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Comment on egusphere-2022-251

Matthew Hiatt (Referee)

Referee comment on "Effect of hydro-climate variation on biofilm dynamics and its impact in intertidal environments" by Elena Bastianon et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-251-RC1, 2022

Review of "Effect of hydro-climate variation on biofilm dynamic and impact in intertidal environment"

Review date: June 2, 2022

Ethics: This is my first review of the manuscript and I identify no conflicts of interest.

Review by: Matthew Hiatt

Summary: This manuscript presents an analysis of the influences of biomass and biostabilization on 1D tidal morphodynamics. A validated hydro-morphodynamic model is presented and amended to include the effects of biostabilization on long-term (~10^4 tidal cycles) tidal channel morphology and depositional/stratigraphic patterns. The influences of hydrodynamic disturbances (frequent, infrequent, small, and large) on biofilm development are also assessed alongside the effects of temperature, biofilm development depth, and biofilm growth rates.

Assessment: Overall, the topic is of interest to readers of ESurf. The paper presents a novel combination of models on an emerging topic addressing the role of smaller scale biological processes on channel-scale geomorphology. This topic fits the journal quite well and is timely. The writing is overall good and clear, albeit in a draft that has many small grammatical mistakes throughout. However, that does not impact the understanding of the science, but certainly needs to be addressed. The motivation and approach to the problem are well-reasoned, but the validity of the biostabilization model is not addressed (see Major Comment 1). The sensitivity analysis is easy to understand when the results are presented, but it is not at all clear what will be tested when reading the Methods section (see Major Comment 2). Alongside these it is unclear from a physical perspective, what the hydrodynamic disturbances represent (see Major Comment 3 & 4). That being said, the results section of the paper quite clearly lays out the results and the outcomes are easily understood. I recommend that there be significant revisions to the paper, primarily focusing on validation/justification of the biostabilization and biofilm growth models (it is mentioned they are validated but with what data?) and clarity surrounding the sensitivity analysis scheme and the physical manifestation of the hydrodynamic disturbances.

Major Comments:

One concern is that the biofilm-dependent erodibility model and the model describing the biofilm biomass dynamics (i.e., all of section 2.3) does not seem to have any backing by observations or data other than the reference to Friend et al. (2002) in line 245. This reference identifies the temperature controls on the growth of MPB. Nevertheless, there is no evidence presented that the behaviors displayed in Figure 2 are representative of reality. They may very well be, but the evidence is not explicitly provided, calling into question what the sensitivity analysis results mean (e.g., if the base parameters in the model are not representative, what does the sensitivity analysis teach us?). That does not mean, in my opinion that the analysis is invalid, but I do think that the authors have an opportunity to more clearly and convincingly address the validity of the models presented in section 2.3. This will enhance the reader's confidence in the results presented in the following section.

It is unclear to this reviewer the ranges of the sensitivity parameters being tested. I see in lines 290-296 that a number of different model parameters are being tested, but I have no clear understanding of how they will be modulated, how many model runs will be performed, if parameters will be tested in isolation or a scenario format. Essentially, I have no idea what to expect to see in the results of than some form of model results. The manuscript would benefit from a clear description of the testing scheme. It's fairly clear after reading the results, but comprehension would have increased with a table of tabled parameters in the methods or perhaps pulling Fig.A3 into the main text to show the tested parameters.

For the "strong" and "weak" disturbances, it would be useful to provide descriptions of what types of events these are representing (storms? Regular tides? Especially high tides?) to go alongside the shear stress values presented. It's also unclear to me whether the intervals of disturbance chosen are representative of anything "real". What does 15/5 day intervals represent? Is it an arbitrary selection? Or is this a spring and neap cycle? I understood that the tidal forcing was only semidiurnal.

How are the hydrodynamic disturbances imposed on the model? Are these done by drastically increasing water level at the seaward boundary or is there a momentum component? Or is there wind in the model? Over a small domain (25m), I would doubt it. I think this confusion could be rectified by addressing the previous comment and then briefly describing how the disturbances are implemented.

Minor Comments:

Lines 11-14: There is something wrong with grammar of this sentence. The list of metrics (or perhaps those are the tuning parameters for the sensitivity analysis – it's not clear) isn't explained. I think I get the point but the sentence needs to be restructured for clarity.

Line 18 & 317 & 465 & 595: Can't say "on the other hand" without a previous sentence saying "on one hand." It's a pet peeve of mine :)

Line 22-23: rephrase to "…predict estuary development and mitigate coastal erosion"

Line 40-48: I'm wondering if the authors could add a quick comment somewhere in here about the global ubiquity of MPD and EPS on intertidal and subtidal channels. Is it present throughout the world? If so, how prevalent is it?

Grammar in Section 2.3 and beyond: I'm noticing several sentence fragments, missing punctuation, issues with parentheses, and noun-verb plurality disagreements. I can still understand the text but it's distracting. (Examples include lines 247-249, line 245, line 221, line 249, and further along with grammatical errors in line 296, line 299, lines 347-352, among others). I stopped noting them after about line 350, but to be increasing in frequency as the manuscript goes on.

Lines 347-350: A reference to Fig A3 is appropriate here. It took me a while to figure out what this sentence meant until I search through the appendix.

Figure 4: The inset figures in c, g , and k are extremely small. I would also suggest using the figure itself to highlight assumptions for each row. Perhaps this means labeling them in the figure by highlighting the rows, or similar (this is already done for the columns). With a full page figure, I keep having to scroll back and forth between the figure and the description to figure out what I'm looking at.

Line 449: I know it's not used in the simulations shown in Figure 6, but I'm still very unclear about what "high hydrodynamic forces" means from primarily the physical perspective (I understand the quantitative shear stress side).

Lines 473-474: "This result in a more stable bed, morphological changes occur in a longer timeframe and, by the end of the simulation, it does not reach an equilibrium condition"…I don't understand this argument. Why would it reach and equilibrium state along the same timeline as the simulation not including the biomass growth and biostability? It seems they are two entirely different simulations that can certainly be compared, but I don't think it's a foregone conclusion that they should reach the same steady state condition simultaneously. However, it does appear in Figure 6c that it is approaching some type of steady state, albeit potentially significantly more slowly than in Figures 6b or d. Were the simulations extended out beyond ~10^4 tidal cycles to verify it does not reach an equilibrium state?

Line 529-530: I'm wondering what the justification is for extending the results presented here to estuarine stability. The results presented only test the effects of disturbances and biomass growth on tidal channel morphology in one dimension. This cannot account for the myriad factors contributing to whole estuarine morphology. In fact, the model presented here does not even model an estuary at all, but a single tidal channel. The connection seems tenuous. I think the authors should either omit this point of discussion or stringently justify the extension to whole-estuarine morphology.

Line 543: The line here states the model is validated for temperate areas. I don't see any comparison to data. Was it validated in another paper? I looked throughout the manuscript and didn't find any mention of this. This is the only mention of validation is in regard to the morphodynamic model along (Appendix Figure A1) This bring me back to the Major comment #1.

Appendix Figure A3: This is not discussed at all in the appendix text.

Line 630: This is a sentence fragment or something.

Lines 577-649: I think this section can be significantly shortened. There is a lot of addition literature review here with only a few lines relating the material studied in this paper to known processes/effects published in the literature. I encourage the authors to trim this down and focus more on contextualizing their results within the current understanding of biostabilization and the state of modeling such processes (as alluded to in 616-617).