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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.

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We would first like to thank reviewer 1 for his or her comments and suggestions. Our response to him/her should be considered as a quick reply and a more detailed response will be submitted together with a new version of the ms when we received the comments from the other referees.

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1) 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.

Original sentence:

"The circulation is characterized by inflowing high saline bottom water from the North Sea and outflowing low saline water from the Baltic Sea. This circulation is largely driven by sea level difference between the North Sea and the Arkona Basin which in turn is controlled by wind and river runoff to the Baltic Sea."

Sorry for the misleading sentence, will rephrase this sentence to the following:

"The circulation is characterized by inflowing high saline bottom water from the North Sea and outflowing low saline water from the Baltic Sea. This two-layer flow is often interrupted by large barotropic transports which are driven by sea level difference between the North Sea and the Arkona Basin.

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2) 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).

Old sentence: "The water column is stratified with a strong persistent halocline and the stratification is enhanced during summer by the development of a thermocline"

New sentences: The water column is stratified with a sharp halocline which is only occasionally destroyed by strong mixing events. The stratification is usually enhanced during summer by the development of a thermocline.

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

Two references to the definition of hypoxia will be given in an updated version of the

ms:

HELCOM: The 2002 oxygen depletion event in the Kattegat, Belt Sea and Western Baltic, Technical report, Baltic Sea Environment Proceedings No. 90, 2003

Diaz, R. J. Overview of hypoxia around the word, journal of environmental quality, (30:2)

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4) 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?

A discussion about the advantages and the limitations will be added together with a motivation of why we use the simplified model in this study. Either we add this discussion in the model description or in the “discussion section”.

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5) 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.

A figure will be added (Fig. 1b) showing the vertical grid structure for the two domains.

More information of the model setup will also be added to Section 2.1: . . . The vertical resolution in the coarse grid varies between 2-50m and in the fine grid 2-8meters. Some special consideration is needed when constructing the vertical grid structure

because the model is not able to handle a completely dry grid point. In order to comply with this constraint the surface layer has to be thicker than the minimum sea level. A surface layer thickness of 8m is used in the coarse domain but in the fine domain the sea level amplitude is smaller and a surface layer of 2m is enough. The vertical grid structure can be seen in Fig. 1b. Climatologies of salinity and temperature are applied along the outer lateral boundaries as well as tidal constituents and sea level elevation from a 2-D version of BSHcmod covering the North Atlantic. 79 rivers are included in the setup of which 9 is enclosed by the fine grid. Daily freshwater input is collected from observational data for 5 German rivers, model data for 43 catchment areas from the HBV model run in Swedish Meteorological and Hydrological Institute (SMHI) and as a last resort climatological data is used. . . . Salinity and temperature data from operational model runs at the Danish Meteorological Institute (DMI) was used as initial fields and the model run 1.5 years (2000/07 – 2002/01) before any experimental simulations was carried out. . . .

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6) p. 1727, section 2.2: The description of the oxygen model is even more incomprehensive. 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.

I think this discussion can be added together with 3)

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7) 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.

The wind speed experiment was carried out in the following way:

In Simulation 3 the model was started with initial field from the reference simulation

in July 2002 and run until November 2002 and the u and v component of the wind in September was multiplied by 1.27.

In Simulation 4 the model was started with initial field from the reference simulation in July 2004 and run until November 2004 and the u and v component of the wind in September was divided by 1.27 to reach the same monthly mean as 2002.

This will be explained clearer in the revised ms.

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8) 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.

You are right. That the model error is lower than the threshold limit of hypoxia does not really prove that the model correctly simulated the hypoxic events. It can still miss or predict a false event. We will consider either rewriting this or adding additional validation to support our statement. A contingency table might be a good idea.

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9) 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

In section 3 we will add the formulas for the remaining metrics and in the text the following will be added/changed:

. . . Here we use a combination of several metrics: bias, root mean square difference (RMSD), absolute average error (AAE), centered root mean square difference (cRMSD) and model efficiency (MEF). The mathematic formulation for each metric is given below in Eq. (1-5). The bias, the RMSD and the AAE is commonly used in model validation but the cRMSD and MEF might need some further explanation. The cRMSD is the unbiased root mean square difference (RMSD) and can be seen as the combined effect of misïňĄts in amplitude and the correlation. The model efficiency (MEF) in Eq. (2) was introduced by Nash and Sutcliffe (1970) and produces a model skill score which can be easily compared to other models. . . .

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

Old sentence: "The average wind speed over 2002–2004 (Fig. 7a) show no spatial variation in the domain and was 6.4 and 8.0 m s−1 during summer (May–August) and winter (November–February), respectively."

It is rephrased by the following: The monthly average wind speed over 2002–2004 (Fig. 7a) in the fine domain (Arkona Basin excluded) was 6.4 and 8.0 m s−1 during summer (May–August) and winter (November–February), respectively.

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11) 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.1732

Basically we calculated the daily transport by summing up the velocities for every

timestep at each transect. We have chosen to define the bottom water as the water column below 15 m because this is where the halocline and the summer thermocline are situated at the location of transects 1 and 2. However, as this might be misleading we will instead clearly state that it is the water transport below 15m that we measure.

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12) 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 presented 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.

This statement will be removed or rephrased.

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13) Technical comments:

All technical comments and misspellings will be considered in the new version.

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Interactive comment on Ocean Sci. Discuss., 8, 1723, 2011.