Review of "Modelling river-sea continuum: the case of the Danube Delta", by Ferrarin et al.

This is the second review of the manuscript "Modelling river-sea continuum: the case of the Danube Delta", by Ferrarin et al. Structure and quality of the text was much improved, but several of the more fundamental points have not been fully addressed. The manuscript would still benefit from the development of a more consistent story line and the formulation of a research question. The authors added a research statement in the introduction focusing on process-oriented model analysis, without providing a definite goal for the analysis. As a consequence, the manuscript demonstrates qualitatively the SHYFEM model's ability to produce realistic results, but it does not necessarily provide sufficient information, for all processes and parameters, to verify the accuracy of the model results quantitatively as well. The scope of the model study is rather broad and covers not just river-sea continuum, but also coastal processes, like upwelling. The model validation study is limited by the amount of data used. This is not a reason to reject the findings. I find them reasonable and qualitatively appropriate. But the limitations of the paper should be stated clearly, preferably in the introduction or method section, rather than, as the authors have chosen, in the result-discussion section (4.1).

The structure of the review is as follow: The first part of this review addresses the manuscript chapter by chapter. The second part responds to the authors' reply to the initial review.

Chapter 1: Line 13-38: Introduction

The novelty of the paper, to resolve the river-sea continuum within a single unstructured grid model, could be more clearly emphasized. It should be more clearly stated how the presented modelling approach differs from previous studies. Some of this could be achieved by moving parts of section 4.1 (line 360-404) to the introduction section.

Chapter 2.1: Line 70-115: The modelling system

The first paragraph (line 71-77) should be modified to remove some errors and make it more readable.

Line 74: Suggestion: The model solves the primitive equations using an unstructured finite element grid in the horizontal and a z-coordinate system in the vertical.

Line 75: The model has been applied ....

Line 76: ... and in other coastal systems.

Line 78: SHYFEM was applied to model the river network ...

Line 90: The model bathymetry is derived by bilinearly interpolating the following datasets onto the model's numerical grid.

Line 99-104: This paragraph contains on the model components, the configuration and the implementation of the model processes all in one. I would suggest to improve the structure of the text and to use references wherever possible. Maybe you can highlight different choices made for dynamical parameter in the open sea and the river network.

Line 103-104: The time frequency of the model output is defined by the dynamic time scales of the processes you intend to resolve and you are able to resolve. If you are able to resolve the processes on the required time scales within the model (wind driven sea level dynamic for example), then you should use a higher output frequency. The point to make is that the dynamic of your model is mainly restricted by the boundary conditions, because the spatial coverage of your model is relatively small. Therefore, the analysis is based on daily mean output files. This might be justified, because you mainly study the longer-term dynamic of the coastal system for the what-if scenarios.

Line 108-109: please correct: …. and is computed by dividing …. with …. .

Chapter 2.2: line 117-144: Numerical experiments

It would be beneficial to include more context regarding the purpose and intended outcomes of the simulations. Reading the text, it is not clear (up to the end) that the main simulation (2015-2019) is the reference simulation for the analysis of the what-if scenarios. Later on you use the term "Reference simulation", but without explaining exactly what it refers to. You could begin with a short overview of the purpose of the simulations followed by a detailed description of the simulation.

The first two sentences (line 117-120) are very general. It's not so clear what these sentences are building towards.

Line 132-135: These lines motivate the numerical studies that have been carried out. They could be presented at the beginning of the chapter.

Chapter 2.3: Line 146-198: Model validation

Line 146-148: I think you can write here using the term "Reference simulation" which you have introduced. You don't compare parameters, which are usually fixed, but you use a statistical metric for validation.

Line 146-148: Your validation study is too limited to assess the model quality with regards to reproducing the hydrodynamic in the entire water body of the Danube Delta and Black Sea continuum. I think you should be open about the limitations of your validation study. You can say that you want to give the reader an impression of the quality of the model. It would be good if you could link the validation exercise to specific applications: sea level validation to the prediction of blocking events, or temperature validation to seasonal model forecasts, etc.

Line 149-168: There might be more data available on EmodNet physics, SeaDataNet or ICES.

Table 1: River discharge: I think there is some rounding involved to get to a correlation coefficient of 1.0. I would suggest to show the first decimal which is not zero.

Table 1: Sea level: Why is the sea level bias zero. Have the time series be de-biased before analysis?

Line 173-180: Suggestion: I think it would be good to know how well the model can predict the total runoff entering the Black Sea. The predicted value can be compared with the total runoff of hydrological models or with the results from the study from Nichersu et al 2025.

Line 181: It should be mentioned that daily averaged time series of modelled sea levels were used for validation.

Line 183: Please see also the reply to R2.27. The sentence should be re-formulated: I don't think it's appropriate to draw conclusions about long-term dynamics, which typically span several years, as well as storm surge-related short-term dynamics, which occur over just a few hours. A suggestion would be: The model can reproduce the variability of the daily mean sea level in the year 2017 including mayor sea level events associated with stormy periods of typically 1-10 days.

Line 188: Please see the reply to R2.37. It would be beneficial to demonstrate the dependency of the model quality with regards to sea level prediction on the quality Black Sea physical reanalysis. The impacts of the model quality on the finding of the paper could be discussed.

Line 197: The comparison with satellite SST data could be provided as supplementary material.

Chapter 3.1: line 204-228: Water division in the river network of the delta

This is an interesting study, demonstrating the capacity of the model. However, I find it quite separated from the rest of the paper. There are no conclusions drawn from the results of this chapter. As I see this chapter, it shall provide evidence for the adequate modelling of the freshwater input into the Razelm lagoon and the Black Sea, which have implications for the what-if scenario study and the coastal dynamic study.

Chapter 3.2: line 230-276: Spatial and temporal variability of coastal dynamics

Line 232: I would rewrite the sentence starting with “Along the coast”. I would not call river plumes a peculiar (meaning: strange, unusual) pattern.

Line 235: Please explain the method of deriving coastal pattern in a number of sentences, with references to figures. It should not be done in a bracket. I think you mean averaging over certain periods: (1.) the entire period of 5 years and (2.) daily periods for in 2019.

Line 235-238: Please reformulate this sentence. It is much too long.

Line 240: …. discharged from the nearby Chilia mouth.

Line 254: …. Danube river inflow extends …. . Or use the word “discharge”. I would associate the river flow with the velocity of the water in the river.

Line 255: During peak river inflow ……

Line 260: “2m thick layer near the surface”

Line 261-262: It should be made clear that both set of plots in figure 6 show configurations with southerly wind, and that the main difference between them is the season.

I’m not sure that you mention somewhere that you use meteorological definition of wind directions, with winds in the direction they are “coming from”, not the oceanographic convention with winds in the direction they are “going to”.

Line 262: When you say “between river mouths”, then you discuss the surface pattern of the combined upwelling and river plume configuration. Vertically, the configuration looks different, as river plumes do not affect deeper layers.

Line 265: I would not use the word “peculiar”.

Line 267-268: Please rewrite “were processed to computed the standard deviation”. I also think that you mean “of all the month of the years 2015-2019”.

Figure 7: I think it would be better to use “Autumn” instead of “Fall”. In the caption it could be mentioned that the plot refers to the model data set covering the period 2015-2019.

Chapter 3.3: line 278-320: Lagoon water exchange, mixing and renewal capacity

Line 283: Could you explain the term “flood river conditions”? Are these situations of high river discharge?

Line 284: Better to use “driven by the wind action”

Chapter 3.3.1: line 322-359: Assessment of the impact of lagoon-sea reconnection solutions

Line 321: Maybe you can use the term “what-if scenario” in the title, to make it more visible.

Figure 11: Maybe you can plot the reference configuration and the difference to the reference configuration, with different y-axis on the left and right side of the plot to make the differences between the 3 solutions clearer.

Line 322-359: I’m missing a bit a discussion of the different solutions (A, B, C), which one is more in line with the expected outcome of the reconnection of the lagoon with the Black Sea.

Recommendation: I think the structure of the manuscript with assessing three different topics: (1.) Danube river and estuary modelling, (2.) Coastal dynamic modelling and (3.) what-if scenario assessment makes it rather difficult to discuss the different topics in one

single chapter. Even more so, as the 3 different topics are presented separately before the discussion. I would present and discuss each topic separately and write a discussion on the integrated river-to-estuary-and-coastal scale in the end.

Chapter 4: line 360-464, River-sea modelling, characteristic, requirements and limitations

There should be a chapter 4: Discussions, before sub-chapter 4.1

Line 361-404: I would recommend to move chapter 4.1 to the introduction or method part of the paper. Lines 364–394 address the requirements for adequate river-to-estuary-to-coastal-sea modelling, effectively justifying the chosen modelling approach and highlighting the novelty of the paper. Line 395-404 deal with the limitation of the validation study and could be moved to chapter 2.3

Line 399: Note: A lot has been done in terms of observation data collection. There are no transnational monitoring systems, because most of the funds come from the national institutions, but there are transnational data collection and cataloguing initiatives, like ICES, EmodNet and SeaDataNet. There are also near real-time ship data exchange initiatives among some of the operational centres. EmodNet is providing river runoff measurements for the Danube from 2022 onwards and is also providing SST observations for several coastal stations in the Black Sea (I didn't check the metadata).

Line 463-464: Could you please explain what you mean with alternative lagoon-sea reconnection solutions. Is this not solution A, B, C. Are you thinking about the timing (design) of the reconnection process, the monitoring of the reconnection process or other activities?

Chapter 5: Line 465-499: Conclusions

Note: The authors could provide their perspective on the strengths of their integrated modelling approach compared to separate assessments using standalone hydrological and ocean models.

Reply to the authors response:

R2.26: Reply: It was suggested that the total runoff into the sea, as predicted by the hydrological model, could be used as a criterion for assessing the quality of the water transport predictions in the Danube river network. The value predicted by hydrological model could be compared to the predictions of the SHYFEM model. This would make it possible to estimate the quality of the SHYFEM forecast not just near the source, but also near the estuary of the river system. However, this was just a suggestion for the case that no observed runoff data was available.

Danube river observations from EmodNet exist, but unfortunately only after 2022.

R2.27: Reply: If I understand the argumentation of the authors correctly, they say that the model's geographical coverage is too small to generate wind driven sea level extremes caused by storm surges within the model domain. Therefore, they rely on the boundary conditions to provide the sea level forcing. But as daily mean sea levels from the Black Sea reanalysis are used as boundary conditions, they can not expect to model wind-driven sea level anomalies characterized by sharp peaks lasting several hours. Using daily mean values will remove most of these peaks from the data set. The highest peak that you are predicting is therefore also about 20cm. I think it would be better to write "stormy periods" rather than storm surges. This reply also applies to R2.34.

R2.31: Reply: thank you for adding a link to Nichersu et al. (2025). Would it be possible to compare the actual values of the runoff calculations?

R2.36: Reply: Thanks for adding a table of statistical parameters. Is the correlation coefficient of the river discharge really equal to 1.0? This would only be possible if the modelled and observed values would coincide. I guess it's rounded up.

R2.37: Reply: Thanks for adding the Black Sea reanalysis time series to the sea level plot. But it's really hard for me to distinguish between the dashed SHYFEM time series and the dotted BLKSEA time series. Maybe you can calculate the correlation coefficient (CC) and the root-mean-square-deviation (RMSD) of the two timeseries (SHYFEM AND BLKSEA) to demonstrate how the daily mean sea level variation at Constanta and Mangalia depends on the boundary condition.

R2.42: Here, I thought about linking the purpose of the validation closer to the applications.

R2.48: Reply: There is a temperature dependency in the conversion, which makes it that 1 PSU is not exactly equal to 1 g/L. I just wanted to make the point that in the oceanographic community usually PSU is used as a unit. But g/L can be used, if it is applied consistently.

R2.58: Reply: Thank you for motivating the choice of the what-if scenario. In the light of this application, the "Numerical experiment" section could be restructured a bit, mentioning the purpose of the simulations, to study different the river-open-sea continuum and different proposals for connecting the Danube delta with the Black Sea in the beginning. Then it would be consistent to present the reference configuration and the what-if scenarios. Right now, the term "reference simulations" is introduced later, with only little connection to the "Numerical Experiment" section.

R2.66: Reply: Thank you for changing the figure 11 (in the current document). It has much improved, but is still rather difficult to see. Maybe it would be possible to present the results of the reference simulation and difference plots, choosing maybe different axis for the different plots. This is just a suggestion.

R2.70: Reply: I'm not sure about the formulation "small scale nearshore feature, situated between river mouths". Maybe you should refer to a figure here. Is this not just the surface

picture that you are describing. It’s more the overlaying configuration of the upwelling pattern and the river plume. The vertical plots show that the rivers only affect the surface, whereas the upwelling pattern extends to deeper layers.