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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 #1**

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This paper is an extension of a recent contribution by Campforts and Govers (2015) that demonstrated the efficacy of using a higher-order flux limiting total volume method (TVD-FVM) for modeling the advective (i.e., stream power law) component of a coupled hillslope-fluvial landscape evolution model. The authors have extended the TVD-FVM method to 2D and they are making the new LEM available to the community as TTLEM.

The main point of this paper is absolutely correct: that upwind differencing with no correction introduces significant numerical diffusion into LEMs. The conclusion that upwind differencing without correction is unacceptably diffusive can be found in every numerical modeling textbook of the last few decades. I don't point this out to minimize the important contribution that the authors have made. Rather, I agree with them



that upwind differencing is overly utlized in the LEM community, often without scrutiny. About this there should be no debate. It should be noted that the numerical diffusion introduced by upwind differencing can be computed and may, in some cases, be mitigated by reducing the diffusivity coefficient D by the same amount introduced by upwind differencing, but this work-around is not commonly performed and is only possible if the prescribed value of D is sufficiently large. I applaud the authors for highlighting the problem of numerical diffusion (first in Campforts and Govers (2015), and again here) and for proposing a robust solution to the problem.

That said, I think the tests employed by the authors do not always allow for a clear assessment of the advantages of TVD-FVM. The authors make comparisons between a first-order upwind method and a higher-order TVD method for computing fluxes. However, unless I have misundertood something, the time steps used are variable within the models, making it difficult to clearly compare the errors associated with temporal discretization and clearly separate them from errors associated with spatial discretization.

Before I discuss this issue further, I think it is important to note that LEMs, like solutions to any other PDE or set of PDEs, should converge as the pixel size goes to zero, or at least be relatively insensitive to the grid resolution over the range of resolutions to which the model is applied. Without this, there is no unique solution for a given set of parameter values, making it impossible to know, in the absence of an analytic solution, if one has achieved the correct solution or to objectively compare results obtained with different schemes (the focus of this paper). Moreover, if a LEM is grid-resolution dependent then the same numerical model operating at different resolutions has to be separately calibrated to data, rendering parameter values such as D and K that should be solely functions of natural processes and material properties also functions of grid resolution. Pelletier, Geomorphology, (2010) has provided some guidance on how to make coupled hillslope-fluvial LEMs grid-resolution independent. His approach involves reframing the stream power as unit stream power (following all sediment transport formulae ever proposed, which is not a trivial rescaling since the contributing area generally scales with the pixel size on planar hillslopes but is relatively independent of the pixel size in convergent portions of the landscape) and modifying the strength of the diffusion term to account for the fact that changes in cross-sectional slope at valley bottoms occur over a distance equal to the valley bottom width (a property of nature), not the pixel size (not a property of nature). The random component of the model used by Campforts et al. poses a special challenge to achieving grid-resolution independence. However, one can maintain grid-resolution independence in a model with spatial random variability by generating random field(s) sampled at a resolution that represents the largest resolution the model will be applied to, then bilinearly interpolating these fields for use in versions of the model run at higher resolution. I am not suggesting that the authors adopt all (or any) of these suggestions, but I do suggest that this issue needs to be addressed in some way. The error calculation (equation (22)) simply assumes that the solution with TVD-FVM is exactly correct and any difference from this solution is an error. Without establishing grid-resolution independence it is really impossible to tell whether outputs such as Figures 8A and 8B are even unique solutions for a given set of parameter values, much less which one is more accurate.

The different methods are only evaluated for a small number of cases (two grid resolutions and cases with and without a maximum time step). Error in a first order method will decrease linearly as you decrease dx and it will decrease with (dx)ˆ2 for a second order method. In moving from a grid with dx=500m to dx=100m, there is a large difference in the computed values of E depending on whether or not a first-order or higher-order method is used. This is expected, but this doesn't indicate a fundamental problem with any of the numerical methods. The error associated with each of the methods is dependent on the grid resolution. So, it is a given that there will be some range of grid resolutions where the differences between a 1st order and 2nd order method appear unacceptable (i.e. numerical diffusion is excessive relative to the prescribed diffusivity). However, what really matters in judging method accuracy is the computational time required to reach a given level of accuracy relative to an ex-



act/converged solution. What would be most helpful is to demonstrate that TVD-FVM saves considerable computational time by providing an acceptable solution at a much higher grid resolution and/or is robust for a much wider range of grid resolutions than first order methods. I suggest the following: First, for one method, perform the simulation for a range of grid resolutions (400m, 200m, 100m, 50m, 25m,12.5m) until the solution converges, i.e. becomes essentially grid-resolution independent. Use a time step that is small enough so that the solution does not depend on the time step (this probably means using a time step that yields a very low Courant number for the coarser grids, but the magnitude of the time step is likely to be similar to the magnitude of the time step needed to keep the model stable on finer grids). Then, it is easy to argue that most of the error introduced into the solution is associated with the spatial component of the problem. Second, repeat step 1 for each of the numerical methods. Assuming all simulations are run on the same machine, keep track of the time required to perform the simulations. This would allow for a more robust comparison of the different methods and would give readers a better idea of the true differences between the methods. For instance, the TVD method should converge to an grid-resolution-independent solution more quickly than the lower order methods. But how much faster? How does this depend on uplift rate or other commonly varied parameters? What are the practical implications in terms of computing time? This would give readers more guidance on the necessity of using one method over the other.

In the discussion the authors imply that their method is really the only acceptable method for the stream-power component of LEMs. Techniques that are widely used to prevent artificial numerical diffusion in many fields of science, including MPDATA and semi-Lagrangian techniques, are implied to be inferior or less robust with no evidence. For example, semi-Lagrangian methods are deemed to be potentially of higher accuracy, but then simply dismissed as inferior to TVD-FVM because "simulation of horizontal topographic shortening would require large amounts of incremental markers to prevent numerical diffusion when interpolating the solution." This sentence confuses two different methods (semi-Lagrangian and particle-in-cell methods are not the same) and is not based on any evidence. I don't see any point in discouraging the community from trying alternative methods until they are clearly tested and shown to be inferior for a wide range of potential applications

Minor issues:

1) The variable x is used for two different things (in eqn. (1) it represents one of the cardinal horizontal directions but in eqn. (2) is represents the along-channel distance).

2) There is some repetition and inconsistency in the equations. For example, there are 6 different equations for one variable (dz/dt). It would be better to use a notation that differentiates among different aspects of dz/dt (tectonic advection versus diffusive erosion/aggradation versus stream-power-driven erosion) and make it clear that dz/dt is the sum of these different components. As written, equations (1) and (6) and (9) are repetitive and incompatible, because they are almost the same equation, yet the left hand side of all of the equations is the same while the right hand side includes uplift in one of the equations but not in the other two.

3) It would be helpful for the authors to address whether the method could be applied to the nonlinear stream power law (n not equal to 1), spatially variable K (e.g., strong over weak layers in sedimentary or metamorphic rocks), transport-limited fluvial processes, landscapes with a finite soil layer over bedrock or intact regolith, and other common LEM variants.

4) The paper is comprehensively referenced, which I appreciate, but some of the references do not support the points being made. To take one example, McGuire and Pelletier (2016) is used to defend the use of a detachment-limited model on the basis that unconsolidated sediment can be easily evacuated from the fluvial network. This is simply untrue. Unconsolidated sediments obviously do get stored in fluvial systems. Whether a detachment-limited model is a reasonable approximation depends on the application (including details such as mean grain size), and I don't think a paper that deals with small channels forming on alluvial terraces is an appropriate basis for



defending the use of a detachment limited model in an LEM designed to model the large-scale evolution of mountain belts.

5) The structure of the paper is good but the sections/subsections could be slightly improved. For example, the issue of artificial symmetry that can arise with rectangular grids is first introduced on line 206 with no prior mention or subsection break. I think this issue should be addressed in its own subsection of section 3 (as it is in section 4.2).

6) The stream power model is introduced using its nonlinear form (the exponent n is general) but the remainder of the paper, including the CFL ondition (eqn. (19)), applies only to the linear case.

7) The use of D8 routing seems unsubstantiated. Dinfinity is the choice of nearly every modern LEM, because it more faithfully represents flow on hillslopes.

8) Please use lat/lon or UTM coordinates in Fig. 2. If these are UTM coordinates, please specify.

9) The method of the paper is referred to as TVD-TVM throughout the abstract but TVD-FVM in the paper. If this is not a typo, please explain the difference between these abbreviations.

10) w_A and w_k are introduced in the equation but then (unless I missed it) never discussed again (not even in the table of parameter values).

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