

## ***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***

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1. Concerning the referee's general comment : "The authors present a lot of figures with results, but the only thing substantive is that the seasonal dynamics of the surface oxygen concentration in the Baltic Sea is better captured using the Liss-Merlivat (1986) + Weiss (1970) formulation than using the standard formulation implemented in GOTM."

We disagree with some of the referee statements. It is true that we have modified the existing GOTM/BIO code in order to simulate better the surface oxygen dynamics, however this is not the main purpose of our study. We address here the problem of

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how important are various factors (physical and biogeochemical) for oxygen cycle in the Baltic Sea. So, we will be glad if the referee gives his/her valuable comments on this problem. Does she/he think that such investigation has been already made?

2. Concerning the referee's general comment: "The authors should certainly inform the GOTM-team (Burchard, Bolding, Villareal) about this finding, but it bears too little general significance to warrant a scientific publication for a wider audience."

We would like to let him/her know that Prof. Burchard has been already informed about our findings.

3. Answers of the specific comments:

1) "Figure 2 in the Manuscript shows that a piston velocity based on Liss & Merlivat (1986) combined with a temperature dependence of the saturation concentration of oxygen based on Weiss (1970) obviously yields a much better reproduction of the observed seasonal surface oxygen dynamics than the standard GOTM formulation (Neumann et al., 2002; Burchard et al., 2006). But to what extent is this improvement caused by the new formulation of the piston velocity and to what extent is it caused by the new nonlinear formulation of the oxygen saturation? My suspicion is that the latter is much more important which could imply that there is no need to replace the constant piston velocity with the Liss & Merlivat formulation. What would the result be, if the authors would use a constant piston velocity, combined with the nonlinear formulation for the temperature-dependence of the oxygen saturation based on Weiss? And what if they would use the Liss & Merlivat formulation for the piston velocity combined with a linear temperature dependence of the oxygen saturation?"

This is indeed a very interesting question to investigate. We have done runs with a constant piston velocity while the saturation concentration of oxygen has been calculated by the Weiss formula. Next, we have made tests with the relationships of Liss and Merlivat, (1986) for the piston velocity and linear relationship between the saturation concentration and temperature. The obtained results for surface oxygen concentration

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are different from these presented in our article. Thanks to the referee's comment, we add a comprehensive analysis of the impact of the different air-sea oxygen transfer models on the surface oxygen dynamics (new Table 1). We would be glad to discuss this issue further with the referee if he/she has any specific ideas.

"In general, the results appear to suggest that the surface oxygen dynamics are much more dependent on the (physical) gas exchange formulation than on the biology/nutrient dynamics. So what do the authors get, if they completely switch off the biological module in their model?"

If we completely switch off the biological module in our model we will not receive any results for oxygen because the oxygen is a state variable of the biogeochemical model (Neumann et al., 2002). However, we have set all constants of the maximum growth and grazing rate of the phytoplankton to zero, i.e. without primary production. The calculations support our finding that at the surface layer the dynamics of the mixed layer and the oxygen exchange with the atmosphere are the controlling parameters of near surface oxygen development. Comparisons with observation data are presented in our new Table 1.

2) "There is a significant discrepancy between the simulated and observed Chl cycles (Fig. 9). Not only that, there even seems to be a systematic discrepancy between observed in-situ Chl (peaking in July) and satellite-derived Chl (peaking in February)! What could be the reason for that?"

The clarity of Fig. 9 seems to have been insufficient, because satellite-derived Chl is not peaking in February as suggested by the referee. Therefore a new and clearer version of Fig. 9 is provided. The model does not simulate very well the Chl cycles (this issue has been discussed in the article, pp. 2130). Nevertheless, it simulates pretty well the oxygen dynamics in the surface and the intermediate layers. For this reason we came to conclusion that the biogeochemical factors are less important than the physical ones. Indeed the discrepancies between the satellite and in-situ data are

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quite large. We are not involved in the collection and processing of the observation data, therefore we can only suggest that the satellite data are often missing the spring bloom peak due to the cloud cover during that time.

3) "The presentation is poor, particularly in terms of style and grammar. In many cases, the articles 'the', 'a' and 'an' are omitted or not used in the appropriate way. I suggest the authors to consult a native English speaker."

We have revised the manuscript in order to improve its style and grammar. The revised copy of the manuscript is attached here as a supplementary material.

Please also note the Supplement to this comment.

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