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Biogeochemical versus ecological consequences of modeled ocean physics

Sophie Clayton, Stephanie Dutkiewicz, Oliver Jahn, Christopher Hill, Patrick Heimbach, and Michael J. Follows

Response to reviewers:

We thank the two reviewers for their helpful comments on the revised manuscript. We have set out our responses to reviewer #3's specific comments below, in blue.

Reviewer # 3

Overall, I find the manuscript much improved from the last version. Some critical details, such as a better description of the model set-up, have been added and the explanations for the model differences are now much more concrete. However, I still think that the key messages of the paper could be improved. There is only a relatively brief discussion (2 short paragraphs) in the conclusion section on what the implications are and it would be nice if the authors spent more time on this. For example, what do their results mean for those developing the next generation of models? Where specifically should limited resources be directed? Can they recommend "optimal" model configurations for different types of investigations, e.g., biogeochemical vs. ecological focused studies, given that researches have limited computational resources and time? Surely this study has given the authors more insight into the implications than the rather generalized statements that they've written. If the key messages and implications can be articulated in a better manner, and some minor recommendations followed (see below), I would recommend publication.

We believe that it is beyond the scope of this paper to provide recommendations on "optimal" model configurations. Such recommendations would also need simulations with simple and more complex ecosystem components, and would indeed be driven largely by the questions addressed. Here rather we have identified the mechanisms which drive both the differences (in biogeochemistry) and the similarities (in ecosystem) between different physical setups. The utility of resource competition in understanding these differences and similarities is, we believe, a useful component for other users of biogeochemical/ecosystem models to understand the controls on their models. Our study has shown important implications of the control of physics in the results. We appreciate the reviewer pushing us to articulate these implications more completely and we do so with the following:

- The last few sentences of the abstract now read (P1, L13-17):

"Although previous work has suggested that complex models may respond chaotically and unpredictably to differences in forcing and resolution, we find that our model responds in a predictable way to differences in ocean circulation, despite its complexity. The use of frameworks, such as resource competition theory, provide a tractable way to explore the differences and similarities that occur. As this model has many similarities to other widely used biogeochemical models that also resolve multiple phytoplankton phenotypes, this study provides important insights into how the results of running these models under different physical conditions might be more easily understood."

- Last sentence of the Introduction (P3, L1-6):

"We explore both the effect on the bulk biogeochemical properties and the community structure of the model solutions. The objective of this study is not to assess which model performs best with respect to reality or to suggest an optimal resolution. Rather, we aim to examine how changes in the resolution and parameterization of subgridscale processes of the model domain alter the emergent biogeochemical and ecological properties of this diverse ecosystem model, and specifically to identify tools to help understand these differences and similarities."

- Additional sentences at end of the first paragraph of the Discussion (P7, L8-10):

"Here we explore what drives these similarities and differences, frequently using simple theoretical constructs. The goal here is to provide a framework that other modellers can use to help understand some of the implications of the physical/biogeochemical and ecological consequences of different forcings and resolutions."

- Additional sentence at the end of the first paragraph of the Conclusions (P11, L16-17):

"We have used theoretical constructs to help us pull apart the reasons for these differences and similarities"

- Additional paragraphs in the conclusions (P12, L6-20):

"Our results show that the physics resolved by the models do matter, regardless of the scope of the question being addressed. The modeled MLD plays a central role in mediating bulk biogeochemical processes, specifically through vertical nutrient supply and the modulation of the light environment, which ultimately control the magnitude of bulk biological rate processes. Notably, although global values of primary production, and to a lesser extent phytoplankton biomass, are similar between models, the bulk biogeochemical properties of the model solutions differ regionally. This shows that whether one is interested in global or regional questions, model resolution is crucially important.

It may be tempting to conclude from the results presented here that, in fact, model resolution is less important when considering ecological problems. However, we would strongly caution against this, as although we have shown that the dominant constituents of the phytoplankton community remain largely unchanged regardless of model resolution, significant differences in phytoplankton productivity, diversity (Clayton et al., 2013) and overall community composition do result from differences in model resolution.

We have shown in this study, that even for complex ecological models, it is possible to explain differences in model solutions, and a similar approach could be taken to evaluate the effect of different model physics on different ecological model formulations."

Specific comments:

Please include the CR results as supplementary material. It is essential to have the absolute values available somewhere for reference if only difference plots are in the main text.

We have a supplemental file that includes the following results from the CR simulation:

- Fig. S1 (a) annual average SST

- Fig. S1 (b) annual average MLD
- Fig. S2 (a) annual average phytoplankton biomass
- Fig. S2 (b) annual average primary production

We have also included the old Fig. 7 as Fig. S3 in the supplemental, as requested below.

I prefer Fig. R2 over Fig. 4. I would recommend that Fig. R2 be the one that appears in the published version.
We have replaced Fig. 4 with Fig. R2 in the revised manuscript.

In Section 3.1 (Pg. 5 line 17) can the authors include the globally integrated primary production rates for the models rather than just saying that they are "similar", i.e., make a quantitative comparison rather than a qualitative one.
We have added a more quantitative comparison of the globally integrated primary production rates for the models. The HR model has 0.6% higher annual primary production than the CR model, this has been added to P5 L16-17 in the revised manuscript.

Figure 6, the Hovmoller diagrams of the seasonal evolution of key model variables, is an improvement that makes it easier to understand section 4.2. However, I would recommend that the authors not throw out Fig. 7 from the original version, as it's nice to also see the comparison of monthly-integrated values. Perhaps this older figure can be kept as a supplemental figure?
We have included the old Fig. 7 in the supplemental, as stated above.