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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

I'm afraid I do not think this paper is appropriate for publication in OS. 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. 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.

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? 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?

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?

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.

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Interactive comment on Ocean Sci. Discuss., 6, 2115, 2009.