GENERAL COMMENTS

This paper is an excellent example how the CMEMS hydrodynamic solutions (or similar) can be useful to support coastal management. A lot of consultancy work is done assuming that the coastal areas do not present relevant 4D hydrodynamic variability. In some cases this can be valid but not in the case of Alfacs Bay and many other. As a consequence, the scientific community should not only be proposing new concepts (e.g. numerical discretizations, different methodologies on quantify the general concept of “water residence time”) but also present methodologies on how these “new methods” should be applied in efficient way and with controlled costs to support complex decisions in highly socio-economic sensitive coastal areas. This paper is an excellent effort in this direction. This paper address areas where some guidance should be given to coastal marine modelers:

How to define realistic boundary conditions? In this paper the focus is in the open boundary conditions but land/surface/bottom boundaries are also properly addressed: how to improve open boundaries integrating regional scale operational model results (e.g. CMEMS); when realistic boundary conditions should be used and when it is acceptable the use of schematic ones. In this paper the authors are also faced with the problem of imposing a freshwater flux along the land boundary based in generic seasonal data: which simplifications can be assumed and how this can influence the model results.

Which valid methods should be followed to have a hydrodynamic model forced with realistic conditions with a proper spatial discretization? In this case a one-way nesting approach was assumed with two nesting levels; How should it be validated a 4D hydrodynamic model? How hydrodynamic model results can be used to support water quality problems? Is it required to implement also a 4D biogeochemical model or computing “hydrodynamic time parameters” based in the model hydrodynamic results can be a good option? How about sub-grid parametrization. How can this impact the “hydrodynamic time parameters” results? In a complex model implementation like the one described in this paper a lot of options must be adopted.

In my opinion the paper will be improve if some of these options are better explained: Why 12 layers and not more or less? Open boundary condition: Clamped vs Flow Relaxation. Options related with the subgrid parametrization (e.g. what values were assumed for the turbulent viscosity and
diffusion of heat and mass coefficients?);

Why an eulerian approach to compute the “hydrodynamic time parameters” and not a lagrangian one that is able to avoid numerical diffusion problems associated with the advection term?

Dear Referee, Thank you very much for your insightful comments and suggestions. These are very valuable and helpful for revising and improving our paper.

Most of the proposed questions by the referee are extremely interesting and useful in order to conduct modeling works in coastal areas. In this sense, the improvement of CMEMS products to nest high resolution coastal models, the impact of the boundary conditions or the influence of spatial resolution will deserved a paper for itself. Therefore, many of the specific comments face these points and we reply properly trying to avoid an enlargement of the manuscript. As we explain in the point-by-point reply some of the decisions made (e.g. spatial/vertical resolution) in the modelling effort are based on our extensive background in to modelling the Bay including calibration and validation excises (see Cerralbo 2014, 2016 and 2018). However many of the questions may involve extensive sensitivity tests, which is out of the focus of our manuscript. In this sense, a revision has been made to our manuscript in accordance with these recommendations.

The response to each one of the reviewer’s comments and the corresponding correction to the paper are explained in detail. Once again, thank you very much for all your help in reviewing our paper. Kind regards,

Scientific significance: The scientific contribution of this paper is focused in the methods. There is a vast variety of concepts, ideas and data being produced by the scientific community focused in the transport of heat, mass and momentum in coastal environments but there is a lack of papers presenting clear methods to support decision making in which concerns the numerical modelling of the momentum, mass and heat transport in coastal areas that I’m more familiar. I rate this paper scientific significance as good. Scientific quality

Specific comments

Page 2 - line 16 – “. . . based on activities that depend on primary production, such as agriculture, fisheries and aquaculture.” The link between marine primary production and agriculture it is not fully clear. In the North of Portugal there was an antient practice of use seaweed as a fertilizer in agriculture. Are the authors referring to something similar?

In this point, the authors are referring to the economy of the area (not only the primary production.). We believe there was a misunderstanding here. For that reason, we have done some minor changes in the text. We believe now it is clearer.

Page 4 – Line 1 – " Cerralbo et al. (2015) found that during warm periods the salinity distribution shows strong vertical gradients . . .". The way this is stated may be a little bit misleading. In fact this happens in periods of low wind intensity that are more frequent in warm periods.

Yes. Thanks. We agree that the way it was written and the place in the text could induce to some errors of comprehension. For that reason, the changed text has been moved to the description of Study Area.

Page 4 – Line 24 – It would be interesting to detail how the nesting it is done between the two ROMS models: the two models run at the same time and every time step the "father model" solution is interpolated for the "son grid" boundary cells or the "father model" runs first and the data is stored
every X seconds in a file and the "son model" runs in a second step?

Ok. We agree and a sentence has been added:
"The nesting is off-line, first D-A simulation is performed and the hourly results are used for the boundary conditions of D-B."

Page 4– Line 25-26 – The justification for the adopted spatial discretization (_70 m horizontally and
12 sigma layers vertically) could be improved. Usually this is a critical point when implementing a 3D (in space) hydrodynamic model. Why dx _70 m is necessary to capture correctly the variability in the inner bay? The same question can be raised for the number of sigma levels. Why 12? They have the same relative thickness?
It was done any sensitive analysis to check if the model results change significantly for different horizontal or vertical discretizations? I'm not familiar with the ROMS model implementation details but I know that it allows the user to do some "vertical stretching" (S coordinate). This way it would be possible to increase the resolution where stratification is more intense (e.g. halocline depth) by aligning the sigma layers with the isopycnic lines and minimize the numerical diapycnal mixing. Was this option considered?

In Cerralbo et al. (2016) there are explained in more detail some of the options (e.g. bottom rugosity height). But it would be beneficial to provide a more detailed explanation for the vertical discretization.

The horizontal resolution are associated at the compromise of the numerical resources and the physical process that we want to solve. The minimum horizontal discretization was established in order to simulate properly the mouth. In this case, the mouth section is discretized in x points and the vertical layers was 12. Also we use vertical stretching for the terrain following coordinates: surface stretching parameter = 7.0 and bottom stretching parameter = 0.4 using Song and Haidvogel (1994) stretching function. This configuration allows to increase the resolution in the upper layer where the surface boundary layer takes place due to the wind action. The transformation function used is described in Shchepetkin and McWilliams (2005) denoted as an unperturbed coordinate system since all the depths are not affected by the displacements of the free surface.

The paper has been improved adding the paragraph:

"A surface stretching parameter (= 7.0) and bottom stretching parameter (= 0.4) for the Song and Haidvogel (1994) stretching function has been used. This configuration allows to increase the resolution in the upper layer where the surface boundary layer takes place due to the wind action. The transformation function used is described in Shchepetkin and McWilliams (2005) denoted as an unperturbed coordinate system. ".

Page 4 – Line 31. It is described the turbulence closure scheme assumed vertically but not horizontally. Additionally it would be important to mention the advection scheme used horizontally and vertically for momentum, mass and heat transport.

Ok. We have changed the sentence in order to provide the aforementioned information:

In order to represent the processes at scales smaller than the grid resolution we used anisotropic horizontal and vertical turbulent schemes based on a Generic Length Scale (GLD) formulation (Warner et al, 2005). K-epsilon parameters are used for GLS formulation. Also Kantha and Clayson stability function formulation is used (Kantha and Clayson, 1994). For advection scheme a third-order upstream horizontal fluxes is used. For heat and mass tracers, a biharmonic mixing scheme along geopotential surfaces is used.

Page 5 – line 6-7. "The variability of currents along the water column (baroclinic component), temperature and salinity are imposed from CMEMS-IBI daily average values with clamped conditions". Two comments: It would be interesting to explain a little better how the baroclinic velocity required to the ROMS boundary condition is computed? U baroclinic (i,j,k,t)= U CMEMS (i,j,k,t) – U CMEMS barotropic (i,j,t) and both CMEMS are interpolated in time for each t instant ? Why had been choose clamped boundary conditions ? Was it also considered the use of nudging layers as an alternative to a clamped boundary condition? If not why? Usually in the literature for coastal and ocean 3D hydrodynamic implementations nudging layers is the methodology recommended. Marchesiello, P., J. C. McWilliams, A. Shchepetkin (2001): Open boundary conditions for long-term integration of regional oceanic models. Ocean Modelling 3, 1-20, 2001. Palma, E. D. and R. P. Matano, 2000: On the implementation of passive open boundary conditions for a general circulation model: The three-dimensional case. Journal of Geophysical Research, 105,. 8605-8627 (2000).

A lot of question here:

1. Ok. The way it was written lead to some confusion. Basically we are referring to baroclinic currents when using the 3D currents, and barotropic when using depth averaged water currents. No more treatment is done. In order to clarify this, we have re-written the text, specifying vertically depth averaged water currents (when saying barotropic) and 3D variables (T, S and water currents). We believe now everything is clearer.

2. 3D values in the OBC are imposed with daily mean values (is the only values CMEMS-IBI provides), and 2D values (depth integrated water currents and sea-level) is provided hourly.

3. We have done many numerical tests trying to define the best OBC for the system (we are not talking about them in the manuscript). Some tests have included nudging schemes as the reviewer proposes (also trying different ways to impose the nudging area and considering different time values). However, the best results (both in skill assessment) and preservation of the continuity with the parent solution (IBI-CMEMS) have been obtained with Clamped for the 3D variables. Other similar applications have used similar configurations (e.g. Penven et al. 2006, Costa et al. 2012.)

Penven, P., Debreu, L., Marchesiello, P., & McWilliams, J. C. 2006. Evaluation and application of the ROMS 1-way embedding procedure to the central California upwelling system. Ocean Modelling, 12(1), 157-187.

Costa, P., Gómez, B., Venâncio, A., Pérez, E., & Pérez-Muñuzuri, V. 2012. Using the Regional Ocean Modelling System (ROMS) to improve the sea surface temperature predictions of the MERCATOR Ocean System. Scientia Marina, 76(S1), 165-175.

Page 5 – line 13. Why was it assumed 18 for the freshwater salinity concentration? This is based in observations? This should be better explained.

The freshwater from the rice-fields is mixed in some areas with water from a coastal Lagoon (L'Encanyissada). The water in this lagoon are considered as brackish waters, but no receantly measurements allows the authors to know or even calculate the mean salinity of these waters. For that reason, an arbitrary value of 18 has been used. However, and considering that the main objective of the manuscript is the comparison between simulations with modification of selected variables (flows or connections with open sea), while keeping the rest immutable, the authors consider that the value of 18 is correct for the purpose of this research. However, some text have been added in the discussion according to the referee suggestion.

In discussion, first paragraph:
"Errors in salinity could be related to the poor knowledge of the freshwater flows (total amount, spatial and temporal distribution) and the salinity of these waters (freshwater from rice fields mixed with brackish waters from coastal lagoon)."

Page 6 – Validation. A table with the statistic parameters (bias, RMSE, R) resulting from the comparison of model results with observations for each water/flow property should be presented.

We agree. We have added some skill parameters to Figure 3.

Page 6 – line 10-11. Why HF radar is only compared for one point? What was the criteria to choose this specific point? Was it considered to compare all HF radar observations intersecting the model domain? See the methodology followed in the validation of IBI CMEMS http://cmems-resources.cls.fr/documents/QUID/CMEMSIBI-QUID-005-001.pdf You can also look in to a conference abstract where it is presented some validation of a model (in this case MOHID model) implemented in the Algarve coast following a methodology similar to the one used in this paper.
http://www.mohid.com/PublicData/Products/ConferencePapers/Leitao_etal_5JEH_2018.pdf

We agree that one option is to perform a 2D validation over the entire domain. In this sense, there is already a similar validation done in a manuscript already in publication process in Journal of Operational Oceanography (Sotillo et al. 2019) for a similar configuration presented here. However, in this manuscript we prefer to show part of the time series in one point in order to clearly observe the good behavior of the model close to the bay in a point with almost data for the entire period.

Page 6 – Water Residence Time. Jouon (2006) do a very good review of the different approaches proposed in the literature to compute what Jouon (2006) calls "Hydrodynamic Time Parameters". In my daily work I usually characterize the "Water Residence Time" based in the parameter that Jouon (2006) named "Water Export Time" using a lagrangian approach (particle tracking model). Braunschweig F, Martins F, Chambel P, Neves R. A methodology to estimate renewal time scales in estuaries: the Tagus Estuary case. Ocean Dynamics. 2003; 53(3): 137-145. Jouon (2006) also follows a lagrangian approach to compute this parameter. The advantage of the lagrangian approach is to avoid the numerical diffusion problems associated with the advection term in the eulerian methods. However, in the eulerian approach the turbulent diffusion parametrization is more straightforward. Additionally the no flux land boundary condition in the eulerian methods is quite simple to impose while in lagrangian case is not so trivial (this problem is also mentioned by Jouon, 2006).

We agree with the referee that lagrangian method could also be used in here. However, in our initial test cases, the utilization of the lagrangian model of ROMS lead us to some problems not so trivial to solve. After performing different tests and methods, the one that provide us with more intuitive and useful results were the ones presented in the manuscript. We have added the reference of Braunschweig et al (2003) in the text to explicitly refer to the lagrangian methods.

Page 7 – line 13-14. It would be important to describe the methods used to compute advection (e.g. TVD ???) and turbulent diffusion (e.g. values of the horizontal turbulent diffusion coefficient) horizontally and vertically in the transport of the conservative tracer. One of the goals of this paper is to compute "hydrodynamic time parameters" using an eulerian method. In this case numerical diffusion associated with: advection numerical discretization, over estimation of horizontal turbulence (e.g. very high turbulent viscosity/diffusion coefficients), numerical diapycnal mixing can have a have a strong impact over the results. The impact of the advection numerical diffusion is briefly discuss by Jouon (2006) (TVD vs Upwind).

Information about the advection scheme used for the mass and tracers are included in the new version of the manuscript (see also previous comment referred to numerical model implementation). These schemes are also selected in previous modelling efforts in Alfacs Bay with good results in terms of skill assessment (see Cerralbo et al.2014, 2016,)

Page 7 – line 14. Why the focus was the surface layers? It is because the main source of stress over the mussel's production is high temperatures? I would aspect the bottom layers would be the ones presenting from a general point of view more intense water quality problems (e.g. oxygen depletion);

The main idea is to study the water quality parameters in the bay (both SST and water e-flushing times) in the well mixed surface layers (above the pycnocline at 3-4m, and where the most of the mussels production is located). For that reason, the analysis have focused on the surface layers.
We agree with the referee that problems like oxygen depletion (not covered by this manuscript) are more related with bottom circulation, and for that reason in the discussion we have added a sentence suggesting to do similar studies but considering the bottom circulation in the framework of problems related to oxygen depletion and turbidity.

Page 7 – line 22. If I understand correctly TFT (total flushing time) is compute averaging the LFT (local flushing time) for the entire bay (surface layer). For me is more consistent to average first the concentration in the entire control volume of interest (in this case the Alfacs bay – surface layer) and compute the TFT to be equal to period necessary to the average concentration to go from C0 to C0/e. This is the methodology proposed by Jouon (2006). Myself when I want to check if my lagrangian approaches are consistent I use a similar eulerian methodology.

We agree. We have re-done the analysis following the suggestion of the referee. In addition, we have changed the text *("(…) being TFT equal to the period necessary to the average concentration of the entire Bay to go from C_o to C_o*e-1 (...)")* and corresponding values in the table.

Technical corrections

Page 19 - Figure 6. Maybe it could be considered another colormap. It is a little bit difficult analyse the figure. A rainbow or similar colormap could be preferable.

Ok. We have changed the color scale and now the analysis is easier.