GMD-2019-197 REVIEW v2

I am very sorry to communicate that I strongly recommend not considering this work for publication. I think you have got great ideas on how CEM could be improved, and you have implemented some of them, but this research is incomplete and presents several inconsistencies in the approach that at least need investigation. Presenting a new model requires validation. There are fundamental processes that I have the impression CEM2D could be messing up. The development of complex bathymetries but the non-inclusion of 2D wave transformation is inconsistent. The cross-shore profile shape evolution requires verification, modelling its evolution is not trivial. The use of a 2D topography scheme with an integrated alongshore sediment transport formula taking sediment from the cell adjacent to the shore is inconsistent, unless you adapt this shore-cell to cover the active zone (that would change in time), or unless you assume a constant profile shape (as in CEM). I encourage the authors to take my comments positively to reinforce their research, and that soon we can read a new resubmitted version of CEM2D with a thoroughly verification of the new processes that is accounting for.

Below my responses to author comments with more fundamental questions regarding their results and presentation of the paper.

11- CEM assumes a constant active cross-shore profile shape and the sediment transport is computed integrated in the surf-zone, for example with the CERC formula. Therefore, the sediment is added/removed horizontally. CEM2D claims to take the sediment from the numerical-grid cells in vertical, this is a great addition but brings inconsistencies when sediment transport is still computed with an integrated formula as in CERC and this sediment is only taken from the adjacent cell to the shoreline, this cell could not be representative of the active profile, could cut it in half, or be double. Therefore, it could introduce changes in the profile shape that would be artifacts of the model grid or the sediment redistribution scheme.

“The evolution of landforms is more gradual in CEM2D…” could you proof this is an improvement over CEM to match natural landform evolution?

12- Figure 17 shows that under sea level rise the landscape feature under the same wave climate is different, could you verify this is a natural behavior? For example, you could find a spot in the world where large scales features developed during a transgressive period, proof that with your model you can mimic the spatial scale and shape of these features, and with CEM the resultant feature would be different.

13- Thanks for the answer, I am curious to know which model is doing alright, could you proof your model is improving the results given by CEM? I do agree that CEM2D evolves different landscapes due to the redistribution scheme, but this could be a numerical artifact, it needs any kind of verification.

14- I am surprised, it is not a numerical instability, however it is a directional bias inherited from the model routines… in any case is a model artifact, I cannot read this in line 239. If CEM2D develops more complex topography and it is different than CEM results, please, proof that your model reproduces natural landscape evolution better than CEM. You could add in Section 5.2 the examples you mention and highlight the differences between CEM2D and CEM in comparison to the natural feature. Nowadays, you can easily obtain accurate wave climates around the world from ERA5 reanalysis (40 years dataset) so that you can compute A,U.

15- In science it is not enough with saying it, we need validation/proof that CEM2D mimics the natural landscape evolution. Please, validate that your model could be used for landscape prediction, validate that the shape, spatial scale, and temporal scale matches the natural behavior.

In section 5.2.1. CEM2D under A=0.55 and U=0.6 doesn't show any feature like the Carolinas, in any combination of A and U is not possible to develop large scale Cuspate Forelands with wave lengths longer than 50km or amplitudes greater than 10km.

15.r The cross-shore profile shape is of great relevance and modelling its evolution is not trivial, proof that CEM2D mimics the profile shape evolution.

16. Presenting a new model requires validation.

17. In case the model takes all the sand from the shore cell, if the shore cell is half the active profile width is taking too much sand from the vertical. I think it is inconsistent this numerical scheme with an integrated sediment transport formula. You need to take sediment at cell level…otherwise you introduce spurious bed level updates (that later you try to smooth out with your redistribution technique).

"activating the sediment distribution method at this frequency causes instability in the model and it is therefore activated less frequently"

Why do you think this happens?

19. It is not enough to say that the results are applicable to timescales of 10 to 100years, show what are the model outcomes at those timescales, do the results mimic shoreline rates found in nature? In general, you must validate that the model mimics the time scale of large-scale feature formation. What is the spin-up of the model?

20. Again, this is an inconsistency, you claim to develop complex bathymetries that is proven to affect shoreline feature development (Limber et al., 2017), but you don't account for changes in wave transformation. I think from your new Figure 17, this affects the results output by CEM2D.