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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
Received and published: 6 August 2016

We thank referee 1 for her/his comments, which helped improving the quality of the manuscript. Our responses the comments are in blue.

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 utilized 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.

We are grateful for the appreciation of the reviewer regarding our work.

1. 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 misunderstood 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.

   It is indeed true that time steps vary between the TVD-FVM and the implicit method on the one hand and the implicit method without a control on the time step on the other. The latter was done on purpose to illustrate how the main advantage of an implicit scheme, i.e. being stable at time steps exceeding the CFL criterion, is counterbalanced by numerical smearing once the CFL criterion is exceeded. If we only compared simulations where the time step obeys the CFL criterion, it would make no sense to use the implicit scheme as the explicit FDM would be as fast or faster (due to the possibility of vectorization).

2. 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).

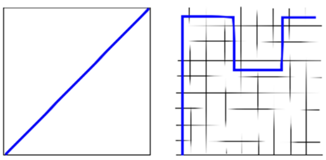
We completely follow the argumentation that numerical models should converge at small resolutions. We applied an analytical solution, which per definition gives the 'the true solution' to illustrate that the different numerical methods applied in our paper indeed converge at small resolutions. Our approach to prove this is further clarified in detail under point 4.

3. 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 trans- port 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.

   Again, we agree with the reviewer that there is a need for a grid resolution independent solution in order to verify and compare the robustness of the different numerical schemes applied in TTLEM. We also appreciate the elegant suggestion to obtain grid independency as proposed in Pelletier 2010 and have modified the discussion of the manuscript to highlight the influence of grid resolutions. The implementation of the proposed methodology to make a numerical model grid resolution independent is however beyond the scope of our paper where we mainly want to illustrate the importance of numerical diffusion when using most frequently applied first order FDM to solve the SPL. The second message we want to bring with this paper is the suitability of a 2D variant of the TVD-FVM to simulate tectonic shortening. Although grid resolution dependency could most surely be investigated in a future release of TTLEM, we follow referee 2 in trying to present our main messages as clear as possible without drawing too much attention to the technicalities of the numerical model. For similar reasons, we decided to remove the part on grid symmetry from the manuscript and no longer discuss the different hillslope diffusion schemes implemented in TTLEM.

4. 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

exact/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.
We consider this remark as very essential and would like to thank the reviewer for his suggestion on developing a grid independent 'true' solution for the SPL and TTLEM in general. We decided that such an approach is indeed most essential and would offer the reader much more guidance in the performance of the algorithms and provides a robust method to compare the different numerical schemes. Moreover, also reviewer 2 requested a robust framework to illustrate the performance of the numerical schemes. However, carrying out the analysis as suggested by the reviewer introduced some complexities and uncertainties which are summarized below. Therefore, we performed an alternative test, also covering a wide range of resolutions and we compared our numerical solution with an analytical one so that resolution effects could be analyzed.
Complications which arise when performing the analysis as outlined above mainly come down to the fact that comparing model runs with similar parameter values at different resolutions is a very tricky business. First, interpolation from the 'starting initial image' to the other resolutions (e.g. from 10 m to 400 m) will change the initial location of the drainage network to a certain extent, depending on the interpolation method used. Hence, catchments and rivers might shift in location which complicates comparison between results. Second, and this one seemed to be very important while doing the exercise, changing the resolution from e.g. 400 to 10 m results in much more possible river paths. This is illustrated in the figure below where it is shown that river distance in higher resolution images might be much longer and can take many different shapes compared to the main resolution (where river length is 400 m or 400 m × sqrt(2)).



For these reasons, when comparing models, executed at different resolutions, one is rather evaluating the effect of raster resolution and the way it is reflected in topography than comparing the performance of numerical schemes. Although the latter is of utmost importance and has been elegantly illustrated in literature (Pelletier, 2010), this is not what is required to evaluate the performance of a numerical scheme.
In order to overcome these problems, we developed the following strategy to evaluate both the computational performance and accuracy of the numerical methods:

- We only consider river cells to quantify the performance of the different numerical schemes. These rivers cells set the base level for the hillslope cells and the way these hillslope cells respond to differences in numerical schemes is illustrated by the erosion rates calculated over several catchments and illustrated in the current figure 7 of the manuscript. We agree however, that our previous approach to document the difference between the TVD scheme and the implicit schemes using a RMSE is misleading. We will therefore no longer refer to the term RMSE to document the difference between two numerical schemes but simply report the difference between the schemes as an offset. E.g. the $O_{TVD\text{-}imp}$ represents the offset between the TVD-FVM and the implicit FDM.

- To document real RMSE values as a consequence of numerical diffusion we performed the following analysis:
    1. We initiate the analysis from the standard DEM, also used to calculate differences in erosion rates plotted in the current figure 7-9.
    2. All river heads with a contributing drainage area exceeding a threshold value are selected (in our case 10^6 m^2)
    3. The drainage network connecting these river heads with the outlet of the catchment is calculated. Very short river profiles <10km are not retained in the analysis to improve computational performance.
    4. For this initial drainage network the initial river elevations are extracted from the standard DEM.
       Steps 1-4 are illustrated in Figure 1.
    5. Next, landscape evolution is simulated for the three numerical models using the same model parameter values and uplift rates (current Fig. 6) as those reported in the paper in order to calculate erosion rates.
    6. At the end of the model runs, river elevations are extracted from the numerically simulated DEMs and compared with the analytical solution described below.
    7. Given that we consider the linear case where n=1 and keep the river network fixed for this analysis, there exists an analytical solution which is calculated with the slope patch method outlined by Royden and Perron (2013). This method will be further detailed in the revised version of the manuscript.
    8. The advantage of this analytical solution is that it is truly grid size independent and is giving the correct solution for elevations along the river profiles.
    9. To illustrate steps 5-8, we plotted the resulting numerical and analytical solutions for 4 selected resolutions in Figure 2.
    10. The previous steps are repeated for a range of resolutions going from 950 m to 6.25 m. For each model run, the CPU time required to perform the analysis is stored.
    11. Given that we have an analytical solution for all the cells of the drainage network, the numerical accuracy of the methods can be evaluated by calculating the RMSE between the three numerical methods and this analytical solution. The result of this exercise is plotted in Figure 3 which is in fact reporting the data required by the reviewer.
        We will discuss these findings in detail in the revised manuscript but note that from this analysis, one can see that it would take for example 12 times longer to obtain the accuracy of the river processes obtained with a TVD-FVM at 500 m (RMSE = 18.17, 2.89 sec) with an implicit method (cfl<1, at 150 m, 36 sec). Such an analysis of course only holds for the river cells as higher model resolutions will also improve model performance in terms of hillslope processes.
- Note that we developed an updated, vectorized, version of the TVD algorithm to perform this analysis which will be released soon on GitHub.

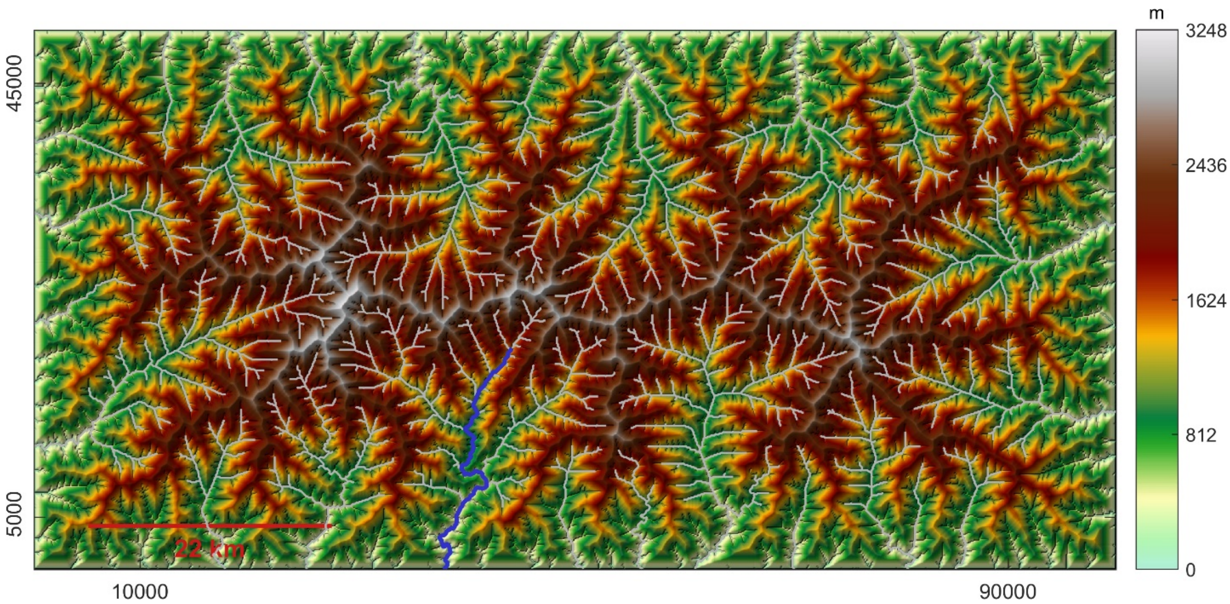


Figure 1: DEM of standard run used in the current version of the paper to calculate catchment wide erosion rates and here used as an initial DEM to run the performance analysis outlined in the comments of the reply. The grey lines indicate the drainage network for which the solution has been calculated analytically. The blue line indicates the river profile for which model results at different resolutions are plotted in figure 2.

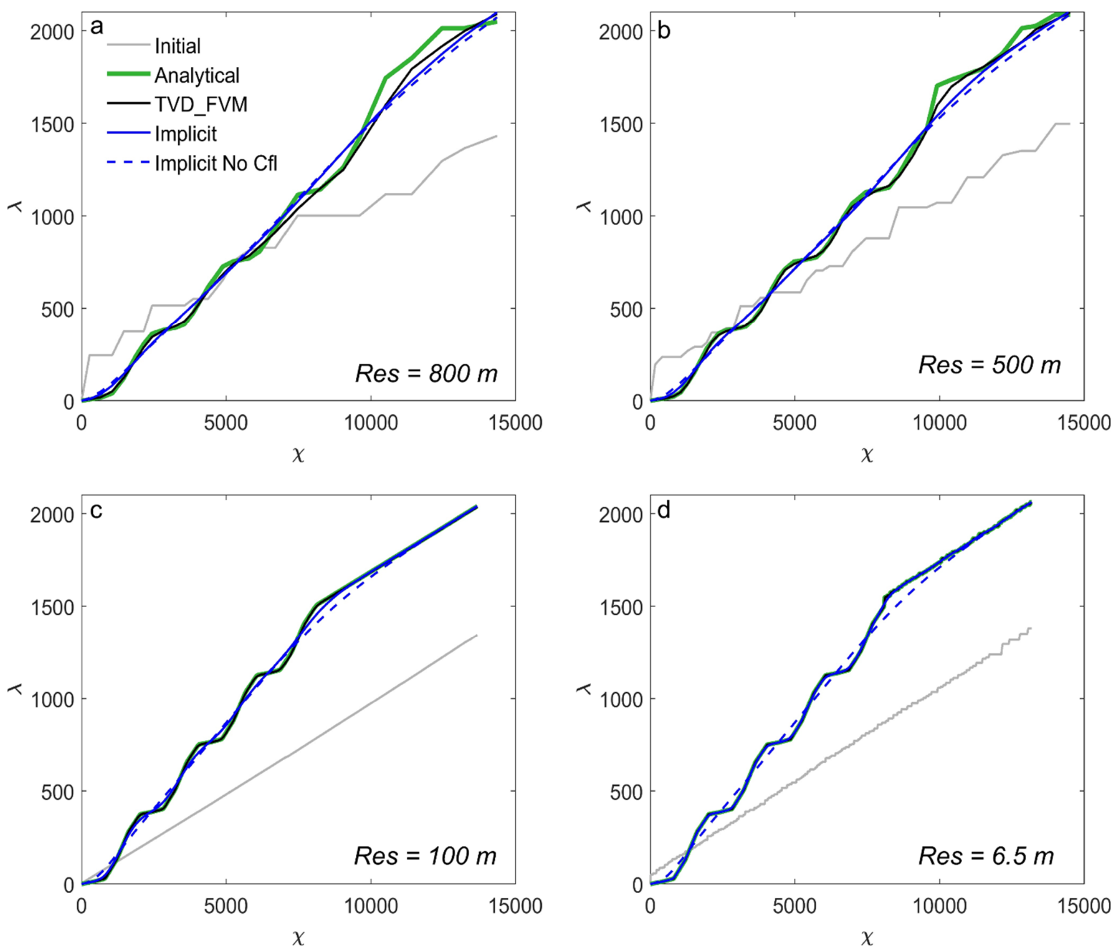


Figure 2: Comparison between different modelled resolutions for the river profile indicated in blue in figure 1. The green line is the 'true' analytical solution, obtained with the slope patch method of Royden and Perron (2013). The solid blue line presents the implicit solution when the CFL<1 and the dashed blue line represents the implicit solution when the time step is left free.

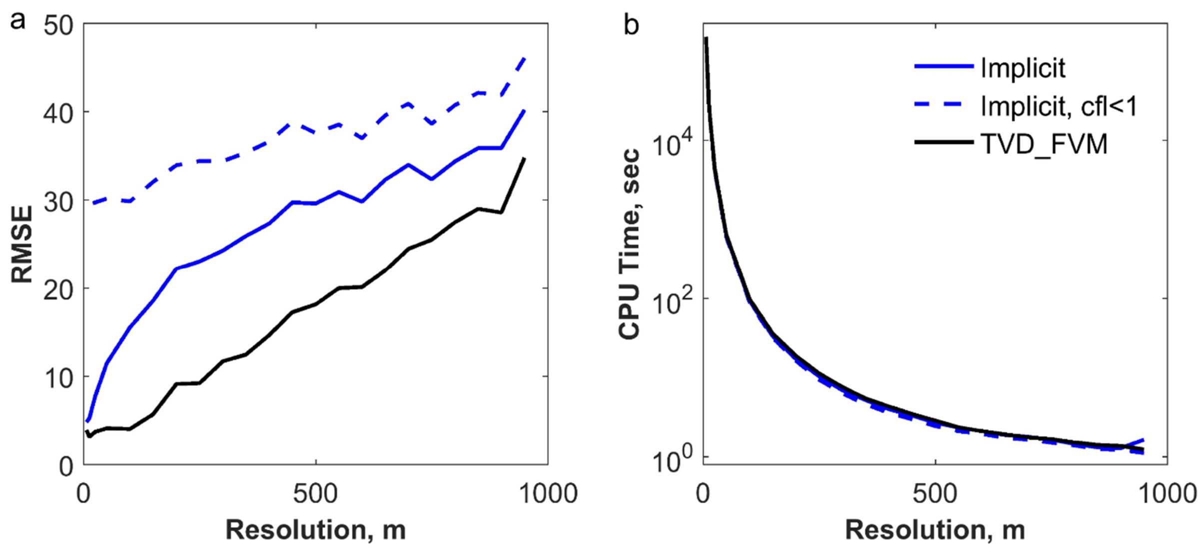


Figure 3: a. Performance of the different numerical schemes calculated with the RMSE between the analytical and numerical methods. b. CPU time required to perform the model runs at the indicated resolutions.

> 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.

We do accept that our considerations were worded somewhat too strongly. We have therefore adjusted this in the new version of the manuscript. That being said, and without the intention to discourage the community from testing other numerical methods, we are confident in stating that the TVD-FVM is a relatively easy to implement numerical solution which does minimize the amount of numerical smearing in the solution. I did implement an adapted version of the MPDATA scheme which ultimately leads to a similar performance compared to the TVD-DVM but only after applying the limiters as pointed out in the manuscript. That makes the scheme heavier and more complex compared to the TVD-FVM and so we concluded that in this particular case, there is no need for using an MPDATA scheme. Regarding the Lagrangian schemes, we agree with the referee that the current text was confusing and we have rewritten the paragraph as follows:

The numerical methods discussed so far are solved on an Eulerian grid. Eulerian grids represent immobile observations points, for which the solution of the variable, in our case topography, is calculated through time. Alternatively, Lagrangian points such as markers or particles are directly connected to the variable (topography) and evolve together with the variable over time (Gerya, 2010). An approach that has previously been shown to be successful in preventing numerical diffusion is the Marker In Cell method. Here, the solution of the system is simulated by interpolating independently propagating Lagrangian advection markers to fixed Eulerian grid points during each time step of the simulation (Harlow and Welch, 1965). In a 1D configuration, this method would produce very accurate results when applied to solve an advection equation such as the SPL. However, simulation of horizontal topographic shortening would require large amounts of incremental markers to prevent numerical diffusion when interpolating the solution to the Eulerian grid (Gerya, 2010).

Some of the weaknesses of the tested numerical solutions can be reduced by LEMs that rely on irregular grid geometries. Irregular grids do, for example, allow to simulate tectonic shortening using a fully Lagrangian *approach where grid nodes are advected with the tectonically imposed velocity field (e.g. Herman and Braun, 2006). …*

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).

We will fix this in the revised version of the manuscript.

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.

We agree that our notation is currently not fully consistent and follow the suggestion of the reviewer to use different notations for the different sub components of the solution (eg. Eq. 6 and 9)

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.

We thank the reviewer for these suggestions. For the moment the model supports (i) non-linear river incision (n~=1), variable K values, different precipitation input. Transport limited fluvial processes as well as a bedrock/regolith interface are currently not supported but are planned to incorporate in future versions of TTLEM.

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.

We agree with RC1. We will change the referencing and wording in this sentence.

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).

We will no longer discuss the issue of artificial symmetry in this paper as suggested by referee 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 condition (eqn. (19)), applies only to the linear case.

All the simulations could be easily performed for non-linear cases. However, we preferred linear examples when demonstrating the impact of numerical smearing on the results to enhance clarity in general. How non-linear slope dependency affects river incision is discussed in Campforts and Govers (2015) in due detail, including the way in which the CFL criterion should be adapted.

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.

Dinf (or D∞) is certainly the flow routing scheme of choice to represent flow on hillslopes. However, in TTLEM fluvial erosion is limited to the channelized domain of the landscape and thus the flow routing scheme on hillslopes of minor significance. Nevertheless, even in the channelized domain Dinf has advantages over D8 since it enables diverging flows on landforms such as alluvial fans and braidplains. The current implementation of TTLEM, however, focuses on the modelling of detachment-limited systems or bedrock rivers where divergent flows are usually confined by valley walls. This is also consistent with other models such as Fastscape (Braun and Willett, 2013) and DAC (Goren et al., 2014) models that use the D8 flow routing scheme. We thus disagree that Dinf is the choice of the majority of modern LEMs. Still, we like to stress that we do not exclude to implement Dinf or other multiple flow direction algorithms in a future version of TTLEM, in particular since the topological sorting algorithm (Braun and Willett, 2013; Heckmann et al., 2015) is equally suitable for the efficient computation of flows on thus derived networks.

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

We will fix this in the updated version of the manuscript.

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.

We will fix this in the updated version of the manuscript.

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).

These parameters are weighting parameters used to scale for changes in precipitation and lithology. We will clarify this.

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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
Received and published: 17 August 2016

We thank referee 2 for her/his comments, which helped us to improve the quality of the manuscript. Our replies are in blue. Throughout this reply, we will also refer to the answers formulated in the author comments on referee 1 (further referred to as RC1) where we also added some figures for clarification.

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.
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 tracking 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:
We are grateful for RC2's appreciation of our work. We also appreciate the constructive comments which will help us to enhance the overall quality and readability of the manuscript.

General comments:
First of all, I think the dual 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.
We follow the advice of the reviewer to focus the entire manuscript on the role of numerical diffusion in landscape evolution modelling. We will therefore remove the two first experiments (e.g. the role of hillslope diffusion and the presence of artificial symmetry) from the paper. Nonetheless, we consider this paper as the first description of the new TTLEM simulation software. Therefore, we will move the flow chart illustrating the different modules of the model to the appendix of the paper along with the picture illustrating the functionality of the different hillslope response schemes. We consider TTLEM as a tool for the community which can be used to reconstruct landscape evolution as well as to test hypotheses. The latter might require a combination of insights in the different existing modules as well as a guidance on how to add new modules. We feel that both objectives, require an overview of the software in its present shape.

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.

In the revised manuscript, we will introduce the readers into the issue of numerical diffusion when solving hyperbolic partial differential equations by adding a paragraph in the introduction. We will also rephrase the sentence in line 378 although we find it important to document these findings which are indeed well discussed in numerical textbooks but less well known/introduced in the earth surface community.

Finally, while I fully appreciate the comparison experiments between the different numerical methods, I suspect that it is not completely fair.

Part of this answer is addressed in the reply to RC1 where we illustrate how the analytical slope patch method (Royden and Perron, 2013) is used to evaluate the performance of the different numerical schemes.

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.

This is an interesting remark that we address in a revised version of the manuscript. We hope that the additional analysis outlined in our comment to RC1 will provide more insight into the trade-offs between numerical accuracy and computational efficiency. The answer to the referee’s question comes in multiple points.

- An essential characteristic of an implicit scheme like that of Braun and Willett is that it fails to allow for ‘vectorization’ which is in contrast to explicit methods (like TVD). By vectorization, we mean ways to exploit single-instruction multiple-data parallelism. Hence, the fact that TVD requires more operations per execution and requires a time step which obeys the CFL criterion may partly compensate for sequential looping through all stream network nodes required by the implicit scheme. From the analysis presented in our discussion of the comments of RC1, we show that both schemes end up running in almost the same time. We will address this point in the new version of the manuscript.
- It is important to note that rivers only occupy part of the landscape. Although TTLEM indeed allows to simulate all cells as rivers cells (as suggested in comment on line 206), we do not test this configuration as we consider it of little use in real world landscape evolution where hillslope processes may dominate where drainage area drops below a threshold value. Hence, while refining the resolution does indeed result in more accurately simulated river elevations, the computational overhead related to hillslopes processes which comes with refining the grid resolution is unacceptably large at the spatial scales and resolutions that we consider. Also notice that even at very high spatial resolutions (6.25 m), the TVD method is still more accurate compared to the implicit (cfl<1) method.
- We appreciate the remark of the reviewer that the higher spatial resolution, which is in principle allowed by the implicit method for similar timescales, is the real strength of the implicit method. This argument is exactly the reason why we simulated the landscape using both an implicit method which is free of any time criterion (and where dt is set by the main model time step, e.g. 2e4 yr) and one simulation where a CFL is applied to the implicit method. The latter was done on purpose to illustrate how the main advantage of an implicit scheme, i.e. being stable at time steps exceeding the CFL criterion, is counterbalanced by numerical smearing once the CFL criterion is exceeded. If we only compared simulations where the time step obeys the CFL criterion, it would make no sense to use the implicit scheme as the explicit FDM would be as fast or even faster (due to the possibility of vectorization). Furthermore, it is not only the inherent nature of an implicit scheme which is not

suited to properly simulate propagating knickpoints. If very large timescales are applied in landscape evolution models, uplift is inserted very suddenly at the beginning of the time step. This results in unrealistic simulations where uplift is a discrete stepwise function rather than a continuous function (e.g. the sine waves used in this paper). In Fig. 2 of this file, we have shown two extremes, i.e. a configuration where CFL<1 and one where CFL >>1. One could argue that intermediate solutions (e.g with CFL closer to 1) would result in more desirable results than the one shown with the dotted lines in Fig. 1-3 of RC1. This is true but, given that computational gains are marginal and numerical accuracy will never be higher than the implicit method simulated at CFL< 1 (solid blue lines), we see little reason to follow such an approach when simulating transient landscape evolution.

- To summarize, a first order implicit scheme is not suited to properly simulate propagating knickpoints in detachment limited erosional basins. First order implicit methods are therefore only suited to simulate configurations where transiency, caused by local base level falls, tectonic faults or lithological contacts can be considered to be minor.

More specific comments:
Line 30: “availability of potential energy”
Line 85: delete “most”
Eqn 1: Why are vx and vy bold?
Because they are representing velocity fields being variable in space.
Eqn 2: Are wk and wa used for anything here? If not flush them out.
They are used as weighing factors to introduce the impact of variable lithological strength an precipitation in the model. We will further clarify this in the updated manuscript.
Line 102: what is “eroding settings”?
Where the detachment limited assumption holds.
Eqn 3: The diverge operator should include a dot between nabla and qs
OK
Line 113: hillslope erosivity and erodibility. What is the difference?
Should be simple erodibility. Erosivity can be removed
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.
Point taken. Section will be removed in the updated manuscript.
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?
Good points, it depends on the way U is defined. We will clarify this in the updated manuscript.
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?
Fixed
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.
We will clarify this further in a revised version of the manuscript. An inner time step is needed because hillslope processes which are diffusive in nature allow the use of semi-implicit methods used to solve them. Here, the implicit nature of the schemes can be fully exploited and large time steps can be used to solve the equations (Perron, 2011). The TVD method which is explicit, on the other hand does not allow such big time steps and does require the main model time step to be split up in so called ‘inner time steps’.

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.

As outlined above, we agree with the reviewer that the experiments on hillslope diffusion and varying values for m are distracting for the main message of the paper. We will also further motivate our choice for the D8 algorithm in the updated manuscript (see also RC1). However, for reasons also discussed above, we did not remove the hillslope processes from our model to explicitly address how numerical diffusion in channel incision affects hillslope diffusion and ultimately basin wide erosion rates.

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.

Fixed, section removed from the manuscript

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.

Fixed, section removed from the manuscript

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

See RC 1

Line 391: overcomes -> reduces

See RC 1

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.

This issue is discussed in the reply to the major comments above.

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

We will consider moving part of the paragraph to the appendix in the revised version of the manuscript.

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

Fixed

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

We will skip this figure and add it to the appendix

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

We will skip this figure and add it to the appendix

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

We will skip this figure

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.

Point taken

Fig. 7: If the authors choose to follow my advice 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.

See discussion above and figures in RC1. We will remove the first three figures from the manuscript.

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. 9 illustrates the difference between erosion rates for the two numerical methods. In our opinion the addition of another figure showing the erosion rates for each method is not very meaningful as the differences in erosion patterns and rates would be less clear. With respect to the knickpoints it is important to consider that the use of a different numerical method does not change the average speed of knickpoint advection (see Campforts and Govers, 2015), but it does strongly affect the evolution of the gradient of the knickpoint: we will add this clarification in the revised version of the manuscript. Hence, it is not meaningful to compare maps of knickpoint locations.

Fig. 10: great figure

Thanks

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