Second round 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).

We would like to sincerely thank the Editor and Reviewers for their thoughtful and constructive feedback. We greatly appreciate the time and effort dedicated to evaluating our revised manuscript. The comments have helped us improve the clarity, balance, and interpretation of our study.

Below, we provide a detailed point-by-point response to the comments and describe the corresponding revisions made in the manuscript. Reviewer comments are shown in **bold**, and our responses follow in regular font.

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Response to Reviewer #1

The authors have taken my comments into consideration and I agree with most of their answers. However, I am not convinced by the conclusions of the manuscript, especially the high importance given to wind effects.

To my opinion the authors underestimate the biological effects (TEP production) during spring and summer and its influence on flocculation and seabed stability (check the literature, where many recent papers highlight the importance of seasonal biological effects on flocculation and SPM concentration). Biological effects explain a large part of the summer decrease in SPM concentration (probably more than the outcome of your analysis). In contrast, in winter the sticky TEPs are absent and as a consequence flocs are smaller than in summer and the critical shear stress for erosion is lower. The absence of TEP in winter explains to a large part the higher SPM concentration to my opinion. Wind increases resuspension, but remains less important than the seasonal biological cycle, because the frequency and duration of storm events are limited.

We fully agree with the reviewer's comment on the role of biological effects in spring and summer. In the previous version of the manuscript, we emphasized that biological processes become increasingly dominant with the onset and progression of the phytoplankton bloom (L536-541). We also acknowledge that the data available for the NN analysis may not have been sufficient to fully capture the biological feedbacks, since more drivers are at play than those we included as proxies in our model (L541-544). Nevertheless, we agree that the original conclusions may have placed relatively strong emphasis on wind forcing, and we appreciate the reviewer's more nuanced perspective. In response, we have adjusted the tone of the Conclusions to better reflect that physical

and biological mechanisms operate on different temporal scales. We now clarify that while wind and tidal forcing remain essential in shaping high-frequency variability, particularly in winter and autumn when biological activity is low, the biologically mediated processes increasingly dominate during spring and summer by suppressing SPM concentrations to lower levels despite similar wind forcing.

What is the role of tidal forces in the seasonal SPM concentration cycle? This has not been investigated or discussed and could be studied by comparing periods in summer and winter with negligible winds. Such a comparison could provide an estimate of biological effects and could strengthen or weakens your conclusion on seasonal wind effects.

Regarding the role of tidal forces in the seasonal SPM concentration cycle, we do not observe notable seasonality in the tidal dynamics. Although tide itself does not change throughout the year, the effectiveness of tidal currents in mobilizing and transporting particles likely varies seasonally because the properties of the sediment and the level of biological stabilization change over time. Therefore, in addition to the other factors with clear seasonal signal, we also included tidal data in the NN analysis to account for its overall influence. However, we acknowledge that we did not specifically quantify the contribution of the tides to seasonal variability of SPM concentrations. We thank the reviewer for raising this point as it adds an interesting point of discussion on the role of tides.

It is actually indirectly shown in Figures 7 and 8: the range of wind speed values is similar during the year, however, there is almost no correlation between SPM concentration and wind speed during the summer months. There is further something contradictory when comparing Figures 7 and 8 with Figure 5. Figure 5 suggests a strong correlation between wind speed and SPM concentration, which is in contrast with Figures 7 and 8, which show a very low correlation. To my opinion and based on literature on biological effects, Figure 5 is misleading as it overestimate the wind influence and does not take biological effects into account. Temperature as a proxy of biological effects would show another correlation. Is the frequency of wind/storm periods enough to explain the correlation shown in Figure 5 between SPM concentration and wind speed? You would need almost every 5 days a storm in winter to explain it, I guess.

The reviewer is also right that the Fig. 5 does not take biological effects into account, because we wanted to analyze the abiotic and biological effects in seperate. The figure was also not intended to represent a direct correlation between wind speed and SPM concentration but rather to illustrate that they both follow a similar seasonal pattern through the year, with high values during winter and lower values in summer.

The Figures 7 and 9, however, provide the monthly correlations which look into the variability of wind speed and SPM concentrations within respective months. We agree that the monthly correlation coefficients between wind speed and SPM concentrations are not particularly strong and we have also not stated that the wind plays dominant role across all seasons, but it is the main driver when the biological activity is minimal. Beginning with and following the phytoplankton bloom in early spring, the biological effects have increasingly larger influence.

In an answer to a comment of mine, you have ascribed the low correlation between SPM concentration and wind speed to the inherent complexity of the system, does this means that the stochasticity of the system is more important than wind effects or does this complexity covers to a larger part the effect of tides?

We thank the reviewer for raising this point. Indeed, the correlation coefficients between SPM concentrations and wind speed are not particularly strong, as winds alone cannot explain the full variability of the SPM concentrations across all seasons. By referring to the inherent complexity of the system, we meant that the observed SPM variability results from the interaction of multiple driving mechanisms including tides, wind, and biologically modulated processes, each operating at different temporal and spatial scales. So the relatively low correlations likely reflect the presence of non-linear interplay among these drivers rather than random stochastic behavior. To clarify this point, we have revised the Discussion section to elaborate that the system complexity arises from the superposition of physical and biological processes, which vary seasonally in their relative importance.

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Response to Reviewer #2

Review overview

The authors have revised their manuscript thoroughly text-wise and I am happy to see that they are much more nuanced now with their statements and conclusions. A few minor remarks remain which are listed below.

Detailed Comments

1. **Line 54-55: I assume the tides will also cause erosion under severe weather conditions, except that waves will cause larger erosion in those circumstances.**

   We agree that the original phrasing could be misleading, as it may imply that tidal erosion occurs only under calm conditions. The revised sentence now clarifies that tidal currents maintain a baseline shear stress and govern sediment transport

under all weather conditions, while during severe weather, waves cause more intense erosion superimposed with the tidal flow.

2. **Line 78-79: I miss references here for the statement about the effect of zooplankton grazing on organic aggregates. And it makes me wonder what the process would be behind it.**

   We thank the reviewer for noting the need for clarification and appropriate references. The revised text now expands on the underlying processes through which zooplankton grazing influences SPM concentration and composition. The effect occurs both directly through the consumption and fragmentation of phytoplankton aggregates and the production of fecal pellets, and indirectly through the release of organic matter that enhances microbial and TEP formation (e.g., Passow, 2002; Toullec et al., 2019; Turner, 2002). These processes collectively alter particle size, cohesion, and settling behavior, which potentially leads to either enhanced aggregation and sedimentation or increased recycling within the water column.

3. **Line 115-116: needs rephrasing. At the very least something like "The basin … embayment called Konigshafen, which has an average depth of ~2m and encompasses large areas which become exposed at low tide", or more simply "encompasses large intertidal areas".**

   We rephrased the sentence as suggested.

4. **Line 160 "displayed in Fig. 1 " gives the impression that the results will be shown there, rather than just the location of the stations. Something like "(see Fig. 1)" would be better.**

   Revised accordingly.

5. **Line 227: not every reader may be aware of what LTER stands for, and it is not explained anywhere in the text currently.**

   Expanded to include the full term *Long-Term Ecological Research (LTER)* at its first mention (L98).

6. **Line 236-238: I do not agree with this. Discrepancies between numerical models and observational evidence (which is what is meant here I assume) can be due to many things, including the spatial and temporal resolution of the model, the spatial and temporal resolution of the observations, indirect observational evidence, lack of processes within the model, lack of accurate forcing data like initial conditions or boundary conditions or the temporal resolution of the applied meteorology. I suggest reading Skogen et al (2021) for a more nuanced view. But my interpretation here was that a**

full numerical model would be costly to run and add little for a first quantification of the different drivers. It would, however, have added better process understanding than a NN model can provide, but would be unable to fully capture the short-term bursts of wind that cause resuspension. Hence I support the choice for the NN model.

We acknowledge the reviewer's comment that the discrepancies between modelled and observational can be attributed to various factors, including the representation of physical and biological processes in models, boundary conditions, and the spatial and temporal resolution of both modelled and observational data. In the revised version, we have incorporated the suggested reference (Skogen et al., 2021) and expanded our reasoning to include more nuanced view. We also thank the reviewer for supporting our choice to use the NN approach as an efficient way to capture complex, non-linear dependencies among the measured variables, while recognizing that it does not provide process understanding.

7. **Line 364: according to the graphs the winter values of Dec-Feb (0.30, 0.44, 0.31) are not much different from those of spring (Mar-May: 0.40, 0.50, 0.10) at the shallow station, average wise. So why is winter listed as having the highest correlation?**

   We agree with the reviewer's observation. The revised phrasing now specifies that the correlations are comparably strong in both winter and early spring. This pattern likely reflects a seasonal transition in which biological effects start to have an increasing influence, yet wind-driven resuspension still significantly contributes to short-term variability of SPM concentrations.

8. **Line 429-430: or highlighting the different source regions? Possibly connected to differences in grain sizes?**

   We appreciate this suggestion from the reviewer. In fact, the difference between the median transit times at the deep and at the shallow stations does highlight the different source zones and associated and transport pathways feeding each station. While our passive tracer simulations do not explicitly resolve grain-size classes, we agree that the different in sediment characteristics would influence the transport timing. In the revised manuscript, we mention these in the text as an additional explanation.

9. **Fig. 11: the right side graph is of very poor quality, both digital and in print.**

   We thank the reviewer for noting the issue with the figure quality. The figure has been replaced with a higher-resolution version.

10. **Line 489-490: you cannot prove a negative. I would say that the regression coefficient dropping to near zero only indicates other drivers, and that your subsequent analysis for all seasons indicates that it is a biological one.**

    This is a valid point, and we thank the reviewer for this correction. A near-zero coefficient indeed does not prove the absence of wind influence, but rather reflects that other drivers become dominant, which is then demonstrated in the following section. We have rephrased the sentence accordingly.

11. **Line 523: "NN related fundings", did the model receive payment for its work?**

    Thank you for noticing the typo, we corrected it to "NN-related findings."

12. **Line 588: I don't understand the use of "as soon as" here, I assume you simple mean "as"?**

    Replaced "as soon as" with "as" for clarity.

13. **Line 695: "from the intertidal and shallow *areas*"**

    Revised the phrase accordingly.

References

Passow, U.: Transparent exopolymer particles (TEP) in aquatic environments, Progress in Oceanography, 55, 287–333, https://doi.org/10.1016/S0079-6611(02)00138-6, 2002.

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