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Ocean Science Discussions

Interactive comment on "The relative importance of selected factors controlling the oxygen dynamics in the water column of the Baltic Sea" *by* S. Miladinova and A. Stips

Anonymous Referee #1

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General comments

The manuscript has certainly improved with respect to the previous version. I am glad to see that the authors carried out the analyses that I suggested and that the grammar is much better now. However, I have basically the same problem with this version as with the previous one: the presented material provides too little new insight to warrant publication. The authors claim that the main point of their paper is not that they implemented into GOTM another formulation for the air-sea exchange of oxygen, but the more general issue of the importance of different factors for the oxygen dynamics in the Baltic. So what are their conclusions with respect to this general issue?

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1) The seasonal oxygen variation at the sea surface is well captured using the 1-D model and it mainly depends on physical factors related to the air-sea exchange of oxygen. This finding is not surprising at all, given the short air-sea equilibration time of oxygen. Due to air-sea exchange, any air-sea disequilibrium in the oxygen concentration caused by either biological dynamics or horizontal advection will disappear on a time scale much shorter than the seasonal time scale that the authors are considering.

2) The model does not capture the seasonal oxygen dynamics near the bottom of the basin. This finding has the potential to provide some interesting new insight. To get there, the authors should bring up one or more specific hypotheses concerning the reason for the discrepancy (which they have not done so far) and these hypotheses should be tested against any relevant observations and simulations available. One idea that came to my mind is that there might be a seasonal inflow of oxygen-rich water from the North Sea in autumn. This idea is consistent with the fact that the seasonal variation in bottom oxygen is largest at the stations closest to the North Sea, that is Stations BY0 and BY1. If such a seasonal inflow is indeed the cause of the large seasonal variations in bottom oxygen at these two stations, then one would expect a seasonal variation in bottom temperature and salinity as well. So to test this hypothesis, the authors should show time series of observed bottom temperature and salinity at BY0 and BY1.

Specific comments

The authors acknowledge that there are large discrepancies between the in-situ measured and satellite-derived Chl data which calls their validity into question. In any case, my suggestion is to delete Figure 9 altogether, because it is really an aside to what the authors state as their main issue. For that same reason, I would delete Figure 13.

In Table 1, the authors have a column with 'Mean absolute error' and one with 'RMSD'. What is the difference between the two?

What is the precise definition of k_min? It is not the vertical (eddy) diffusivity as it is commonly defined, because that has units m^2/s, whereas k_min has units m^2/s^2.

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