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

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1 Reviewer 1

We first thank the reviewer for his very careful reading of the manuscript, his relevant comments and valuable suggestions. We have taken into account all the remarks concerning typos, notations, or rephrasing. We refer to the modified version of the article for those points. We now give more complete answers to your more involved comments.

. "*137-138. This presumes we know what "most classical cell-to-cell MFD algorithms*" you are talking about, and I am unsure." : we have added the corresponding references.

. "*173. You have just said that the specific catchment area (SCA) is an approximation of q_w, but now you say that the specific catchment area a is in fact equal to q_w? I am confused. Then later you are treating a as interchangeable with h_w? And q_w with Q_w ?*": Our previous notations were indeed quite confusing about discrete and continuous versions of the water discharge and various catchment areas. We believe that this was the origin of the small misunderstanding. In the new version, we use different notations for continuous quantities and their discrete counterparts. It should not be clear that the specific catchment area computed from MFD algorithms was a discrete amount, and thus an approximation of a continuous one. On the other hand, a and q_w are defined at the continuous level. The quantity a can be formally identified with $h_w\eta_w(h_w)$, just by looking at the expressions: this is how the GMS model can be viewed as a generalization of the model underlying a. Finally, we used $\mathcal{Q}_w$ as a generic "local discharge of water" to encompass all the possible choices of the literature, without choosing a specific one. At the end, we set $\mathcal{Q}_w = q_w$ to emphasize that we use for the remaining of the paper the consistent q_w. We have tried to make it clearer in the new version.

. "*209-225. This whole discussion of flat areas and the bowl is a little obtuse, and seems to me outside the main point of this paper. Can you just say that you will focus on well-posed problems, which are not completely flat or closed depressions, and move much of this to the supplement? (Edit after reading the response to previous reviewer: I am sorry to be asking you to downplay something added to address a past reviewer comment. Do as you please, but I do think that this section is a barrier for readers)*": we have postponed it to the beginning of the section explaining how to overcome the limitation of the GMS model, i.e. the very last section of the paper. We hope that proceeding this way will be enough to ease the reading.

. "*247-255. This is really hard to follow with the stand-in variables, and the coefficients with plusses and minuses, much of which goes unexplained. I am not even sure why we need to see equations 12 and 13*": we have removed the stand-in variables and eliminated the technical requirements to ease the reading.

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. "*324-328. This is a helpful description of the implications of this analysis. However, I do find myself wondering, if the continuous scheme has these self-amplifications, and the numerical scheme results in perturbations that are similar enough to those found in nature (which is full of heterogeneity and general messiness) then should we be so worried about the numerical errors that amplify ? Or is there some difference between the perturbations derived from the numerical scheme? Perhaps I am missing something here, or this is more of a philosophical point, but you might add something about this*" and "*544-547. This seems like an essential point, and possibly an answer to my above comments on the realism of self-amplification. It might be worth highlighting that connection.*" : We thank you for this comment, and are happy that the perturbation analysis was helpful. Since the numerical noise grows from the physically based self-amplification mechanisms, it finally has a "realistic" good look. However, the numerical noise arises for instance from linear and non-linear solvers, and is influenced by software or hardware choices, which has obviously no relation with nature. Moreover, numerical noise is generated at each time step, and thus accumulates over time. Its statistical signature will never coincide with the physical observations: it would be like throwing the parts of a puzzle and hoping that they will correctly reconstruct the puzzle when going down. This is in our opinion a methodological key point: a numerical simulation is not supposed to reproduce directly natural observations, but only to compute a correct approximation of a model for a given dataset. This is the model and its data that should reproduce nature, and in this case if solved correctly the numerical solution will be a useful approximation. If we do not willingly add randomness in the model or its data, the numerics should not introduce it out of nowhere and bypass our modeling. It is not reasonable to rely on numerical hazard to recover the missing elements in a model. Worst of all, numerical noise lacks two essential modeling requirements: reproducibility and explainability. The first one since numerical noise depends on the softwares/algorithms used, the number of processors, etc. The second one since it is almost impossible to track how the numerical errors are generated. We have added a paragraph in the discussion section on this point.

. "*387-388. This also makes me think of how steady-state solutions to the imperfect numerical model do generate steady-state topography that appear to satisfy the governing equation. For example, the relationship between incision height and curvature (Figure 7) in (Theodoratos et al., 2018), or slope and area in many other studies. There is sometimes some scatter around the expected relationship though. So does your work suggest this scatter is the result of the error you describe, or are you saying we are even using the wrong measuring stick of success ?*": It is indeed possible that the amplified numerical noise is reflected in this scatter. However the only way to be sure of it would be to reproduce their experiments with a filtered model.

. "*397. What would it mean for the cartesian mesh to not be symmetric ?*": we intended this to emphasize the fact there is no symmetry problems coming from the mesh. We have simply removed this confusing comment in the new version.

. "*419-421. I think I understand this from the perspective of reproduceable LEM simulations, but as you have shown, aren't these self-amplifications likely reflecting a natural phenomenon ? I am not familiar with the CFD world to know how they accept or handle such features, but I suspect geomorphologists using these LEMs will wonder about this*": For CFD, numerical methods are usually tested on analytic solutions and well-established benchmark solutions (such as the Taylor-Green vortex). Symmetry is systematically tested, and numerical methods that do not control efficiently numerical noise are simply discarded. Self-amplification of numerical noise is indeed avoided at all costs, since noisy numerical results are quantitatively useless.

. "429. "refer the reader to a the quite recent review Zhiyin (2015)" Remove "the quite". Here and elsewhere your citation style should be checked too. I would expect (Zhiyin, 2015).": we have removed the quite. As for the citation style, we use the style file provided by the journal, and consequently have no control on the chosen style.

. “*463. Are there existing formulations for steady state shallow water equations that have already used such filtering, or is this also new?*”: When shallow water alone is considered, the topography is in general assumed to be a data without noise. Thus, there is no reason to filter the topography. Notice that the full shallow water system because of the non-linear terms poses some specific but well-known numerical challenges.

. “*Figure 15. Is the x-axis here correct? Everywhere else when you have presented a “convergence curve” the x axis has been grid spacing*”: there was indeed a mistake in the x-axis, it was supposed to be $\ln \Delta_{xy}$ as everywhere else.

. “*495. This makes sense, as τ is similar to Pe in Perron et al. (2008). The length scale derived from Pe=1 describes the scale at which advection (destabilizing) begins to exceed diffusion (stabilizing). Of course, this is just an analogy, the underlying model is different, as you discuss.*” : Indeed, τ and Pe seems to play similar roles, and we believe that this is how our results could transpose to the model of Perron et al.

. “*575-576. “Undoubtedly the correct solution” Rather than saying this, can you support with other evidence ? For instance, what does Perron et al. (2008) suggest should be the length scale spacing between ridges and valleys, and how does that relate to the filter parameter ?*: In fact we had conducted a convergence study on this case, drastically reducing both the filter parameter and the mesh size. The solution remains unchanged for the refined situations, which is a strong argument that the uniform solution is the correct one for the large value of k_g. We have added this comment in the new version.