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Ocean Science
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.

Anonymous Referee #3

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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.

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?

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.

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.

d. I understand that there is no tide gauge in Alfacs Bay in order to validation the model against the water 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

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, ln. 12)? If so, prescribing the salinity of 18 implies much lower effective freshwater input. This needs to be clarified

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)

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

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

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.

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)^B, 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.

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

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.

Technical comments: 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" p.1, ln. 15: leading "to" high rates p.1, ln.19: change "consists in" to "consists of" p.1, ln.26 change "low renovation" to "poor water renewal" p.2, ln.1: change "-" to "," p.2, ln.1: insert "and are" after "aquaculture" p.2, ln.2: change "-" to "," and add "e.g." after the comma p.2, ln.2: change "communication" to "exchange"

p.2, ln.16: "Ebro delta" should read "Ebro Delta" here and throughout the manuscript p.2, ln.29: "Alfacs bay" should read "Alfacs Bay" here and throughout the manuscript p.2, ln.30: change "sense" to "context" p.3, ln.13: change "on the east" to "in the east" p.4, ln.25: comma missing before "respectively" here and throughout the manuscript p.4, ln.26: change "transference" to "transfer" p.6, ln.6 expand IRTA (despite it being explained in the affiliation) 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" p.10, ln.30: change "increase" to "improving".

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