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 might have misunderstood the way discussion on esurf works, and probably have answer too early to the first reviewer. Should this be the case, we apologize about that.

We understand from both reviews that publication under the present form is very unlikely, as well as that there are doubts on the fact that we can produce a good enough revised version. We have nevertheless tried to produce a revised version since it is our belief that the first version was quite misunderstood. In the revised version we have prepared, most parts have been completely rewritten, taking into account the remarks of the reviewers (notice that this made the latexdiff produced file requested by the journal quite useless). We felt that there were sufficient similarities between the reviewers remarks, therefore before giving separate answers to each of them we start by giving some precision on the two main topics of the first version of the paper, in the hope that it will clarify things. We have rewritten the major part of the article including the introduction, basically keeping only the model description and the numerical results. The first major modification of the new version consists in the fact that we have decided to postpone the publication of the mathematical details on the consistency correction for MFD and its extension to node-to-node methods to a future paper in a more suitable journal. Thus the large majority of the mathematical technicalities have been suppressed, allowing to focus on the treatment of numerical instabilities. Since it is clear that our example involving symmetry was not convincing enough, the second major modification is that we have added both a short theoretical analysis of the stability issues as well as numerical illustration on analytic test cases on which it should be obvious that the simulation does not produce the correct solution as well as the fact that the erroneous solution without considering a filtering strategy is more appealing to the eye than the correct one, leading to the treacherous situation we were trying to describe in the first version of the paper.

The MFD algorithms and the GMS model

We believe that because of the mathematical formalism, there has been a fundamental misunderstanding concerning the MFD algorithms. In [2], it was rigorously established that at the very least on cartesian meshes the cell-to-cell MFD is exactly a mesh-dependent mean of the water flux associated with the discrete Gauckler Manning-Strickler (GMS) model obtained through the two-point flux finite volume approximation. We perfectly understand that the MFD algorithms were not designed with the GMS model in mind, and that they are believed to be a very different model. They nevertheless unexpectedly truly happen to be a discretization of the GMS model, even if they were not intended to. Using this identification, in [2] it was proposed to replace the mesh-dependent mean of the water flux with a correct estimate of the discrete water flux, leading to a consistent, convergent and as mesh-independent as possible approximation of the continuous water flux. The GMS-MFD approach can then be extended to general polyhedral meshes, as detailed in [2]. A numerical convergence study

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was conducted there, in particular it illustrates the problematic behavior of the outcome of MFD algorithms, with a very striking bad behavior on Voronoï meshes despite the fact that they fulfill the requirements of the two-point flux finite volume approximation. We originally wanted to recall this result and explain how this allows to correct along the same path most of the MFD algorithms of the literature, at least those for which no additional artificial mesh dependency is introduced, still using the GMS discrete model as a reference. This excludes for instance single flow direction methods, as within the context of the GMS we can see that they introduce a very strongly mesh dependent coefficient. This reinterpretation also allows to directly link the MFD algorithm with the specific catchment area a of [4, 1], since the GMS model is a generalization of the model defining a. The work of [2] was mostly dedicated to correcting the non consistency of MFD algorithms, rather than proposing a new and correct definition of the unit catchment area as in the works of Porporato and al, nevertheless leading to an abstract unification of both approaches through the general GMS model.

We were hoping that this would come as good news, as it provides a possible way to get rid of the mesh dependency of most classical MFD methods. The catchment area produced by the MFD algorithms, corrected using the formula of [3] to get a unit catchment area was historically used in our stratigraphic model. We observed the usual mesh dependency, which created much difficulties for obtaining reproducible results in particular in a parallel environment and completely prevented convergence with the mesh size. We have replaced it a long time ago by the discrete water flux coming from the GMS model, once we had understood the link between MFD and GMS.

This being said, the main objective of our paper was to discuss the problem of numerical instabilities arising from the self-amplification mechanisms of the coupling between water and sediments and how to control them using the LSS approach. Since it is necessary to first get rid of any $O(1)$ error, in the first version of the paper we felt that it was important to first discuss the consistency correction for the MFD as using a non consistent discretization for the water flow model would prevent from obtaining any meaningful result for the coupled water and sediment model. However it has apparently created much confusion and has diluted the main message that was supposed to be on the coupled system. This is the reason why we have decided in the proposed revision to simply replace this section on MFD by a short subsection explaining why we have chosen to use the GMS model to compute our water flow, simply emphasizing the link with MFD algorithms in what we hope will now be an understandable way.

Numerical instabilities

We mentioned in the introduction of our first version that in the absence of analytic solution, it is hard to decipher if the obtained results are not the consequence of numerical noise. Of course we did not wanted to say that it is impossible to construct analytic solutions (one can always do that by prescribing a function that satisfies the boundary conditions, inject it in the system and compute the corresponding source terms) but rather than when performing real life simulations on complex topographies, we precisely do not know what is the correct solution and thus if we cannot certify that the numerical scheme is reliable, it is hard to be sure that the obtained result is trustworthy. This is the reason why we had chosen in our first version to illustrate the numerical stability issues on our "three rivers" tests case, as despite the fact we do not know the exact solution thanks to the symmetry preservation properties of the equations the continuous exact solution will be symmetric. Thus, a good numerical approximation must be symmetric up to the expected discretization error, which should tend to zero with the mesh size and should be relatively small (say $O(\Delta_{xy})$) for a fine enough mesh. This is not what we have observed, the error is of the same order of magnitude than the solution, as the main water flow is not located where it should. Obviously, this was not sufficiently convincing but we still believe that this example is important as it is an easy to perform test. This the reason why in the proposed revision, we have derived analytic solutions on which we study the numerical behavior of the system. We have added a short theoretical analysis of the evolution of perturbation's energy, from which the ratio τ between the product of the diffusive water-driven coefficients and the water flow and the diffusive gravity coefficients naturally appears. The ratio τ

seems to be a key control parameter of this equation system. As expected, when the parameter τ is relatively small, we obtain a good approximation and the overall method is converging with the mesh size (showing by the way that our numerical scheme is convergent and that our implementation is correct). However when τ is large, not only do we loose convergence but the numerical solution is visually very different from the analytic one. We have chosen a quite artificial looking solution, a stationary monodimensional solution to which we add analytic stationary smooth bumps. For large τ, we obtain a non stationary numerical solution with complex topographic structures that could be reminiscent of valleys or rivers and have a "landscape" look. However this is totally artificial and the result of self-amplified numerical noise. Using filters, those perturbations disappear and we recover the correct solution even for large values of τ. In addition to the three rivers test case, we hope that those new examples will be convincing enough.

Reviewer 1

We thank the reviewer for his time and valuable comments.

. In the revised version, we have chosen to completely remove the technical discussion on the MFD algorithms and to focus on illustrating why numerical instabilities are an issue and how to control them. We are aware that even the revised version might in some respects be better suited to a community of pure modelers. However, the modeling approach based on MDF algorithms being one of the corner stones of the community, it is our belief that this type of publication, which provides these kinds of recommendations on the deployment of LEMs, should be visible by this community.

. As mentioned above, in the revised version of the manuscript we have added a comparison between numerical and analytic solutions, that should emphasize more clearly that the self amplification of numerical errors is a true issue.

. It is well documented in the mathematical literature that the stationary transport equations like the continuous Gauckler-Manning-Strickler (GMS) model considered here are well posed if a sufficient condition on the topography is satisfied, the two we proposed being probably the simplest ones. Otherwise the problem can have several solutions, infinite solutions or no solution at all. As the MFD algorithms are truly a discretization of this equation, even if they were not originally designed to be one, they suffer from the same deficiencies. The additional "and > 0" was a typo, our apologizes for that. By "drainage basin" we wanted to designate basins with no accumulation or flat areas which would then satisfy one of the sufficient conditions (the conditions on the Laplacian being the simplest one), and we are probably wrong on the use of the terminology "drainage" so we have simply chosen to avoid the use of "drainage" in the revised version. Valleys are not necessarily a problem provided they possess a downstream spill point. Since from a practical point of view on cartesian grid the MFD algorithm computes a value for the water flow even in the problematic areas, this creates a model error that if overlooked can also lead to non-physical flow patterns because of the self-amplification mechanisms of the coupled sediment/water model. This is why in the numerical experiments of the paper we have been careful to always use topographies that satisfy one of the sufficient conditions. In particular, this is the reason why we have avoided using random noise in our initial conditions. We hope that the much lighter discussion of the GMS model will make things clearer.

Reviewer 2

We thank the reviewer for his time and valuable comments.

. We have used the word chaos in the usual mathematical sense (see for instance Differential Equation Dynamical Systems & an introduction to Chaos, M. Hirsch, S. Smale and R. Devaney) which may be quite different from the notion you have in mind. To avoid any misunderstanding, we completely removed any use of the word chaos in the revised version. Roughly speaking, what we wanted to say is that even very small perturbations in the data can result in large differences in the solution, including perturbation arising from numerical errors. Anyway, we are in fact quite worried by our own findings: we do believe that there is a risk that some numerical results are indeed blurred by numerical errors. Fortunately it depends on many factors like the use of high order methods, explicit schemes, etc... Of course our intention was not to criticize former works but rather to raise an alarm on the risks inherent to the coupling between water flow and sediment erosion and transport. We have deeply modified our introduction and abstract in this sense.

. The coefficient r_s in our model represents the exponent of the non linearity linking the water flow and sediment model. We do not see any direct link with the concavity index.

. One of the consequences of turbulence is that small physical perturbations will be amplified and have a major impact on the final solution. The channel instability studied by Porporato and al indeed corresponds to a positive feedback loop between water and sediment. This is however not unrelated with numerical instabilities. If perturbations of the continuous solution are unstable and amplify with time, the same will happen for perturbations arising from numerical approximations. Continuous level "turbulent" behavior is always linked to unstable numerical behavior. For the coupling between sediment and flow the same phenomenon will occur: a small error on the sediment distribution, for instance a small lack of sediment somewhere will result in a modification of the water flow, slightly focusing the flow where the numerical error had creating a small noise. This will result in more erosion at this point, increasing the originally small perturbation in the sediment. The physical instability has necessarily a numerical counterpart. This is the reason why in computational fluid dynamics at high Reynolds number high order methods are used to reduce as much as possible the numerical errors and some numerical dissipation mechanism is always added to simulations (for instance sub grid scale diffusion). We wanted to emphasize that the similarities between CFD and LEMs will necessarily result in the need of similar numerical treatments.

. We might have missed some part of the literature. In the works of Porporato and al. as that we were citing, as well as the older works of Smith and al on the model we use, the stability of solutions of the continuous model is evaluated through a linear stability analysis under periodic perturbations. This analysis leads to monodimensional EDO that they solve to obtain the value of the parameter controlling the time evolution of the perturbation, hence classifying stable and unstable regime. We were absolutely not saying that those results do not represent the physical process with accuracy. What we wanted to express with this sentence is, as explained above, when performing a numerical situation in a realistic, complex setting, we do not have any tool to discriminate between correct and incorrect solutions, precisely because we do not know the exact solution. Of course, this is inherent in any interesting numerical simulation. In a "turbulent" context, if no special treatment is applied it is very likely that numerical error amplification can occur. This is not always the case, but if it is not taken into account by design in the numerical method, the risk exists. In the revised version of the paper, as explained above we compare our results to carefully designed analytic solutions, which consists in a stationary monodimensional solution similar to the ones usually used in the literature, but perturbed by analytic smooth bumps. We show that for the "non turbulent" regime everything is ok and the simulation converges to the exact solution. However, in the "turbulent" context, numerical instabilities are amplified up to the point that the numerical solution is very different from the correct one, and any case up to a point where it cannot be considered a reasonable approximation. However, the resulting error has a "landscape" look that is treacherous. In this well controlled setting, it is easy to see that this good looking solution is wrong. The intended meaning the sentence you cited was precisely to warn that it becomes very difficult on a real test case to identify this kind of errors.

. We have decided to remove almost all the section on the MFD for the reasons explained above, and we simply motivate our use of the GSM model.

. We have added appropriate citations in the introduction. We also postpone to the discussion section the question of the generalization to other LEMs. We have tried to better motivate why we believe that our findings will carry to those models.

. A complete answer requires to enter into mathematical details, and we hope that the above section on the MFD will have clarified things. We nevertheless repeat that although they were not designed for this purpose, most of the MFD algorithms in fact exactly coincide with well chosen discretization of the GMS model, and can thus be analyzed within the GSM framework. This allows to understand easily why they have such a strong mesh dependency and how to correct it. Notice that the GSM model we speak of directs flow with the topographic and not the hydraulic gradient. We discuss its generalization at the end of the paper, as a potential way to overcome its limitations.

. Unless we have misunderstood their paper, which is of course possible, we understood that they derived and studied the evolution of the mean value in one direction of the model. This is a limit case of filtering: where the mean is taken over a large domain rather than a small one. This might be a strong simplification, but this can be seen as a LES model. Our intention was to avoid pretending that we were the first one to use a LES approach, as their work approached very closely the idea.

. We have rewritten this section entirely, and we hope that you will find that it does now justice to those papers. Again, our intention was to justify the use of the GSM model and this associated water flux q_w, which because of its many parameters is a generalization of the model underlying a.

. Provided the source terms, initial value and boundary conditions as well as the domain satisfy some symmetry, then this symmetry should be kept at all time. For instance, if the solution is symmetric with respect to the axis $x = 0$, then it is not difficult to see that $(h_s(-x, y), h_w(-x, y))$ solves the very same equation than $(h_s(x, y), h_w(x, y))$ with the same data, and thus that the two are equal (assuming uniqueness of solutions of course). If the mesh is symmetric, this property should carry easily to the numerical solution. However, even if the mesh is not symmetric, if the numerical solution is a reasonable approximation of the continuous one then symmetry should be obtained up to the numerical error, which should behave like $O(\Delta_{xy})$. This is incompatible with a water flow being at the wrong position: this is an $O(1)$ error. Furthermore, on a fixed grid, the position of the flow should never depend on the linear solver chosen to compute it. We hope that the added comparison with analytic solutions in the revised version will be more convincing, and that this will give more strength to this symmetry test, that we believe to be important. Indeed, symmetry testing can be done in situations where we do not know the exact solution. Concerning valley spacing, notice that we do not say that the system is incapable of producing patterns: we just say that if there is no significant instability seed in the physical data, no patterns should spontaneously emerge. We do not understand the end of your remark: if we add significant noise, there is no reason for the solution to remain symmetric as the data will no longer be symmetric.

. We have taken care of the minor comments.

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