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Interactive comment on "Accurate simulation of transient landscape evolution by eliminating numerical diffusion: the TTLEM 1.0 model" *by* Benjamin Campforts et al.

Anonymous Referee #2

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Campforts et al. addresses an important problem for fluvial landscape evolution models: numerical diffusion of the solution to the stream power advection equation. The authors first of all present a solution to the problem based on a higher-order flux-limiting method (TVD-TVM), and secondly, they outline a new modeling platform (TTLEM), which makes use of TVD-TVM and is available to everyone as part of the TopoToolbox 2.0.

Overall, my opinion is that numerical accuracy of fluvial landscape evolution models has received too little attention in the past, and it is therefore good to see the authors address it here. The method proposed to reduce numerical diffusion is convincing, and the damping of numerical diffusion in stream-power advection as well as in track-

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Discussion paper

ing horizontal tectonic displacements is significant. I hope that this contribution gets published in Esurf, although I do have some concerns and suggestions, which I list below:

General comments:

First of all, I think the duel purpose of the manuscript: 1) discussing numerical diffusion and presenting TVD-TVM, and 2) presenting TTLEM as a more general landscape evolution model leads to a rather diffuse and ackward structure of the text. The main strength of this text is in my opinion the focus on numerical diffusion and the presentation of TVD-TVM, but the TTLEM presentation calls for many details that are not needed to address this issue (see for example Fig. 1). For example, because the introduction focuses mostly on the influence of numerical diffusion, it is hard to understand the motivation for the first couple of experiments focusing on drainage networks and the influence of different hillslope models. I would strongly recommend simplifying the flow of the manuscript focusing more exclusively on the issue of numerical diffusion. Likewise the authors should consider skipping the first two experiments and in stead perform more like the one shown in Fig. 7. I think that it would increase the impact of the contribution, and the presentation of TTLEM could perhaps be saved for another manuscript in a more software-oriented journal.

Secondly, I suggest the authors give a short introduction to basic knowledge about numerical diffusion in advection problems. This could be inspired by simple textbook material and use linear advection as a starting point. By this the authors could avoid some awkward reflections, like in line 378: it is not at all counterintuitive that time steps smaller than the CFL criterion leads to more numerical diffusion. Most numerical analysis textbooks I know of give very simple explanations for why numerical diffusion is minimized exactly at the CFL criterion. Overall, I think the authors can make better use of basic textbook wisdom to prepare the reader for the main points of the manuscript.

Finally, while I fully appreciate the comparison experiments between the different numerical methods, I suspect that it is not completely fair. The main advantage of the implicit method (as FastScape by Braun and Willett) is that it becomes more compute efficient at high spatial resolution than the explicit methods, simply because it is not similarly constrained by the CFL condition. Thus, if explicit and implicit methods were compared in experiments with similar compute time (which I think they should be), would the implicit method not allow for finer spatial resolution than the explicit method? If so, would the finer spatial resolution in combination with the larger time steps not reduce the numerical diffusion of the implicit method? I am not questioning the advantages of TVD-TVM here. I just feel that the authors are not appreciating the real strength of the implicit method, which is how the compute time scales with spatial resolution.

More specific comments:

Line 30: “availability of potential energy”

Line 85: delete “most”

Eqn 1: Why are vx and vy bold?

Eqn 2: Are wk and wa used for anything here? If not flush them out.

Line 102: what is “eroding settings”?

Eqn 3: The diverge operator should include a dot between nabla and qs

Line 103: hillslope erosivity and erodibility. What is the difference?

Eqn 7: Again, is the variability on m really needed to demonstrate the points of numerical diffusion? If not skip it to clean the text. More complicated means less convincing.

Eqn 8: I do not understand the effect of densities here. Is U not simply uplift of the surface? If so, I guess the densities should be on the second term, right?

Line 153: “. . .transforms returns. . .”

Eqns 11-17: The use of subscripts seems inconsistent.

Line 192: ".. is similar than the one..."

Eqn: 19: I guess A varies by several orders of magnitude in the grid. Please discuss the CFL criterion in the light of this. Is max(A) used here?

Line 199: Description of the inner time step is confusing, and I do not understand why it is needed. Again I suspect that it is the general presentation of TTLEM that stands in the way for a clear and concise presentation of the numerical experiments.

Lines 206-205: This kind of randomness should be avoided here. The authors are documenting the level of numerical diffusion in different numerical techniques, and in this process it is very important that we know what advection equation is solved. m seems to be varied in order to make the drainage networks look more realistic. But that is not important here. And by the way: varying m randomly does not remove the grid dependency (which is inherent to stream-power advection and D8 drainage), it just obscures the close links between the grid, the (random) variability of m, and the drainage network. Please keep m fixed and the equations as simple as possible!

Section 3.4 is not well written. In spite of carefully reading the text I am still confused about how hillslope processes are implemented. But more importantly: Can the experiments documenting numerical diffusion not be run without hillslope processes? This would require that Ac=0 in Eqn 8, but why not? It seems a bit silly to deliberately add physical diffusion to an experiment were one wants to measure numerical diffusion? The authors should consider if the experiments can be made simpler (see first general comment above). Skipping hillslope processes and deleting this section could be a quick fix.

Section 4: I recommend skipping the first two experiments on hillslope processes and drainage networks (or save them for another paper). This would free up space to dig deeper into advection and numerical diffusion.

Line 276: I am not impressed by this strategy. I agree that the artificial symmetry is a problem, but at least we know where it comes from. Fixing this by introducing variability in the exponent m obscures the link between model input and model output, which is otherwise critical for use of computational experiments. Variability on K is better, because the linear scaling does not alter the form of the equation.

Line 344: So, what happens if the grid resolution is lowered to 10 m?

Line 391: overcomes -> reduces

Line 403: A small time step is not the essential factor here. The implicit method first of all offers a fine spatial resolution in combination with a large time step. The advantage of this combination should be explored more.

Line 464-474: All of this seems rather irrelevant to the main points of this study. see first general comment.

Line 481: "... the current debate..." calls for references.

Fig. 1: I almost get dizzy by looking at this. What is the point of showing this level of complexity in the first figure?

Fig. 2: While this is interesting I do not understand the motivation. The introduction spins me up to read about numerical diffusion, not this.

Fig. 3: Same comments as for Fig. 2.

Fig. 4: This is a nice, simple figure and to me the extension of this existing result to 2D simulations is the essential contribution of this study. This figure could be a great opening figure.

Fig. 7: If the authors choose to follow my advise and skip the first experiments, then more like this could be performed. It would be useful to see experiments with different setting of m and n (linear vs. nonlinear). Also to have experiments at finer spatial resolution where the advantages of the implicit method should start to kick in.

Fig. 9: It is good to see the difference between methods here, but it would also be great to see pictures of the two separate erosion rates. I wonder if knickpoints can be recognized in both?

Fig. 10: great figure

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