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Ocean Science Discussions

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 #1

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General comments:

The authors address the topic of oxygen variability in the North Sea/Baltic Sea transition zone with special emphasis on temporary occurring oxygen deficit events. A recently developed oxygen model is attached to a well established 3D ocean circulation model to tackle the problem. A sensitivity study is carried out in order to separate the impact of the physical processes from the biochemical ones. During the last decades the area under investigation has become well-known for suffering from hypoxic conditions in the bottom water in late summer and the problem is often discussed in relation to eutrophication due to very high nutrient inputs. The problem addressed in this paper

is therefore of high scientific and social relevance. The applied methods are generally appropriate and used according to common scientific standards. The paper is therefore recommended for publication after minor revisions.

Specific comments:

p. 1724, l. 22: River runoff is not know to be one of the most important drivers in the Arkona Basin. The most important factors driving the dynamics in this region should be the inflow of high saline water over Darss Sill and Drogden Sill, wind induces mixing, either direct or by generating upwelling and internal waves, and hydraulic control of the transport to the east in the Bornholms Gate.

p.1724, l.23: Although haline stratification is an important feature in that area at least the shallower parts show more an intermittent than persistent halocline (see fig. 3).

p. 1725, l.1: Why is hypoxia defined at a limit of <63umol l-1? Either explain or give a reference.

p. 1726, l.5-6: The simplification of the biogeocemical processes should be thoroughly discussed. The applied oxygen model is very simply which has of cause some advantages, but causes on the other hand a severe limitation in the generality and robustness of the approach. There are well established biogeochemical models like ERGOM or SCOBI available in that region and at the involved institutes. Why have these models not been applied, at least for an intercomparison? What are the advantages and the limitations of the applied model?

p. 1726, section 2.1: The description of the physical model is not comprehensive, a reference to a previous paper is not a convenient solution. Further important information should be provided on: Vertical resolution (layer thickness), handling of tides, model initialisation (T&S) and spin-up, river run-off especially in the area covered by the fine grid.

p. 1727, section 2.2: The description of the oxygen model is even more incomprehen-

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sive. The motivation to build the model the way it has been done is completely missing. Although the model is highly parameterized nothing is said about the calibration and the limitations due to the severe simplifications.

p.1728, l.22-23: How is the increase/decrease in wind speed done? This should be described in some detail, because the method of the applied change might have an important impact.

p. 1729, l.4-5: Why? I can't follow the argument that it should be sufficient that the model error should be less than some threshold value assigned to the simulated system. If the model e.g. has a very low error during a large part of the season, but always misses the hypoxic events it might have a very low overall error but is completely inadequate for the problem at hand (see e.g. fig. 4g). If a validation strategy shall be applied to a specific problem, than it should be done in a specific way. Applied to the problem of a threshold value for hypoxia this means, that the model skill with respect to this feature should be tested explicitly. If the aim of the model is to predict hypoxic events and a threshold value for this event has been defined it should be handled like a categorical forecast and the well know methods of categorical verification statistics (e.g. contingency tables) should be applied.

p. 1729: The formulas for all metrics should be given (e.g. AAE). In order to be comprehensive it would be preferable to give all contributions to the cRMSD, i.e. the RMSE and the bias, separately

p.1732,l. 21: Averaged over which domain? The whole North Sea/Baltic Sea area?

p.1723,l.23-24: How is the transport calculation done? How is 'bottom water' defined? (The water column below 15 m depth is not well mixed, so this whole column can't be defined as bottom water altogether.)

p. 1734, l. 17-18: That 'the transport through the Danish Straits is mainly regulated by the barotropic signal outside the modelled domain' can't be concluded from the pre-

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sented sensitivity experiments. The outcome of the experiments will crucially depend on the way the wind field is modified. The observed barotropic transport through the Danish straits is not a steady state problem. To conclude that the differences in the observed transports in 2002 and 2004 are related to differences in the wind field during that time in a steady-state manner is completely misleading.

Technical comments:

p.1726, l.6: mush =>much

p.1728, l.8: pronounce=>pronounced

p.1730, l.9, are=>is

p.1730, l.12, and it=>as

p.1731, l.6, range=>ranges

p.1732, l.10, decreased only=> decreased by only

p.1732, l.12, cover=>covers

p.1732, section 4.1, flow => transport

p.1732, l.27, stagnant=>stagnation

p.1733, l.2, continuous=>continues

p.1733, l.4, water=>layer

p.1733, l.21, because of=>for

p.1734, l.15, show=>shows

p.1735, l.4, weaken=>weakened

p.1737, l., boarder=>border

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