Responses to the Editor Comments and Suggestions

Journal: Ocean Sciences (OS)
Manuscript number: egusphere-2025-606
Manuscript title: Modelling river-sea continuum: the case of the Danube Delta

The original Editor's comments and suggestions are shown in regular typeface, while our responses are shown in italics. The line and figures numbers we use refer to the revised document.

ED.1 I now have the referees' comments on your revised manuscript; I assume you have seen them (please tell me if you do not have them). Below I have some italicised comments on how you might respond to them. This looks like a lot but I should tell you that both referees rated this manuscript as "good" and asked for (only) "minor revision"; please do what is practicable to address these comments.

Response. We thank the editor for the careful reading of the manuscript. We improved the manuscript in accordance with the suggestions.

ED.2 First paragraph of the review "The manuscript would still benefit from the development of a more consistent story line and the formulation of a research question." In your line 30 "investigates" is rather vague and would be clarified by immediate connection to lines 34-38 where - implicitly - you state the aims by stating what you quantified. "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)." I think probably in the method section, but only as limitations known before doing the work. It is quite conventional to discuss limitations as part of a Discussion section, perhaps especially the limitations that appear by doing the work.

Response: We appreciate the comments and we improved the manuscript in accordance with all suggestions. In particular, we modified the introduction at lines 45-54 to better clarify the scope and content of this study.

The limitations of this modelling study are primarily presented in the Methods section (e.g., the exclusion of the delta floodplain system from the computational domain).

ED.3 "Chapter 1: Line 13-38: Introduction . . ." I mostly agree. Your present introduction is quite short and could include more about "the state of the art" and how you intend to advance it. However, in the introduction you should avoid referring to what the manuscript achieves or to the results obtained.

Response: In response to the referee's comment, we have moved the general statements from Section 4.1 to the Introduction. See also the response to comment R2.31.

ED.4 "Chapter 2.1: Line 70-115: . . ." The specific points on lines, 74, . . 90 are all improvements.

Response: The text has been corrected following the referee's recommendations.

ED.5 Line 99-104: . . ." I am not sure about this. Implementation values are most naturally kept with the formulation they relate to. But perhaps aspects of formulation (wind drag, bottom friction, vertical diffusivity - and boundary conditions?) should come before time step and output frequency. More references would help.

Response: We improved the description of the parameters and settings by restructuring the original paragraph into the following sentences.

- *Lines 91-92: "Vertical viscosity and diffusivity are calculated by the $k-e$ turbulence closure module of the General Ocean Turbulence Model (Burchard and Petersen, 1999)."*
- *Lines 118-121: "The drag coefficient for the momentum transfer of wind has been set to a constant value of 2.5 10^{-3}. The friction in the model is parameterized by a quadratic bottom friction expression following the Strickler formulation (Umgiesser et al., 2004, 2022). Due to the lack of data on bottom sediment characteristics, no spatial variation in bottom friction was applied, and the Strickler coefficient was uniformly set to 32 $m^{1/3}$ s^{-1}."*
- *Lines 138-140: "The main simulation (hereinafter referred to as the reference simulation, or REF) covers the period from 2014/01/01 to 2019/12/31, with the year 2014 considered as model spin-up time. The results are analyzed over the period 2015-2019."*
- *Lines 151-156: "The maximum allowable time step in the simulation was set to 60 s, and the model adopts automatic sub-stepping over time to enforce numerical stability with respect to advection and diffusion. Model outputs are saved at a daily frequency. This choice was made to limit the volume of model outputs and is justified by the fact that shorter time-scale processes are not relevant in the study area. Indeed, tides along the northwestern Black Sea coast are negligible; therefore, coastal dynamics are primarily influenced by open sea conditions, river discharge, and atmospheric disturbances with typical time scales of 1 to 10 days. Additionally, the model is forced at the boundaries of the Black Sea and the Danube River using daily datasets."*

ED.6 "Line 103-104: . . ." Maybe the point is that (for example) stress resulting from a daily- average flow is not equal to (usually greater than) daily-average stress. So it is necessary to use variables at the frequency of the time step for all (especially non-linear) calculations before "final" output.

Response: To clarify this aspect, we have revised the text as follows (lines 152-156) "Model outputs are saved at a daily frequency. This choice was made to limit the volume of model outputs and is justified by the fact that shorter time-scale processes are not relevant in the study area. Indeed, tides along the northwestern Black Sea coast are negligible; therefore, coastal dynamics are primarily influenced by open sea conditions, river discharge, and atmospheric disturbances with typical time scales of 1 to 10 days. Additionally, the model is forced at the boundaries of the Black Sea and the Danube River using daily datasets."

ED.7 "Line 108-109: please correct: .... and is computed by dividing .... with .... ." I think ".... and is computed by dividing .... by .... ."

Response: The text has been corrected following the referee's recommendations.

ED.8 "Chapter 2.2: line 117-144: Numerical experiments . . ." I generally agree with these comments.

Response: Section 2.2 has been restructured following the referee's recommendations. The first paragraph of this section now reads "The main purpose of the model application is

to reproduce the seasonal and interannual variability of the Danube Delta hydrodynamics under the influence of river discharge, heat and momentum fluxes at the water surface, salinity and sea temperature gradients and open sea forcing (sea level oscillations and currents). Moreover, the concurrence of intense atmospheric forcing, direct morphological interventions within the delta territory and freshwater inflows results in the Danube Delta being characterized by a wide range of transport phenomena. The main simulation (hereinafter referred to as the reference simulation, or REF) covers the period from 2014/01/01 to 2019/12/31, with the year 2014 considered as model spin-up time. The results are analyzed over the period 2015-2019."

Section 2.2. is now structured as follows: first, the purpose and characteristics of the main simulation are introduced; second, the simulation setting (boundary and forcing conditions, simulation and output time steps) are described; finally, the additional what-if numerical experiments are presented.

ED.9 "Chapter 2.3: Line 146-198: Model validation . . ." I generally agree with these comments so please consider them. Of course for validation you can only use data that you can find. You should put enough of your previous responses in the manuscript for other readers not to ask the same questions that you responded to.

Response: We are aware that the model validation is limited and constrained by the availability of observations during the 2015-2019 period availability on the study site. To clarify this aspect, we inserted the following sentence in lines 169-171 "Limited spatial and temporal coverage of existing monitoring networks, along with restricted availability of freely accessible data, are critical issues in the Danube Delta. Consequently, model validation was constrained by the availability of observations during the 2015-2019 period." Moreover, the issue of limited model validation is also addressed in at the end of the "Model validation" section.

ED.10 "Chapter 3.1: line 204-228: Water division . . ." This is probably fair comment but the last paragraph of this section 3.1 is important regarding some form of validation and its first and last sentences. Respond as best you can but do keep this section in some form.

Response: To stress the importance of accurately capturing water distribution in the delta's river network, we added the following two sentences to section 4.2.:

- *lines 400-401: "An accurate representation of water distribution within the delta's river network, along with its temporal variability, is crucial for correctly reconstructing the freshwater input into the Black Sea, and consequently, the river plumes and coastal dynamics."*
- *lines 421-422: "Capturing the spatial and temporal variability of water distribution in the delta's river network is essential for accurately modelling the amount of freshwater entering the Razelm Lagoon via the Dunavăţ and Dranov canals."*

ED.11 "Chapter 3.2: line 230-276: Spatial and . . ." I agree with a majority of the comments. In lines 232, 265 maybe "peculiar" -¿ "particular". However, I think you should choose your wording for lines 240, 254, 255, not necessarily follow the referee. In line 260, best to avoid "surficial" but you might simply want "2 m surface layer".

Response: The text has been corrected following the referee's recommendations. See the responses to comments R2.20, R2.21 and R2.22.

ED.12 "Chapter 3.3: line 278-320: Lagoon . . ." Line 283 I agree with the referee but Line 284 either "induced" or "driven" is OK.

Response: We replaced flood river with high discharge.

ED.13 "Chapter 3.3.1: line 322-359: Assessment . ." Line 321 - I agree. Figure 11 - I am not sure; there is a strong point that "Sol. A" and "Sol. B" are very close and "Sol. C" and "REF" are very close. Difference plots (A-B or C-REF) would need a different scale. Line 322-359 - is there an "expected outcome of the reconnection"?

Response: The 3.3.1 sections's title has been modified to "Assessment of the potential impact of what-if lagoon-sea reconnection scenarios".

We prefer to retain Figure 11 as it is, as it effectively illustratea the impact of the considered reconnection measure on the absolute salinity values.

We improved the discussion of the potential impact of the different reconnection measures section 4.2. The text at lines 441-451 now reads "In the RSLS, efforts to enhance ecological status and improve water circulation have prompted exploration into the potential impacts of creating a new inlet to strengthen the lagoon's connection with the sea. The findings presented in section 3.1.1 suggest that even a localized morphological modification can significantly influence the overall hydrodynamics of the lagoon system. Introducing a new inlet in the Razelm Lagoon (scenarios A and B) leads to a 20 % reduction in the RSLS renewal time, which helps mitigate stagnation and enhances ecological conditions. However, it also results in elevated salinity levels up to 9 and 16 g L^{-1} in the Razelm and Sinoie lagoons, respectively. While fisheries and tourist activities would benefit from this intervention, the increased salinization of the lagoon's waters poses a considerable risk to agricultural freshwater resources. In contrast, the impact of solution C on WRT and salinity is limited to the southern part of the Sinoie Lagoon. To help local authorities and communities manage these issues, the model will be used to explore lagoon-sea reconnection solutions with flow regulation based on seasonal and meteo-marine conditions."

ED.14 "Recommendation . ." I think this is a suggestion that you might move the discussion relating to (1), (2), (3) to the respective sub-sections of section 3. I do not insist on this but you should think about it.

Response: Following the referre's suggestion, we restructured the Discussion section to focus on the processes driving the exchange between water bodies. A brief discussion on the implications of the considered reconnection solutions is included at the end of this section.

ED.15 "Chapter 4: line 360-464, River-sea modelling . ." What you do about "There should be a chapter 4: Discussions, before sub-chapter 4.1" depends on what you do about "Recommendation . .".

Response: See the response to comment ED.14.

ED.16 "Line 361-404:" See above about "First paragraph of the review"

Response: In response to the referee's comment, we have moved the general statements from Section 4.1 to the Introduction. See also the response to section ED.14.

ED.17 "Line 399" and "Line 463-464". Please respond to these comments in the revised manuscript.

Response: We concur with the reviewer that significant progress has been made in observational data collection, and that CMEMS, ICES, EMODnet, and SeaDataNet are highly valuable data portals. However, the Danube Delta remains a poorly monitored environment, and not all data collected by local and national authorities is publicly accessible.

We modified the mentioned sentence as "To help local authorities and communities manage these issues, the model will be used to explore lagoon-sea reconnection solutions with flow regulation based on seasonal and meteo-marine conditions."

ED.18 "Chapter 5: Line 465-499: Conclusions . ." I agree

Response: We modified the first paragraph of the Conclusions section as "This work presents the first cross-scale hydrodynamic model implementation covering the entire Danube Delta to investigate the river-sea continuum. To study the hydrodynamic processes driving water exchange and connectivity among the various interconnected water compartments of the delta, the 3D unstructured hydrodynamic SHYFEM model was applied to a domain representing the delta river network, the Razelm Sinoie Lagoon System coastal, and part of the western Black Sea shelf. The variable model resolution is of fundamental importance for reproducing the complex morphology of the Danube Delta and achieving a seamless transition across spatial scales, from river branches to the coastal sea. Compared to standalone hydrological and ocean models, the river-sea continuum approach is essential to accurately represent the non-linear and bidirectional interactions among the various water compartments of the delta. In particular, cross-scale modelling is essential in the coastal sea near the river mouths to accurately capture plume dynamics. By contrast, most regional models of the Black Sea (e.g., Lima et al., 2020; Miladinova et al., 2020) employ coarse resolutions (greater than 2 km) and simplified representations of river inputs, which are inadequate for describing the complex coastal circulation patterns revealed in this study."

ED.19 "Reply to the authors response:" Not all of this asks for changes, but do consider what requested changes would be easy to do.

Response: We have addressed all the concerns raised by Referee#2.

ED.20 Referee 1. Please take account of these comments as best you can. The references to L105 and L121 might be looking at a different version of the manuscript

Response: We have addressed all the concerns raised by Referee#1.

ED.21 Line 276. This part of the sentence does not relate to the previous part. Maybe ". . and of the winds . ." but the sentence is too long.

Response: The mentioned sentence has been modified as "The freshwater discharged by the different branches determine a similar salinity standard deviation pattern in winter (Fig. 7e) and fall (Fig. 7h). These findings reflect the seasonal variability in the strength of the main drivers: (i) the Danube River discharge, which usually peaks in spring or early summer, while drought conditions are generally observed in autumn (Fig. 2a); and (ii) wind forcing (both northerly and southerly), which tends to be stronger in winter and autumn (Bajo et al., 2014)."

ED.22 Line 318. "which resulted mostly influenced" needs correction.

Response. The sentence has been shortened. The revised version reads as follows: "The input of marine waters through the Edighiol and Periboina inlets has a limited effect on the local WRT."

References

Bajo, M., Ferrarin, C., Dinu, I., Stanica, A., and Umgiesser, G.: The circulation near the Romanian coast and the Danube Delta modelled with finite elements, Cont. Shelf Res., 78, 62–74, https://doi.org/10.1016/j.csr.2014.02.006, 2014.

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Miladinova, S., Stips, A., Macias Moy, D., and Garcia-Gorriz, E.: Pathways and mixing of the north western river waters in the Black Sea, Estuarine Coastal Shelf Sci., 236, 106 630, https://doi.org/10.1016/j.ecss.2020.106630, 2020.

Umgiesser, G., Melaku Canu, D., Cucco, A., and Solidoro, C.: A finite element model for the Venice Lagoon. Development, set up, calibration and validation, J. Mar. Syst., 51, 123–145, https://doi.org/10.1016/j.jmarsys.2004.05.009, 2004.

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Responses to the Referee#1 Comments and Suggestions

Journal: Ocean Sciences (OS)
Manuscript number: egusphere-2025-606
Manuscript title: Modelling river-sea continuum: the case of the Danube Delta

The original Reviewer's comments and suggestions are shown in regular typeface, while our responses are shown in italics. The line and figures numbers we use refer to the revised document.

R1.1 The authors have made clear efforts to address the comments; the revised manuscript shows significant improvement in structure and clarity. The messages are more explicit and effectively conveyed and the discussion is substantially strengthened. The responses are detailed and most of the requested information has been incorporated. I am generally satisfied but I have some minor remaining concerns that would be good to address before acceptance.

Response. We thank the referee for the in-depth and useful review. We appreciate the comments and we improved the manuscript in accordance with all suggestions.

R1.2 L105 and R1.2 is the reanalysis only available at daily frequency or do you chose to select daily input for the boundary. If that is the case, you're reasoning is circular. If you chose to use save daily outputs and higher resolution data is available for open boundary conditions, your choice is because smaller scales are not that relevant (i.e. no tide and meteorological events larger scale). If the dataset is only available at daily scales is also likely that it is because shorter scales are not relevant. You explain this better in the reply to reviewer. Could you add this in the manuscript L105?

Response: Following Reviewer#1's indication, we modified the text as "Model outputs are saved at a daily frequency. This choice was made to limit the volume of model outputs and is justified by the fact that shorter time-scale processes are not relevant in the study area. Indeed, tides along the northwestern Black Sea coast are negligible; therefore, coastal dynamics are primarily influenced by open sea conditions, river discharge, and atmospheric disturbances with typical time scales of 1 to 10 days. Additionally, the model is forced at the boundaries of the Black Sea and the Danube River using daily datasets."

R1.3 In R1.8 the authors state that SHYFEM performs slightly better than BLKSEA, however in Table 1 RMSE is larger and CC is lower for BLKSEA than it is for SHYFEM which contradicts their statement. Looking at Fig 1 it would seem that BLKSEA presents slightly elevated values than SHYFEM across the time seriessith BLKSEA overestimaring peaks and SHYFEM reproducing better the low values. Please better indicate the reason for discrepancies between BLKSEA and observations.

Response: We do not really understand this reviewer's comment. As reported in Table 1 of R1.8, SHYFEM has a lower RMSE and higher CC than BLKSEA. Therefore, SHYFEM is performing better than BLKSEA.

R1.4 R1.9 and Fig3 I am curious as to why include station 15360 instead of Constanta when you already have sea levels for Constanta. I understand that you the figure may look unbalanced with 3 sea levels and 2 salinities but I would like to see Constanta time series in the figure. Please do include the time series temperature for Constanta.

Response: We decided to include station 15360 instead of Constanta because it is located closer to the Danube Delta study area. We preferred to present a balanced Figure 3, with two panels for sea level and two for sea temperature. However, to meet the referee's request, the temperature time series for Constanta is now included in panel (d) instead of Mangalia. This modification does not significantly affect the presentation of the results, as Constanta and Mangalia have similar sea temperature validation statistics, as shown in Table 1.

R1.5 R1.31 please add the example at the beginning of March (L287).

Response: We modified the sentence as (line 322) "(as occurring at the end of February 2018; Figure 8c and d)".

R1.6 L121 I think it should be "leads"

Response: We modified the sentence as (lines 136-138) "Moreover, the concurrence of intense atmospheric forcing, direct morphological interventions within the delta territory and freshwater inflows results in the Danube Delta being characterized by a wide range of transport phenomena."

R1.8 L178 please avoid constructions such as "underestimate (overestimate the peak discharge values in the Chilia (Tulcea)" Could you reformulate? This was highlighted as well by R2d.

Response: We modified the sentence as (lines 201-203) "It is worth noting that the model tends to underestimate peak discharge values in Chilia and overestimate them in the Tulcea, as indicated by the slopes of the linear regression best-fit lines: 0.90 for Chilia and 1.10 for Tulcea."

R1.9 L301 you could use WFT instead of flushing time since already defined and you use the acronym afterwards. You have also described what it is, so no need to define it again here.

Response: Corrected.

R1.10 L319 I think you can remove "where".

Response: We modified the sentence as (lines 352-353) "Salinity shows limited variability across the RSLS, with values ranging from 1 to 8 g L^{-1}. The highest values are observed in the area of Sinoie Lagoon, near the Edighiol and Periboina inlets (Fig. 10e)."

R1.11 L334 I think it should be "does not".

Response: Corrected.

R1.12 ·L367 I don't think Zhang and Yu 2025 is relevant here, their model is 2D barotropic so it does not resolve the type of processes you are looking at in your paper. Please remove it. If you wanted more Pearl River Delta examples, the following would be more relevant since they do resolve baroclinic processes, you could cite https://doi.org/10.1007/s44218-022-00008-0, or more recent https://doi.org/10.1029/2021JC017523 or https://doi.org/10.1016/j.ocemod.202

Response: We thank the referee for bringing these references to our attention. We replaced the Zhang and Yu (2025) with Payo-Payo et al. (2022).

R1.13 L412 It is the first time you talk about the standard deviation of the relative discharge among branches and temporal variability, in Section 3.1 you're giving averages for the whole period. How accurately these averages are is also relevant for the plumes you obtain later. Perhaps you could develop more the discussion on that aspect.

Response: To stress the importance of accurately capturing water distribution in the delta's river network, we added the following two sentences to section 4.2.:

- *lines 400-401: "An accurate representation of water distribution within the delta's river network, along with its temporal variability, is crucial for correctly reconstructing the freshwater input into the Black Sea, and consequently, the river plumes and coastal dynamics."*
- *lines 421-422: "Capturing the spatial and temporal variability of water distribution in the delta's river network is essential for accurately modelling the amount of freshwater entering the Razelm Lagoon via the Dunavăţ and Dranov canals."*

R1.14 L419 "configuration" is confusing here.

Response: We modified the sentence as (lines 404-406) "This type of coastal dynamics is common among many of the world's major river deltas - such as the Mississippi and the Nile (Horner-Devine et al., 2015) - as well as in coastal regions where multiple river mouths are located in close proximity (Warrick and Farnsworth, 2017)."

R1.15 L426 presumably the temperature of the smaller scale patterns between river mouths either warmer or colder has to do with the background temperature of the ocean and that of the river discharge. Is you river temperature constant? Or does it change throughout the year?

Response: As mentioned at lines 149-150, water temperature at the Danube River boundary was taken from the daily results of the wflow catchment model implemented over the Danube River basin (van Gils et al., 2025). The vertical alongshore sea temperature transect presented in Figs. 6c and and 6d indicate an upwelling-driven transport of offshore marine waters from deeper layers to the coastal zone.

R1.16 L432 the salt intrusion length values are introduced here for the first time and it is difficult to see where you show those values, perhaps point to the figure or introduce those values in the result section so the discussion here makes more sense.

Response: Compared to the first version of the manuscript, we have removed the section describing saltwater intrusion. Although this phenomenon poses a serious threat to several coastal areas - compromising freshwater supplies for agriculture and human use (Li et al., 2025) - it has not yet been reported as a major issue in the Danube Delta. Therefore, we decided to limit the presentation of the saltwater-related findings to a paragraph in the discussion section.

References

Horner-Devine, A. R., Hetland, R. D., and MacDonald, D. G.: Mixing and Transport in Coastal River Plumes, Annu. Rev. Fluid Mech., 47, 569–594, https://doi.org/10.1146/annurev-fluid-010313-141408, 2015.

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Zhang, A. and Yu, X.: Development of a land-river-ocean coupled model for compound floods jointly caused by heavy rainfalls and storm surges in large river delta regions, Hydrol. Earth Syst. Sci., 29, 2505–2520, https://doi.org/10.5194/hess-29-2505-2025, 2025.

Responses to the Referee#2 Comments and Suggestions

Journal: Ocean Sciences (OS)
Manuscript number: egusphere-2025-606
Manuscript title: Modelling river-sea continuum: the case of the Danube Delta

The original Reviewer's comments and suggestions are shown in regular typeface, while our responses are shown in italics. The line and figures numbers we use refer to the revised document.

R2.1 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).

Response: We thank the referee for the in-depth and useful review. We appreciate the comments and we improved the manuscript in accordance with all suggestions. In particular, we modified the introduction at lines 45-54 to better clarify the scope and content of this study.

The limitations of this modelling study are primarily presented in the Methods section (e.g., the exclusion of the delta floodplain system from the computational domain).

R2.2 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.

Response: In response to the referee's comment, we have moved the general statements from Section 4.1 to the Introduction. See also the response to comment R2.31.

R2.3 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 108-109: please correct: .... and is computed by dividing .... with ..... .

Response: The text has been corrected following the referee's recommendations.

R2.4 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.

Response: We improved the description of the parameters and settings by restructuring the original paragraph into the following sentences.

- *Lines 91-92: "Vertical viscosity and diffusivity are calculated by the $k-e$ turbulence closure module of the General Ocean Turbulence Model (Burchard and Petersen, 1999)."*
- *Lines 118-121: "The drag coefficient for the momentum transfer of wind has been set to a constant value of 2.5 10^{-3}. The friction in the model is parameterized by a quadratic bottom friction expression following the Strickler formulation (Umgiesser et al., 2004, 2022). Due to the lack of data on bottom sediment characteristics, no spatial variation in bottom friction was applied, and the Strickler coefficient was uniformly set to 32 $m^{1/3}$ s^{-1}."*
- *Lines 138-140: "The main simulation (hereinafter referred to as the reference simulation, or REF) covers the period from 2014/01/01 to 2019/12/31, with the year 2014 considered as model spin-up time. The results are analyzed over the period 2015-2019."*
- *Lines 151-156: "The maximum allowable time step in the simulation was set to 60 s, and the model adopts automatic sub-stepping over time to enforce numerical stability with respect to advection and diffusion. Model outputs are saved at a daily frequency. This choice was made to limit the volume of model outputs and is justified by the fact that shorter time-scale processes are not relevant in the study area. Indeed, tides along the northwestern Black Sea coast are negligible; therefore, coastal dynamics are primarily influenced by open sea conditions, river discharge, and atmospheric disturbances with typical time scales of 1 to 10 days. Additionally, the model is forced at the boundaries of the Black Sea and the Danube River using daily datasets."*

R2.5 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.

Response: To clarify this aspect, we have revised the text as follows (lines 152-156) "Model outputs are saved at a daily frequency. This choice was made to limit the volume of model outputs and is justified by the fact that shorter time-scale processes are not relevant in the study area. Indeed, tides along the northwestern Black Sea coast are negligible; therefore, coastal dynamics are primarily influenced by open sea conditions, river discharge, and atmospheric disturbances with typical time scales of 1 to 10 days. Additionally, the model is forced at the boundaries of the Black Sea and the Danube River using daily datasets."

R2.6 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.

Response: Section 2.2 has been restructured following the referee's recommendations. The first paragraph of this section now reads "The main purpose of the model application is to reproduce the seasonal and interannual variability of the Danube Delta hydrodynamics under the influence of river discharge, heat and momentum fluxes at the water surface, salinity and sea temperature gradients and open sea forcing (sea level oscillations and currents). Moreover, the concurrence of intense atmospheric forcing, direct morphological interventions within the delta territory and freshwater inflows results in the Danube Delta being characterized by a wide range of transport phenomena. The main simulation (hereinafter referred to as the reference simulation, or REF) covers the period from 2014/01/01 to 2019/12/31, with the year 2014 considered as model spin-up time. The results are analyzed over the period 2015-2019.".

R2.7 The first two sentences (line 117-120) are very general. It's not so clear what these sentences are building towards.

Response: Se the response to comment R2.6.

R2.8 Line 132-135: These lines motivate the numerical studies that have been carried out. They could be presented at the beginning of the chapter.

Response: Section 2.2. is now structured as follows: first, the purpose and characteristics of the main simulation are introduced; second, the simulation setting (boundary and forcing conditions, simulation and output time steps) are described; finally, the additional what-if numerical experiments are presented.

R2.9 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.

Response: We modified the mentioned sentence as "The reference simulation was validated by comparing various variables to assess the skill of the SHYFEM model in reproducing the hydrodynamics of the different water compartments within the delta."

R2.10 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.

Response: We are aware that the model validation is limited and constrained by the availability of observations during the 2015-2019 period availability on the study site. To clarify this aspect, we inserted the following sentence in lines 169-171 "Limited spatial and temporal coverage of existing monitoring networks, along with restricted availability of freely accessible data, are critical issues in the Danube Delta. Consequently, model validation was constrained by the availability of observations during the 2015-2019 period." Moreover, the issue of limited model validation is also addressed in at the end of the "Model validation" section.

R2.11 Line 149-168: There might be more data available on EmodNet physics, SeaDataNet or ICES.

Response: We checked the EmodNet and SeaDataNet and they contains the same sea level and sea temperature time series we obtained from the in-situ ocean thematic centre of the Copernicus Marine Service.

R2.12 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.

Response: The correlation coefficients were rounded to the second decimal, as for the other variables.

R2.13 Table 1: Sea level: Why is the sea level bias zero. Have the time series be de-biased before analysis?

Response: Yes, due to unknown reference datum, the sea level time series were de-biased prior the analysis. This aspect is now mentioned in lines 207-208.

R2.14 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.

Response: The SHYFEM numerical grid does not include the delta floodplain system - comprising channels, wetlands, lakes, and marshes - as simulating flood dynamics over these areas is beyond the scope of this study. Therefore, un our model application, the total runoff entering the Black Sea is equal to the total water discharge imposed at the upstream river boundary of Isaccea, excluding the evaporation-precipitation fluxes over the river network. We would like to point out that there is no exact estimate of the total runoff, as no monitoring network or hydrological model currently has the capacity to resolve the hydrological complexity of the delta.

R2.15 Line 181: It should be mentioned that daily averaged time series of modelled sea levels were used for validation.

Response: Corrected.

R2.16 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.

Response: Corrected.

R2.17 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.

Response: As already specified in out previous reply, it is not our intention to present a validation of the Black Sea Physics Reanalysis in this manuscript. A proper model comparison was provided in our previous report, which also include, in Table 2, a statistical analysis (in terms of RMSE and CC) of daily sea levels from both SHYFEM and BLKSEA at the monitoring stations.

R2.18 Line 197: The comparison with satellite SST data could be provided as supplementary material.

Response: We respectfully disagree with the reviewer's statements. The comparison with satellite SST data is the only model validation we were able to perform for the Razelm Sinoie Lagoon System; therefore, we prefer to retain it in the main manuscript.

R2.19 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.

Response: To stress the importance of accurately capturing water distribution in the delta's river network, we added the following two sentences to section 4.2.:

- *lines 400-401: "An accurate representation of water distribution within the delta's river network, along with its temporal variability, is crucial for correctly reconstructing the freshwater input into the Black Sea, and consequently, the river plumes and coastal dynamics."*
- *lines 421-422: "Capturing the spatial and temporal variability of water distribution in the delta's river network is essential for accurately modelling the amount of freshwater entering the Razelm Lagoon via the Dunavăţ and Dranov canals."*

R2.20 Line 232: I would rewrite the sentence starting with "Along the coast". I would not call river plumes a peculiar (meaning: strange, unusual) pattern.

Response: The mentioned sentence has been modified as "Along the coast, these processes create distinct hydrodynamic patterns, the so-called river plumes, having thermohaline characteristics and buoyancy that allow to distinguish them from seawater".

R2.21 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.

Response: The mentioned sentence has been modified as "Off the coast of the Danube Delta, the overall coastal circulation - averaged over the entire period of 5 years - reveals distinct freshwater plumes formed by the various branches of the multi-mouth delta. The shape and size of these plumes are influenced by both the volume of water discharged from each river branch and the specific features of the coastline (Fig. 5a)."

R2.22 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"

Response: Corrected.

R2.23 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.

Response: We modified Figure 6 caption as "Maps of sea surface temperature and north-to-south alongshore transects of sea temperature under southerly wind conditions during winter (10 January 2015; panels a and c) and spring (15 May 2015; panels b and d). The transect location is indicated with a red line in panel a."

R2.24 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".

Response: For wind direction, we applied the standard meteorological convention. In contrast, the oceanographic convention is typically used for waves and currents. We believe it is unnecessary to explicitly state this in the manuscript.

R2.25 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.

Response: We didn't understand the referee's comment. We never state that river plumes affect the deeper part of the water column.

R2.26 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.

Response: Corrected.

R2.27 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"

Response: We replaced "flood river" with "high discharge".

R2.28 Line 321: Maybe you can use the term "what-if scenario" in the title, to make it more visible.

Response: The 3.3.1 sections's title has been modified to "Assessment of the potential impact of what-if lagoon-sea reconnection scenarios".

R2.29 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.

Response: We prefer to retain Figure 11 as it is, as it effectively illustratea the impact of the considered reconnection measure on the absolute salinity values.

R2.30 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.

Response: We improved the discussion of the potential impact of the different reconnection measures section 4.2. The text at lines 441-451 now reads "In the RSLS, efforts to enhance ecological status and improve water circulation have prompted exploration into the potential impacts of creating a new inlet to strengthen the lagoon's connection with the sea. The findings presented in section 3.1.1 suggest that even a localized morphological modification can significantly influence the overall hydrodynamics of the lagoon system. Introducing a new inlet in the Razelm Lagoon (scenarios A and B) leads to a 20 % reduction in the RSLS renewal time, which helps mitigate stagnation and enhances ecological conditions. However, it also results in elevated salinity levels up to 9 and 16 g L^{-1} in the Razelm and Sinoie lagoons, respectively. While fisheries and tourist activities would benefit from this intervention, the increased salinization of the lagoon's waters poses a considerable risk to agricultural freshwater resources. In contrast, the impact of

solution C on WRT and salinity is limited to the southern part of the Sinoie Lagoon. To help local authorities and communities manage these issues, the model will be used to explore lagoon-sea reconnection solutions with flow regulation based on seasonal and meteo-marine conditions."

R2.31 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 onesingle 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.

Response: Following the referre's suggestion, we restructured the Discussion section to focus on the processes driving the exchange between water bodies. A brief discussion on the implications of the considered reconnection solutions is included at the end of this section.

R2.32 There should be a chapter 4: Discussions, before sub-chapter 4.1.

Response: See the response to comment R2.31.

R2.33 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.

Response: In response to the referee's comment, we have moved the general statements from Section 4.1 to the Introduction. See also the response to section R2.31.

R2.34 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).

Response: We concur with the reviewer that significant progress has been made in observational data collection, and that CMEMS, ICES, EMODnet, and SeaDataNet are highly valuable data portals. However, the Danube Delta remains a poorly monitored environment, and not all data collected by local and national authorities is publicly accessible.

R2.35 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?

Response: We modified the sentence as "To help local authorities and communities manage these issues, the model will be used to explore lagoon-sea reconnection solutions with flow regulation based on seasonal and meteo-marine conditions."

R2.36 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.

Response: We modified the first paragraph of the Conclusions section as "This work presents the first cross-scale hydrodynamic model implementation covering the entire Danube Delta to investigate the river-sea continuum. To study the hydrodynamic processes driving water exchange and connectivity among the various interconnected water compartments of the delta, the 3D unstructured hydrodynamic SHYFEM model was applied to a domain representing the delta river network, the Razelm Sinoie Lagoon System coastal, and part of the western Black Sea shelf. The variable model resolution is of fundamental importance for reproducing the complex morphology of the Danube Delta and achieving a seamless transition across spatial scales, from river branches to the coastal sea. Compared to standalone hydrological and ocean models, the river-sea continuum approach is essential to accurately represent the non-linear and bidirectional interactions among the various water compartments of the delta. In particular, cross-scale modelling is essential in the coastal sea near the river mouths to accurately capture plume dynamics. By contrast, most regional models of the Black Sea (e.g., Lima et al., 2020; Miladinova et al., 2020) employ coarse resolutions (greater than 2 km) and simplified representations of river inputs, which are inadequate for describing the complex coastal circulation patterns revealed in this study."

R2.37 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.

Response: See the response to comment R2.14.

R2.38 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.

Response: We did not state that the model's geographical coverage is too limited to generate wind-driven sea level extremes caused by storm surges within the domain. Our coastal model requires sea level forcing at the open sea boundary, which, in the presented simulations, is derived from the daily Black Sea Physics Reanalysis. As demonstrated in our previous report, boundary conditions have a significant influence on sea level fluctuations within the model domain. However, the model is forced by 3-hourly meteorological fields, which produce sub-daily variability. Nevertheless, we chose to use daily averaged sea levels for model validation, as atmospheric disturbances in the study area typically occur over timescales ranging from 1 to 10 days. This is clearly illustrated in Figure 1, which shows time series of hourly and daily sea level measurements at Constanța.

R2.39 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?

Response: See the response to comment R2.14.

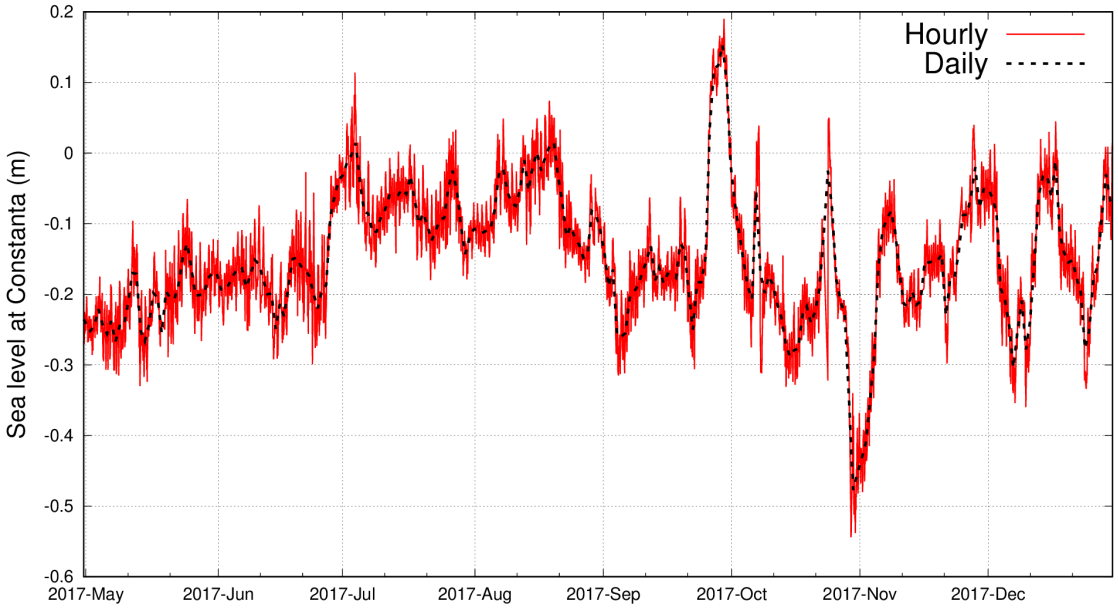

Figure 1: Time series of hourly and daily sea levels measured at Constanta.

R2.40 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.

Response: See the response to comment R2.12.

R2.41 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.

Response: See the response to comment R2.17.

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

Response: We believe that the revised text addresses the referee's concerns.

R2.43 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.

Response: Yes, we are aware of the temperature dependency in the conversion.

R2.44 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.

Response: See the response to comment R2.6.

R2.45 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.

Response: See the response to comment R2.29.

R2.46 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.

Response: Following the referee's recommendation, we included the a reference to Figure 6.

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