

GMD-2019-197 REVIEW

In this manuscript, the authors introduce a 2DH version of the Coastal Evolution Model (CEM) that is able to account for, among other variables, a dynamic sea level over extended spatial scales (tens to hundreds of kilometres) and "meso" time scales (decades to centuries).

I am familiar with the original (proto) CEM, based on Ashton *et al.* (2001) and its conversion since to an open-source landscape-evolution model at the core of undertakings like the Community Surface Dynamics Modelling System (CSDMS).

Overall, I found this manuscript readable and digestible – which is excellent for a model description. The Introduction grounds the model in a helpful context, and provides the tool with its *raison d'être*.

I have one major comment and a few minor ones – remarks that I hope will improve the manuscript that much more.

Major comment

I ended up confused about how the "variable water level" advance gets presented here. At L111, the authors write that "CEM2D contains a significant number of modifications to enable it to model the evolution of coastal features including their topographic profiles and to study the influence of a variable water level," but then at L179 state that "we do not examine the influence of a variable water level on coastal morphodynamics but explore the changes that happen with a two-dimensional evolution of the coastal profile."

What wasn't absolutely clear to me is what we gain atop what CEM will already demonstrate. I would encourage the authors to show, as explicitly as they can, the difference in results yielded by CEM vs CEM2D. I see Figs. 10 & 11 – I mean some kind of quantitative demonstration of the differences in output?

I think part of what I don't understand is why the "new" parts of CEM2D are underplayed here. Most of this paper comes across as a kind of reassurance that the new version still does everything the old version does. I appreciate the addition of elevation contours in Fig. 11 (and others), but I finished the manuscript still waiting for the other shoe to drop. Here's what the old model did; and here's the amazing thing this one does. I wanted a clearer demonstration of the latter. This manuscript seems like exactly the opportunity to showcase everything that the inclusion of a variable water level now allows – instead of a conservative assurance that there's been no loss of benefits from CEM.

In CEM, is it possible to impose a linear erosion rate to simulate sea-level rise? And then, in CEM2D, could the authors show the equivalent experiment with the addition of an actual landscape gradient? Seems to me that, given its emphasis on variable water level as the key motivation for CEM2D, this paper needs to focus on what that yields. I think the authors are starting to get there in Section 5.3 (L287) and at L323 in the Discussion, but we need clearer and more specific supporting evidence than Fig. 14 provides.

Minor comments

L47 – "specific research questions" – Theoretical explorations can also derive from "specific" research questions. Do you mean questions that pertain to spatially explicit problems? (Simulating a particular reach of coastline?) Suggest rethinking this paraphrasing of Murray (2007).

L323 – clarify sentence – (see "systems", plural?)

L335 – Confusing paragraph (and see punctuation of "waves") – I think the authors are trying to convey what was absent from CEM but is now present in CEM2D, but the paragraph doesn't read that way to me. Sounds like it's all still "to be calculated", as though these capacities don't yet exist in CEM2D. (But they do, correct?)

Fig. 2 – more labels? alongshore/crossshore, etc., to match Fig. 4?

Fig. 9a, d – x axes? It's not clear to me what's plotted here.

Fig. 10 – Labels for land versus sea? Strange visual inversion when sideways (Fig 11 clearer).

Fig. 12 – White band? I'm missing it, I think?

I wish the authors a fruitful revision, and look forward to seeing this work in print.

– *Eli Lazarus*

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