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Biogeosciences Discussions

Interactive comment on "Spatio-temporal variability of marine primary and export production in three global coupled climate carbon cycle models" *by* B. Schneider et al.

Anonymous Referee #1

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General comments

The paper by Schneider and co-authors presents a comparison of the outputs of three coupled climate carbon cycle models, in terms of primary production (PP) and export production (EP). The three models have been already published and used for other similar work. The central idea is to analyze the relationships between PP and EP and environmental factors such as temperature or stratification of surface waters, as a basis for future studies of the response of the ocean to climate change (warming and stratification in this peculiar case).

The subject is, therefore, timely and appropriate for BGD. A major problem, however, is I don't understand how the authors came to the conclusions they are proposing considering the results they show. I have, therefore, several comments and suggestions, as detailed below.

In its present form, I think that this paper would rather confuse people about the response of PP and EP to climate change.

In terms of presentation: the writing of the paper lacks concision, and the presentation of the results and the discussion are permanently mixed, which makes it hard to read.

My recommendation is, therefore, to reject the paper in its present form.

Specific comments

This paper suffers from often drifting away from the displayed objective (at least as it is indicated by the title). The title indicates that the spatio-temporal variability of PP and EP is the subject. The response of the model to climate variability is, however, recurrently brought into the picture, so the paper lacks focus. If the objective of this paper is to analyze the relationships between PP and EP and environmental factors (SST, stratification etc...), then it should do this only. It looks like this paper is part of a larger study and that another paper is in preparation about the impact of future climate change (last sentence of the introduction). The separation between both subjects is apparently not optimally performed between the two papers. Although I understand that the present work is a first step towards making some predictions of the response of PP and EP to climate change, the discussion of this part of the subject should probably just appear near the end of the paper (discussion) rather than being spread across the entire paper (this is also probably because the results and discussion sections are intermingled).

Apart from this issue about the structure of the paper, the first problem I have is with the conclusions, i.e., when the authors say (abstract, lines 10-12) "Two of the models also reproduce the inverse relationship between stratification (SST) and PP...". This is

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contradictory to what is said page 1895. As far as I have well understood, the IPSL model is the only one to reproduce this relationship. Neither the MPIM nor the NCAR models reproduce this at all: line 16 page 1895: "For MPIM no correlation can be found between the respective anomalies of PP and stratification (SI) or SST,...", and line 29 of the same page: "In NCAR both slopes are weaker and correlations insignificant";. My conclusion would be that only one model reproduces what was shown in the Behrenfeld et al. paper (2006; nature, vol 444).

The second major issue is with the effect of temperature on PP. We know that the various existing parameterizations of the effect of temperature on photosynthesis diverge in their predictions and that the impact on global PP simulations is dramatic (e.g., Sarmiento et al., 2004, J. Geophys. Res., vol 18; Carr et al., 2006, Deep-Sea Res. II, vol 53). Some recent models rather go in the direction of temperature-independent PP rates (Behrenfeld et al., 2002, Mar. Ecol. Prog. Ser., vol 228). Being the most recent does not qualify them as being closer to reality; this variety of approaches simply illustrates the lack of understanding about the effect of temperature on photosynthesis when the global scale is considered. It is, therefore, symptomatic that the MPIM model, into which photosynthesis is temperature-independent, doesn't show at all the relationship between stratification and PP anomalies. This could mean that the change in nutrient supply due to the change of stratification has no significant effect in the MPIM Model? I understand that the problem is extremely complex because of the myriad ways global coupled models can react to the change of a given parameter. Would it be feasible to perform some case studies, for instance by running the biological models offline, with controlled parameters (i.e., some kind of sensitivity studies)? This type of experiment may help understand the response of these models when embedded into the global 3D climate models. The very minimum would be at least to better explain which type of temperature dependency is used in each of the two models that consider this dependency (IPSL and NCAR). Doing this doesn't require long developments. Considering the importance of this effect, it would be preferable than to simply provide references to other papers where it is not necessarily easy to find the answer.

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Considering these uncertainties, my recommendation here would be to remove the temperature dependency of PP in all three models, in order to look specifically and solely to the impact of the change in the nutrient supply. Doing this would probably remove some of the ambiguities that come with the present results.

The weaknesses of the "satellite models", and the uncertainties attached to the estimates of primary production (PP) by these models, are recurrently mentioned (introduction, lines 7-20 page 1879, then lines 15-16 page 1880; section 3.2, lines 15-22 page 1892, then lines 29-30 page 1893; line 20-22 page 1894). I think this should be either stated once and for all in introduction or mentioned during the discussion of the results. When model outputs and observations (or observation-based estimates) don't match, one cannot just explain this by questioning the quality of the observations. A full paragraph (lines 7-20 of introduction) is devoted to the uncertainties attached to "satellite-derived" PP: why the authors don't write the same thing about model-derived PP? In addition, the Carr et al (2006) paper is cited to explain that the uncertainty in these "satellite PP" is at least of a factor of two, without mentioning that a large part of the uncertainty is brought by GCM-derived estimates (Fig. 5 in Carr et al., 2006). All this is just to say that the comments about uncertainties in satellite-derived and model-derived PP must be more balanced. The same comment applies to the mixed-layer depth, when it is said (lines 23-25 of page 1887) "Observation-based estimates of MLD, however, are uncertain, and...". Why the authors use these estimates if they believe they are incorrect? They can either use them (and then they should quantify their uncertainty with some numbers; this could be feasible from the paper of DeBoyer et al) or they simply say nothing.

Another problem I have is with the models' mixed layer. All three models overestimate the mixed layer for latitudes $> 20°$N. Starting simulations with too deep mixed layers may overemphasize the stratification effect of warming in longer simulations, and then the impact of this warming on PP.

The use of "El Niño-like conditions" to describe the future state of the ocean under

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a possible global warming is somewhat oversimplifying. The El Niño phenomenon is much more than simply a change of stratification or temperature.

More generally, I think that the reading of the paper would be much easier if the presentation of the results would be separated from the discussion (as far as possible).

My last comment is about the appropriateness of the three models to address the questions the authors are considering: I think that the problem should be tackled in parallel on a regional scale by using models with a much better spatial resolution and much better controlled physics (to get a better understanding of the relationships between PP, EP and environmental factors) and at global scale with models of a coarser resolution and including a simplified biology (to look at the multi-decadal response). The three models used in the work by Schneider and co-workers are maybe "too simple" for the first type of study, and too complex for the second one. Maybe some discussion on this issue (appropriateness of the model) could be provided.

More detailed comments, plus technical corrections

o Abstract, line3: "three different coupled climate...". If they are three we suppose they are different, so "three coupled climate..." would suffice (should be checked in the rest of the paper).

o Page 1880, lines 17-18: I don't think the "satellite algorithms" and the GCMs are as independent in their ways of deriving PP as said here. The equations for irradiance propagation, and, more importantly, for the parameterization of the P versus E curve (or the growth rate equations) are essentially the same in both approaches, and the parameters entering into these equations are derived from analysis of in situ data sets. The main difference is that the satellite algorithms are diagnostic (they use the chlorophyll concentration as it is, and as derived from the satellite observations), while the PP modelling in the GCM is in essence prognostic, so the chlorophyll is determined at each time step (well, nutrients are computed, and chlorophyll is derived from nutrient-to-chl ratios).

o Page 1883, line 7: "Amount" should be "Aumont".

o Page 1885, line 12-13: I am not sure it's acceptable to refer to an unpublished paper here; this is an Editor's decision that I cannot anticipate, however.

o Page 1885, line 21: why the regenerated contribution would be lower than in the real ocean?

o Page 1886, line 6: is this "Doney et al (2006)" (There is no Doney, 2006 ref in the ref list).

o Page 1890, lines 18-20: looking at Fig. 2, one really cannot say that "..all models show a reasonable agreement with observations of .. " MLD max, PO4...". The model MLDs are seriously overestimated in several latitude bands.

o Same page and lines: again, how can the authors say about the model PO4 "..reasonable agreement with observations..", when the maps displayed in Fig. 4 show more than a factor of 2 in several areas. These differences might be acceptable depending on what te authors are looking at, but this is not said.

o Page 1890-1891: the lowest value of annual PP reported in Carr et al (2006) for the GCMs was 35 GtC. How the authors explain that the maximum is here of 31 (IPSL) and the minimum as low as 24 (MPIM)? The PISCES model was part of the Carr et al inter-comparison, and it was providing a value as high as ˜75 GtC. Are such differences simply produced by the different forcing or the different ways of running the simulations (spin up, coupling etc...)? How can we "live" with such enormous differences in PP even from one single model? There is for sure an explanation and the relevant pieces of information should be provided for the reader to understand.

o Page 1894, lines 13-17: something (a verb probably; maybe part of a sentence) is missing in this sentence, which is not understandable.

o Page 1894-1895 and Fig. 10, left panels: anomalies in PPstrat and PPglob are shown to be positively correlated. Let us call PPmix the quantity PPglob-PPstrat. Because

PPstrat is about 60% of PPglob for the IPSL model (Table 1), which is by the way not very different (as said in the paper) from its relative contribution to the total area, I think one would obtain a similar correlation between anomalies in PPglob and anomalies in PPmix. The conclusion would be, therefore, that global ocean PP anomalies are mainly driven by PP anomalies in "mixed areas". This is something the authors should seriously consider in their discussion. It is probably not the case, but, for the moment, it looks a bit like a spurious correlation.

o Fig. 10., right panels: why the El Niño - La Nina transition in 1997-1998 is not better reproduced? I have read the comment page 1895 that says we should not concentrate too much on the phasing of events, but this one is a major event and I am surprised that the models don't do a better job.

o Page 1896, beginning: all this is quite speculative. The authors should be more specific or say nothing.

o Page 1897, 10 first lines: again, this is long and not really informative.

o Page 1897, lines 18-24: this is typically where the paper drifts away from its central topic. Maybe a specific sub-section discussing the implication of what is shown with the three models in terms of future evolution of PP and EP is needed.

o Page 1899, line 7: "This result is very robust across the models...". How can the authors say that when only one model reproduces the relationship?

o Page 1899, line 21: Sarmiento et al. say the converse!!, i.e., "..The three algorithms give a global increase in primary production of 0.7% at the low end to 8.1% at the high end, ..".

o Page 1900, after line 10: please remove all these "strong". This is really too much (by the way, this comment is valid for the entire paper; authors should check the use of such qualifiers, and remove most of them).

o Page 1900, lines 11-13: this sentence sounds like this work has illustrated the link between satellite-derived productivity and satellite-derived climate variability (i.e., what was shown in the Behrenfeld paper). Should be rewritten.

o Fig. 2: maybe it would be useful to recall that the average is for the 1985-2005 time period.

o Figs. 4-7, 10, 12 and 13 are ridiculously small. It's impossible to see anything on these figures. I don't know whether this is due to the editing process or to the original size; the size has to be increased anyway.

o Fig. 10: "overlain" to be replaced by "overlaid".

o Fig. 12; where is the "shaded area"?

o It might be useful to use equal-area projections to display the global maps.

Interactive comment on Biogeosciences Discuss., 4, 1877, 2007.