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Discussions

Interactive comment on "Projected 21st century decrease in marine productivity: a multi-model analysis" *by* M. Steinacher et al.

Anonymous Referee #2

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General comments:

The paper shows results of three different biogeochemical models and attempts to analyze and explain simulated changes in "marine productivity" during the 21st century. Having read the title I was very curious learn more about this important scientific issue. Having read the paper, I am confused and disappointed about this missed opportunity to clarify why some models/methods give similar answers and others don't. As earlier studies (e.g. Maier-Reimer et al., 1996; Bopp et al., 2001) have already investigated the predicted decrease in export production (EP) under global warming, the current study rightly concentrates on the predicted changes in primary production (PP). An essential difference between PP and EP is the microbial loop, which contributes to PP but not to EP. In order to understand why "mechanistic" models predict a decline in EP

and "empirical" models predict an increase in PP, I would first look at the response of the microbial loop to global warming. It is problematic that the mechanistic analysis is based on results of a model (the only one among the three) that does not include any representation of the microbial loop.

A second concern is that the authors do not discuss the possible role of iron in causing changes in EP and PP. What do the models assume about the evolution of aeolian iron supply during the 21st century? As shown by a previous analysis of the same models (Schneider et al., Biogeosciences 2008), at least two of the models are severely limited by iron (leading to way too high surface phosphate values e.g. in the Pacific). Similar to earlier studies of predicted changes in EP, the mechanistic investigation focuses on nutrient supply from below. If iron supply from above is relevant and production is iron limited, changes in stratification or mixed layer depth may have opposite effects: Iron added to a shallower mixed layer may have a better chance to end up in biomass than iron added to a deeper and darker mixed layer. Is this relevant?

In many places there is a lack of precision and detail in the terminology and in the analysis. Several times the authors use very vague statements and circular arguments from which they jump to unjustified and presumably partly wrong conclusions. I'd like to encourage the authors to perform a more careful analysis of the available model results and to clarify why some models differ in predicted changes in primary productivity and others don't, but I am afraid I cannot recommend the current paper for publication in Biogeosciences.

Major concerns:

1. The paper pretends that there is a "general" conflict between a recent empirical-statistical approach (Sarmiento et al., 2004) and what the authors call "mechanistic" models. As A. Schmittner in his comment rightly points out, this conflict is less general than stated in the paper and may just be a coincidence of the particular choice of models used here. The interesting question is why different "mechanistic" models can

predict opposite changes in primary production in response to global warming. The authors ignore this question by referring mostly to changes in "productivity" without clarifying that they look more at new production rather than primary production (see point 2 below).

2. The terminology is confusing. Although a definition of primary production (PP) and export production (EP) is given at the end of chapter 1, the repeated use of "marine productivity" or "productivity" makes it difficult to follow the paper. It doesn't help that one of the models (NCAR) represents "the carbon flux associated with net nutrient uptake". Depending on the decay time scale of the semi-labile dissolved organic matter (not given in the paper), this should be close to net community production or new production (NP). In the NCAR model there is essentially no representation of the microbial loop. Without any attempted justification stating that the NCAR model's "PP" "is a reasonable proxy for the time and space variability of PP" and "for reasons of simplicity...is considered here as PP" (line 9-10, page 7943) does not make it such.

3. The entire analysis of the mechanisms of shifts in PP (section 3.2) is based on results of the NCAR model. It is therefore rather an analysis of mechanisms of shifts in NP, similar to previous studies of changes in EP, except for the possible change in the export of dissolved organic matter (which is not commented on in the paper). It is not an analysis of changes in PP, and there is no justification given why "the mechanisms identified for the NCAR model are also key for the productivity changes in the IPSL and MPIM model" (line 20, page 7948) if "productivity" is to be equated with "primary productivity".

4. As shown by Schneider et al. (2008), two of the three models do not (or much too weakly) show the observed anti-correlation of "PP" with temperature on interannual time scales. Why then should one have confidence into the "PP" response to global warming? Also shown in the Schneider et al. (2008) paper is that two of the models have way too high surface phosphate concentrations in large areas of the Pacific (0.5-1.0mmol m-3 too high in the MPIM model, 1.0-1.5 mmol m-3 too high in the NCAR

model). Doesn't this indicate the the models' response to spatial differences in nutrient and/or light supply is wrong? Why should this result in any reasonably response to temporal changes in nutrient/light supply?

Specific comments:

p.7935,l.5: According to my knowledge, the first study coupling a marine ecosytem model to an oceanic GCM was Sarmiento et al. (1993) and Fasham et al. (1993).

l.22: include "from SOME mechanistic models", though one may argue whether any of the empirical ecosystem models are mechanistic at all....

l.24ff . What is "marine global productivity"? Many of the models cited here do not include an ecosystem model and cannot make any statement about primary production. I would agree that there is a general decrease in simulated export production, but since your refer to "marine global productivity and organic matter export" you seem to refer to something else.

p.7936,l.1: again, the statement that "global productivity and export" are reduced is not supported by the references given.

l.5 Here "productivity" probably refers to "new production" (in C units?) or "export production"

p.7937, l.21/22 : "the use if a multi-model ensemble increases the robustness of the results". This remains to be shown. At closer look none of the models is very good. You may get a more robust result from averaging many bad models, but, if anything, it will remain a bad result.

p.7939 top: A description of the controls of PP and any explicit temperature dependencies, as discussed for the MPIM and NCAR models, should also be given for the IPSL model. If I remember correctly, some heterotrophic processes also depend on temperature in this model. If this is the case, this should be pointed out (also for the other models).

p.7941, eq.2,3: The text prior to the equations suggests that P_sil + P_car=1, but this is not the case for equations 2 and 3.

p.7942, eq.4 : What is the reasoning behind this temperature function? Since this paper is about the effects of global warming, the reader should be able to understand this formulation which does not seem to be commonly accepted standard.

p.7943, l.25: What do you mean by "Gaussian interpolation"? Is there some spatial scale larger than the grid spacing involved?

l. 25ff: I do not understand this. First you use the control run to detrend model results (i.e. by substracting the control run results from the global warming results?). Do you then, in a second step, detrend T, S, and nutrients? Does this mean you detrend the difference global warming minus control? Why should one do this? Also, in the case of the NCAR model you detrend T, S, and nutrients, but not EP and PP. Doesn't this affect your analysis that assumes consistent T, S, nutrients, EP, and PP fields?

p. 7945, l.23: How do you know this is the MAIN reason? Bopp et al. (2005) have "only" shown that this is ONE reason.

p.7946, l.26: Isn't the "biomass proxy derived form phosphate and iron concentrations" just the phosphorus equivalent of the limiting nutrient? Under what conditions does biomass equal the nutrient concentration?

p.7947, l.15: Does the "unrealistically strong iron limitation" change during the simulated 21st century?

l.26: what is the relative importance of changes in cloudiness and mixed layer depth?

p.7948, l.19-20. Why does a correlation suggest that the mechanisms are the same?

p.7950, l.1. Does the Fe:C ratio show a similar change in the exported particles?

l.13: EP decreases because nutrient supply from below goes down. Iron is mostly supplied from above (is it?). Therefore, assuming no changes in aeolian iron supply,

I would expect iron concentrations to go up when EP goes down. Why do iron and macro-nutrients show parallel changes here?

p.7951, l.20-21. This is a circular argument: first you say that the mechanisms of the NCAR model can be applied to the IPSL and MPIM models, and then you conclude that the multi-model analysis confirms the results of the NCAR model analysis.

p. 7952, l.8-11. Just picking those particular grid points from each model that agree best (or disagree least) with the observational estimates does not mean the any of the models or the models as a class can represent most of the features. Would you apply this to the global warming simulations, you could possibly find a picking scheme that results in a decrease in PP (or EP?). The statement that the mechanisms are the same is again a circular argument, because it was assumed earlier that the mechanisms identified for the NCAR model apply to the other models.

p. 7952, l.11: what is the reason for using an influence radius of 10 degrees?

p.7960, l.16-17. The assumed temperature difference is also very different among the models used here. Why does this seem to play such a little role in the "mechanistic models" compared to the large role in the satellite-based estimates?

Interactive comment on Biogeosciences Discuss., 6, 7933, 2009.