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Biogeosciences Discussions

Interactive comment on "On biotic and abiotic drivers of the microphytobenthos seasonal cycle in a temperate intertidal mudflat: a modelling study" *by* Raphaël Savelli et al.

Anonymous Referee #2

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Review of 'On biotic and abiotic drivers of the microphytobenthos seasonal cycle in a temparate intertidal mudflat: a modelling study' by Savelli et al.

Summary The manuscript describes a box model of combined mudflat temperature dynamics and benthic diatom growth. Snails are included as grazers. The model was constructed mostly from existing elements. The model was applied to a mudflat site on the Atlantic coast of southern France, using forcing from a meteorological station and tide gauge data for the year 2008. Simulated mud surface temperatures, diatom biomass and snail biomass and abundance were compared with two short data sets from a few weeks in February and July. Also, visual comparison was made between

simulated diatom biomass and a vegetation index derived from remote sensing data. The sensitivity of the model results to variations of a subset of the model parameters was assessed by a set of monte-carlo simulations analysed by calculating correlation coefficients between the varied parameters and diatom primary production, showing primary production collapse for most of the monte-carlo simulations, and fairly weak correlations for the simulations that did not collapse. The reference run was analised predominantly in terms of the seasonal cycle, limiting effects of light and temperature in relation to submergence/exposure and grazing, showing that temperature inhibition occurs around mid-day, light limitation in the early morning and late afternoon, and predominantly low levels of grazing except for a few episodic events.

General comments

I found this an interesting and well-written manuscript, which is appropriate for the journal. However, I have a number of major concerns regarding i) the validation, ii) the sensitivity analysis, iii) water temperature calculations, iv) snail model.

i) Validation

The two short periods of in-situ data are not sufficient to constrain the seasonal cycle. With these data, many other potential modelled seasonal cycles, including constant values (straight lines) could be equally valid results. The authors mention a monthly data set of chlorophyll observations from the same mud flat covering March 1992 to February 1993. A simulation for this period should be included and compared with the observations. The remote-sensing data are not really a substitute for this because they may have their own issues, and in the current manuscript are not the same variable.

ii) Sensitivity analysis

The sensitivity analysis leaves me puzzled. Why calculate correlation coefficients which assume linearity if the model equations are clearly non-linear? For which areas of the varied parameter space does the primary production collapse? Why? Why

was this subset of parameters selected and not others? Instead of randomised monte-carlo simulations and questionable statistics, I would think a series of graphs where primary production is plotted as a function of a varied parameter (with others set to reference values) would be much more instructive, or should in the least be used to analyse what happens in the monte-carlo simulations. Such an approach could even be used to restrict the range of variation of the parameters such that not so many simulations collapse (if the collapsing simulations occur towards the extremities of the parameter space). These new ranges could then also be critically compared with the ranges reported in the literature.

iii) Water temperature calculations

Eq. A7 should include S(mud_to_water). Also, the heating/cooling of the water column should be related to the instantaneous water depth. I can't find this in the equations. Are these just issues with the representation in the manuscript, or is the heat balance model flawed? This should be corrected.

iv) Snail model

I'm puzzled by the few sharp peaks in ingestion. Is this realistic behaviour or an artifact of the model? If the latter, could it be related to the exponent in Eq B11, which can change sign depending on the temperature? This seems odd from a mathematical perspective. Was this kind of behaviour of the equation envisaged/included in the range of values considered in the publication in which this relationship was proposed?

I have a number of additional concerns and suggestions that I will detail below. Overall, I recommend major revisions or rejection with encouragement to re-submit.

Further important issues

-It is suggested (p. 6, l. 10) that the mud temperature (fig 4) closely follows the air temperature (fig 3). This is difficult to see. Please include the air temperature in fig 4 for better comparison.

-The point above triggers the question if a full mud temperature model is necessary. This question could be easily addressed by driving the microphytobentos model with the air temperature (or air temperature when exposed and water temperature when submerged) and comparing the results with the reference simulation.

-p. 7, l. 30-p. 8, l. 2. 4xsignificant. I disagree. These differences are not significant, because the model mean is within the confidence interval of the observations.

-p. 8, light limitation. The definition is confusing. Also during the night, light is the limiting factor. Please use the full 24 hr period, not just daylight hours to represent this.

-Discussion. The authors provide a substantial number of numeric comparisons with published results throughout the discussion. This information is very difficult to digest in this way. Please compile a table of all these data/values/references, and present as part of the results.

-Table 1. I'm not sure if figures are allowed within a table - check journal requirements. This table doesn't seem to contain new information compared with the text (appendix B1). Ensure there is no duplication (delete table?).

-Appendix B1. dZ/dt is identical in the three cases. Please print only once. Also B4 is identical to B2 except for the formulation for tau - find an alternative way to present this without duplication.

Detail

-I've spotted several typos - please use a spell checker.

-p. 1, l. 10. events of biomass reduction when

-p. 1, l. 14. export flux: from, to?

-p. 2, l. 10. migrate towards

-p. 2, l. 27. De Jonge

-p. 3, l. 6. the composition of the benthic diatom assemblage?

-p. 4, l. 4. each set of measurements

-p. 4, l. 7. The reference to fig 3 occurs before the first ref to fig 2. Swap figures.

-p. 4, l. 7. consisted of

-p. 4, l. 25. spatially averaged

-p. 5, l. 31. This sentence is unclear.

-p. 6, l. 13. This sentence is unclear.

-p. 6, l. 15. First use, write out the names of the variables. Why these - there are many others (Table A3)?

-p. 6, l. 23-24. This sentence is unclear.

-Fig 5. Label graphs. Also plot 'original data' in the main figure for better comparison. Rephrase caption to make it clear what these original data are.

-p. 7. l. 2-5. Fall bloom. This seems less evident in the 'original data'? Is that true and if so why?

-p. 7, l. 28. one month

-Figure 9. The white colour is missing from the legends. For graph b, there is no grey. Is this actually the case or an issue with the figure? It seems that in graph b, T_opt was plotted, not T_opt_z?

-p. 9, l. 3. limiting

-p. 9, l. 17. key: why are these 'key' (and how is that defined)?

-p. 12, l. 29. Here, a section starts on salinity (it's not entirely clear to me where this ends). This is the first mention of salinity, and as far as I understand salinity is not represented in the model. So this paragraph seems a bit out of place. Either delete, or

argue why salinity was not included in the model in Methods, and then move this bit to a separate heading in the discussion.

-p. 30, l. 31-35. This contradicts statements in Results.

-Figure 8. Why does the vertical axis start at 5? The plot seems to suggests that this truncates the data in mid-summer?

-emersion (is it emergence?) and immersion are easily confused, please use exposure and submergence.

-Figure 1. the font size used for latitude and longitude may be too small.

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