Biogeosciences Discuss., 11, C710–C715, 2014
www.biogeosciences-discuss.net/11/C710/2014/

Interactive comment on "Using biogeochemical data assimilation to assess the relative skill of multiple ecosystem models: effects of increasing the complexity of the planktonic food web" *by* Y. Xiao and M. A. M. Friedrichs

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Received and published: 3 April 2014

Response to Reviewer #1:

We appreciate Reviewer #1's input on this manuscript, and hope that we've fully addressed the comments/questions provided.

General comments: This study compares five lower trophic level ecosystem models with increasing numbers of P and Z state variables in terms of their ability to reproduce observed patters in remotely sensed surface chlorophyll and POC. The comparison

is made by assessing changes in the cost function following data assimilation for a set of four test sites, and by computing the cost function for models with optimised parameters at four crossvalidation sites. The approach is useful in that it isolates the effects of model complexity on fit to real data (without confounding with other aspects of model architecture), and the results are consistent with previous findings that models of intermediate complexity tend to perform best in terms of their ability to reproduce observations. The results are clearly presented and the discussion is succinct. For several components of the methods the reader is referred to Xiao and Friedrichs (2014), which is currently in review. While the submitted version of this manuscript appears to be available online, the relevant aspects of the methods referred to in this discussion paper have not yet undergone the full peer review process.

Response: We are pleased to report that Xiao and Friedrichs (2014) is currently accepted by JGR and should be published online soon.

Specific comments:

1. Methods section 2.2 (satellite-derived data): at what scale (spatial and temporal) were these data extracted?

1. Response: We used 9 km daily data and have added this information in Section 2.2. Specifically we had roughly 100 data points distributed over the 365 days, as shown in Figure 2.

2. Page 487, lines 15-18: is there a reference for this statement?

2. Response: We intended this to be a very general statement about optical depths, but feel it is somewhat out of place here and have removed the statement.

3. Methods section 2.4: can the authors provide a brief justification for the selection of sites?

3. Response: Sites were selected such that they represented various locations throughout the MAB (northern, central and southern) as well as at varying depth levels:

on the shelf, near the shelf break, and off the shelf in deep (>2000m) waters. This text has been added to Section 2.4.

4. Discussion and Conclusions: while the authors do provide a useful evaluation of the tradeoffs associated with overtuning as the number of parameters being optimised increases, I am a little concerned that the assessment of a posteriori costs is confounded with (a) the level of fit to data with the initial parameter set (which may vary between models), (b) the efficiency of the optimisation for different models, and (c) the actual skill of the model after optimisation. If the authors could provide some commentary on this issue I think it would help with interpretation of their findings.

4. Response: How are the a posteriori costs affected by:

(a) A priori level of fit for the five models?

We actually attempted to have roughly similar a prior costs for the five models, but it was nearly impossible to have identical values. We feel that the magnitude of the a prior costs are not significantly affecting the a posteriori costs for two reasons: (1) We conducted some experiments modifying the initial guesses of the parameter values, and typically this affected the a priori costs, but not the a posteriori costs, i.e. the same cost function minimum was obtained, and (2) the model with the highest a priori cost produced the lowest a posteriori cost (Table 3). For these reasons we feel the a posteriori results are not substantially affected by the fact that the five models vary in regards to their initial costs.

(b) Efficiency of optimization for the five models?

We are not completely sure what the reviewer means by the "efficiency of optimization". The five models undergo the identical optimization routine, so in that sense the efficiency is identical for all five models. The only additional factor that affects the optimization is the a priori level of fit, which is discussed above.

(c) Actual skill after optimization for the five models?

Here we define the skill after optimization as the a posteriori cost, primarily at the cross validation sites. We feel that if the models are tuned to the DA site observations, then their skill can be adequately tested by seeing how well the models fit the unassimilated CV site observations.

5. Page 550, line 10: but MZ (medium zooplankton) is not a state variable in any of the models specified (unless I've missed something).

5. Response: We thank the reviewer for their careful reading and catching this typographical error!

6. Appendix A: It would be helpful to have a list of the symbols for parameters that were optimised (and are named in Table 1).

6. Response: Excellent idea. The symbols for the optimized parameters is now included in Table 1.

7. Figure 2b: the fit to data for a posteriori simulations is fairly poor from just visual examination, at least in comparison with examples of optimisation for NPZD models that I am more familiar with (Kidston et al. 2011, 2013, Melbourne-Thomas et al. 2013). Can the authors make a general comment on the ability of their models to capture seasonal cycles for this system?

7. Response: This is a valid point and a couple sentences have been added to address this issue at the end of the second paragraph of the Discussion: "However, even after assimilation all five models still had difficulty reproducing the seasonal cycle of this system. This is most likely due to issues related to the physical fields obtained from the 3D simulation used to force these models."

Technical corrections:

1. Page 483, line 13: should read "lower trophic level model".

Done.

2. Page 484, line 20: unclear to me what is meant by the term “community species”.

Deleted.

3. Page 484, line 22: suggest replacing the word “truth” with “observations”.

Done.

4. Page 487, line 3: the word “have” should be “has”.

Done.

5. Page 487, line 15: abbreviation “MAB” hasn’t been defined.

This sentence was deleted, but “MAB” is now defined before it’s first usage.

6. Page 488, line 2: the word “include” should be “includes”.

Done.

7. Page 488, line 13: the word “represent” should be “represents”.

Done.

8. Page 495, line 23: suggest replacing “a deterioration in the Total_cost” with “an increase in the Total_cost”.

Done.

9. Table 1: units for maximum Chl:C ratio – mg Chl mg C-1 – is this right? Not just Chl C-1 or mg Chl mg-1 C-1?

The correct units are (mgChl)/(mgC) or (mgChl)(mgC)-1. We’ll see if the typesetter can make this clearer.

References:

Kidston, M., R. Matear, and M. E. Baird. 2011. Parameter optimisation of a marine ecosystem model at two contrasting stations in the Sub-Antarctic Zone. Deep Sea

Research Part II: Topical Studies in Oceanography 58:2301–2315.

Kidston, M., R. Matear, and M. E. Baird. 2013. Phytoplankton growth in the Australian sector of the Southern Ocean, examined by optimising ecosystem model parameters. Journal of Marine Systems 128:123–137.

Melbourne-Thomas, J., S. Wotherspoon, S. Corney, E. Molina-Balari, O. Marini, and A. Constable. 2013. Optimal control and system limitation in a Southern Ocean ecosystem model. Deep-Sea Research Part II. DOI: 10.1016/j.dsr2.2013.02.017.

Response: Thank you!

Interactive comment on Biogeosciences Discuss., 11, 481, 2014.