Biogeosciences Discuss., 10, C1978–C1984, 2013
www.biogeosciences-discuss.net/10/C1978/2013/

Biogeosciences
Discussions
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Interactive comment on "Long-term trends in ocean plankton production and particle export between 1960–2006" *by* C. Laufkötter et al.

Anonymous Referee #1

Received and published: 17 May 2013

Review of Biogeosciences manuscript bgd-10-5923-2013: "Long-term trends in ocean plankton production and particle export between 1960–2006" by C. Laufkotter, ÌĹM. Vogt, and N. Gruber

GENERAL COMMENTS

The authors investigate the impact of climate change on primary production and export of particulate organic carbon (POC) in the ocean for the period 1960-2006 using a hindcast simulation from a global coupled physical-biogeochemical model. The authors perform a thorough analysis of how changes in the physical environment affect ecosystem structure and lead to changes in primary production and POC export in different regions of the ocean, and present some interesting results. However, there are some issues with clarity and specificity in the methodology and presentation of

the results that make the manuscript hard to follow in some places, and potentially raise questions about the validity of some of its results and conclusions. More detailed comments follow below.

SPECIFIC COMMENTS

The CCSM BEC model has been extensively described in Moore at al 2004 and Doney et al 2009a, 2009b. So the first part of Section 2.1 (Model description, page 5927) can probably be shortened. It would suffice to include a basic description of the model, cite the previous papers and comment on the differences between the version used in this study and the versions used in those papers. I also do not think it is necessary to include the full equations and parameter values for the BEC model in the Appendix.

In Section 2.2 (Forcing), the authors state that the 3000-year spin-up run was forced with "climatological means" of the inter-annual forcing (CIAF). Was the spin-up run forced with the averaged CIAF fields or was it forced with with the CORE CNYF v2 (Corrected Normal Year Forcing version 2)? This is a very important distinction. Averaging inter-annual forcing into an annual climatology removes high frequency variability, and that has severe negative effects on the quality of the simulation and model skill. The CNYF v2 is a reconstructed "normal year" that maintains high frequency variability. Another point is that 3000 years is a fairly long time for a spin-up. There could be significant model drift. Was there any attempt to quantify model drift? This is particularly important given how small the changes in global NPP and export production are. The authors should elaborate more and be more explicit on the forcing used for the spin-up run and the control runs that were made, so these issues can be clarified.

On page 5931 line 5, the authors state that global NPP is 4.8 Pg/y. Either the value or, most likely, the unit is incorrect. It should probably be Pg/month.

On page 5931 lines 17-18, regarding the BEC estimates of total export being significantly lower than other models. More recent studies (Henson et al 2012 and Lutz et al 2007) have total export estimates of the order of 5 PgC/y, which are consistent with the

CCSM BEC results presented in the manuscript.

In Section 2.4 (Calculation of trends), it would be helpful if the authors stated explicitly the temporal frequency of the model data used to compute the trends. Did they use weekly, monthly or annual model output? The mention of specific months and seasons in the text suggests that they used monthly data. But it's not clear. If they used monthly or weekly data, was the seasonal cycle removed before computing the trends? The authors should provide more detail on how the trends were computed so the reader can better evaluate the results presented. Do the time-series plots (Figures 2 and 9) show annual means or deseasonalized monthly means? It would also be helpful if, in addition to the definitions of export production, NPP and phytoplankton and zooplankton biomass, the authors also included the units used for these quantities (between parentheses, perhaps).

The units in Figure 1 are a bit confusing. Panels a, c and e show percentages and panels b and d show mol C/mˆ2/yr. So are the trends/percentages in panels a, c and e %/year or percent over the 47 years of the simulation? In the caption, the authors should be more explicit about what is being shown in the figure. I would also change the color of the land to something other than light blue. This makes it look like the trends in the light blue regions in the ocean are not significant. The same comments apply to Figure 4. The authors use "PP" in Figure 1a,b and "NPP" in the text. The same acronym should be used everywhere.

On page 5932 line 16, the authors state that the decline in NPP and EP in the Southern Ocean is greater than the inter-annual variability. Looking at Figure 2, that is not very clear. There is considerable inter-annual variability in the Southern Ocean and other regions as well. But the authors do not quantify nor show any estimates of the model's inter-annual variability to contrast with the computed trends. Given the model's inter-annual variability, are the observed trends and changes significant? This is a very important issue given how small the changes in NPP and export production (6% and 7%, respectively) are in that 47-year period. For example, Henson et al 2010 and Yoder

et al 2010 argue that longer time-series of at least ∼40 years are needed to distinguish a climate change signal from natural variability. So according to Henson et al 2010, the 47-year hindcast run is barely long enough to detect a climate change signal.

Figure 2 lacks units for the variables shown in panels c, d and e. It's also not clear what is being shown in the time-series plots. Are these annual means or deseasonalized monthly means? More information about the plots should be included in the caption.

On page 5932 line 22, it looks like it should be Fig. 1e NOT Fig. 1c.

On page 5933 lines 1-4, the authors talk about the relationship between "changes" in NPP and "changes" in SST and refer to Figure 3. What exactly is being shown in Figure 3? From the magnitude of the values and number of data points, it looks like a plot of global NPP vs global mean SST from a series of model runs. If so, where do these model runs come from? How were they made? If these are not model runs, how did the authors obtain the global NPP and SST values in the plot? I did not find any mention of it in the methodology or figure caption. In the text (lines 1-4), the authors refer to "changes" in NPP being correlated with "changes" in SST, which implies the figure shows delta NPP vs delta SST. But the figure caption says "annual NPP as a function of changes in SST", which implies NPP vs delta SST. And yet the magnitude of the values suggests that these are global integrals of NPP vs global averages of SST. In addition, in the text the authors mention the relationship/correlation changes with latitude, but I don't see any information on latitude in the figure. The reader cannot properly evaluate the results and arguments presented without knowing exactly what is shown in Figure 3.

In Figure 4, the labels in the panels say "small phyto trends", "diatom trends" but the caption says "changes in small phyto NPP" and "changes in diatom NPP". The labels in the panels are misleading because they suggest that the trends are in biomass not NPP.

On page 5935 line 1, there is an extra "zooplankton biomass".

In Figure 5, how were these changes computed? Are these trends (slope of linear regressions) or differences between annual or decadal means? The authors should be more explicit about what is shown in the figure in the caption.

On page 5936 line 9, there is a typo "...weak oh phytoplankton...".

On page 5936 lines 24-25, the authors state that small phytoplankton have higher light requirements than diatoms. Are they referring to the small phytoplankton's lower max Chl:C ratio?

The caption in Figure 6 has "a" and "b" labels but I don't see any "a" or "b" labels in the panels.

Figure 7 does not have any units for the biomass shown in the "y" axis of the plots.

Figure 8 shows a distribution map of the different sources of POC. Is this an average for the period 1960-2006? How exactly was this computed?

The word "through" is misspelled on page 5940 line 5.

On page 5940 lines 25-27, there is not much of a trend in the sources of POC, particularly in the Southern Ocean and North Atlantic. Perhaps adding the regression line would help see the trends. This also relates to my previous comment on inter-annual variability and the climate change signal.

On page 5942 line 15, the authors state that they also see a "global decline in chlorophyll". Is this surface chlorophyll or an average for the upper 100 m or mixed layer? The authors should be more specific.

The studies by Henson et al 2010 and Yoder et al 2010 are particularly relevant to the statement made on page 5944 lines 22-25.

In the different sections of the Discussion, the authors provide a very nice and thorough analysis of how changes in the physical environment impact the ecosystem dynamics and global NPP and export production and compare their results to other studies.

However, questions remain regarding the significance of the observed trends given the model's temporal variability. In general, the authors should also be more explicit and include more information, including units and exact definitions, about what is being shown in each figure in the figures' captions. In many places not enough information is provided to properly evaluate the study's results and conclusions.

RECOMMENDATION

In summary, I find that the manuscript is potentially acceptable for publication in Biogeosciences after a major revision addressing the issues raised in the comments above.

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