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

Manuscript overview

The manuscript combines evidence from observational correlations, particle tracking and a neural network model to derive likely drivers for observed SPM dynamics in the Sylt-Rømø tidal inlet located in the German/Danish Wadden Sea. The correlations from 2 long term stations in the inlet are primarily used to illustrate a decrease in correlation during certain seasons, showing stronger correlations during winter. The particle tracking is applied to identify regions within the inlet that supply sediments to the 2 observational stations by tidal processes, demonstrating a local supply of sediment to the shallow station and a (Sylt) basin-wide supply of sediment to the deep station. The neural network model is applied using varying predictors and seasonal training sets, displaying decreasing winter correlation values when all seasons are used for training and increasing winter correlation when biological proxies are included, all relative to the winter trained abiotic NN model application to winter observations. The authors conclude that wind time lag is important for the sediment supply to the deep station while wind is important for the shallow station, that physical drivers determine the main winter sediment dynamics and that in summer additional biological controls account for roughly half of the 80% observed reduction in SPM levels, attributing the other half to wind reduction.

Review overviews

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.

Detailed Comments

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

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?
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.
4. Line 153: please be table or figure specific when referring to the supplementary materials. This applies to the whole manuscript.
5. Line 161: needs rephrasing, e.g. "calculated … were released" or 'We released …'
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.
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.
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.
9. Line 250: I miss references here to more work regarding EPS, its production by phytoplankton and its role in flocculation.
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.
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).
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.
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.
14. Line 309: please include a list of predictors used in each separate NN model application.
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.

16. Line 353: "*The stronger relationship*" is more accurately a less weak relationship.
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.
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.
19. Line 515: a reference to Table 2 is missing here as a source for the 80% reduction mentioned.
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.
21. Lines 520-522: or it speaks to the stronger winds in autumn?
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.
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.
24. Section 3.3.4: the title is not semantically correct.
25. Line 542: please reference figure S2 directly.
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.
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.
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.
29. Line 590: "*constrained*", do you mean unknown?

References

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)

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.