Biogeosciences Discuss., 11, C4507–C4508, 2014
www.biogeosciences-discuss.net/11/C4507/2014/

Biogeosciences Discussions
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Interactive comment on "iMarNet: an ocean biogeochemistry model inter-comparison project within a common physical ocean modelling framework" *by* L. Kwiatkowski et al.

Anonymous Referee #2

Received and published: 19 August 2014

General Comments

I do not think that the manuscript, in its current form, represents a substantial contribution to the field of ocean biogeochemical (BGC) modeling. The manuscript does not make clear how its findings are substantially different from previous studies of a similar nature, such as: Kriest et al., 2010, doi:10.1016/j.pocean.2010.05.002 Friedrichs et al., 2007, doi:10.1029/2006JC003852

I think that the authors need to present a strong case about how their work is new, compared to existing literature.

Specific Comments

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Discussion Paper

We know from previous work that the fidelity of the ocean physical model plays a large role in the behavior of ocean BGC models. Some studies that put the same OBGC model into different GCMs are: Doney et al., 2004, doi:10.1029/2003GB002150 Najjar et al., 2007, doi:10.1029/2006GB002857 Dunne et al., 2013, doi:10.1175/JCLI-D-12-00150.1 Séférian et al., 2012, doi:10.1007/s00382-012-1362-8 With this in mind, it is important for the authors to describe how well their configuration of NEMO, and how well it performs. What is the spatial and vertical resolution of the model? What physical parameterizations are used? Describe the biases in the fields: SST, MLD, MOC. This is particularly relevant to the Southern Ocean comparisons, where it is suggested that ocean physics deficiencies are causing the OBGC biases.

How much were the BGC model parameters tuned? There is a comment in the discussion "model developers were afforded a a limited opportunity to tune parameter settings". Please elaborate on this in the model descriptions. Previous work, like Kriest et al. (2010) and Friedrichs et al. (2007) demonstrate that models generally perform poorly if they are not tuned. If their 'limited opportunity' was not sufficient, then what's the point of this analysis? If these models were serious candidates for inclusion in a CMIP class ESM, they would be given more than a 'limited opportunity' to tune parameter settings.

The model evaluation is too brief. Please relate biases in surface fields to processes, e.g. primary productivity and biological export. The evaluation makes almost no mention of previous literature on OBGC model skill assessment that can guide the analysis. For instance, please see the special issue of Journal of Marine Systems on this topic http://www.sciencedirect.com/science/journal/09247963/76/1

A drawback of the Taylor diagrams is that it omits information on mean bias. For plots 1-3 and S1-S4, please add mean field values for models and observations to the plots. This could be done in the corner of the maps or in the legend.

Interactive comment on Biogeosciences Discuss., 11, 10537, 2014.