Second round 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).

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.

Recommendation

Minor revision, recommending publication.

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.
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.
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".
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.
5. Line 227: not every reader may be aware of what LTER stands for, and it is not explained anywhere in the text currently.
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.
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?
8. Line 429-430: or highlighting the different source regions? Possibly connected to differences in grain sizes?
9. Fig. 11: the right side graph is of very poor quality, both digital and in print.

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.
11. Line 523: “NN related fundings”, did the model receive payment for its work?
12. Line 588: I don’t understand the use of “as soon as” here, I assume you simple mean “as”?
13. Line 695: “from the intertidal and shallow *areas*”

References

Skogen, M.D., Ji, R., Akimova, A., Daewel, U., Hansen, C., Hjollo, S.S., van Leeuwen, S.M., Maar, M., Macias, D., Mousing, E.A., Almroth-Rosell, E., Sailley, S.F., Spence, M.A., Troost, T., van de Wolfshaar, K. (2021) *Disclosing the truth: are models better than observations?*, Marine Ecology Progress Series, DOI: 10.3354/meps13574