Answers to the review of the article: Large structures simulation for landscape evolution models

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To all reviewers

We first thank the reviewers for their careful reading of the manuscript, their relevant comments and valuable suggestions. We now give here our separate answers to their new remarks.

1 Reviewer 1

. Thank you for your comments. To ease the reading, we have tried to reduce the amount of mathematical details, essentially by moving to the appendix some of them also by removing some unnecessary ones inside the appendix sections.

2 Reviewer 2

. The main objective of our paper was to explain how to obtain correct numerical results for LEMs, that are not dominated by amplified numerical errors. We might not solve the more general issues of LEMs that the reviewer has in mind, however we strongly believe that it is necessary to start by ensuring that numerical results are reliable for simple models before considering more advanced issues. In particular, we consider and illustrate in this paper that one of the major problems regularly encountered in LEMs is the anomalous mesh dependency and its implication in terms of non-physical results. In this context, we are confident on the added value of our approach that follows well-known principles used by the computational fluid dynamics (CFD) community, since unfortunately the issues of LEMs are very close to those of CFD for turbulent flows. As you mentioned, on observed landscapes channelization can be modeled as occurring "randomly", since very small scales heterogeneity can have a huge impact on the flow. Reproducing this phenomenon would require a detailed knowledge of very small scale details of the landscape (such as boulders, vegetation, etc..). Numerical models that do not incorporate such data or any randomly generated data should not artificially become random out of numerical error amplification. Of course, the approach presented in the paper is only a first step in this direction, this we only filter small scales without adding any "small scale model". This is what we propose as a perspective in our conclusion: to complement the model with sub grid scale modeling.

. The mathematical requirements you insist on (Eq. 8) are introduced as sufficient conditions, not necessary ones. The mathematical references we gave provide well-posedness results under such conditions that essentially consist in requiring the positivity of the zero order operator. Since it is was obviously not clear enough, we have detailed in the new version of the paper (section 1.1) situations for which the system is still likely to be well-posed despite not fulfilling one of those requirements (in particular saddle-points and valleys) and thus not having a comprehensive mathematical theory. We have also explained why if the

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topography has some flat or accumulation areas then the system will not be well-posed, in particular by providing an analytic example for the less obvious case of accumulation areas.

. We are quite surprised by the way you mentioned that we replace the grid size dependency by a smoothing parameter dependency. What we have tried to explain is that smoothing allows to get rid of the anomalous grid dependency due to numerical error self-amplification, and that it allows to recover the normal mesh dependency which is the convergence of the solution to the correct result when making the grid and filter size go to zero. This is the best kind of "mesh independency" achievable, since as long as we are discretizing on a mesh, this mesh will have an impact on the results. In particular, in any simulation the mesh will always control the size of the smallest details accurately reproduced in the results (that will ultimately be several cell-size large). When LEMs are designed without any filtering strategy, it corresponds to consider that the cut-off length scale is the mesh size. In other words, the scale you called as an "additional artificial length scale" is already implicitly considered, but it lacks calibration and leads to the recurring problems in LEMs. In our case, since we need a filter to ensure correctness of the results, the resolution of the model will indeed be controlled by the filter size. Here we show that the calibration of this length must simply respect an elementary principle: to be largely lower that the size of the geomorphic structures the LEM aim to reproduce. Based on this calibration, the mesh resolution must be chosen so that the filter is correctly discretized. Thus we have not deteriorated the situation: we still have a unique discretization parameter that governs the resolution of the model. We have added a paragraph at the end of section 3.3 to emphasize this point.

. Asserting that the Gauckler-Manning-Strickler equation (which is the continuous equivalent of corrected MFD algorithms, and thus roughly speaking corresponds using one of the MFD algorithms) is not able to simulate the water flow in valleys would challenge many previous studies. Fortunately, thanks to what has been said on the difference between sufficient and necessary conditions, we hope that it will be clearer that the limitation pointed out by the reviewer is no more relevant.

3 Editor

. We hope that we have given satisfactory answers to the remarks of reviewer 2 (see the above section and the new version of the paper).

. We appreciate that despite the abundance of mathematical formalism, you consider that the subject of this manuscript rightfully belongs to the scope of ESURF and have the potential to be of interest for the LEM community. We have tried to reduce the amount of mathematical details, essentially by moving to the appendix the computational steps of section 1.3 devoted to perturbation study and also by removing some unnecessary details inside the appendix sections.