Response to Reviewer #2

Manuscript title: *"Wind and Phytoplankton Dynamics Drive Seasonal and Short-Term Variability of Suspended Matter in a Tidal Basin"*

We thank Reviewer #2 for the thoughtful and thorough evaluation of our manuscript. We value the recognition of our combined methodological approach and appreciate the constructive comments provided. In the following, we respond to each comment in detail and explain the revisions carried out in the manuscript.

Reviewer comments are shown in **bold**, and our responses are given in regular font below each comment.

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Review of the manuscript "Wind and Phytoplankton Dynamics Drive Seasonal and Short-Term Variability of Suspended Matter in a Tidal Basin" by Konyssova et al., submitted to Biogeosciences (egusphere-2025-2135).

The manuscript is well written (though a bit repetitive in places) and has many supporting graphs. The study is essentially very local but has useful conclusions for similar systems, at least throughout the Wadden Sea. The main conclusions are not new but quantify the separate contributions from the separate drivers for this particular inlet during winter and summer. The approach is novel, combining observations, particle tacking and a neural network (NN) model to identify changes in (usually) already weak corelations. Personally I found the particle tracking study the most interesting part, and a good way to identify source regions without doing a full morphodynamic study (and truly considering SPM dynamics). The manuscript lacks some information in places (like the predictors included in the NN), seems to contain some errors in listing results, does not always include enough evidence for statements, does not specifically reference the supplementary materials and relies in many parts on weak and even weaker correlations. The latter I consider a suboptimal bid for information, but as sediment resuspension is a notoriously difficult process to predict given the large range of spatial and temporal scales involved I do find it understandable and, to be fair, a brave choice. Overall the authors make a relatively convincing case for the identified drivers and their seasonal impact, though they should be cautious with their conclusions when considering observational correlations and biological proxies. They are however much more cautious with these results in their discussion and conclusions. More detailed comments are listed below.

Recommendation

Major revision, though new simulations are not necessary.

We thank the reviewer for their thorough and constructive evaluation of the manuscript. We appreciate the recognition of our combined methodological approach and the potential relevance of our findings to similar coastal systems.

In response to the reviewer's detailed comments, we have revised the manuscript to enhance the clarity and focus of the storyline, remove repetitive text and overstated claims. Some supporting figures have been moved to the Supplementary Material, and we have improved cross-referencing throughout the text to guide readers more precisely to the relevant figures and tables. We also appreciate the reviewer's point about the difficulty of resolving SPM dynamics across scales and have taken care to present our conclusions with appropriate caution.

Detailed Comments

1. **Line 56: I get the point but the wording is awkward, suggesting there is a sediment balance to begin with.**

   We have rephrased the sentence to avoid confusion and misinterpretation. We see how the original wording may suggest that a defined sediment balance already exists.

2. **Section 2.1: in the area description I miss mention of possible anthropogenic activity in the region that could influence observed SPM levels. Is there local ship traffic (commercial, recreational), ferry service, dredging or trawling activity?**

   Thank you for this important remark. Indeed, anthropogenic activities in the Wadden Sea such as maritime traffic, dredging, and trawling have been shown to influence sediment dynamics and turbidity. Unfortunately, there's no specific data available on these activities in the research area to include directly in our analysis. However, we have now added a brief contextual disclaimer in the Area Description section, noting that such activities may contribute to local SPM variability and should be considered in future studies.

3. **Line 147: I am a little bit surprised that M6 is not included here, but have not checked the listed references for its importance.**

   The M6 constituent is not included when generating elevation at the open boundary, which is situated largely in a relatively deep area of the domain, as its magnitude is much smaller than that of the M4 overharmonic, which is included. However, M6 is, of course, generated within the domain.

4. **Line 153: please be table or figure specific when referring to the supplementary materials. This applies to the whole manuscript.**

   Thank you for this helpful suggestion. We have reviewed the manuscript and revised all references to the supplementary materials to explicitly mention the corresponding table or figure numbers where applicable.

5. **Line 161: needs rephrasing, e.g. "calculated … were released" or 'We released …'**

   We rephrased the sentence for clarity and grammatical correctness.

6. **Line 233: here winter is mentioned for the first time outside of the abstract and introduction, so I would expect a definition of the seasons used here. Note that different research fields can have different delineations of the year into seasons. The Wadden Sea Quality Status report of 2017 even used January-March for winter (van Beusekom, 2017), and astrological definitions do not follow whole months. So please specify which delineation was used in this work.**

   Reference: van Beusekom, J.E.E., Bot, P., Carstensen, J., Grage, A., Kolbe, K., Lenhart, H.-J., Patsch, J., Petenati, T., Rick, J. (2017), Eutrophication, in the Wadden Sea Quality status report, version 1.01 (https://qsr.waddensea-worldheritage.org/reports/eutrophication)

   We appreciate this critical remark. This nuance has been missed in the original version and now has been clarified explicitly in the revised text. Since the neural network analysis aimed to separate periods of minimal biological activity in winter from spring bloom, summer, and autumn, we defined the seasons based on observed seasonal cycles in Chl-a concentrations at the study site rather than using calendar-based or astronomical definitions. Specifically, we used the following delineations:
   - Winter: November 20 – February 19 (low biological activity, low Chl-a)
   - Spring: February 20 – May 31 (phytoplankton bloom initiation and peak)
   - Summer: June 1 – September 19 (post-bloom conditions, high light, reduced Chl-a)
   - Autumn: September 20 – November 20 (transitional period).

7. **Line 239: I'm not sure what is meant here with "current elevation", but I assume the authors mean SSH? And a table overview of the predictors of the NN model is necessary, likely in the supplementary material. Counting the listed predictors here I find 5 directly mentioned predictors or 19 when counting the individual lag components separately. But not the 21 mentioned in 3.3.3 for the non-biotic**

version of the NN. Please list the basic and added (biological proxy) NN model predictors clearly.

Yes, by "current elevation" we indeed meant sea surface height (SSH), and we have corrected the terminology in the revised manuscript for clarity. We have now also included a table in the Supplementary Material that explicitly provides an overview of all input variables used in both the abiotic and full (biotic + abiotic) NN models.

8. **Figs 4, 6, 7: these could go into the supplementary materials with simple a table in the main text listing the correlation metrics in one table for all figures. This would facilitate comparison of the individual month correlations over the different observation drivers with observed SPM.**

   These figures illustrate not only the correlation between parameters but also the variability and scatter structure, which are important for interpreting the seasonal character of the relationships. While the text highlights some key quantities, we have now added a table in the Supplementary Material summarizing the monthly correlation metrics, following the reviewer's suggestion.

9. **Line 250: I miss references here to more work regarding EPS, its production by phytoplankton and its role in flocculation.**

   We have added a reference to support the role of EPS production in flocculation. As the paragraph is primarily descriptive, we have moved it to the Introduction in response to the suggestion of Reviewer #1.

10. **Line 274: the data also shows an autumn bloom which seems to have a different impact on SPM dynamics compared to the spring bloom mentioned here. Some explanatory text on this phenomenon (why is there an autumn bloom in this shallow inlet and why does SPM increase at the same time?) would be useful here.**

    In the revised manuscript, we have moved the paragraph originally located at L270–276 to the Introduction, following a suggestion by Reviewer 1. Nonetheless, we agree that this point deserves attention, and we now address this in the Discussion section by highlighting the seasonal differences in biological activity and its interaction with physical drivers such as wind. Elevated SPM concentrations are observed during secondary phytoplankton bloom in autumn, likely due to the combined effects of stronger wind conditions promoting the resuspension process and organic matter availability to sustain the secondary phytoplankton bloom. This explanation is supported by the NN results (Table 2, Fig. 14), which show that during autumn, predictions overestimate observed SPM, suggesting additional biological regulation at play.

11. **Line 291: the use of "*winter (December-January) … autumn (August-October)*" suggest a seasonal definition that has not been specified before and which suggest unequal length of the seasonal delineation. And which is at odds with other parts of the manuscript (e.g. line 299 where winter is suddenly December-February, while in line 312 late autumn consists of November and December but in line 328 it is listed as October-November).**

    We acknowledge the inconsistency in how seasonal terms were used in the manuscript. In our neural network, we employed seasonality based on observed Chl-a seasonal cycles, as mentioned in our response to comment 6 (L233). We have revised the manuscript to avoid the inconsistent use of the terminology and removed any ambiguous references, such as "late autumn", instead referring directly to the corresponding months.

12. **Line 302: the listed do not match those in fig. 4 where December has 0.84 at the shallow station and January has 0.75 at the shallow station.**

    We thank the reviewer for noting the mismatch. The initial manuscript incorrectly attributed the highest correlation in January (R^2 = 0.84) to the deep station, while it is observed at the shallow station. We have corrected this in the revised text and now clearly state that the highest correlations occur in December-January: R^2 = 0.84 and R^2 = 0.75 at the shallow station, and R^2 = 0.52 and R^2 = 0.56 at the deep station.

13. **Line 306: all R^2 are < 1 as R^2=1 would be a perfect fit. So not sure why this is included here.**

    We agree that this statement was odd and have removed it from the manuscript.

14. **Line 309: please include a list of predictors used in each separate NN model application.**

    We appreciate the reviewer's suggestion. We have revised the manuscript to clarify that "current elevation" refers to sea surface height (SSH) and now provide a complete overview of the NN model predictors in a table, including both the abiotic and extended biological models. This also clarifies the total number of inputs used in each case.

15. **Line 339: R^2=0.44 is by no means a strong correlation and it is not even the highest correlation found as April lists R^2=0.50 for the shallow stations correlation between wind speed and SPM levels. And it should be mentioned**

clearly in the text that a correlation between 2 timeseries is by no means a causal relationship, as demonstrated by the strong correlation between winter Chl-a and SPM levels. Therefore I find line 341 rather contentious: it was simply tested here if there was a correlation between 2 timeseries, and for January only 44% of the SPM variability was explained by the instantaneous wind speed and in April 50%. Hardly a dominant control in winter I think. In my opinion the authors should be more careful in their statements regarding the observational time series correlations.

Following the suggestion, we have corrected this overstatement in the revised text and adjusted the wording to more accurately reflect the strength of the correlations without implying causation. Rather than suggesting a dominant control, we now interpret the seasonal pattern in wind–SPM correlation more cautiously, emphasizing that wind forcing appears to contribute more substantially to SPM variability during winter than in other seasons.

16. **Line 353: "*The stronger relationship*" is more accurately a less weak relationship.**

    We agree that "stronger" may be misleading and have revised the sentence to reflect a more accurate interpretation of the correlation strength.

17. **Line 357: I would say that wind plays a role in controlling SPM variability on different temporal scales, but a crucial role suggests higher correlation factors than have been found here. Besides, the authors only consider tides in the particle tracking study because they are the dominant resuspension mechanism (line 428), and do not include wind waves here.**

    The relatively low correlations can be explained by the non-linear nature of the wind's impact on SPM dynamics, which is, however, captured by the neural network. Indeed, when we include wind stress in the neural network, we account for different mechanisms of its influence on SPM dynamics, as wind stress determines shear in the water column and mediates wave-breaking processes. Tidal forcing, of course, also induces resuspension and the information about tidal phase and elevation is included in NN as well. In addition, tides strongly contribute to net transport (particles do not return to their original location after a tidal cycle) or advection of material within the domain due to its shallowness, complex topography and large tidal amplitudes (all non-linear terms - bottom friction, advection of momentum and non-linearity in the continuity equation- play significant role therefore such a strong net transport exists). Our Lagrangian simulations show indeed tidally induced transport but not mixing. The sentence has been revised to avoid overstatement and more accurately reflect the wind's role in modulating SPM variability.

18. **Line 373: I'm sorry but I don't see this in Figure 8 (rather the opposite) and any kind of metrics to support this statement are missing. Please include more supporting evidence here.**

    We thank the reviewer for this observation. The visual difference between low and high tide distributions in Fig. 8 may appear subtle, especially when considering the total number of data points. However, the distinction becomes more evident when examining the upper envelope of the data distribution. At both stations, particularly the shallow one, the highest observed SPM concentrations occur during low tide. This pattern is consistent with the expectation that, at lower water levels, the sample intake is closer to the sediment surface and thus more likely to capture freshly resuspended material. While we recognize that this is a secondary aspect of the study, we retained this analysis in the Supplementary Materials.

19. **Line 515: a reference to Table 2 is missing here as a source for the 80% reduction mentioned.**

    Revised accordingly.

20. **Lines 518-520: there is no reference here to figure S1 and the supplement does not contain any metrics for a direct comparison of seasonal wind speed. Indeed, autumn winds look a lot stronger in S1 than the spring or summer winds. The figure quality of S1 is too low as well. But I see no evidence for the statement in these lines.**

    We thank the reviewer for pointing this out. We have replaced the original low-quality Figure S1 with a higher-resolution version. We also clarified that Figure S1 presents wind directions (wind rose) rather than wind speeds. The wind speed seasonality has been described in the dedicated section "Role of wind forcing in seasonal SPM variations."
    Regarding the explanation, we revised the text to clarify that the discrepancy between model predictions and observations cannot be explained by wind forcing alone, particularly in biologically active seasons. The abiotic-only model systematically overestimates SPM in spring and summer, highlighting the increasing role of biological processes, while better correlations in autumn reflect conditions more similar to winter, when the model was trained.

21. **Lines 520-522: or it speaks to the stronger winds in autumn?**

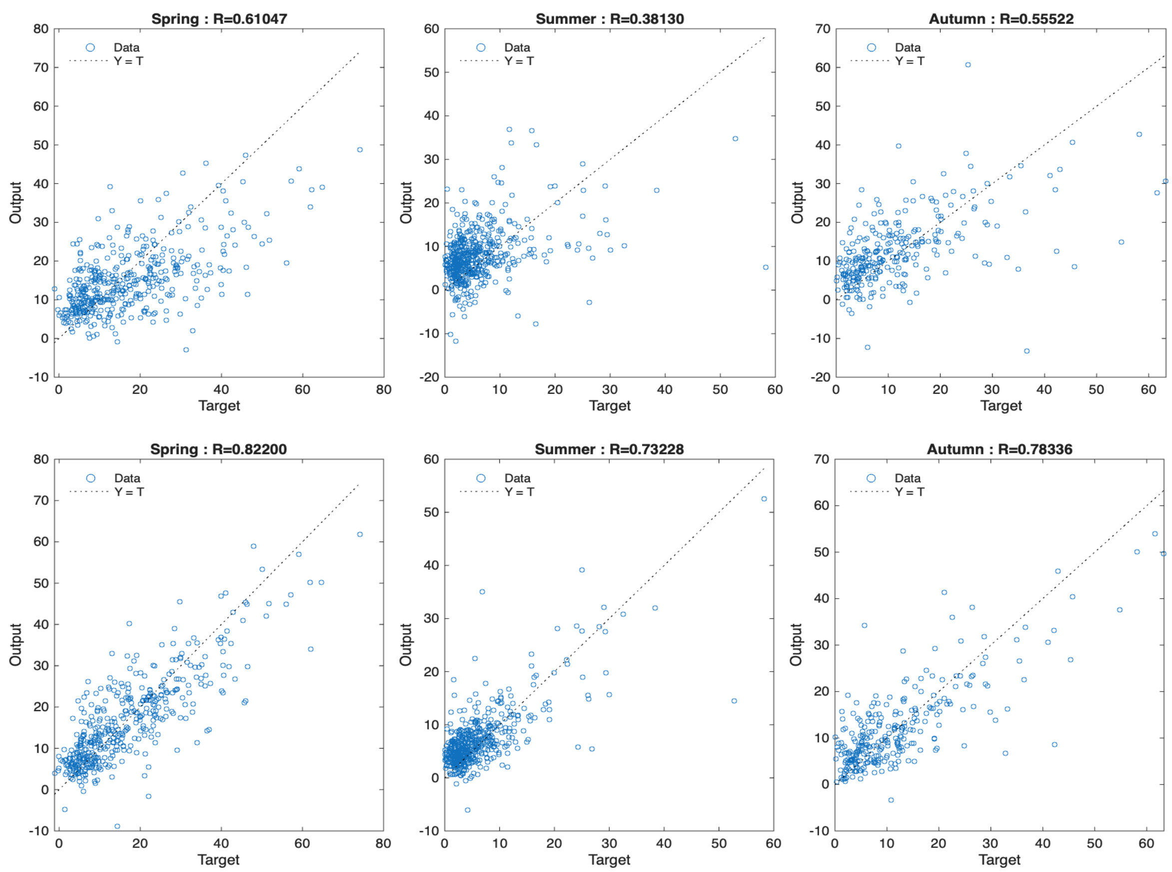
    Thank you for the comment. The text now clarifies that differences in correlation between spring and autumn are interpreted in the context of the NN trained on winter abiotic conditions. The lower correlation in spring is attributed to stronger biological influences that are not represented in the abiotic-trained model. In contrast, autumn conditions, with lower biological influence and similarly strong wind conditions, more closely resemble the physical environment in winter.

22. **Line 523: tidal dynamics was already mentioned as the dominant driver of SPM conditions, now biological processes are mentioned here as being dominant in summer. I would suggest that biological processes become the primary driver of SPM variability in summer, not of SPM dynamics.**

    Yes, you are absolutely right. The sentence has been rephrased to clarify that biological processes are considered the primary driver of SPM *variability* during summer.

23. **Section 3.3.3: it would be good to include seasonal plots for the 2 additional NN model applications (all seasons physics and all seasons plus biological proxies) in the supplementary materials. That is the equivalent of figure 14.**

    We appreciate this suggestion that seasonal plots for the two extended NN models would improve the interpretability and allow more precise comparison with the baseline winter-trained model. We have now added tables with seasonal predicted values and figures with regression plots for both the all-seasons physical-only and the all-seasons physical + biological proxy models to the Supplementary Material:



Upper row: Regression analysis for NN trained on all seasons' abiotic data and applied to spring, summer and autumn data vs observations.
Lower row: Regression analysis for NN trained on all seasons' abiotic data *with biological proxies* and applied to spring, summer and autumn data vs observations.

24. **Section 3.3.4: the title is not semantically correct.**

    Thank you for pointing this out. We have revised the section title to "Neural network trained on all-season data including biological proxy features".

25. **Line 542: please reference figure S2 directly.**

    Revised accordingly.

26. **Lines 553-554: Amen to that. I think the statement here is more accurate than other statements throughout the manuscript mentioning crucial or dominant drivers. And the authors should not sell themselves short: this is not new in itself but they have helped quantify the parts played by different drivers for this inlet and for these data years.**

    We thank the reviewer for this supportive and thoughtful remark! In line with this suggestion, we have revised some of the earlier sections in the manuscript to avoid overstatements regarding "dominant" or "crucial" drivers. We now more consistently emphasize the variable and seasonally dependent *roles* of different processes, while highlighting the study's contribution in quantifying their relative influence rather than attempting to resolve all processes involved.

27. **Section 4.1: I miss a more detailed discussion about the biologically induced flocculation process here, as for instance TEP (transparent exopolymer particles) production occurs under certain circumstances and therefore cannot be predicted by Chl-a levels.**

    The discussion in Section 4.1 has been expanded to include the role of transparent exopolymer particles (TEPs) in biologically mediated flocculation. We note that TEP production can be influenced by factors beyond phytoplankton biomass, such as nutrient availability, species composition, and abiotic conditions, adding complexity to the relationship between SPM and Chl-a. This perspective puts our findings in a broader context while maintaining the interpretation that Chl-a patterns provide meaningful insight into seasonal SPM variability.

28. **Line 571: I miss a reference here to Hommersom et al (2009) who report a correlation between SPM and Chla due to resuspension in their work. A discussion of this work and the difference with the present study should be included.**

Reference: Hommersom, A., Peters, S., Wernand, M. R., & de Boer, J. (2009). Spatial and temporal variability in bio-optical properties of the Wadden Sea. Estuarine, Coastal and Shelf Science, 83(3), 360-370.

A reference to Hommersom et al. (2009) has been added to expand the discussion with their findings of a strong Chl-a–SPM correlation caused by resuspension that are consistent with our winter observations.

29. **Line 590: “*constrained*”, do you mean unknown?**

    Yes, with "constrained" we meant that the role of introduced species is not well known in the study site. We agree that the original phrasing is ambiguous and have rephrased it to “remains less clear”.