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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.

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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

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Interactive Discussion

Discussion Paper

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.

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.

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.

Specific comments:

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

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

BGD

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Interactive Comment

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.

Page 5672 (Fig 4). Boundary problem?

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.

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

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.

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

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