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

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Dear Reviewer,

thanks for your constructive and encouraging comments! It is good to see that the problem with the grid-resolution dependence is still an important issue as long as we do not disregard the feedback of the hillslopes to the rivers. Let me take the chance to clarify your points immediately, starting from the discussion of the alternative approach suggested by Pelletier (2010).

I agree to your first point in recapitulating Pelletier's (2010) approach – *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, ...* They should, and this would make things easier. However, the relation that is used in almost all models is an erosion rate as a function of channel slope and total catchment size (some proxy for total discharge, but not for discharge per unit width). This old relationship was empirically derived from river profiles and has the advantage that it immediately bridges between model results and real river profiles via the concavity index and the steepness index. It is a lumped equation that already includes the downstream increase of river width without specifying this increase explicitly. This means that we must be careful when starting from this relation and bringing river width into play afterwards. In my opinion, this applies to both the concept suggested by Howard (1994) and adopted by Perron et al. (2008) as well as Pelletier's approach.

My interpretation of Pelletier's idea was slightly different from your explanation. I thought of taking the entire flux from both hillslopes into the river and distributing it not over a width δx, but over the river width w. However, the representation in the equation is the same in both cases, and I guess that you know better than me what the original idea was. The scaling is opposite to the other concept as it rescales the divergence of the hillslope flux at the river by $\frac{\delta x}{w}$ instead of rescaling the fluvial erosion rate by $\frac{w}{\delta x}$. This was the reason for my sloppy argument about the "problem obviously coming from the fluvial incision term."

The main problem, however, is that Pelletier's concept is not independent of the spatial resolution at least according to my preliminary findings. The results of a simple numerical experiment are attached as figures. It describes a river segment of unit length and width (one slope of length $\frac{1}{2}$ at each side of the river). All parameters are set to unity (including the uplift rate and the catchment size in the stream power law) except for a diffusivity of 0.1. Figs. 1–3 show the topography (with the scaling approach suggested in my manuscript) at different times, starting from a flat topography. Figs. 4–6 show the river profile for $\delta x = 0.01$ and $\delta x = 0.1$ at different times with both scaling concepts. The profile should approach a straight line with a slope of 1 for $t \to \infty$. While the profile becomes independent of δx for large t for both scaling concepts (although too steep in Pelletier's approach), the time scale of adjustment strongly depends on δx for Pelletier's scaling concept. This means that the time scale of response to changes in uplift etc. depends on the spatial resolution in Pelletier's scaling concept, so that I am not convinced that is solves the scaling issue. However, I may be wrong, and if you ever tested the scaling properties of Pelletier's approach and obtained different results, I would be happy to know.

Now about the specific points addressed in your review.

Grid-resolution dependence in coupled colluvial-fluvial models can be seen most readily as a dependence of drainage density on pixel size.

I do not fully agree to this statement. If we assume that rivers start at points with a given minimum catchment size A_c (in m^2, not in DEM pixels) and a well-organized dendritic network (not parallel flow on slopes), the dependence of drainage density on DEM resolution is rather weak.

If I understand correctly, Hergarten is proposing to use this variation/error in drainage density to scale the fluvial erosion term.

The dominator is indeed something like drainage density except for two differences: (i) Area is not total area as it is in drainage density, but only the part of the area not draining to leaves of the river network. This is the part that makes my analysis a bit complicated at first sight. (ii) Total river length is area of the DEM that covers the network divided by mesh width. For a square grid, this means that diagonal river segments have the same length as those in direction of the axes.

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.

As mentioned above, I would immediately buy this argument if the widely used model was derived from physical principles. Then the rivers would not know about properties such as drainage density. However, it comes from empirical data of "typical" rivers eroding "typical" landscapes. My conjecture is that the expression for the fluvial erosion law, in particular the value of the erodibility, refers to an equilibrium of erosion and uplift in the catchment and does not describe the river as an isolated object. If this conjecture holds, scaling must be like the one described in my manuscript.

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 am afraid that you did not miss it. I thought it would be clear from the analyzed network properties alone, but accept that it is not. So I can include a more serious version of the simulated river segment and the results of a series of larger simulations that I have just started. In order to make it a bit more interesting, these simulations use irregular triangular grids with 10^5 to 10^7 nodes and a threshold model for the hillslopes. I think they will be ready in a few weeks.

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?

It is much easier than you think, and the practical relevance of the value of A_c is limited in most applications. The result of my approach is that the erodibility K as it is usually considered is not the parameter that we need, but K multiplied by a length constant (which is not the river width) instead. I suggest $\sqrt{2A_c}$ as a simple estimate of this length scale. If we use a given erodibility K, we expect a certain channel steepness in equilibrium with a given uplift rate. The only prediction of my concept is that we can define any value A_c and let fluvial erosion act only at catchment sizes $A > A_c$, we will arrive at the correct channel steepness. In many applications there will be hillslope processes affecting scales larger than A_c. If these are strong, fluvial erosion will lose relevance even for for $A > A_c$, and the value of A_c also becomes less relevant. If it is much smaller than the scale of the hillslope process, it even only defines the reference topography that would occur if the considered hillslope process was switched off.

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.

Not really – it is all only about bringing empirically determined values of K into the model. We are free to assume any model for fluvial erosion at small scales such as a spatially variable threshold or a continuous decrease of erosion rates at decreasing catchment sizes. We just have to keep in mind that the value of K is the one that

we would measure from equilibrium river profiles if we assume that fluvial erosion is switched on for $A > A_c$. In this approach, differentiating hillslopes from channels on a given topography would only be useful if we want to use a specific value of K measured in a given catchment. If we knew the spatial distribution of erosion in this catchment, we could use it for assigning a "realistic" value of A_c to this value of K. However, this is hopeless in most cases, so that we have to accept the problem that measured values of K many unresolved dependencies (including something like A_c) as you already mentioned.

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.

Finally, at least one point where I agree without any reservations.

Best regards,
Stefan Hergarten

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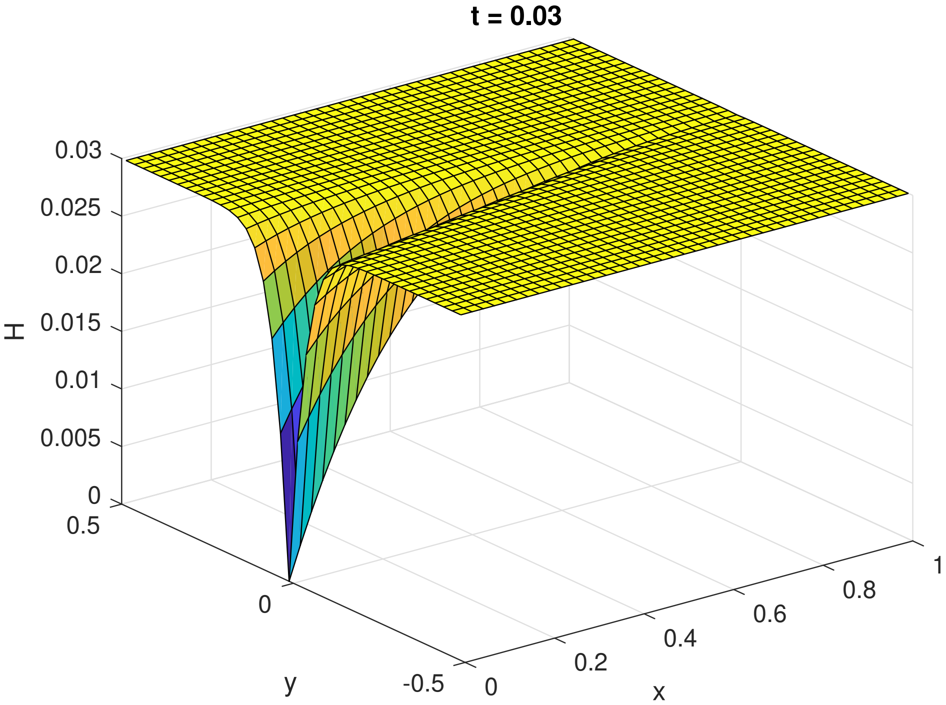


Fig. 1.

C7

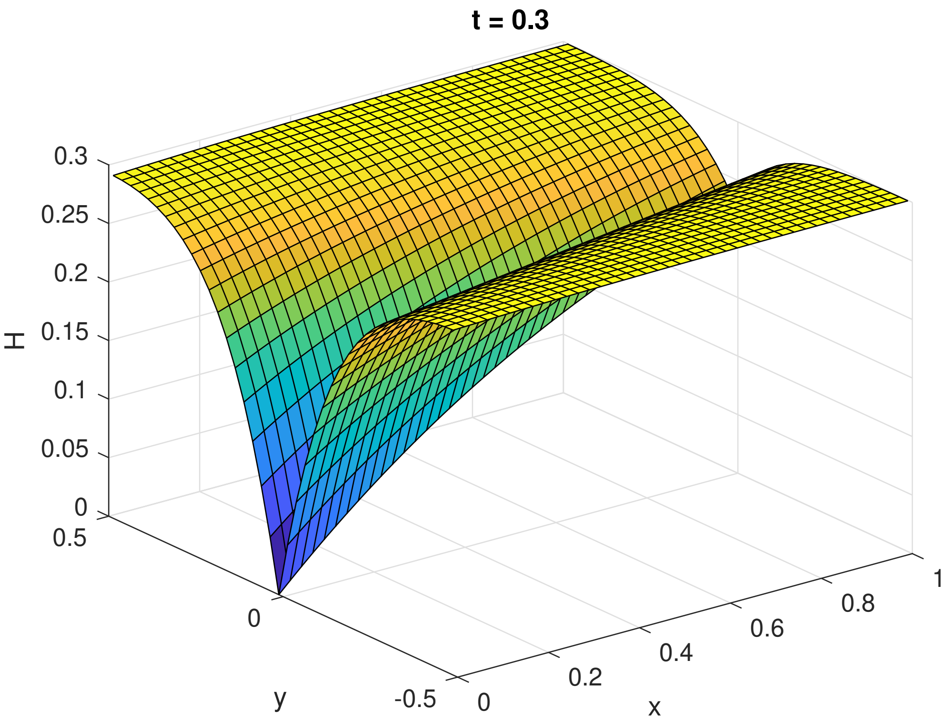


Fig. 2.

C8

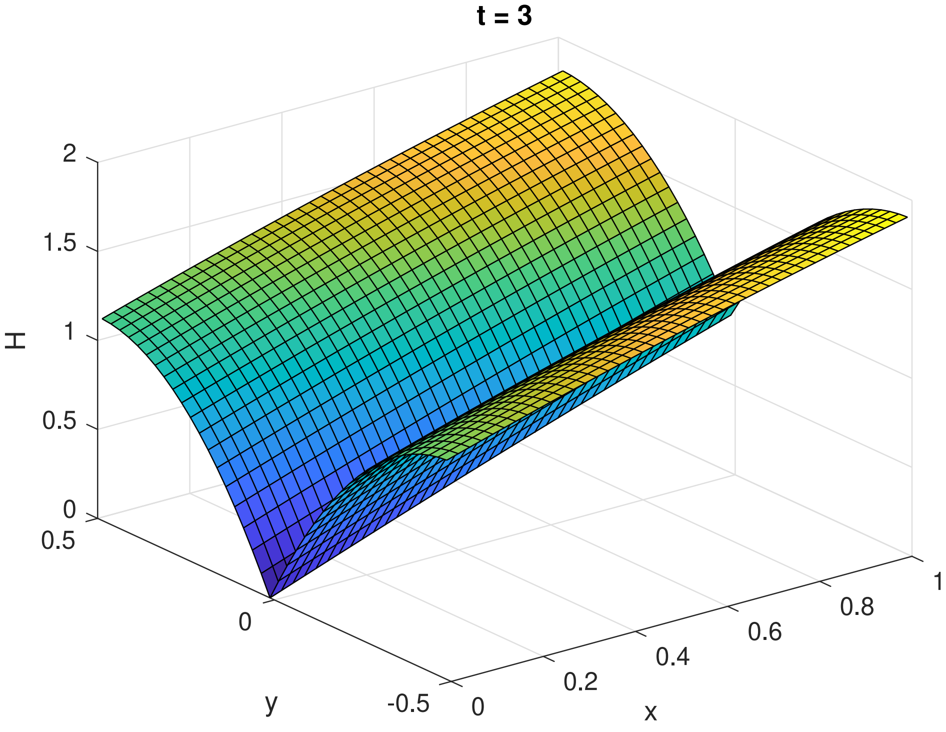


Fig. 3.

C9

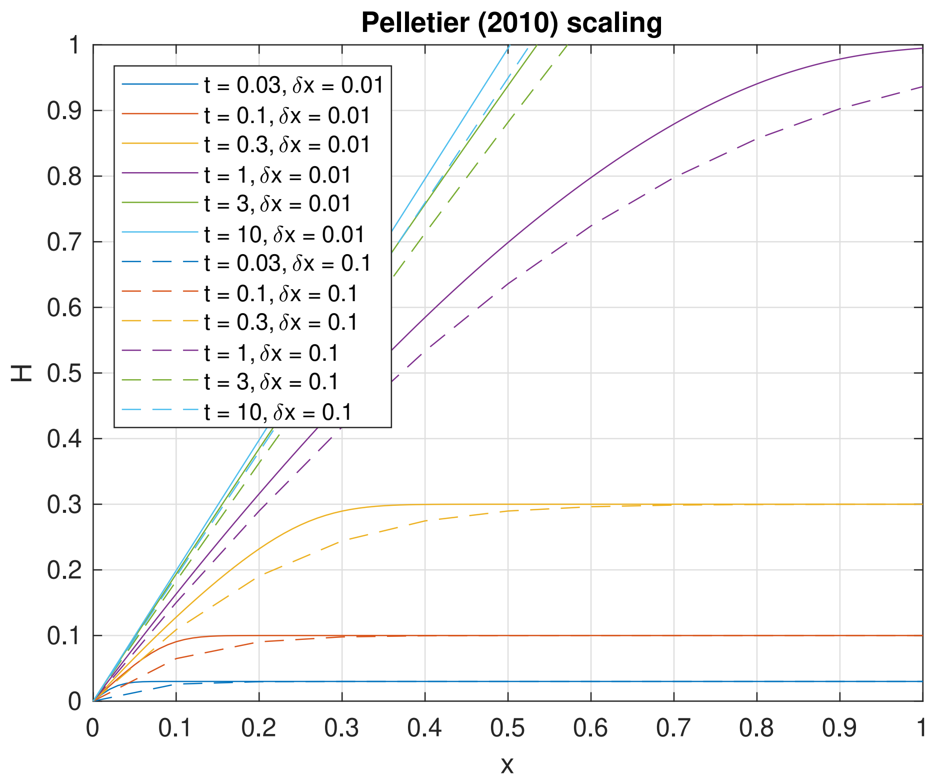
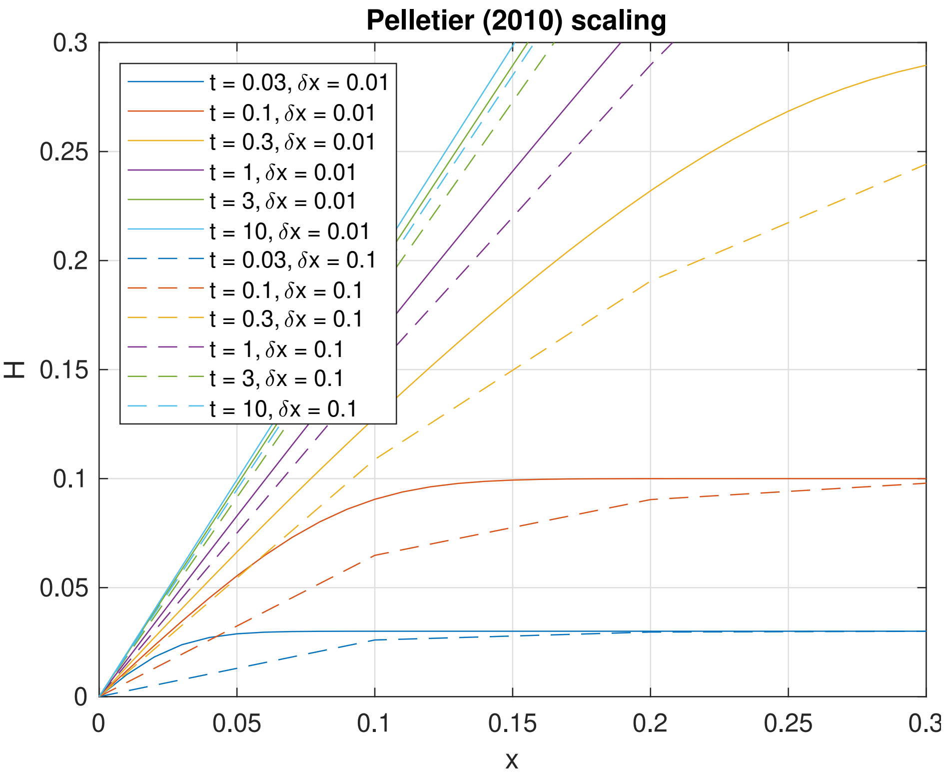


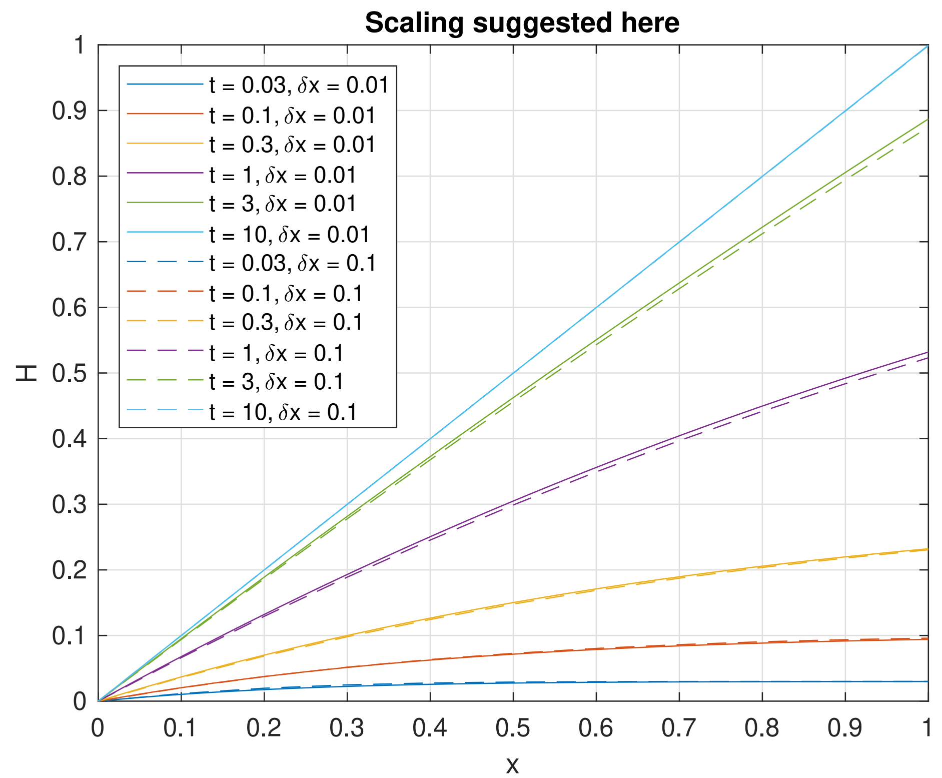
Fig. 4.

C10



Pelletier (2010) scaling
t = 0.03, δx = 0.01
t = 0.1, δx = 0.01
t = 0.3, δx = 0.01
t = 1, δx = 0.01
t = 3, δx = 0.01
t = 10, δx = 0.01
t = 0.03, δx = 0.1
t = 0.1, δx = 0.1
t = 0.3, δx = 0.1
t = 1, δx = 0.1
t = 3, δx = 0.1
t = 10, δx = 0.1
H
x

Fig. 5.



Scaling suggested here
t = 0.03, δx = 0.01
t = 0.1, δx = 0.01
t = 0.3, δx = 0.01
t = 1, δx = 0.01
t = 3, δx = 0.01
t = 10, δx = 0.01
t = 0.03, δx = 0.1
t = 0.1, δx = 0.1
t = 0.3, δx = 0.1
t = 1, δx = 0.1
t = 3, δx = 0.1
t = 10, δx = 0.1
H
x

Fig. 6.