

Interactive comment on “The impacts of physical processes on oxygen variations in the North Sea-Baltic Sea transition zone” by L. Jonasson et al.

Anonymous Referee #2

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This paper presents results of a 3D hydrodynamic model for the transition zone between the Baltic Sea and North Sea. Based on their findings, the authors propose that physical, rather than biogeochemical, processes determine the distribution of O₂ within this zone. Whilst the results are interesting and provide new information on the factors controlling O₂ dynamics in this region, the paper is poorly prepared with numerous basic omissions, lack of attention to detail and improper data presentation. This significantly devalues this manuscript. I do not recommend publication until these issues are addressed.

Comments

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Firstly, the style of English is somewhat unorthodox for a scientific paper. The authors mix tenses randomly, with a tendency to write everything in the present tense. This makes it difficult to easily distinguish between what is already known from what the authors did and have found. The Methods should be in the past tense because this is what the authors *did*. Similarly the Results should be in the past tense because this presents what the authors *found*. The present tense or present+past should be reserved for the discussion where the findings are discussed in the context of previous studies. Although the authors cannot be blamed for English language errors, there is an excess of them in this paper, some of which I have listed below. I strongly urge the authors to have the manuscript proof read by a native English speaker.

In relation to this comment, Section 3.1 and 3.2 are model results. Why are they being presented in the Methods section?

The presentation of the model and its application is lacking vital basic information, or if it's there then it is not clearly stated. This makes it hard to follow the results and assess the significance of this paper. For example, for what period is the model applied? From the figures, the results are presented for different periods: 2002-2004, 2002-2006 and 2002-2007. How was the model spun-up and what are the initial conditions? What is the vertical and spatial resolution? The wind speed scenarios are not clear at all. Why focus on August and September? I recommend a small table summarizing the sensitivity analysis experiments in section 2.3.

What is the justification and criteria for simplifying the biogeochemical model as much as possible (P1726,L5)? I agree that a sound physical model is necessary to simulate the biogeochemical dynamics, but since the authors have this, why opt for simple biology? The assumption that constant pools of organic matter are available is in contrast with known dynamics of temperate systems where carbon dynamics are highly seasonal. Moreover, it is well known that respiration is limited by substrate concentration rather than O₂. Perhaps this is partly due to the lack of biological imprint on the O₂ dynamic in this transition zone.

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P1729,L5. Certainly the accuracy of the model should be better than 63 $\mu\text{mol/L}$! The fact this is the threshold for hypoxia is no justification at all for the required model accuracy.

P1730,L3. Why only the autumn months? Surely, using the whole yearly data would better constrain the model. It is also not clear how this improves the spatial error distribution.

L1731, L10-12: I think the authors should at least comment why the model does not capture the O₂ dynamics in Arkona Basin. Is this a failure of the physical or biogeochemical model, or both?

P1732,L8: O₂ looks higher in winter and lower in summer, which is expected due to higher mineralization rates in summer.

P1736,L13: Why assumed? I thought that respiration depends on temperature? If respiration is quasi-fixed, it is little wonder that transport processes account for the inter-annual variability.

Fig. 5. The spatial variation is binned in a 0.5x0.5 degree resolution. Why is a finer resolution not chosen? The fact that the results also include land area is poor presentation of the data.

Fig. 6. Why average O₂ concentrations over the whole transition zone, rather than compare to the individual stations (Fig. 4)?

Minor comments and typos

P1725,L1: ref needed for hypoxia threshold.

P1725,L6: are believed . . .

P1725,L18: anaerobic

P1725,L28: water dominates. . .

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P1725,L30: indicate Great Belt on map

P1726,L6: as much as

P1726,L13: DMI not defined

P1726,L17: coarse not course

P1727,L9: time splitting = operator splitting?

P1727,L15-16: sentence unclear

P1728,L2: experiment

P1728,L8: pronounced

P1728,L23: by 27%

P1729,L7. Define 'bottom water layer'.

P1729,L14: mismatch

P1729,L22: 'gb' in Fig. 2.

P1732,L12: values

P1732,L20: showed

P1733,L2: ... S1 continues through the Great Belt.

P1735,L26: considerably

P1735,L10: Laeso island not indicated on map.

Table 2: has a column header of 'Number of observations', which is obviously incorrect. Proper column headers should be added.

Table A1: 'Bubble effect' is a poor description of this parameter.

Table A1: kb and kp are half-saturation constants

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Table A1: $O2_{SAT}$ not defined.

Fig. 3. Units are missing

Fig. 4. Oxygen units are either μM or $\mu\text{mol L}^{-1}$, but not a combination of both. Keep the same units throughout the paper

Fig.6. Check O_2 units. Define bottom water.

Fig.8. Check O_2 units.

Fig.10. To what do the percentages refer to?

Fig. 11. No definition of right hand axis is given.

Fig.12. State that these are flows through S1 and S2.

Fig.12. It would be insightful to split the respiration sinks into the benthic and pelagic parts.

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