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Discussions

Interactive comment on "Use of a hydrodynamic model for the management of the water renovation in a coastal system" *by* Pablo Cerralbo et al.

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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: âĂć 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. âĂć 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; âĂć How should it be validated a 4D hydrodynamic model? âĂć 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? âĂć 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: âĂć Why 12 layers and not more or less? âĂć Open boundary condition: Clamped vs Flow Relaxation âĂć Options related with the subgrid parametrization (e.g. what values were assumed for the turbulent viscosity and diffusion of heat and mass coefficients?); âĂć 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? 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

The followed methodology (from a general point of view) is the right one to support the questions the paper wants to answer. Some options in the numerical model should better explained and discussed. I rate this paper scientific quality as good.

Presentation quality The paper is very easy to read. The results and conclusions are clear. The references are relevant and in the proper amount. The figures have good quality (there is an exception that will be mentioned in the technical corrections section). I rate this paper presentation quality as very good.

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 practise of use seaweed as a fertilizer in agriculture. Are the authors referring to something similar? 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. 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? 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

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Discussion paper

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

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).

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).

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);

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.

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.

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