Biogeosciences Discuss.,
doi:10.5194/bg-2016-337-RC3, 2016

Biogeosciences Discussions

Interactive comment on "Biogeochemical versus ecological consequences of modeled ocean physics" *by* Sophie Clayton et al.

Anonymous Referee #3

Received and published: 2 December 2016

In this study the authors investigate how model resolution influences simulated ecosystem and surface ocean biogeochemical properties. Two physical ocean configurations are used; a high-resolution eddy permitting model and a lower resolution version of the model that does not resolve eddies. When the same "emergent" biogeochemical ecosystem model is coupled to these different physical configurations the authors find that phytoplankton biogeography is similar, while other biogeochemical properties have much larger differences. Investigating the "biogeochemical versus ecological consequences of modeled ocean physics" is important and I was eager to read such a study. However, I was somewhat disappointed with what was presented. The writing is clear and the analysis to show how the model results differ is mostly acceptable. However, I was not satisfied with the explanation of why there are similarities and differences between the two set-ups. The authors did not conduct a deep enough

investigation and it seemed more that they were simply showing similarities and differences and then hypothesizing why this occurred. I realize that the authors made some attempts to figure out the reasons behind the similarities and differences using a resource competition framework, but they did not take this investigation far enough and instead often ended up concluding their investigation by saying things like, "we hypothesize..." or "this may...". This is rather unsatisfying since one should be able to examine the model results in detail to actually determine why any similarities or differences occurred. Moreover, after reading Clayton et al., 2013 again, it seemed to me as if the authors are merely trying to extend their earlier work and publish a few new details that probably should have just been included in the earlier publication (i.e., not much seems to be new except for a few plots of biogeochemical differences). Am I wrong in this or is this a new set of experiments? The methods section of the paper was also lacking a few details and I had to assume that the set-up was the same as in the earlier paper based on what was stated in the results and discussion section. Without some of the critical information on how the model was spun-up and more crucially, for how long it was spun-up, I also had a difficult time interpreting some of the presented results. If the model was only run for 8 years as in Clayton et al., 2013 then, I highly doubt that steady state or even quasi-steady state conditions were reached. This makes it challenging to investigate biogeochemical properties because of model 'drift'. While it may be possible to somewhat account for such 'drift' the authors have not attempted to do so and thus, have only provided a snapshot of a system that would likely be quite different if the simulations were run for a longer period of time. I realize that there are computational limitations that prevent high-resolution models from easily being run to steady state, but the authors need to address the issue of 'drift' if they want to investigate differences in biogeochemistry. This is an issue even in an idealized case where the goal is not to reproduce observations, but to only compare differences due to model resolution. Overall, I also found myself wondering what the important insights from the study were. Yes, the message is that there are some similarities and differences that could be important, but what does it mean for the marine biogeochemical modelling community? The few concluding statements are not very satisfying.

Specific comments:

As mentioned above, some critical information is missing from the Methods section. Information on how long the model was spun-up for is needed. More information is also needed on the biogeochemical forcing data. What biogeochemical data sets are used to initialize the model? Is this World Ocean Atlas data, etc.?

An analysis should be conducted to address the issue of model 'drift', i.e., how much drift is occurring and what it might mean for interpreting the results. The resource competition framework that is used to explain some of the differences depends on the system being at steady state to work. If this is not the case as I suspect then it's difficult to see how such a framework can be used to explain the differences. The authors will need to provide more evidence for this to be believable.

Is annual averaging the best way to evaluate the similarities and differences that are seen in Figs. 1-5? As Figure 7 shows there are striking monthly differences at higher latitudes. Perhaps it would be more informative to compare and show the key physical, ecological, biogeochemical properties in seasonal plots (e.g., winter, summer, fall, and spring)? Or maybe carefully selected Hövemoller type plots would be informative? I would be particularly interested in seeing if phytoplankton diversity and differences are more pronounced seasonally or during the progression of the spring bloom in the Atlantic.

In Figs. 1 and 2, it would be nice to see the CR results too.

Fig. 3. I find that this lone figure made it difficult to really see the differences between the model configurations and found that I had to refer to Clayton et al., 2013 to really understand what was going on. This is somewhat frustrating, as some of this information seems to be necessary to understand the study. It would be really nice to have this all in one publication.

Fig. 4. What is the actual concentration? Is it realistic? I realize that the purpose of the study is not to figure out which is the 'best' simulation, but it would still be nice to see the absolute values.

Page 5 line 15 'tus' needs to be 'thus'

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