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

Anonymous Referee #2

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I applaud Hergarten for taking on an important problem in geomorphic modeling: how to achieve grid-resolution independence in coupled colluvial-fluvial landscape evolution models. All numerical models must be grid-resolution-independent solutions of the underlying equations. That is, they must converge to some validated solution as the pixel size decreases. Many landscape evolution models, however, are not grid-resolution independent and others achieve grid-resolution independence using ad hoc means. This is a significant issue that hinders our ability to model landscape evolution and to compare the results of one model to another.

Before providing my comments on the manuscript, I wish to first review a key alternative approach to the problem as a means of introducing the general issues at play. Pelletier (2010) addressed the problem of grid-resolution dependence in coupled hillslope-channel landscape evolution models by first noting that contributing area scales linearly with pixel size on hillslopes and other areas of non-convergent water

flow but independently of pixel size in fluvial valleys and other areas of convergent water flow. Given that all fluvial erosion formulae are based on unit or specific discharges (i.e., discharges per unit channel width) or related quantities such as shear stress or unit stream power, Pelletier proposed that landscape evolution models should also be based on unit or specific contributing area, both as a means of more faithfully acting as a proxy for unit or specific discharge and for minimizing the general grid-resolution dependence of flow-routing algorithms he documented. With the different widths of water flow on hillslopes (where flow occurs as sheetflow or rill flow) and channels (where flow is confined to a width that may be smaller than a pixel width) taken into account, Pelletier (2010) demonstrated how grid-resolution-independent fluvial incision rates can be computed. It is important to emphasize that any fluvial incision rate must depend, implicitly or explicitly, on the width of water flow since putting more total discharge into a narrower flow pathway will increase the fluvial erosion rate if steepness, climate, and rock characteristics are unchanged.

Pelletier (2010) also demonstrated that it was necessary to scale the divergence of colluvial sediment flux by the ratio of the width of water flow to the pixel width because otherwise colluvial deposition rates (which, together with the fluvial erosion rate, controls the rate of elevation change) in narrow valley bottoms are systematically underpredicted in models. Take, as an example, a location where the valley bottom in nature that the model is trying to represent is only 1 m wide but the pixel width is 10 m. The divergence term will be dominated by the gradient in valley side slopes in the cross-sectional direction. The model will predict that gradient to be the difference in slopes on either side of the valley divided by 10 m, while the actual gradient in nature will be the difference in slopes on either side of the valley divided by 1 m. The colluvial deposition term will, therefore, be too small by a factor of 10 in the model relative to nature and hence must be scaled up accordingly. It is important to note that such a modification to the colluvial deposition term is not some indirect way of scaling the fluvial term as Hergarten implies. Far from being a "problem obviously coming from the fluvial incision term" (line 60), it addresses a limitation of the model to represent the cross-valley curvature and the effect of that limitation on the resultant model-predicted colluvial deposition rates in valley bottoms.

Grid-resolution dependence in coupled colluvial-fluvial models can be seen most readily as a dependence of drainage density on pixel size. If I understand correctly, Hergarten is proposing to use this variation/error in drainage density to scale the fluvial erosion term. I am wary of this approach because there is no clear (at least to me) physical basis for why the fluvial erosion term would need to be scaled in this way and because there is no indication that the drainage density predicted by the model, even if it can be shown to be grid-resolution independent, is the correct one for a given set of model parameters after such scaling.

I apologize if I missed it, but I didn't see that Hergarten demonstrated that his approach actually leads to grid-resolution-independent results. I was expecting to see model results with similar topography as the pixel size varies over a wide range. No such figure appears in the paper. I recommend that Hergarten present such a figure along with any other analysis (e.g., predicted steady state drainage density as a function of pixel size) needed to demonstrate grid-resolution independence of the model predictions. I would like to see such grid-resolution independence also demonstrated for cases on non-uniform uplift rates, as such applications are common in landscape evolution models.

I had a hard time following the description of the scaling approach. My understanding is that the hillslopes and channels in the model output are first differentiated using a user-defined threshold area, Ac, and then the fluvial erosion term is modified by an amount equal to a power-law function of Ac. The power-law modification to Ac is clear but how is Ac chosen? Does the model have to be run first without scaling the fluvial erosion term in order to determine Ac and then rerun with the scaling? Please provide a step-by-step guide for performing the proposed scaling that is applicable not just to the case of steady uniform uplift to steady state but for other potential landscape evolution model applications. It may be that for the case of steady uniform uplift,

channels and hillslopes can be differentiated based on a threshold contributing area, but many landscape evolution models are of non-uniform uplift and hence non-uniform drainage density. Moreover, there is a large literature on how to differentiate hillslopes and channels both in models and real-world DEMs, and the use of a single contributing area threshold is universally regarded as an inadequate approach to such differentiation. Assuming that choosing Ac involves differentiating hillslopes and channels before scaling the fluvial erosion term, this manuscript glosses over a very complex topic, the implications of which likely influences the applicability of the proposed method.

A minor issue: it is incorrect to state that the erodibility coefficient K depends on rock characteristics and precipitation (line 25). K is influenced by any factor other than channel slope and contributing area that influences detachment-limited erosion rates, including channel width, all of the factors that influence rainfall-runoff partitioning (including vegetation, soil texture, the distribution and sequence of storm events), snowmelt dynamics (for some catchments), etc.

Again, I applaud Hergarten for taking on this important problem and look forward to seeing clarifications in due course.

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