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Comment on esurf-2022-30

John Armitage (Referee)

Referee comment on "Testing the sensitivity of the CAESAR-Lisflood landscape evolution model to grid cell size" by Christopher J. Skinner and Thomas J. Coulthard, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-30-RC1, 2022

Review of “The sensitivity of Landscape Evolution Models to DEM grid size” by Chris Skinner and Tom Coulthard

In this manuscript the authors discuss the sensitivity of the LEM CAESAR-Lisflood to grid cell size. They find that for a series of model functions that the numerical model is roughly as sensitive to DEM resolution as two other important parameters, the sediment transport law, and the TOPMODEL m parameter (that controls the peak and recession curve for the transformation of rainfall to runoff). There is the important observation that despite resolution having some, but perhaps not a significant impact on model functions such as total sediment yield or time to peak sediment yield, there is the potential that the model gives the “right answer for the wrong reasons”.

In this manuscript the authors re-use the experimental method developed in Skinner et al. (2018) to use the Morris Method to explore the relative importance of parameters to one and other. This is achieved (please correct me if I am wrong) by first selecting a representative sample of models to run (1220 models in this case) and then plotting the standard deviation and mean effect of each parameter relative to a model function (Table 2 in the manuscript). From this the most important factors might become apparent. From reading up on the subject, I see that the Sobol method however gives a quantified effect of each parameter on the model result, however in Skinner et al. (2018) it is stated that the Morris Method is good enough given evidence from previous studies. It would be interesting to see this point demonstrated, but perhaps that is a technical point for some future study.

I think this manuscript is a useful contribution to understanding the operational use of landscape evolution models that are process based, such as CAESAR-Lisfood. From using this code, my colleagues and I have noticed that for example the Mannings coefficient needs to be adjusted if a higher resolution DEM is used to replace a coarse resolution DEM. This manuscript starts to put these sorts of “tunings” into context of the limitations of the model approach.

I have a few comments that the authors might find useful to further improve this manuscript (in no specific order):

- It would be useful to see to what extent the change in DEM resolution impacts the spatial distribution of erosion and deposition. There is a focus on the model functions in Table 2, for obvious reasons, however these are spatially lumped. The biggest advantage of using a code like CAESAR-Lisflood is it can be used to model the spatial distribution of erosion and deposition. If I were interested in only gauging station measurements of water flux and sediment yield, I could turn to one of the many 1D models that treat the river network as a line and get over the problem of resolution. Therefore, it would be ideal to get some feeling for how resolution impacts the spatial distribution of landscape change. If the authors think it possible, perhaps some analysis of the DEMs of difference between the start and of each model could be analysed. A plot of the distribution of the mean and standard deviation of elevation change for each DEM resolution? Or the same exercise for each sub-catchment in the DEM as a function of model resolution? It would be interesting to see at what resolution the spatial distribution of erosion and deposition starts to converge.
- The sensitivity analysis is carried out on an existing landscape, where the landscape features already exist. Another application of LEMs is to try and model landscape formation. Here the impact of model resolution might be more acute, as the channels have not been carved into the landscape, and the model equations are free to form the landscape features. It would be interesting to run the same sensitivity analysis on a simple slope, perhaps with some noise to localize the flow routing algorithm. This would confirm the robustness of the results from the Morris Method that suggest DEM resolution is as important as the sediment transport law and the TOPMODEL m parameter. Spatial statistics, such as the wavelength of valley spacing, could also be measured to discover if below a certain resolution the model reproduces the same topography (e.g. Armitage, ESurf, 2019; https://doi.org/10.5194/esurf-7-67-2019).
- Why was the Meyer-Peter Muller sediment transport model introduced in this study? How has it been included? What are the benifits of using it over Wilcox and Crowe? What are the drawbacks?
- I recently read the chapter "Transport of gravel and sediment mixtures", in Sedimentation Engineering: Theories, Measurements, Modeling, and Practice, by Gary Parker (2008; https://ascelibrary.org/doi/10.1061/9780784408148.ch03). In this chapter he states that "Einstein (1950) was the first to execute such an analysis for the bed-load transport of mixtures. The relation cannot be considered appropriate for the purposes of calculation due to the gross inaccuracies in the hiding function." I am curious as to why the Einstein model remains within this analysis if it is known to badly represent the hiding effect of large grains on smaller grains?
- In Figure 5, what is the "vegetation critical shear" and the "grass maturity"?
- I think it is important to stress that it is not surprising that outputs that are totaled over the duration of the model run are not sensitive to the DEM resolution, such as total sediment yield. The area of the catchment has not changed, and neither has the average slope of the catchment (I presume). What is more interesting is the response of the model to change, such as how well flood events are recreated. I don't feel that this is really covered by the application of the Morris Method here.
- There is a typo on line 42, "someone".
- Why is bedrock erosion ignored? This was also the case in Skinner et al. (2018).

Summary

Overall, I think this manuscript is highly valuable, if focused to users of CAESAR-Lisflood. The results could be possibly extrapolated to other process-based models, such as LAPSUS, PARALEM (?), but this has not been tested. It could be published with some minor improvement, and act as a starting point for more research. Or with some more thought into the question of the spatial distribution of erosion and deposition could make for a bigger piece of work. I would prefer the latter, hence my choice for "major revisions" however being realistic, I would leave that choice to the authors as the day job can get in the way of big revisions.

I hope these comments are helpful.

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