landscape evolution modeling: how to eliminate or minimize grid-scale dependence that can arise from the combination of flow algorithm, water erosion law, and gravitational transport law. The paper makes the case for a new approach. In my opinion, this is a valuable contribution that should be published, after suitable modifications. I have some recommendations, below, for how the value and impact of the paper could be increased. The most important of these is a recommendation to show examples of model simulations with and without the proposed scaling solution, to demonstrate that the solution does indeed minimize mesh-size dependence (and also to illustrate the nature of the problem generally).

General comments:

(1) The scaling problem: the paper describes in words the scaling problem as it manifests in landscape evolution models, but a picture would be worth a thousand words. I think the impact of the paper would be greater if the author added a figure showing visually the effect discussed section 2: the steepening of topography with decreasing pixel size. Pelletier (2010) has a figure showing the (relative) lack of such effects when using his proposed solution, so one idea would be to mirror that figure (his Fig 6) but without any attempt to scale the problem away. You could even use the same parameters. I would also suggest including a plot showing equilibrium slope-area scaling for models at different pixel resolutions.

Then, follow up by showing the same examples, but now using the proposed scaling solution. This would (presumably) demonstrate that the solution works. Adding such a 'before and after' pair of figures seems really key to selling the core idea of the paper; otherwise, readers might be left wondering 'if I bother to do this, will it really work?'.

(2) Some parts of the paper come across as if written for readers who are already well familiar with the relevant literature. I recommend making a few small additions / modifications that would make the paper accessible to, for example, graduate students who are just starting out, or people in other fields who have a new interest in the topic. Mostly this is a matter of adding example references to the literature and/or expanding on some points, as noted in the specific comments below.

(3) A general question, which would be worth answering somewhere in the text, is whether the scaling analysis still holds if the drainage patterns are qualitatively different on hillslopes versus channels. In this Discussion version of the paper, it is not clear whether the simulation in Figure 1 includes any local transport (ie hillslope) processes; if so, it is not apparent in the drainage patterns.

Notes keyed to text by line number:

19-20 For readers unfamiliar with this idea, it would be helpful to add one or a few example references (one of the earliest I am aware of is Andrews and Bucknam, 1987;

another option would be to cite a review paper that discusses mathematical representations of various geomorphic processes)

23 'has become some kind of paradigm' - this will not mean much to readers who are just joining the conversation. Suggest giving one or a few example references.

24 I disagree that equation 2 (I think that is what is meant by 'it') requires the assumption of constant precipitation. First, I would argue that it is runoff rather than precipitation per se that matters (though obviously the two are correlated). Second, there are quite a few papers that discuss the relation between precipitation characteristics and formulas like equation 2, going back at least to Willgoose et al. (1991 'part 1' in Water Resources Research, their Appendix B), and continuing more recently with papers like Deal et al. (2018) (and lots of literature in between). In any event, the text about 'constant precipitation' (constant in space or time or both?) seems like just a side comment, and maybe the best approach would be simply to delete it.

26 As above, I would argue it is the spatial distribution of runoff that matters most; precipitation has some influence on this, but there are other factors too.

33 The relation predicts m/n = theta ONLY if the erosion rate and erodibility are uniform in space and steady in time. You allude to that in the next sentence, but the way this is worded would be confusing for a reader who does not understand that you are referring to a special case here. I recommend re-wording this section to be more precise.

35-38 I would argue that the condition of equilibrium is more general than the word 'uplift' implies. The key is that the erosion rate is space-time uniform. This could be due to actual tectonic uplift relative to, say, sea level. Or it could be an equilibrium relative to a given rate of base-level lowering at the boundary of a given system (and in fact the former is a subset of the latter).

42-43 'the total area covered by large rivers decreases with decreasing mesh width': can you provide evidence for this, or otherwise clarify this concept? It seems to disagree with the view of Pelletier (2010), who showed examples where the drainage area of the larger catchments remains invariant to grid resolution, whereas the drainage area associated with hillslope 'patches' decreases with pixel size. My sense is that the area of the larger catchments in a given DEM or landscape model is probably strongly influenced by the domain size and geometry. Maybe what you actually mean here is that the surface area covered by stream segments ('channel pixels'), rather than drainage area, shrinks as pixel size shrinks (tending toward zero when the network segments become infinitesimally wide linear features).

46 - I think there is a bit more to it than that. If you omit local transport (ie, diffusion or diffusion-like modification of the topography), you have the odd circumstance where for the equilibrium case the equations predict that H => infinity as A => 0. In practical terms, then, a model with just eq 2 would have increasing relief with decreasing pixel size. Might be worth pointing out, as the current text ('scaling problem may not be critical') could be misinterpreted as meaning there is no pixel size dependence without local transport.

58-61 I don't think this summary quite does justice to Pelletier (2010). I suggest adding something like 'where the factor is unity on cells identified as hillslopes, but greater than unity for cells that represent valley features'. Also, you might add that a reason to suspect it doesn't work for 'all types of local transport' is that his derivation was (heuristically at least) based on a linear model.

62 Can you expand here to say why large mesh widths would be immune? Large relative to what? Is the idea that if all cells are conceptually valley cells, then you don't need special treatment for hillslopes versus valleys?

75 For what it's worth, I would argue that 'bedrock incision' just means what it says, and does not (or at least should not) imply any particular mechanism or model thereof. I think the idea you are trying to get at here is that there is a difference between assuming that a channel must entrain and remove only the material on the channel bed,

or that it must entrain and remove that plus the sum of material transported into the channel from surrounding hillslopes. I do not think the term 'bedrock erosion' is all that helpful in articulating the difference between these two possibilities, but it would be worth expanding on the idea: for example to note that it depends on the degree of contrast between the 'mobile' material coming from side slopes and the 'intact' or 'original' material in the channel floor (one example of highly resistant material coming from side slopes is Shobe et al. (2016 GRL)).

86-88 Consider noting here that Pelletier (2010) described an alternative approach based on comparing computing drainage area on the DEM grid, and on a 2x higher resolution interpolated version of the DEM. That approach has the advantage of allowing the processes to determine the drainage density. I suspect that the mechanism for identifying channel versus hillslope pixels probably does not matter much for the technique you propose, and if that is the case, then it would be worth pointing out. For the sake of developing the idea, using a fixed Ac seems totally fine. But as a reader I would like to know whether I can still use the approach if I use a different method for distinguishing channel and hillslope pixels.

89-90 I got confused at first by the definition of Ae. A key aspect of the definition is that it includes only those pixels that drain DIRECTLY to a given channel pixel, and not ones that 'pass through' another channel pixel upstream. If that understanding is correct, it would be worth stating this (because other readers, like me, are probably used to thinking of contributing area as something that accumulates downstream).

Eq 5: unless you are changing the definition of K, this equation seems to change the meaning of E: in eq 2 it seems to be length per time, but in eq 5 it seems to become volume per time. If that is correct, I recommend using a different symbol than E to avoid confusion. Note I am assuming that Ae is a surface area. The text says 'number of sites', so I guess it is actually meant to be dimensionless (just a count), as text later in the paper implies. But in that case then you're no longer talking about a physical law. Why not treat Ae as a surface area, and either have the equation represent the

volumetric erosion rate over the area concerned, or divide by cell area to arrive at a length per time. At any rate, clarification of these issues in the text would be helpful.

94 - It would be very helpful to add more information about this model and the conditions under which it was run to generate figure 1. Is OpenLEM in this example solving just eq 2 or does it include diffusion too? What flow routing algorithm does it use? How are closed depressions handled? Was it run until steady state balance between erosion and uplift/baselevel was reached? Does the fluvial threshold Ac actually apply in the numerical model, ie, are areas smaller than Ac treated exclusively with local transport? Is local transport applied to all pixels or just those A < Ac? Or, alternatively, was the model run without any threshold or hillslopes? In addition, please list all the input parameters so readers could reproduce or replicate the experiment.

121 I think you mean 'site' not 'size'

120-125 and eq 6: I found this section confusing. I understand Ae to be a spatial field, with a different value at every pixel. Yet if P(Ac) is just a scalar fraction, then eq 6 implies a unitary value for Ae. Is your aim here a derived distribution of the cumulative probability of Ae? ... Ah ok, reading later, you mention Ae is dimensionless (but perhaps you can see why it is confusing given that A and Ac refer to areas).

134 I recommend a more extensive explanation here. Clearly figure 4 shows that the Ae-Ac relation follows a power law with about the same slope as that of the cumulative area distribution. But the underlying scaling argument is hard to follow.

151 It would be helpful to know the parameters used to generate these synthetic topographies.

eq 9: if you used this directly in a model, would it not break equilibrium slope-area scaling? Or are you suggesting that the leading factors compensate for deposition by material sourced from surrounding hillslopes? I can imagine such an argument being quantified as:

equilibrium => fluvial erosion rate = uplift (baselevel) rate + hillslope deposition rate

=> E = U + D

and the deposition rate is (Ae - 1) U, ie deposition from the hillslope area but not the pixel itself, so you have

E = Ae U

...etc. This is basically the argument you're making, right? That effectively a fluvial grid cell has to drill through not only its own material, but also all the material coming from the surrounding hills. I think the idea would be conveyed more clearly if you added some math along the lines of the above.

183-4 reference for this number?

185-eq 10: I can see the advantage of this approach, but would like to see some discussion of how to reconcile the concept of a threshold area Ac with the actually valley head area that emerges from a model. To mirror my questions above, are you suggesting that this approach should be paired with using a model that only applies fluvial erosion to locations with A > Ac, where Ac is a parameter? Or could one allow Ac to emerge from the dynamics, as in Pelletier (2010)?

205-6 good point, and some models I'm aware of allow for diffusion-like transport to be applied ONLY to convex locations, with the assumption that the material is instantly carried away in concave-up locations.

Code availability: I do not know what the policy of Esurf is, but 'available on request' is no longer generally considered best practice. Better to place code in a community repository, or at least a public repository. Better still to have it under version control. Even better yet to provide input files, examples of usage, etc., in an open repository (see Wilson et al. below).

Some example literature on open and reproducible research software:

Nick Barnes. Publish your computer code: it is good enough. Nature, 467(7317): 753?753, 2010.

Irving, D. (2016). A minimum standard for publishing computational results in the weather and climate sciences. Bulletin of the American Meteorological Society, 97(7), 1149-1158.

Wilson, G., Bryan, J., Cranston, K., Kitzes, J., Nederbragt, L., & Teal, T. K. (2017). Good enough practices in scientific computing. PLoS computational biology, 13(6).

Benureau, F. C., & Rougier, N. P. (2018). Re-run, repeat, reproduce, reuse, replicate: transforming code into scientific contributions. Frontiers in neuroinformatics, 11, 69.

Stodden, V., Krafczyk, M. S., & Bhaskar, A. (2018, June). Enabling the verification of computational results: An empirical evaluation of computational reproducibility. In Proceedings of the First International Workshop on Practical Reproducible Evaluation of Computer Systems (pp. 1-5).

Chen, X., Dallmeier-Tiessen, S., Dasler, R., Feger, S., Fokianos, P., Gonzalez, J. B., ... & Rodriguez, D. R. (2019). Open is not enough. Nature Physics, 15(2), 113-119.

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