Review of "Large structures simulation for landscape evolution models" submitted to ESurf

Summary

The authors demonstrate and provide a means to address the amplification of numerical errors in a basic landscape evolution model. They do so by drawing comparison between this model and the Navier-Stokes model, both of which have mathematical characteristics that support the amplification of small perturbations. Their solution involves filtering the water and sediment fluxes, effectively smoothing them to a scale greater than the grid cell width. They show that real perturbations above the scale of the filter can and do amplify, while results suggest those below the scale do not. They demonstrate that re-introducing heterogeneity in physical parameters can create the satisfyingly complex simulation results we expect from landscape evolution models, but acknowledge that in practice it can be difficult to discern the complexity that arises from numerical artefacts versus introduced heterogeneity.

Overall, I found this to be a compelling work. By the end of the paper, I was convinced that filtering such as they have done could help achieve reproducibility in landscape evolution models. As previous reviewers and the editor have noted though, the paper is challenging to read. While some of this is due to the use of mathematical formalism that is unfamiliar to the geomorphology community, I think the authors could still improve clarity of the formalism by working on the text in Section 2. Notation that is not defined should be defined, and a greater attempt should be made to distinguish between similar quantities, or at least remind the reader of their definitions along the way. Some notes on this are listed below.

A previous reviewer argued that there was limited value to the work because it simply introduced another length scale. I think they have clearly argued that some length scale is introduced whenever a numerical solution is implemented, and it would be best the solution were dependent on this scale rather than the grid cell size.

A note on the response to reviewers – the section numbers you give seem to be off by one, so section 1.1 is in fact 2.1, and 3.3 is 4.3.

Comments by line number

19. Define MFD
35. "from of a"
43. gravity-driven transport
44-40. This sentence needs restructuring
53. I do not know what the parenthetical means, and I suspect many other readers will not as well.
58. target length scale \alpha
96. "the principle of the conservation of mass" -> the principle of mass conservation
105. "b is a data" Rephrase.

108-109. Can you be clearer that Ss is in-situ production and Bs is boundary input?
111. “Let us precise that in the following the xy” Rephrase.
119-120. This sentence is unclear to me. Can you rephrase or describe in more detail?
137-138. This presumes we know what “most classical cell-to-cell MFD algorithms” you are talking about, and I am unsure.
144. thought
145 truly
146. For which choice of source? Are we talking about the case in the previous sentence, which you are not considering, or something else?
147. Definition of a. Is the h_w in parenthesis indicating \eta_w is a function of h_w?
173. You have just said that the specific catchment area (SCA) is an approximation of qw, but now you say that the specific catchment area a is in fact equal to qw? I am confused. Then later you are treating a as interchangeable with h_w? And qw with Qw?
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)
245. Where u is a stand-in variable?
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.
280-281. Rephrase for clarity.
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.
357. Should this include Fig. 8 too?
Figures 4-8. It would be helpful to title the subplots with the parameter combination, rather than just listing them in the caption.
Figures 9. Again, it would be helpful to title the subplots with the parameter combination. In the caption, you can then reiterate which previous figure (4-8) shows the corresponding analytical solution.
371. “Which is no more high enough” It might be better to say “Problems are potentially more severe in finer meshes because numerical diffusion that can dissipate residual numerical errors declines with grid spacing” or something like this.
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?

397. What would it mean for the cartesian mesh to not be symmetric?
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.
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).
433. Remind us, or define \delta and d
457-459. Rephrase for clarity.
463. Are there existing formulations for steady state shallow water equations that have already used such filtering, or is this also new?
Figure 14. Again, it would be helpful to have title labels with the parameter information.
Figure 15. Is the x-axis here correct? Everywhere else when you have presented a "convergence curve" the x axis has been grid spacing.
495. This makes sense, as \tau 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.
522. "implicitly"
539. "of the same magnitude *as* the analytic ones"
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.
548. "loose" -> "lose"
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?
604. "similar numerical stability issues that" -> "similar numerical stability issues to"
659. "physcally"
678. I would remove the double negative.
680. Repartition seems like the wrong word here.
684. "Llet"

Works Cited

Perron, J. T., Dietrich, W. E., & Kirchner, J. W. (2008). Controls on the spacing of first-order valleys. *Journal of Geophysical Research: Earth Surface*, *113*(4), 1–21. https://doi.org/10.1029/2007JF000977

Theodoratos, N., Seybold, H., & Kirchner, J. W. (2018). Scaling and similarity of a stream-power incision and linear diffusion landscape evolution model. *Earth Surface Dynamics*, *6*(3), 779–808. https://doi.org/10.5194/esurf-6-779-2018