Biogeosciences Discuss., 6, C1806–C1813, 2009
www.biogeosciences-discuss.net/6/C1806/2009/

Biogeosciences
Discussions

Interactive comment on "Statistical validation of a 3-D bio-physical model of the western North Atlantic" *by* M. K. Lehmann et al.

M. K. Lehmann et al.

katja.fennel@dal.ca

Received and published: 3 September 2009

Below we repeat the reviewer's comments with our responses interspersed in ***bold-italics***.

General comments: The manuscript "Statistical validation of a 3-D bio-physical model of the western North Atlantic" by Lehmann et al., uses the results of a size-structured ecosystem model in a high-resolution, regional circulation model of the northeast North American shelf and adjacent deep ocean in order to assess whether the added functional complexity of two functional phytoplankton groups improves the model's ability. They used satellite derived SST and sea surface chlorophyll for model-data statistical comparison.

The topic is suitable for the journal with broad international interest. The paper is in general properly organized, well written and explanations are clear. It can be seen that a lot of work went into model development (coupling of Lima and Doney with ROMS, parameterization of the ecosystem model etc.) and analysis of model results. However, I have a few concerns that are given below. Depending on how authors respond to my comments, I support publication in BG after revision.

1) My main concern is that the ecosystem model used in Fennel et al., (2006, 2008) is rather different than the current model (Lima and Doney, 2004). In the current model phytoplankton growth follows Geider's formulations and it includes phytoplankton cell quotas etc. Also it seems like considerable tuning is made on the current model parameters to fit the data (cf. Table 1). The main conclusion of the paper is that "improvement in model skill can be directly attributed to an additional phytoplankton group". However, it is not really proven that an additional phytoplankton group is really improving the model skill or a better model with improved tuning is doing part of the trick.

Response: We agree with the reviewer's comment that "...it is not really proven that an additional phytoplankton group is really improving the model skill..." and we modified the text on page 5680 (first sentence of the conclusions) to make this explicit: "The implementation of the size-structured model by Lima and Doney (2004) improved the agreement between model-predicted fields of surface chlorophyll with chlorophyll estimates from the SeaWiFS satellite compared to the model by Fennel et al. (2006, 2008) across an ecological gradient from the productive MAB to the oligotrophic Sargasso Sea."

We would also like to point out that it was not our objective to prove that added complexity improves predictive skill. In decreasing order of importance our objectives were: 1) to implement a size-structured model for a high-resolution domain in the northeast North Atlantic (to the best of our knowledge this is the first implementation of a size-structured model in a high-resolution domain for a continental shelf), 2) to assess the model results in a statistically rigorous analysis,

and 3) to contrast the results with previous simulations for the same region that use a simpler biological model.

We feel that the contrast with the model of Fennel et al. (2006, 2008) is a useful addition to the core objectives 1) and 2), but not the primary purpose. Also, we don't intend to make general statements about size-structured models performing better than single phytoplankton models and we have modified potentially ambiguous sentences in our revision.

2) If we consider that the inclusion of an extra phytoplankton group improves the model results considerably, it would be good to see what the authors think of the implications of these results on the conclusions made in Fennel et al. 2006. Do they think these conclusions are still valid? This could be discussed in the manuscript which would contribute some scientific aspects on top of the pure technical results of the paper.

Response: In the Fennel et al. (2006) paper a nitrogen budget for the MAB was presented. While the details of a MAB nitrogen budget derived from the present model (i.e. the individual nitrogen flux estimates) will differ from the estimates in Fennel et al. (2006), not only because of model differences, but also because of interannual variability, we have no reason to believe that the main conclusions are affected. We did not carry out a nitrogen budget analysis with the present model yet – it is outside the intended scope of this study.

3) I appreciate that the authors did a very thorough statistical comparison of model chl with observed chl. However, I am curious to see how the other fields compare, for example isn't there any nutrient observations from the region that the authors can use to compare with model nutrient fields? In Fennel et al. 2006 some climatological values were used for this comparison.

Response: We prepared a comparison of simulated nitrate fields with climatological data for the inner and outer shelf of the MAB and for the slope region (i.e. the regions for which we have climatological nutrient data available). The Figure

is included with this response. The observed climatological monthly means are shown in green; the monthly means simulated by the present model are shown in blue; and the simulated means from Fennel et al. (2006) are shown in red. The errorbars indicate 1 standard deviation. The values simulated by the present model agree well with the observed means and are within one standard deviation. The simulated values in the present study differ from the values in Fennel et al. (2006) in the winter and early spring (i.e. Dec, Jan, Feb, Mar and Apr). While data is lacking in Dec, Jan and Feb, it appears that the simulated fields of the present study agree better with the observed climatology in March and April.

Unfortunately nutrient data are not available at sufficient resolution to attempt statistical comparisons similar to those presented for sea surface chlorophyll and SST.

Specific comments: Page 5665 lines 8-13. This part does not seem to belong to introduction

Response: We removed the paragraph.

Page 5668 (Table 1). Authors should briefly mention the justification beneath the modification of parameters on Table 1. How are these parameters set/adjusted?

Response: We inserted the following text on page 5668 "We modified the parameters given in Table 1 from their original values because these modifications improved model-data agreement. For the purpose of parameter tuning we implemented 1-dimensional models for several locations within our model domain, which allowed us to perform many simulations with different parameter sets."

Page 5668 line 19. Does silicate limit growth in this region? If yes the implications of not including Si for large phytoplankton group should be discussed.

Response: We are not aware of any evidence for systematic or widespread silicate limitation in our study region. Malone et al. (1980) observed silicate deple-

tion during a diatom bloom in the plume of the Hudson River, however, as can be seen in Fig. 3 of their paper, silicate depletion was local and limited only to the freshwater plume. Silicate levels of 5 micro gram per liter were observed outside the plume at salinities above 32.

T. C. Malone, C. Garside and P. J. Neale, (1980) Effects of silicate depletion on photosynthesis by diatoms in the plume of the Hudson River, Marine Biology, 58(3):197-204

Page 5672 (Fig 4). Boundary problem?

Response: A boundary artifact is present at the southwestern open boundary where the Gulf Stream enters our domain. We prefer to refer to this as artifact rather than problem, because effects of this nature are a reality in nested applications. We would like to point out that we did not have simulations of the biological variables from a larger scale model with the same biology available.

We added the following text to our manuscript on page 5672: "A boundary artifact is noticeable at the southwestern boundary where the Gulf Stream enters the model domain and simulated chlorophyll concentrations are lower than observed."

Page 5674 lines 5-10. I believe it is a success that the model can simulate shifts in phytoplankton for different regions. For example diatoms dominate MAB North and MAGOM shelf break and MABGOM slope during fall-spring and small phyto dominate Sargossa Sea during this time. I think it would be useful if authors can discuss how relative phytoplankton distribution (small vs large) compare with available data. For example, I know that the Sargossa Sea is dominated by small phyto even during spring, so model results in this region should be consistent with observations.

Response: We added references to the pertinent literature and changed the text from the sentence beginning in p. 5674, line 8 to the end of the paragraph as

follows:

This agrees with the succession of phytoplankton size classes observed on the continental shelf by O'Reilly and Zetlin (1998), who report that the end of the spring bloom is marked by a decline of the chlorophyll contribution by large phytoplankton and increasing importance of small phytoplankton. In contrast to shelf waters, picoplankton dominates throughout the year with a slight increase in the contribution of large phytoplankton during the winter-spring bloom in the model for the Gulf Stream and Sargasso Sea regions (Fig. 6). This dominance of picoplankton and a diatom bloom following winter mixing is a classic feature of the open ocean as observed at the Bermuda Atlantic Time Series site (DuRand et al. 2001, Li Harrison 2001).

Page 5674 line 25. This boundary problem is obvious on Fig. 4

Response: Yes, on page 5675, line 25 we mention the boundary artifact. See also comment above.

Page 5677 line 14-17. I guess, here the authors refer to ME of Sargossa Sea SST, because ME of Sargossa Sea chlorophyll (Fig. 9) is negative as frequently as others.

Response: Yes, we clarified this in the text.

Page 5678 lines 4-6. This part is not clear to me. How large phyto is fueled on the shelf is not really explained.

Response: We added the following text: "This shift is in line with a well-established paradigm in plankton ecology, reviewed and extended by Cullen et al. (2002), which states that small phytoplankton with high ratios of surface area to volume and, hence, low sinking rates and an advantage in competition for nutrients dominate in stable, nutrient depleted waters like the Sargasso Sea. The microbial loop, which relies on recycled production of small phytoplankton is present everywhere in the ocean, but dominates biomass only in stable,

low-nutrient regimes like the Sargasso Sea. The relatively nutrient-rich, coastal waters are characterized by food webs with larger cells that transfer a greater proportion of primary productivity to higher trophic levels and allow for a greater proportion of primary productivity to be exported vertically."

Cullen, J.J., Franks, P.J.S., Karl, D.M., Longhurst,A., Chapter 8: Physical influences on marine ecosystem dynamics, In: The Sea (eds. A. R. Robinson, J. J. McCarthy and B. J. Rothshild), John Wiley and Sons, New York, Vol. 12, p. 297-336, 2002.

Interactive comment on Biogeosciences Discuss., 6, 5661, 2009.

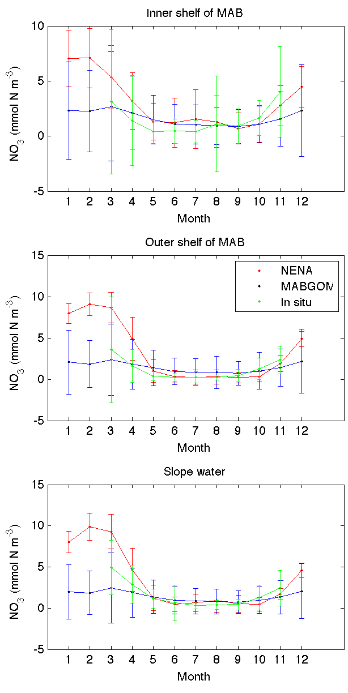

Fig. 1. Comparison of simulated nitrate fields with climatological data for the inner and outer shelf of the MAB and for the slope region.