REVIEW OF "LARGE STRUCTURES SIMULATION FOR LANDSCAPE EVOLUTION MODELS"

I have detailed first, what I believe, are major points of concern in the manuscript and then I have listed some specific minor comments. I have tried to express my reservations clearly. Some of my concerns may seem direct, but I offer them constructively. Overall, I think the submitted work would need at least (maybe more than) a major revision.

1 Major Comments

1.1 Presentation

The quality of the writing needs improvement. With the current way the paper is written, it will be unreadable to a vast majority of readers who know a bit about landscape evolution modeling (I am not even counting users or experimentalists here). I had to do multiple rounds of reading to understand the work; which led to a delay in submitting my review here. I am giving this opinion as a person whose research has focused on landscape evolution modeling. I understand that the authors have tried to bring mathematical correctness to their explanation, but I don't think it will be conveyed to the earth science community in this format. I have written papers that contained very few fractions of mathematical jargon that is here and the papers are not well received. So, just in terms of presentation, a major rewrite is required. The current draft feels to me like a paper from the SIAM journal on applied mathematics.

1.2 Motivation/Premise

I feel after reading the abstract and introduction multiple times that jargon and references to previous works are used without careful consideration. If anyone reads the abstract of this draft, it will seem that due to nonlinear (the authors say "chaotic" - I don't understand how that feedback is chaotic, which means a completely different phenomenon of the equations), the previous LEMs are likely full of (or blurred with) numerical errors. This is a very big claim in my understanding, without proper justification. Again, if authors have some interpretation of "chaotic" behavior, that should be written clearly and in the particular context they are analyzing.

Result from Line 505: The issue of a "chaotic" drainage network is only for $rs > 1$ in the authors' model. If that is correct, it should be made clearer way earlier in the manuscript rather than here. And for $rs < 1$, there is no numerical issue without filtering, Is that correct? Again this information is crucial that is lost in the middle of the manuscript. How is this observation related to the work of Shelef et al. and Tucker et al. based on the concavity index? Is rs for your model working the same as the concavity index used in other landscape evolution models that are referenced in the beginning?

In the abstract, the authors discuss and compare "numerical instability" to "turbulence instability". I am familiar with the works of Porporato and co-authors cited in the introduction to which current authors attribute this comparison. Those authors introduced "channel instability" in reference to the properties of the landscape evolution equations and the physical feedback of eroding more where the accumulation of more water and then again accumulation, which results in a branching cascade, similar to the energy cascade in a turbulent flow. This is very different than numerical instability propagation, as claimed by the authors.

The authors write on page 2: "However, in the absence of reference analytic solutions, it is hard to decipher if the obtained landform results from physical processes or from the self-amplification of initially small numerical errors." For the same group of studies by Porporato and co-authors, analytical results have been presented and numerical solutions have been tested to agree well with the solutions. In Bonetti et al. (2020, PNAS) and Anand et al.(2022, JGR), linear stability analysis of the smooth solutions is matched with numerical solutions under different exponent values. I don't understand how these results don't represent the physical process with certain accuracy, as the current authors claim. I completely agree that numerical errors/grid orientation errors may not be completely absent, but they are not detrimental as appropriated by the authors in motivating their work.

In my understanding, the presented work attempts to improve the impact of grid orientation on the numerical approximation by flow-direction algorithms for the GMS mode. After reading Section 3 carefully, this work focuses on extending the work of Coatléven (2020), which I could not guess from the introduction. If I am not completely wrong here; I am getting more convinced that the title would need changes to convey that message clearly.

If I have understood this work correctly; the authors' new results are shown for the sediment model of Granjeon (1996); Eymard et al. (2004, 2005); Peton et al. (2020). These papers are not discussed/cited in the manuscript until the main result section 4. Also there, these papers are just cited quickly. I feel authors should focus explicitly more on these models and their specific results in a clean way. The approach of using different models mentioned before for motivation and using different models for explaining results is very confusing. I can see the urge to show the relevance and generalization of these results to other models, but the current presentation is not the way to do it.

1.3 Technical Points

In para 50-60, the authors say that flow-direction algorithms can not approximate well their GMS model (which is a shallow-water approximation). This does not look unexpected to me because the flow-direction algorithm are describing the kinetic flow of water under surface gradient (gravity balancing friction), which is different than the model using shallow water approximation. The physics of the two cases is different, I don't think that can be just attributed to big numerical error. Again, I am not saying grid resolution and numerical errors due to imposed orientation are completely absent, but this attribution does not seem accurate. Please correct me if I am wrong here.

For the LES part of the motivation, the authors claimed and described in lines 80-85, Hooshyar and Porporato (2021a); Porporato (2022) implemented LES. They described the potential use of LES based on the dimensional analysis results for different detachment-limited erosion exponents and they verified those claims using a DNS-type numerical approach (Anand et al. 2020), not an LES approach.

In line 150 the authors write, "Setting Qw = a, this allows to reduce the mesh dependency to the usual consistency errors of numerical schemes." This is already shown in the works of Porporato and co-authors, which is not attributed clearly here. They used a specific drainage area (defined at a point) instead of total catchment area A which by definition is grid-size dependent. Bonetti et al (2020) and related modeling papers showed that using the specific catchment area a is the correct use of variable for water flux at a point rather than the total catchment area, A, and the resulting numerical results using a are not grid dependent.

Line 515: If I have understood correctly, the claim of not having any numerical error in the presented results relies on the observation of symmetry for "three rivers" cases with LSS compared to solutions without the filter. I am not totally convinced by this argument. In my understanding, the authors have taken three water influx boundaries, with one in the middle and hence they say that the solution should be symmetric. However, this would be the case only when solution geometry matches the domain geometry, and if that is not the case, the obtained solution would try to adjust on the discrete grid and it will have some defects (symmetry breaking). This is also observed in other pattern-forming systems (see Debenedetti, Nature (2001)). How I see this is that the numerical solutions without filter bear some memory of initial conditions, which get removed by using the filter. I am curious to see if the average first-order valley spacing changes a lot for with/without filter cases for the long rectangular domain case. Or, is there a big change in the statistics of the landscapes (hypsometry, power spectra, drainage network statistics) for with/without filter cases? Based on the color labels of the solution in figures with and without filters, the range of water flux and elevation height seems to be in the same range for both scenarios. For this reason, does the author know what happens when you add additional noise to their governing equations, even when there is no filtering? Do we see a symmetrical case appearing in those simulations as well?

Minor Comments:

Figure 1 can be made much more intuitive than its current form.

Figures 3 and 4 could be combined.

The label in Figure 6 is missing.

Line 155: Spelling mistakes "though of".

Lines 160: Awkward sentence: "this choice of source, k_m has the unit m.s-1 of a speed."

Figure 19: Missing the literature source "approximately 35.9°N, 120.8°W extracted from Dietrich."