Anonymous Referee #2

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 acknowledge the helpful comments and corrections of Reviewer 2, which helped to improve the quality of the manuscript. Below, each comment is answered point-by-point. We have analyzed your comments carefully. We have included the main corrections in the manuscript and the response to the reviewers' comments which we hope to meet with your approval. The answers or explanations are written in blue, and the newly added contents are in italics blue.

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.

We agree that the improvement and/or specific contribution of this paper is not sufficiently highlighted in the current version. The third paragraph of the introduction has been modified. The objective has been clarified and the link with previous works (e.g. F-Pedrera Balsells et al., 2021 and Llebot et al., 2011) rewritten. So, the objective of this work is now explained in lines 75-77.

The meaning and application of the ecosystem function in the bay is also explained in the new version of the manuscript (see new paragraph in L77-82). It is possible to combine this type of models, where the biogeochemistry of the bay is analysed together with the hydrodynamics, with simpler models such as those of carrying capacity (Weitzman and Filgueira 2020; Guyondet et al., 2022) for better aquaculture management including harvest planning and early warning systems to avoid mortality (Hargreaves 1998; Yu and Gan 2021). They can even be extended to socio-economic study models of the area to cover all aspects related to aquaculture activity. Also, the use of hydrodynamic and biological models supports Nature Based Solutions (NBS) as an alternative to traditional engineering, with growing relevance to design integrated solutions for building coastal bay resilience (Pontee et al.,2016; F-Pedrera Balsells et al. 2020b) under climate change. Initial set of environmentally adapted alternatives in Fangar Bay may be: i) self-regulating connection with the open sea, ii) adjustable connection with land discharges or iii) adaptive reallocation of aquaculture activities; whose will require specific investigations on the hydro-biogeochemical response (L372-386).

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).

About the vertical variability, it has been observed in other works (Ramón et al., 2007; Soriano-González et al., 2019) that when strong winds blow, the surface data are similar to the integrated data of the whole water column consistent with our model results. In addition, it should be noted that this is a very shallow bay (4m max) and it is often difficult to distinguish the surface/bottom layers in the sampling. Our work focuses on spatial and temporal distribution instead of vertical variability which also is of great importance and not still understood in very shallow domains.

The new version of the manuscript includes a new qualitative comparison with numerical results and observations including order of magnitudes of the vertical variability (L256-267) using specific conversion factors. Throughout the discussion, previous work is cited and compared with the new results provided in this manuscript. E.g. lines 287-292, 294-298, 328-330.

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.

For this investigation of the influence of the physical variables on the biological variables, we decided to show “idealized” or “simplified” simulations results where the wind was constant but varying the direction in order to observe the dynamics of phytoplankton biomass as a function of wind throughout the water column. This is because the system is extremely complex in terms of hydrodynamic and biological variables as we stated in previous papers and similar domains (i.e. coastal shallow embayment’s and small estuaries) leading strong temporal and spatial variability (second paragraph of the Introduction reflects this behaviour and also encountered in the cited works: Demers et al., 1979; Díez-Minguito & de Swart, 2020; Masson & Peña, 2009; Mishra, 2012). To run “idealized” (i.e., controlled) conditions has proved more effective to understand the processes in contrast to using realistic cases (see F-Pedrera Balsells et al., 2020a).

However, we agree that this is a critical point that must be clarified in the new version of the manuscript. We have added two additional sentences highlighting this point in the Discussion which we think that may help the reader to understand the strategy used in our investigation (L298-309). Additional simulations have been carried out paying attention to long-term evolution (10 days). These simulations show pointless results (see figure R1), and even though we mention the analysis in the new version of the manuscript (see L301-310) we preferred not to extend this point to not confuse the reader. Figure R1 shows that the Chl *a* concentration reaches values up to 500 mg·m^{-3} at some of the control points. Probably, the reason for the senseless results is based on the fact that the model is not taking into account the phytoplankton consumption, so the values increase. It also does not take into account the interaction of nutrients, phytoplankton and zooplankton from inside the bay to the open sea. Also, we have tested long simulations (10 days) where the wind stops after 5 days. We have observed that it does not provide any direct conclusions and requires further analysis.

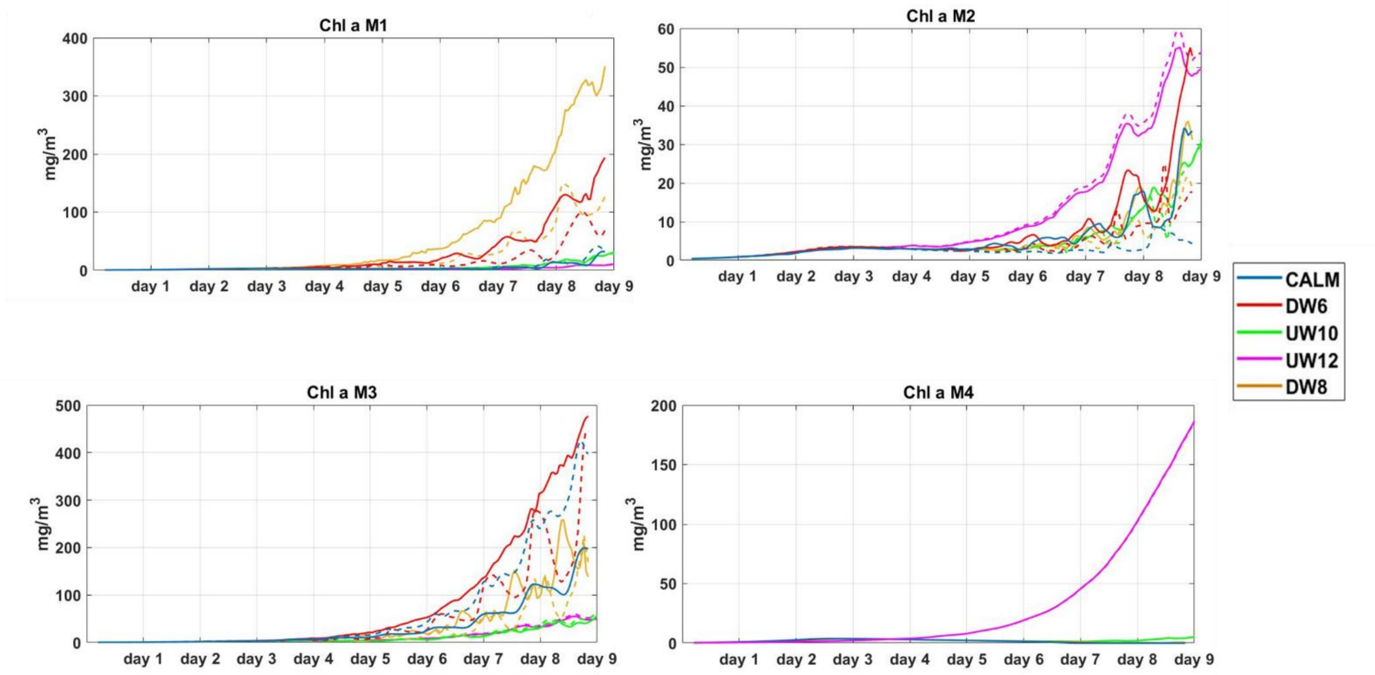

Figure R1. Time series of the Chl a at different points of the bay: (a) M1, (b) M2, (c) M3 and (d) M4. The different colours show the different simulations with in function of the wind. Solid lines show the numerical results at the sea surface, dashed line shows numerical results at the sea bottom.

Second paragraph of the Conclusions also addresses this point of using idealized simulations for an accurate analysis. As future work a new sentence has been added including the realistic simulations configurations set-up (see L423-426).

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.

As we pointed out before, the analysis is supported on idealized runs to discern the driving mechanics of the hydro and biological evolution. Because we are not using realistic (long-term) simulations results we think that including the validation in the core of the paper may mislead the reader. We think that the previous comments about idealized vs realistic simulations may also help the reader.

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?

L77: Ok, changed. It is a coupled model. Seems more suitable than forced, because the NPZD scripts run online is not "externally" 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

That is a typing error. The correct wind is SE. Corrected already in the text.

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

Added in the manuscript.

	Latitude (º)	Longitude (º)	Depth (in m)
M1	40.775306	0.720305	4.05
M2	40.767762	0.742785	4.02
M3	40.771534	0.735841	1.79
M4	40.758125	0.771917	0.93
BO	40.785970	0.709483	-
IM	40.766413	0.738546	-

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.

Modified.

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"?

L140: The 10 sigma levels are distributed according to the plot referenced in the article F-Pedrera Balsells et al. (2020b). Subsequent to that work we were able to obtain a more realistic bathymetry which is the one used for this work.

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

L153: 893.51 g/mol. Added in the text.

L. 155 and Table 1: For experiment UW12fr, why choosing a channel flow of 3 m3/s?

This was a mistake: 3.75 is the proper value that is half the flow rate that usually flows out of the drainage channels (7.5 $m^3 \cdot s^{-1}$). Clarified also in the new version of the manuscript (L162).

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. Tis point isn't discussed in the manuscript, I suggest that you add the impact of wind duration variability in your discussion.

The complexity of the hydrodynamic and biological variables suggests to face the analysis using "idealized" conditions instead of realistic (and long-term) simulations. Also, we proved that these short simulations (of the order of the wind duration events) have been useful to understand the main coupled hydro-biological processes. A simulation length of 5 days has been chosen because it corresponds to the average duration of the wind events (Ràfols et al. 2017). This wind duration seems to be too short, but in order to understand the fundamental processes and the link of biological and hydrodynamic variables this duration was enough. Results of larger simulations have been also analysed (10 days), but those become unfeasible according to the observations of primary production (see figure R1) (L301-310).

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 sown on figure 3 of the present paper.

L164: The ROMS model requires initial and boundary conditions data for both temperature and salinity. The initial and boundary values introduced in the simulations have been taken from the campaigns of the mentioned article (F-Pedrera Balsells et al., 2021), where you can see the bottom temperature values in figure 2 and 3 and the salinity values in figure 6, as you indicate. These values are not an average but the exact values, which have been interpolated in the numerical mesh.

Added in the manuscript, line 166-167 "*Figure 3 shows the horizontal distribution of modelled salinity based on initial conditions interpolated from the observation shown in F-Pedrera Balsells et al. (2021).*".

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?

The freshwater flows in these channels are in principle only driven by rice cultivation activities. This chosen value is the average of the data measured in the channels (i.e. *in situ)*. Likely heavy rainfall increases the freshwater flow and may generate an interesting situation within the bay (including eventual mixing and saltwater flushing), but unfortunately no observations are available during these events. In the new version of the manuscript this point has been included as future work (see second paragraph of the Discussion).

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

L188: Done. Thanks.

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 upbay wind), both surface and bottom phytoplankton time series coincide at all control points ". Which of these two statements is correct?

L192-196: Right, this leads to confusion. The text has been amended to clarify this issue: "*stratification conditions (CALM and DW6 simulations) show higher phytoplankton biomass concentrations at the surface due to freshwater fluxes. Substantial differences in phytoplankton biomass between the surface and bottom layers are evident in M1, where stratification tends to be greater in contrast to the shallowest point (M4)*".

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?).

In this paper we focus in the primary production instead of zooplankton which has a larger growth rate in comparison to phytoplankton. The graphs do not show much of the data (Figure R2), so we have not included them in the manuscript.

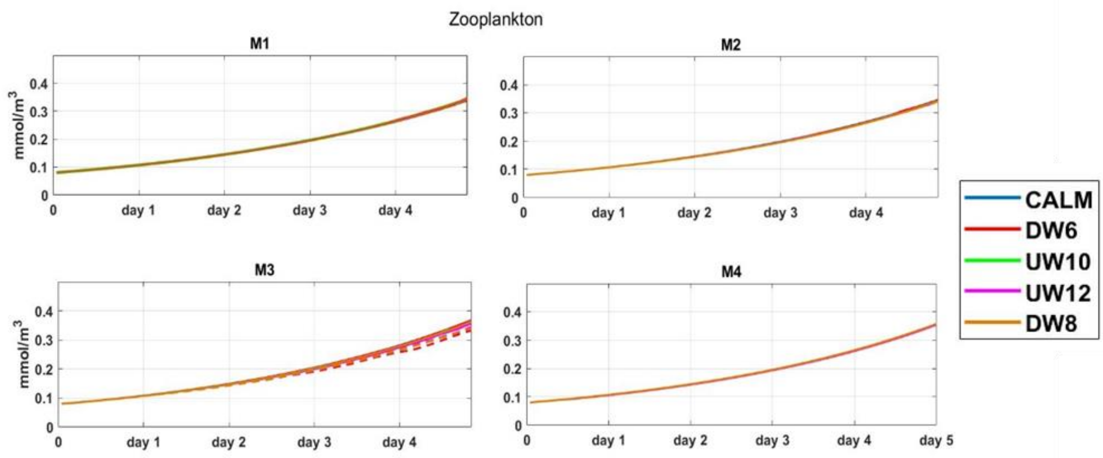

Figure R2. Zooplankton evolution at different control points for all the simulations.

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.

The explanation refers to figure 4 (before figure 3). The vertical profiles of phytoplankton biomass show how winds of the same intensity (12 $m \cdot s^{-1}$) but different direction (NW (UW12) - SE (DW8)) cause a completely different horizontal distribution of phytoplankton concentration. We have corrected the name of the simulation (UW12) in the text (L226).

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.

Ok, changed. Nitrates at the end of the simulations also have been added in Figure 4. We modified also the manuscript according to these results: *As it can be seen in Figure 4c, there is nutrient input at the initial moment (continuous lines) and then there is a consumption by phytoplankton that reduces this concentration (dashed lines). With this simple model, where there is no further contribution of nutrients neither by the suspended sediment nor by the input from the open sea they are almost depleted by the end of the simulation* (L246-250)

L. 264: add “and nutrient” after “freshwater”.

284: Done, thanks.

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

Done, thanks.

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

L321-323: "*When strong NW up-bay winds blow in Fangar Bay, the water column homogenises, making nutrients available throughout the column, both at the surface and at the bottom (i.e., UPW10 and UW12 simulations)".* → this can be seen by looking at the continuous and dashed lines, pink and green, overlapping at points M1, M2 and M4 in figure2.

L325-328: "*With strong SE down-bay winds (DW8 simulation), phytoplankton biomass increases near the discharge channels and the phytoplankton biomass distribution follows the water circulation driven by SE winds*" → The same is true for the DW8 simulation (brown line). The solid and dashed lines overlap showing how the water column homogenises when these winds blow.

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.

Qualitative comparison of the idealised coupled simulations and *in situ* data and remote sensing provides robustness to our analysis. The increase of phytoplankton in the first days of the wind event numerical simulations are consistent with the conclusions drawn by F-Pedrera Balsells et al. (2021) which pointed out that wind episodes causes an increase in the concentration of surface Chl *a*. Chlorophyll *a* field data collection were obtained using seawater samples (F-Pedrera Balsells et al. 2021) and the range obtained after wind events were 4 $mg{\cdot}m^{-3}$ and 7 $mg{\cdot}m^{-3}$. These values agree with the values obtained after 5 days of wind simulations (e.g.~ 3 $mmol{\cdot}m^{-3}$) assuming a 1.59 g chlorophyll per mole N conversion suggested by Gong et al. (2015) (L256-263). We can see in F-Pedrera Balsells et al. (2020b) that the magnitude of the current of the simplified model (figure 11) is similar to that of the observations (figure 6) (0.1 $m{\cdot}s^{-1}$, added in L299), so we were able to make a qualitative assessment.

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 435-438: U and V are the zonal and meridional components of the current at a random offshore position. This has been specified now in the manuscript, and a reference to a paper describing the HFR system and its characteristics has also been added.

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.

L443: No. The initial state of the different domains is obtained by interpolation from the respective parent domains.

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 cannot 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).

L455-457: Yes, the authors agree that it is difficult to validate a model when both measured and simulated values are so small, and possibly within the error range of the instrument and the model for a significant portion of the observational period. In addition, measurement errors can be aggravated due to the likely presence of very fine suspended sediment in the water column (Fangar loosely translates as "place of mud"). Under these conditions, small variations in the velocities can lead to large errors in model performance. We believe that a Lagrangian validation scheme would have been preferable, instead of a Eulerian approach, but this was not possible to do.

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.

459: The idea behind the validation procedure was to validate each one of the domains to ensure the robustness of the nesting scheme. There are no field data that can be used simultaneously for both grids, and there is some difficulty in knowing all the natural data needed to enter them into the model. On the one hand, the HFR spatial coverage does not include the bay (as can be seen in Lorente et al., 2015. *Evaluating the surface circulation in the Ebro delta (northeastern Spain) with quality-controlled high-frequency radar measurements*, Oc. Sci., 11(6), 921-935) and, on the other one, the 350-m grid is too coarse for us to expect a decent hydrodynamic description inside the bay. Therefore, a separate process validation has been carried out for domains A and B.

Technical corrections

Typos and spelling: All done, thanks for the corrections.

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: All done, thanks for the corrections.

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”.