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Ocean Science Discussions

Interactive comment on "Influence of climate parameters on long-term variations of the distribution of phytoplankton biomass and nutrient concentration in the Baltic Sea simulated by a 3-D model" *by* L. Dzierzbicka-Głowacka et al.

Anonymous Referee #1

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This manuscript present results from three long term experimental simulations with a coupled hydrodynamical-ecological model in the Baltic Sea. The purpose of these simulations is to estimate the influence of altered atmospheric forcing on phytoplankton and nutrient concentrations. This is a highly relevant topic but unfortunately the overall quality of the manuscript is poor regarding both presentation and scientific content and can not be recommended for publication in Ocean Science. The main concerns are stated below.

Validation of both physical and ecological model:

One of the most important aspects of ecosystem modelling is the underlying physical model. The authors only present model-observation comparison of SST and only for the southern Baltic Sea and no reference is given to a more thoroughly validation with this model set up. The SST is mainly determined by the atmospheric forcing and this really does not give much information about the model performance. For instance, how well is the vertical stratification resolved? Is 9km horizontal resolution in the Danish Straits enough to model the water exchange between the North Sea and the Baltic Sea?

The validation of the ecological model is also far from sufficient. A comparison of modelled and observed Chlorophyll concentrations is seen in fig.6b but it does not give much information. DIN concentration is not even compared to observations. The model needs to be thoroughly and quantitatively assessed before this kind of sensitivity analysis can be made.

The ecological model is too simple:

It is questionable if such simple model can be use in this kind of long term simulations. Neither phosphorus nor oxygen is included in the model. Increasing the wind 30% will definitely have a large impact on the bottom oxygen condition which in turn will have an effect on the sediment phosphorus release and the N:P ratio in the water. This change could affect the nitrogen fixation (which is also not included in the model) an in turn the primary production and phytoplankton concentrations. The authors mention this at p.538 "...although cyanobacteria overcome N shortage by N-fixation, so primary production is limited by available phosphorus" there is no justification on how this process can be neglected.

To my understanding the zooplankton biomass is prescribed by observation. What is the reason for not including it as a state variable? It is highly unlikely that the zooplankton biomass will stay constant while the phytoplankton biomass increases. At page 542 "the zooplankton biomass is prescribed as a force and it uses abundance data from the

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Mankowski (1978), Ciszewski (1983) and Mudrak (2004) for the southern Baltic Sea". But the model domain covers the whole Baltic Sea, the Kattegat and the Skagerrak. What data is used here?

What kind of data is use for the lateral boundary condition in Skagerrak?

Conclusions are not supported by the presented results:

All conclusions are based on surface nutrient and chlorophyll data, what about the subsurface concentrations? A more accurate analysis would be to compare the vertical integrated primary production. A fourth reference simulation would also be beneficial.

P. 548 "The results show significant changes in phytoplankton biomass Phyt distributions, which take place in areas (open sea), where there is a considerable increasing in currents"

- This change in currents is not presented at all.

P. 548: "It is the result of the rise in nutrient concentration Nutr (Fig. 11) in the upper layer caused by the increasing of the wind speed, i.e. by mixing deep."

- This increased mixing is not documented. At least some salinity profiles should be presented.

Next sentence says P.548: "With the parameter values in scenario 2 and 3, for increasing turbulence (mixing) (30% increased wind speed and western component of wind speed, ...)".

- I do not understand this at.

P. 549: "The results are consistent with in situ observations for temperature and chlorophyll-a for five years (2000-2004)."

- Only SST have been compared to observations and it has a significant bias of 1.4 degrees C. From figure 6b I would not say that the observations are consistent with

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observations.

Other remarks:

The manuscript is sometimes difficult to follow. The language needs to be improved and the structure is sometimes confusing. This is especially true for the model description. Page 539 - 541 includes four model equation but only 3 of them are numbered. Integral limits are missing, misprints and new equations are presented in the section “parameters”.

At page 540: “The state Eq. (2) for nutrient includes the first four terms on the right hand side (/. . ./) and the four processes nutrient uptake (UPT), dark respiratory release (RELE) . . .”, but (RELE) and (UPT) is not visible in eq.(2). Instead one need to look in Table 1 to see that RELE=RESPdark. This section must be made clearer.

Presentation of the results and discussions are mix together which leads to confusion of what is actually modelled.

Interactive comment on Ocean Sci. Discuss., 8, 533, 2011.