GENERAL COMMENTS

In this paper, the authors present an application of a numerical model ROMS to a small bay in NE Spain in order to study the water renovation times and possible implications on water quality. The model is used to examine several coastal zone management scenarios that can be undertaken in order to improve the exchange of water in the bay. These include increased freshwater inputs from rice fields and a construction of an artificial channel of various widths through the Trabucador Bar in order to connect the inner Alfacs Bay with the sea. It is a very interesting contribution and the paper is well structured and easy to follow. Presentation of the results is clear, especially the figures and tables. It is also a very nice demonstration of the usefulness of having the Copernicus Marine Environment Monitoring Service as an enabler of downscaling of numerical models to a coastal zone in order to assist with the coastal zone management. I would like to see this paper published, as I think it will be of wide scientific interest. However, I recommend the following revisions to be undertaken by the authors before this paper is accepted for publication, especially that there is still scope (in terms of the size of the paper) to expand the paper to include some more and important, in my opinion, details.

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

Specific comments:

1. Validation: The quality of the paper will be strengthened if more validation results of the numerical model are presented. In particular:

a. Why validation against the HF Radar is only limited to the sampling station T and why validation is only limited to 3 months, whereas validation against temperature and salinity is presented for a full year?

We think that there is a misunderstanding here. The HF-R is validated in one point (HF-R, in Figure 1), and for the entire year 2014. However, the Figure only shows a period of three months in order to facilitate the understanding of the image (one year long does not allow to clearly observe the good behavior of the model compared with the HF-Radar). In order to clarify this point, some text has been modified in the validation paragraphs and figure captions.

b. Some basic stats would be very useful, e.g. RMSE, for T, S and currents to accompany the results presented in Figure 3, especially that the authors claim a 'remarkable' agreement between the model and observations (p.9, ln. 2), which is a very firm statement and should be confirmed by very high values of stats. Otherwise, I recommend not to claim a remarkable agreement, or define the scale somehow. See Sutherland et al. (2004) for an example of a model skill assessment method: Sutherland, J., Walstra, D.J.R., Chesher, T.J., vanRijn, L.C., Southgate, H.N., 2004. Evaluation of coastal area modelling systems at an estuary mouth. Coastal Engineering 51, 119-142. The standards of model skill assessment are not very well established and remarkable, vey good, poor, etc., model scores are too frequently used subjectively.

We agree with the reviewer. Some statistics have been added in the Figure 3 and text in order to explain the behavior of the model.

c. From section 2.5 I understand that some good salinity measurements exist across the Alfacs Bay, since it was possible to apply the Officer (1980) box model to it. If so, the authors should present validation of the model against salinity, not only at location T, but also at other available locations. The authors also state that there were weekly CTD casts taken, and location T is only one of them.

The field campaigns used for the calculation of the box model are for different years (2012-2013). For that reason, it was impossible to use it for the validation.
The other weekly CTDs casts taken during 2014 were performed close to the T point (not covering a wide area). For that reason, the authors consider that is enough with validation at T point. In the next figure it is shown the SST and SSS validation for another point inside the bay (note that the behavior is very similar).
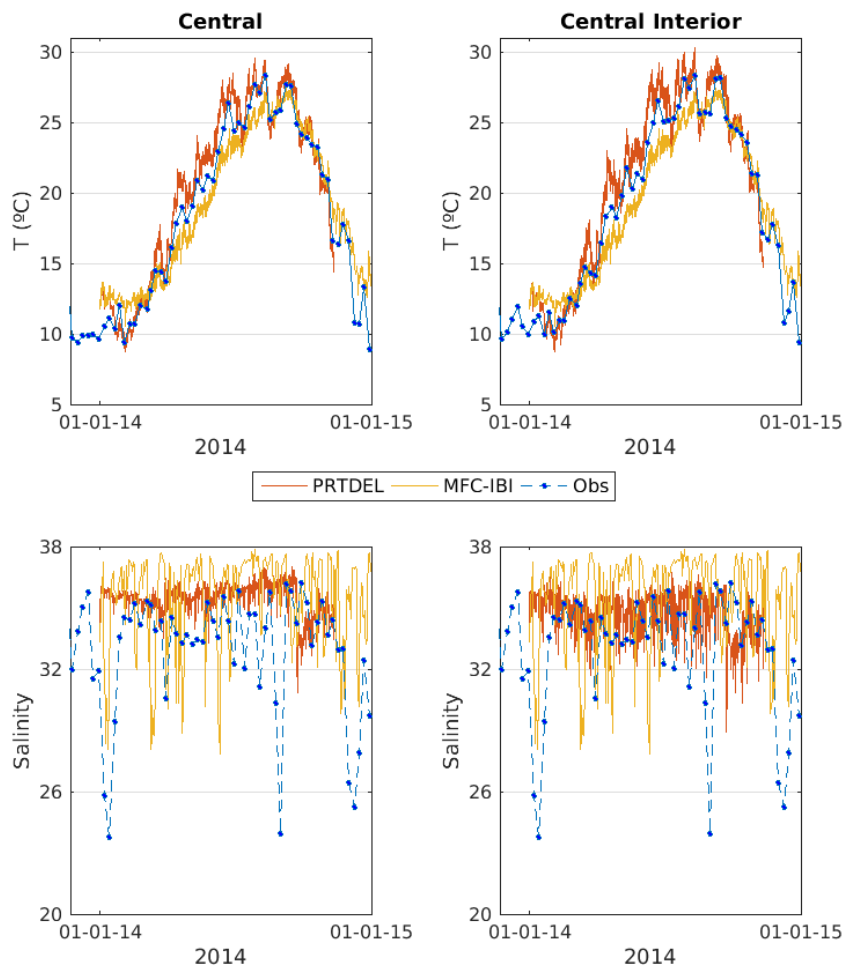
d. I understand that there is no tide gauge in Alfacs Bay in order to validation the model against the water level?
Yes. There is no data for sea level.

2. Numerical model: I have three comments here that I would like to see addressed:

a. This comment is related to 1(d) above. From the description of the model set-up, I understand the model is forced with 1-hourly data from the CMEMS-IBI model. What is the amplitude of tides in the region? The high and low water levels can be cut-off when using 1-hourly forcing resulting in not so-good representation of tidal circulation in the bay. This information will be of wide interest to the scientists trying to force coastal models with 1-hourly data in strongly tidal regions.

Yes. The model is forced with 1-hourly data from CMEMS-IBI model. Although not presented in this contribution, we have done different tests trying different OBC (open boundary conditions) in similar configurations for differents Spanish harbours. One of the options was to use only the CMEMS-IBI for the water velocities, salinity and temperature, and use the tidal information from an atlas (amplitude, phase, ellipses) to allow ROMS to compute internally the tides. That method presented some inconsistencies at the contours due to the tidal currents from CMEMS-IBI and the tidal currents computed from the atlas. In the Mediterranean sea harbours no difference where observed.

b. Why is the salinity of incoming freshwater flows set at 18? I know that for stability reasons it is generally advised not to use salinity of 0 in ROMS, but some small value, e.g. 1-2. However, 18 seems excessive. Are the intended freshwater input 1m3/s and 10m3/s (p.5, In. 12)? If so, prescribing the salinity of 18 implies much lower effective freshwater input. This needs to be clarified.

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)."

c. It will also be of wide interest to the modelling community if the authors provided more details on 'to avoid land contamination of the atmospheric forcing . . ..' (p.5, ln.11).

OK, the text has been modified in order to clarify this point.

" To avoid land contamination of the atmospheric forcing on coastal areas (e.g. heat fluxes and winds), a prior land mask is applied to the forcing data, and then variables over the sea are interpolated on the land. "

3. Water residence times:

a. Related to comment 1(c) it would be good if authors included a Table with the values of S, Q and E used in the Officer (1980) box model.

Ok. We have added the information of the salinities in the different layers in the manuscript, whilst the Q from the rice fields was already described (10m3/s). We also have added in the response the figure summarizing the salinities and the corresponding flows from the model.
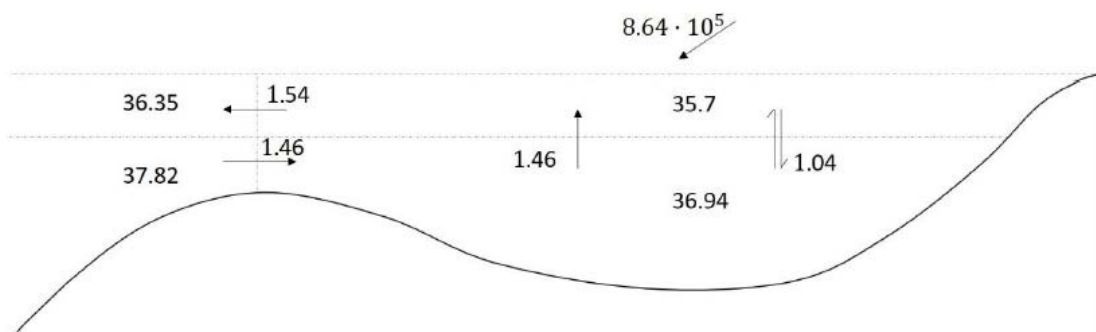
b. It will also be beneficial if the authors provided more details on the definition of LFT and TFT for quick reference for the readers. I appreciate it is provided by Jouon et al. (2006), but a brief overview will be useful. There is a plethora of the definitions of the flushing, e-folding, residence, renewal, etc., times, and the reader will benefit of a precise definition of LFT and TFT in this paper, even if it entirely follows Jouon et al. (2006). See also my related comment 4(a) below.

This point has been addressed following the suggestion in 4.a (see below)

4. Results:

a. P.7, ln.21 'When the total flushing time (TFT). . . '. I am not convinced that TFT is simply an average of LFTs. We are dealing with exponential functions describing the decrease of tracer concentration in the bay or sub-region of the bay (see Figure 4(a)). If TFT is defined same way as LFT, e.g. as a time needed for tracer concentration to drop to 1/e of C0 then this time should be computed separately for the entire Alfacs Bay by finding the time needed for the average concentration in the entire Bay to drop to 1/e of C0. This will not be the same as averaging LFTs. This is one of the reasons I asked for precise definitions of LFT and TFT in my comment 3(b) above.

We agree. We have re-done the analysis following the suggestion of the referee. And 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.

5. Discussion:

a. The authors say that there are many ways to compute residence times (p.9, ln.9) and further they claim that the most complete method is to compute LFT and TFT using a passive tracer simulations in a numerical model. Given that LFT and TFT are defined as e-flushing times (time needed for the concentration to drop to 1/e of C0) and we have a luxury of having a numerical model of the bay, there are actually more accurate methods. The e-flushing time approach as a representation of residence time is valid under the assumption of complete mixing in the bay at all times, i.e. tracer is evenly distributed in the bay at all times, which is simply not the case in a real situation, and in the Alfacs Bay. The residence time being equal to e-flushing time in the case of a fully mixed waterbody can be derived analytically. Having the numerical model in place and the predicted tracer decay in it, there is actually a more accurate method to calculate flushing (residence) time. This is the approach proposed by Takeoka (1984), whom authors actually quote. Residence time is an integral of a remnant function (from zero to infinity). The remnant function can be approximated by an exponential function proposed by Murakami (1991), r(t) = exp(-A*t)ˆB, which can be easily integrated to obtain residence time (Murakami, K., 1991. Tidal exchange mechanism in enclosed regions. In: Proceedings of the 2nd International Conference on Hydraulic Modelling of Coast Estuary and River Waters, vol. 2, 111-120.). This is certainly more complete than simply using the 1/e condition. It is still fine for the authors to use the e-flushing time, but precise definitions are needed and it is certainly not the most complete method and it should be discussed in the paper. E-flushing time is e-flushing time and it is not the same as residence time or water renovation time unless we are dealing with a fully mixed waterbody, as explained above. Several examples of the application of Takeoka and Murakami methods exist for the Irish Sea, e.g. Dabrowski et al. (2012). Determination of flushing characteristics of the Irish Sea: a spatial approach. Computers and Geosciences, 45: 250-260.

Ok, we agree with the reviewer. In order to clarify this point we have modified some text reflecting the concept that other methods are also adequate for environments like this, and the fully mixed water body constraints the e-flushing time related to residence times. However, we still believe that the e-flushing time, as it has been used here is a good proxy for the idea (or concept) of residence times. The referee affirms that the e-flushing times is only valid as a residence times when dealing with a fully mixed waterbody. For that reason, our analysis focuses on the surface layers, which is well mixed above the pycnocline at 3-4m. We have added some lines in both methods and discussion about this topic. The reference of Dabroswki et al. 2012 has been added.

6. Conclusions:

a. Conclusions can be expanded to include recommendations for the future research and developments in the area of research covered by the paper

Ok, we agree. In this sense, we have added the following text:

"Future works should include the analysis of the wave effects on water the circulation, as well as the consideration of different initial conditions and met-ocean conditions on the determination of water renewal in Alfacs Bay."

b. I am in doubt as to the following conclusion drawn in the paper, namely 'only the modification of freshwater flows is recommended due its lower impact on the environment. . .'. How about the impact of freshening of the bay? Surely it will exert some, possibly significant, stress on marine biota. Also, high temperature is identified as one of the stressors, and yet, as stated by the authors, the freshwater from rice fields is of high temperature and so it will make matters even worse? How about nutrient enrichment? Is the freshwater from rice fields not rich in nutrients? I think it deserves a more thorough discussion and more thoughts should be given to the conclusions drawn. Some discussion of a relationship between residence time and water quality is presented, for example, in Nash et al. 2011. Modelling phytoplankton dynamics in a complex estuarine system. Water Management, 164(1): 35-54.

Ok, we agree. For that reason we have added some new text.

However, the effects of increasing the freshwater sources could lead to some disturbances over the bay: e.g. stress over the marine biota and nutrient enrichment (increasing the risk of HABS under some conditions). For that reason, future works should consider the application of biogeochemical models (e.g Nash et al. 2011) in the bay characterizing the ecological behavior of the bay and performing numerical simulations in order to understand the effects of such modifications.

<u>Technical comments:</u> Overall the paper is well structured, easy to follow and English is good. Figures and Tables are nicely presented also.
p.1, ln.1: change "Delta Ebro" to "Ebro Delta"
Ok. Thanks. Done

p.1, ln. 15: leading "to" high rates
Ok. Done.

p.1, ln.19: change "consists in" to "consists of"
Ok. Done.

p.1, ln.26 change "low renovation" to "poor water renewal"
Ok. Thanks. Done

p.2, ln.1: change "-" to ","
Ok. Thanks. Done

p.2, ln.1: insert "and are" after "aquaculture"
OK.

p.2, ln.2: change "-" to "," and add "e.g." after the comma
We have added e.g. But we prefer to keep the '-'

p.2, ln.2: change "communication" to "exchange"
Ok

p.2, ln.16: "Ebro delta" should read "Ebro Delta" here and throughout the manuscript
Yes. done

p.2, ln.29: "Alfacs bay" should read "Alfacs Bay" here and throughout the manuscript
Ok. Done

p.2, ln.30: change "sense" to "context"
Ok. Done.

p.3, ln.13: change "on the east" to "in the east"
OK

p.4, ln.25: comma missing before "respectively" here and throughout the manuscript
Ok. done

p.4, ln.26: change "transference" to "transfer"
OK.

p.6, ln.6 expand IRTA (despite it being explained in the affiliation)
OK. Done

The remainder of the manuscript seems to be mostly free from the small errors like above, except:
p.10, ln.18: insert comma after "regions"
OK. Done.

p.10, ln.30: change "increase" to "improving".
We believe increase is more adequate here.