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Biogeosciences Discussions

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

Anonymous Referee #1

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General Comments

The manuscript, "Projected 21st century decrease in marine productivity: a multi-model analysis" by Steinacher et al provides an important contribution as a prototype for the kind of intermodel comparison that will be necessary for IPCC fifth assessment. While much of the results are not different from those with earlier, more rudimentary models, it is important to know that the results of the more mechanistically complex models are consistent with those earlier results. Still, it is disconcerting how low the primary productivity is in all of these models compared to field 14C observations. In that sense, the authors might have been better of focusing more on export production as in Gnanadesikan (2004) given the trouble that these models have in representing the microbial loop. While it would have also been advantageous to include some of the other models

from different groups, the paper stands when as a prototyping approach. The authors advance a statistical metrics for regional skill approach for synthesis into a single, optimal projection that seems clearly superior over using the more conventional use of the ensemble mean, but I have a problem when the authors consider VGPM alone as 'truth' in interpreting satellite chlorophyll. Ideally, given that OBGCMs represent macro and micro nutrient controls, one might think that the OBGCMs should be able to represent PP much better than any individual satellite model driven only by T and Chl. I realize this is still not the 'state of the yet' yet, however. While the authors reference the Carr et al., 2006 study that compared models of PP, they ignore the implications of that paper in the uncertainties related to the interpretation of satellite chlorophyll. I sympathize with the authors quandary, however, and find that this manuscript clearly highlights the need to have a follow-on study to the Carr et al (2006) work to provide a clearly superior satellite-based PP estimate that is optimally consistent with field 14C observations… perhaps using a similar statistical metrics for regional skill approach as the one described here! In leiu of that, I would much rather see the authors compare to an ensemble of satellite algorithms, or to chlorophyll itself (noting the need to interpret the NCAR results into chlorophyll), than just to VGPM. Still, I am reluctantly willing to see the manuscript published without this modification so long as the authors make an explicit statement that they are using VGPM (i.e. Behrenfeld, 2006) only as an example. It might be helpful to move some of the discussion of these issues with respect to the Sarmiento 2004 paper from the discussion earlier. In the end, the fact that the three models seem almost unidirectionally biased low suggests to me that at this phase, such refinement is probably not essential. As models improve, this will hopefully become more of an issue in the future.

Specific Comments:

Page 7937, line 11-12 – With 'The MPIM model, and to a lesser degree, the NCAR model, suffer from a too strong iron limitation compared to the real ocean.' it is unclear to what aspects of iron limitation the authors are referring here. Do they mean that the concentrations of iron are too low? That the half saturation value was too high? That the formulation of iron limitation was ill-posed? In any case, more specific support for the statement would be helpful given the number of times this point is necessarily returned to when describing the climate sensitivities of and differences between these models

Page 7942, line 20 – this temperature functionality is odd for two reasons, first, the shape is convex down instead of the usual Epply or Q10 convex up, secondly, it severely curtails productivity in the coldest waters. What is the justification for this? The Doney et al (2006) simply references HAMOCC which seems insufficient.

Page 7944, lines 10-14 – If the bias in MPIM were due to too intense iron limitation, one would expect to see too much nitrate at the surface, is this the case? Also, what is the explanation of the IPSL low bias? My guess is that the real answer is that none of these models can adequately represent the microbial loop and the role of cyanobacteria and other pico and nanoplankton.

Page 7944, line 20 – Filtering a potential comparison of modeled Chlorophyll and temperature with satellite estimates through the Behrenfeld (VGPM) algorithm seems inappropriate here. As the authors note on line s 24-26, the satellite algorithms are highly uncertain. It is extremely misleading to treat VGPM as a standard, unless one is dealing exclusively with the North Atlantic region to which that algorithm was calibrated. Comparisons with field 14C PP estimates show the VGPM algorithm to almost always underestimate productivity in regions outside of the North Atlantic. Better would be to compare to an ensemble of satellite PP algorithms, and better yet to chlorophyll and temperature themselves, if the NCAR model could be used to derive a chl value consistent with it's estimate of PP from the 'biomass proxy' described on page 7946, line 25.

Page 7945, line 28 – Why use globally averaged air temperature rather than SST as one would surmise the regression to be even better?... okay, on Page 7958, line 21 the

authors justify this choice as a means to facilitate comparability with other metrics in order "to account for the different climate sensitivities of the models." It would be good to have this kind of explanation in this earlier section.

Page 7947, lines 16-17 – I'm not sure what the authors are getting at by saying that, "On the other hand, reduced nutrient concentrations in combination with increased export are indicative of a sustained nutrient input into the euphotic zone." I think this sentence could be eliminated unless there is an additional point the authors are trying to make beyond the one in the sentence that follows this one.

Page 7947, lines 22-24 – To say that nutrient supply in the North Atlantic is 'linked' the thermohaline circulation is far too vague. Is it simply that both depend on the occurrence of deep wintertime convection which decreases, or is it that the waters that supplant those exiting in the thermohaline circulation supply the nutrients? The sentence that follows suggests the former, but is also vague.

Page 7947, line 26 – In describing why light limitation increases, by 'changes in cloudiness and changes in MLD', do the authors mean 'increases in cloudiness and increases in MLD'? If so, why does MLD increase under intensified stratification... wind mixing? Or do the authors mean that increased cloudiness overwhelms the decrease in MLD in increasing limitation?

Page line 16 – While I am very supportive of this objective statistical approach, again, the idea that VGPM provides a robust estimate of PP (assuming that B&F alone was used) is inappropriate. The only justification I can think of for the use of B&F is that all of these models are so strongly biased that anything is better than nothing, and suggests that the authors lack any confidence in the model PP formulations. The authors should either be using chlorophyll directly and avoid filtering through algorithms such as VGPM that necessarily parameterize variability induced by nutrient effects on PP through temperature and general down-regulation or creating an ensemble pp algorithm with a variety of those used in Carr et al., 2006 such as was done in Sarmiento et al. (2004).

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