

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-322

Anonymous Referee #2

Referee comment on "Biological response to hydrodynamic factors in estuarine-coastal systems: a numerical analysis in a micro-tidal bay" by Marta F.-Pedrera Balsells et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-322-RC2>, 2022

General comments

This paper deals with the influence of wind and freshwater discharge over the dynamics and phytoplankton distribution in a microtidal bay (Fangar Bay). Modeling experiments using a coupled physical-biogeochemical model with varying wind and freshwater input are used to show the large variability in phytoplankton concentration in the bay depending on the wind intensity and direction.

This paper is well written and clear. Figures are of good quality and a conceptual diagram of the processes affecting the Fangar bay is proposed.

The authors previously published a few papers investigating the hydrodynamics and biogeochemistry in the Fangar Bay, and this paper is presented as a complement to this previous series. In the 2020a paper, the authors used *in situ* and model data to study the hydrodynamics of the bay and to assess the influence of bathymetry on wind-driven circulation, the wind being constant. In the 2020b paper, the influence of river outflow on residence time is assessed. In the 2021 paper, *in situ* data of hydrodynamics and Chlorophyll-a are used to show the influence of the wind and stratification on Chl-a surface pattern. However, even if there is an attempt of explanation, more should be made on how this paper brings substantial new results compared with the papers 2020a, 2020b and 2021. Results on phytoplankton biomass could also be emphasized for their meaning in explaining the ecosystem functioning of the bay.

Another important concern is the model validation. Why showing only model/data comparisons at the surface (with satellite imagery for phytoplankton concentration) although the bay can be stratified, the surface/bottom biogeochemistry is contrasted and the model results are also explored at the bottom? In the previous papers 2020a and 2021, *in situ* data time series are available in summer and autumn along the water column, for physical and biogeochemical variables. Some of the wind situations shown in the present paper may have been encountered during the *in situ* experiments (indeed this is the case for NW winds). How does the model handle realistic situations close to the idealized experiments in this paper, why not considering realistic cases for comparison with *in situ* data? Is this left to a future paper? I suggest to add a discussion on how the results of the previous paper compare with previous observations.

I would recommend publication in Biogeosciences only if the previous and following comments are adequately addressed.

Specific comments

L. 136-137: In the introduction, the authors say that the model has been validated in the Fangar Bay, and refer to the appendix. This may mislead the reader, as the validation is presented only in this paper and was not done before.

L. 72: is there really a biological model? I think that you mean biogeochemical model. You claim that it is embedded into the hydrodynamical model, is it not rather coupled or forced?

L. 101: the authors mention NE winds of great intensity in the bay. However on page 4 (L.159) and in Figure 6, the wind is SE

Figure 1: Please add the position of control points M1-M4 in a separate Table.

L. 130-136: These lines are exactly the same than in 2020a (section 2.3, first paragraph). Please avoid copying the exact sentences from a previous paper.

L. 133: How are the 10 sigma levels distributed on the vertical? How is the bathymetry set up, as in the previous paper 2020b you used an idealized bathymetry “due to the difficulty of achieving good bathymetry”?

L. 151: What is the mole fraction of Chl-a?

L. 155 and Table 1: For experiment UW12fr, why choosing a channel flow of 3 m³/s? The wind blows for 3 to 5 days, which explains the choice of the experiments. However, from Figure 2, the results can be quite different after 3 or 5 days for phytoplankton biomass. This point isn't discussed in the manuscript, I suggest that you add the impact of wind duration variability in your discussion.

L. 161 and Figure 3: How is the initial stratification set up? You mention a previous paper (2021) as an explanation for the choice of your stratification profile. However it is not clear to me how you chose it. Is the salinity an average of the two profiles shown on Figure 6 of 2021 paper? I did not find any T plots on this paper, except at the bottom. Please provide more information, and mention that the initial salinity profile is shown on figure 3 of the present paper. For freshwater fluxes: the authors take a constant value of 7.5 m³/s for most of the cases, without differentiating between cases of NW and SE winds. However, as stated by the authors, SE winds are associated with local rain events, could this induce an increase of the channels outflow, or are the fluxes only driven by the rice cultivation activities?

L. 183: add reference to “(Figure 1)” at the end of the sentence.

L. 188: the authors say that “all simulations show larger concentrations of phytoplankton biomass at the surface due to freshwater fluxes”, and refer to later discussion. But at L. 196-197, they say that “for the UW10 and UW12 simulations (moderate and strong up-bay wind), both surface and bottom phytoplankton time series coincide at all control points”. Which of these two statements is correct?

L. 210: The authors say that there is a difference in growth rates observed between phytoplankton and zooplankton, without any reference to a figure. Not shown? It could help to add a Figure for zooplankton (in Appendix?).

L. 241-242: I think that you refer to “Figure 4” instead of “Figure 3”, and the difference shown by the figure may not be “UW10” at L. 242 because there is not any plot showing UW10 results, I guess it is “UW12”. In this case, you should not write “winds of similar intensities” at L. 241.

Figure 4 gives the spatial pattern of phytoplankton biomass and salinity at the surface and bottom of the bay. For the reader, it is easier to understand the spatial gradients and the bottom/surface differences from this figure than from vertical profiles at control points. I suggest to place this figure before Figure 3. Indeed you first use Figure 3 to explore horizontal differences between control points and this should be placed after the general horizontal maps. Also, it is not clear to me why nitrates do not appear anymore.

L. 264: add "and nutrient" after "freshwater".

L. 274-281: This part of the discussion could be moved to the introduction to better explain the purpose of the present study.

L. 298: The difference in nutrients availability along the water column depending on the wind direction is not discussed from figure 2.

L. 390-391: You mention the comparisons of modelled currents with observed current profiles in 2017. Where are the results for the comparison of currents along the water column? You should definitely add some model/observed data (shown in the 2020a paper for example) comparisons in the water column and not only at the surface.

L. 389-393 and Figure A2: What is the spatial coverage of the HRF radar? What are U and V on the plot, is it a spatial average or at a particular location?

L. 402: is the initial states of smaller domains only obtained from interpolation or do also you have to perform extrapolation? If the answer is yes, please add it in the text.

L. 413-414: I agree that the model reproduces well some events (in November especially), but the comparison is not so good for the U component around 15 October for example from Figure A2 and Figure A3. Moreover, Figure A3 shows that the v component is out-of-phase, and even if values are relatively small, the authors can not validate the model from this figure. To compare the general trends, I would suggest to show filtered data sets. Also, adding statistics (RMS, mean, bias, correlation...) would be very useful to show the model validation (this can be added in a table).

L. 418: the authors show a model validation for the 350m model resolution, is the comparison is for the Fangar Bay? (in this case, why not showing a comparison of HRF radar data with the embedded configuration B that is used in the present study (L. 418)?) This does not really make sense for me, the configuration used for the study is not validated in the paper. And there is no validation of hydrology, is there any SST image that could be used? There are hydrological data from the *in situ* campaigns.

Technical corrections

Typos and spelling:

L.68 : replace "spatial-temporal" by "spatio-temporal"

L. 65: replace "on" by "in"

L. 78: add "of" before "satellite images"

L.79: add "of" before "phytoplankton"

L. 137: replace "Annex 1" by "Appendix A"

L. 156: add "input" after "freshwater"

L. 215: replace "B1" by "Figure B1"

L. 229: add "input" after "freshwater"

L. 298: add "water" before "column"

Figures:

Figure 2: in the legend, replace "Solid lines shows surface numerical results, dashed line shows bottom numerical results" by "Solid lines shows the numerical results at the sea surface, dashed line shows numerical results at the sea bottom". It is very difficult to distinguish between the different curves on the plot. For a to c, bottom dotted lines are hardly distinguishable from surface curves. For all plot, the orange/red lines are hardly distinguishable. Please find a way to improve the figure. The concentration in nitrates has a maximum value of around 3 mmol/m³, so I suggest that you take 3 or 4 mmol/m³ as a maximum in the plot.

Figure 3: Replace "vertical Chl a profiles" by "phytoplankton biomass" in the legend to be consistent with the text describing the results.

Figure A2: add "horizontal resolution grid" after "350m".