

Interactive comment on “Restoration of the Baltic Proper by decadal oxygenation of the deepwater” by Anders Stigebrandt

Anonymous Referee #1

Received and published: 19 May 2016

Using a conceptually simple model, the author argues that oxygenation of the deep waters of the Baltic Sea could substantially reduce the phosphorus content of the water column. The arguments presented are plausible and mostly convincing, even though there is not much new science in the manuscript. A perhaps novel aspect is the hypothesis that because of positive feedbacks the artificial oxygenation could be terminated after several years without destabilizing the restoration of oxygen and phosphorus to less eutrophic conditions. Still, all quantitative arguments rely on numbers that are just thrown in by the authors, often without reference, justification or error bars/uncertainty estimates. The model is used as convincing argumentation, but it is only an incremental advance with respect to earlier studies, including some by the same author. I value the manuscript as a summary and review paper, and thus think that there is some value in publishing it.

C1

A major criticism regarding the presentation is the lack of absolute clarity required in the joint analysis of the P and O₂ cycles. The manuscript begins (p.2, l.2) with the statement that "vertical circulation" transports P from the deep to the surface waters. This is needed for the positive feedback on production, export etc. However, "vertical circulation" would also transport oxygen to depth, thereby acting as a negative feedback loop, which is not mentioned in the manuscript at all. Later, it is even said that the deep box is assumed well mixed for P but stratified for O₂. Again, this implies different transport agents for P and O₂, which is not correct (and, I believe, not necessary for the main argument). The treatment of P and O₂ transport in the argumentation and story line must be perfectly consistent. This needs careful attention in the revision.

Unfortunately, the presentation is not very convincing, some arguments are not justified, some assumptions are not stated clearly enough and some terms in the equations are not defined. Thus I cannot recommend publication of the manuscript in its present form and ask for a substantial revision addressing the individual points below. The terminology is not always precise. E.g. what do restoration (e.g. title) or recovery (e.g. p.2, l.13) mean? To have scientific value, such terms should be defined better (e.g. with respect to what reference?).

Individual points: p.3, l.6 & l.22/23. Line 3 speaks of water column, line 23 of surface concentrations. This is not the same and thus contradictory to "as already mentioned" in line 22.

p.3,l.24 This does NOT show that the model lacks a source. It could also mean that the model overestimates some sink(s).

p.4, l.2. What is the conceptual two-layer model? Does this include stratification of the deep layer?

p.4, eq.1, eq.2: explain the different Q's. Int sin k should probably read "Intsink" all in italics?

C2

p.5, l.12-13. Where do the estimates for Extsource and Intsource come from? How well are they constrained? What methods has been used? What are the error bars?

p.6, l.4/5 "total P" would be a concentration (perhaps times volume), but the text uses rates.??? And why now 25000-40000 tonnes/yr and not 60000 as used one page above?

p.7 l.15 Where do the numbers come from? What do they mean? Would this the O₂ that is required to make the entire deep box of the model oxic? Or does this assume some bottom boundary layer (which would be inconsistent with the P model part).

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-17, 2016.

C3