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Ocean Science Discussions

Interactive comment on "The 2007 North Atlantic spring bloom in operational analysis from the TOPAZ system" *by* A. Samuelsen et al.

A. Samuelsen et al.

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Reply to reviewer # 1

This reviewer had no specific criticism of the manuscript.

Reply to reviewer # 3

Reviewer: The main concern is that "The model is not able to reproduce the spring bloom" and "the key ecosystem components driving the spring bloom is neither identified nor reproduced in the model"

Reply: The main purpose of this study was to examine the impact of assimilation of physical variables on the biological model and we are confident that the conclusion of the paper still holds whether the primary production is over- or underestimated. We also evaluated the model performance and it was found to be good in some aspects

and not so good in other. This coupled model is under development and improvement, but in view of the main goal of the paper, we do not agree that further improvement is necessary for this particular paper. The reviewer suggest that the errors when compared to the data need to be non-significant, we are not certain what is meant by this, but assume that the reviewer mean that the model error should be within the limits defined in the first paragraph of "3.1 General performance". We think that this is a quite strict criteria for a medium-complexity coupled biological physical model and particularly for a large scale model, as parameters will not be optimal everywhere. In addition, we did not account for errors in the climatological and satellite data that the model was compared to. For the satellite data the coastal areas are most likely overestimated because the algorithm used was for case I waters. For the nutrient climatologies, the open ocean values are probably more uncertain because these areas are under-sampled. Unfortunately very few in-situ data are available to us. However the ones that we had (Figure 5) actually showed a better agreement with the model than indicated by the satellite and climatological data.

The spring bloom in the Norwegian Sea is well studied, it is the result of deep mixing during winter providing nutrients, increased light-availability in the spring, and finally the shoaling of the mixed layer. All of these processes are present in the model, although we are not saying that they are perfectly reproduced in the model. The model does actually produce the spring bloom. The cause of the largest errors in this regard is the timing of the spring bloom which is later than observed. This is primarily a problem with the physical model, which has a delay in the onset of the seasonal mixed layer compared to the actual timing. Assimilation at the surface does not correct this problem, but the assimilation of T and S profiles from ARGO floats, which will be included in the system, may improve the representation of stratification in the model. I.e. the mismatch in the timing is not a problem of the biological model, but a problem of the physical model.

We will clarify the purpose of the paper both in the abstract and introduction. In addition

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the title will be changed to emphasize the focus on sensitivity to data assimilation rather than a model validation of the spring bloom.

Specific comments

Reviewer: The authors should clearly indicate the statistical significance of the differences between model results and the (satellite) observations instead of restrict their comments to expressions such "too high, too low" (page 344, lines 16-18).

Reply: Numbers will be inserted throughout the manuscript where the somewhat vague expression "too much" and "too little" have been used.

Reviewer: Although the ecosystem model is described in literature, some additional information about it is required here as the core of the paper is just assessing the phytoplankton chlorophyll, a biological ecosystem component, during springtime. A figure showing the ecosystem model structure and interactions will be helpful to better understand the model functioning.

Reply: A figure showing the structure of the ecosystem model will be added to manuscript.

Reviewer: A table showing model parameters will be useful to identify eventual model parameterisation deficiencies.

Reply: The model description, complete with parameters is already published (Skogen et al., 1995), in addition the derivation of the model is described in Aksnes et al. (1995). Therefore we think it is unnecessary to include these here, however we did some small changes to the code and we will include a table with the new parameters. In addition we will include some of the key aspects of the model in the text.

Reviewer: The fact that surface chlorophylls are underestimated in spring and overestimated in summer suggest a failure the ecosystem model identification of key state variables, state variable interactions or model parameterisation. The authors suggest an excess of primary producers consuming surface nutrients and preventing them-

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selves to hold and increase their biomass in spring. The lack of grazers is also pointed out as a possible reason to explain higher chlorophyll concentrations than those expected in summer (page 354, lines 24-28), etc. What about a possible limitation of the hydrodynamic model to simulate the biological state variables? Were the 22 layers in the hydrodynamic model enough to reproduce the processes in the vertical dimension?

Reply: There is definitely a possibility that deficiencies in the physical model affect the result. One example is excess mixing that delay the spring bloom. The 22 layers is not much, but the layers are most dense in the upper water column. Because this is a hybrid layer model the vertical resolution will vary with region and season, for example in the northern regions there are generally about 10 layer in the upper 100 meters, which is probably sufficient vertical resolution, but in the tropics there can be as little as 3 layers in the upper 100 meter (typically 5-7) which probably is not enough. In the next version of this forecasting system the upper 5 layers will be fixed thus assuring the upper water column is resolved, this is still being developed on the physical side. However we expect the conclusions will still hold using higher vertical resolution.

Reviewer: What about the vertical profiles of chlorophyll concentration? No information is provided about the model performance regarding this topic.

Reply: We are currently unable to asses the vertical profiles of chlorophyll concentration because we do not have many vertical profiles to compare the model to. The chlorophyll measurements in the Faroe-Shetland channel are only at 3 depths and don't really qualify as a profile. However, we will change figure 5, so that the reader can get a better impression of how the model variables vary with depth compared to the data.

Reviewer: The expression -underestimated mortality- (page 354, line 28) if referred to phytoplankton should be changed by any other such as -low phytoplankton consumption by zooplankton-, as referring to phytoplankton mortality should not be appropriate.

Reply: Expression has been changed.

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Reviewer: According to the authors, the (coupled) model -is not optimised for coastal regions- and suggest that the present results should provide nesting conditions to a coastal model (page 355, lines 9-11). If this was a goal of the paper this should be indicated at the end of the introduction section and not here. Moreover, TOPAZ is for -operational prediction of North Atlantic European coastal zones- (page 344, lines 3-4) what makes this point confusing.

Reply: We will point out that the model is meant for open ocean regions in an earlier in the paper. The acronym for the operational system TOPAZ was inherited from a previous project by the same name (2001-2003) that also considered coastal zones. The system has gained visibility as such. We do not wish to change the system name and we will simply remove the definition of the acronym.

Reviewer: The authors indicate that -the model will be set up for operational forecasting in the Atlantic and Arctic Ocean- (page 356, lines 20-21). (i.e. to obtain non significant differences) to the observations.

Reply: Other efforts in model parameterization and data assimilation are ongoing within the European MyOcean project (2009-2012) that will lead to more realistic demonstrations.

References Aksnes, D. L., Ulvestad, K. B., Balino, B. M., Berntsen, J., Egee, J. K., and Svendsen, E.: Ecological modeling in coastal waters - towards predictive physical-chemical-biological simulation-models, Ophelia, 41, 5-36, 1995. Skogen, M. D., Svendsen, E., Berntsen, J., Aksnes, D., and Ulvestad, K. B.: Modeling the primary production in the North-Sea using a coupled 3-dimensional physical-chemical-biological ocean model, Estuarine Coastal and Shelf Science, 41, 545-565, 1995.

Interactive comment on Ocean Sci. Discuss., 6, 343, 2009.