

Biogeosciences Discuss., author comment AC2
<https://doi.org/10.5194/bg-2021-31-AC2>, 2021
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Reply on RC2

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Author comment on "Retracing hypoxia in Eckernförde Bight (Baltic Sea)" by Heiner Dietze and Ulrike Löptien, Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-31-AC2>, 2021

General:

A: We thank the reviewer for time and effort. We agree with the reviewer that boundary conditions are a key element of the model setup and will clarify the rationale of our choice in the revised version of the manuscript. There are ongoing intense discussions on potential spurious effects of all sorts of boundary conditions and the optimal choice seems to depend on the considered problem (cf. e.g. Blayo and Debreu (2005), Herzfeld et al. (2011), Jensen (1998) and many more to follow). We chose closed boundaries because these are (1) easy to interpret, (2) generate in our case realistic results and (3) set a lower bound for the results of our study – i.e. our findings would hold even more if the cross-boundary transports were stronger. In our case switching to open boundary conditions would render model simulations inconclusive rather than adding value because it is not per se clear whether and under which conditions we might over-and underestimate the exchange with the open Baltic Sea.

Please note in this context that we ensured that the variability of our simulated ocean currents is statistically in a reasonable range. Unfortunately, the years of observations did not coincide with the simulation period in this comparison and we thus rely on an assessment of the statistical ranges (e.g. the standard deviation of the simulated meridional surface flow was 0,071 m/s in the simulation LowMix and 0,098 m/s in the observations at station Boknis Eck, when considering two distinct 10-months periods). We will add a table with the respective numbers for different years to the revised version of the manuscript. We also realized that we should, in addition, add a note that tides are negligible in the Baltic Sea and that we excluded the region close to the boundary from our analyses.

We must admit that before reading the reviewers comment, we assumed that we have clearly shown that our conclusions are backed by our boundary conditions. After carefully studying the reviewer's comments, however, we think that there is a misunderstanding and realized that we may have failed to explain the links between horizontal wind-driven circulation, vertical processes and remote versus local effects on oxygen concentrations. We think that confusion may be rooted in the fact that we interchanged these terms frequently in the text and we will edit the text thoroughly in this respect. Further we will add a paragraph or two discussing the reviewer's concern about our boundary conditions.

Our respective argumentation is as follows: One of our main results is that suboxia deep

in EB is the consequence of intermittent inflows of suboxic waters reaching deep into the bay. These wind-driven inflows develop - despite our tank-like boundary which features a no-slip boundary condition (necessitated by our Arakawa B grid discretization). Hence, even in case we underestimate the inflows of suboxic waters because we take out kinetic energy at the boundary of the model domain, the inflows dominate over local oxygen consumption and ventilation deep in the bay. We conclude that the effect of inflows as identified in our analysis is a lower bound. By switching to open boundary conditions, we expect that we may get an even more vivid in and out going circulation because the circulation would be no longer obstructed by the no-slip condition at the boundary of the model domain. While being a more plausible approach at first sight it would render the interpretation more difficult because we could not know whether we under or overestimate the effect of intermittent inflows of suboxic waters reaching deep into the bay.

Blayo, E., & Debreu, L. (2005). Revisiting open boundary conditions from the point of view of characteristic variables. Ocean modelling, 9(3), 231-252.

Jensen, T. G. (1998). Open boundary conditions in stratified ocean models. Journal of Marine Systems, 16(3-4), 297-322.

Herzfeld, M., Schmidt, M., Griffies, S. M., & Liang, Z. (2011). Realistic test cases for limited area ocean modelling. Ocean Modelling, 37(1-2), 1-34.

Specific Comments:

R: 1. The manuscript in its current state lacks literature on general circulation in the EB or the Baltic Sea to support later findings that low oxygen within this bay is imported and not due to local process. I'm assuming there is literature on the physics close to EB to give the reader a general idea to draw conclusions?

A: *We will add a thorough and comprehensive review of what is known about the physics close to EB.*

R: 2. Due to the rigid walls in the northern and eastern boundaries the model acts like a "tank" and to a certain extent its not suitable for resolving remote processes. While restoration can be effective in constraining the model to prescribed values it does not replace the effectiveness of open boundary conditions.

A: *The reviewer argues that our model boundaries act like walls and that the restoration we apply does not replace the effectiveness of open boundary conditions. We agree that it is likely that our implementation of the northern and eastern boundaries dampens in and outgoing currents to a certain extend. Even so, we find a surprisingly vivid connection between the inner EB and the entrance of the EB and, additionally, found a good statistical agreement between simulated and observed current variability at the entrance of the bay (not shown in the original manuscript; we will add a respective table to the revised version of the manuscript).*

Our major finding, i.e. the strong effect of circulation in EB which exchanges water from deep within EB with water at the entrance, is robust towards the choice of the boundary condition. Moreover, we expect that by switching from closed boundaries to open boundaries an even more vivid circulation, unobstructed by walls, might develop. To this end our results of the importance of wind-induced flushing of the bay are a lower bound. Please note that switching to open boundaries would render the interpretation of our results more difficult because we could no longer argue that the choice of our boundary conditions dampens the circulation which protrudes deep into the bay and, hence, that our estimate of the importance of this circulation is a lower bound meaning that in reality it may be even more important.

Note that using closed tank-like boundary conditions is a pragmatic approach which has been successfully applied earlier (e.g. in Dietze et al. 2014; Metzger et al, 2013; Carton and Chao, 1999), is easily interpretable and avoids spurious sources and sinks in cases where restoring fields and prescribed in and outgoing currents are mismatched.

R: 3. The model is able to capture temperature, salinity and oxygen concentrations because its not allowed to drift freely due to the restoration. Hence the good representation from the MedMix and LoMix experiments in the Taylor diagrams. These two experiments may be representing less "aggressive" diffusivity which is also not countered by inflow as compared to the HiMix hence the good agreement. So in its current state the model is mostly suitable for resolving vertical processes as drawn from conclusions of the evaluation.

A: *We will clarify our setup in the revised version of the manuscript: The model is only restored at the entrance of the bay, while our manuscript essentially describes the process of how these conditions at the entrance of the bay are protruding into the bay. The model fidelity is measured at Station Buoy 2a deep inside the bay, far away from the restoring at the entrance. We find that a vivid circulation exists that effectively connects the conditions deep inside the bay (at Station Buoy 2a) with conditions at the entrance of the bay (at Station Boknis Eck). In an ensemble of simulations, we find that the best fit to observations deep in the bay (at Station Buoy 2a) is achieved by using a low vertical diffusivity. We will clarify this in the revised version of the manuscript and put more effort into explaining which processes can be trusted in our model.*

R: 4. The higher oxygen concentrations between 30–31°N and beyond 10°E in Figure 16 suggest that there is an almost permanent feature at the boundary possibly due to the restoration and potentially weak boundary conditions which may not be strong enough to push this into or out of EB. Hence the comment that this model is currently suitable for investigating vertical processes. A hovmuller plot of dissolved oxygen in addition to the water age and residence time in Figure 17 could paint a better picture of how these factors are related.

A: *We disagree. The enhanced O₂ concentrations between 54°30'N - 54°31'N and beyond 10°E are caused by a shallow region, called "Mittelgrund". Hence, the enhanced O₂ is not a spurious feature caused by a dodgy boundary condition: "Mittelgrund" raises up to a few meters short of the surface. It is surrounded by depths of the order of tens of meters depth. Hence, the bottom oxygen concentrations at "Mittelgrund" are typically higher and closer to saturation levels than ambient concentrations, because conditions at the bottom of the shallow "Mittelgrund" are more exposed to ventilating air-sea fluxes of oxygen. We will clarify this in the revised version of the manuscript. Please note also that the actual boundary conditions are further east than the region shown in Figure 16 which may have caused confusion (which we will fix in the revised version of this manuscript).*

R: 5. Machine learning techniques are generally good in forecasting if fed enough data. The reason why there is discrepancies and between ANNs and the model is because wind may not be driving the flow in the model in EB, again, due the rigid walls and weak boundary conditions. The ANNs are able to perform well with just temperature and not wind, and this should be investigated further as its concerning. It may imply that the oxygen is consumed within EB but this can only be resolved if the boundary conditions issue is resolved.

A: *We are sorry for the misunderstanding. The ANN is applied and trained using only model output and not observed values. In our model, we know (as shown e.g. in Figure 20) that there is a strong effect of the wind. This effect is, however, not picked up by our ANN which is trained with the model output. Still, the ANN performed surprisingly well and suggested that the seasonal cycle of oxygen concentrations is surprisingly predictable*

using information on temperature and stratification only (especially given the limited amount of training data). A straightforward explanation is that winds, temperature, stratification are not statistically independent from one another. Note that refinements of the presented ANN approach – such as extending the data base or applying Convolutional Neural Networks (CNNs) (that might be better suited for sequential data) – will be explored in studies to come and are outlined in the discussion. We decided against the use of Long short-term memory (LSTM) artificial recurrent neural networks, because the current state is that they typically underperform at time series forecasting tasks.

R: 6. The authors can use the model in its current state to investigate vertical processes or address the boundary conditions issue which is evidently persistent in the results.

A: *We disagree. We think that the configuration is well suited to showcase the importance of horizontal circulation in relation to local mixing and oxygen consumption. Our model provides realistic results within the typical bound of uncertainties endemic to the current generation of ocean models. Driven by surface winds and the prescribed sea level elevations at the boundary, the model develops a vivid circulation. To make this point more convincing we will add a statistical assessment of the simulated and observed ocean currents at Boknis Eck to the revised manuscript. Certainly, the resulting strong vertical fluxes in the restoring zone are unrealistic and we thus do not consider any model results in the vicinity of the boundaries in our analyses. We will clarify this point in the revised version of our manuscript. Note that this pragmatic approach was successfully used earlier (e.g. in Dietze et al. 2014; Metzger et al., 2013; Carton and Chao, 1999), is easily interpretable and avoids spurious sources and sinks.*

Literature

Dietze, H. , Löptien, U. und Getzlaff, K. (2014) MOMBA 1.1 - a high-resolution Baltic Sea configuration of GFDL's modular ocean model. Open Access Geoscientific Model Development, 7, pp. 1713-1731. DOI 10.5194/gmd-7-1713-2014.

Carton, J. A., & Chao, Y. (1999). Caribbean Sea eddies inferred from TOPEX/Poseidon altimetry and a 1/6 Atlantic Ocean model simulation. Journal of Geophysical Research: Oceans, 104(C4), 7743-7752.

Metzger, E. J., Wallcraft, A. J., Hurlburt, H. E., Zamudio, L., & Chassignet, E. P. (2003). Pacific Ocean Modeling Using the HYbrid Coordinate Ocean Model (HYCOM). In AGU Fall Meeting Abstracts (Vol. 2003, pp. A52F-05).