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Biogeosciences Discussions

Interactive comment on "Phytoplankton competition during the spring bloom in four Plankton Functional Type Models" *by* T. Hashioka et al.

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1 General comment

This paper illustrates the results of a multi-model analysis done over 4 different Plankton Functional Types (PFT) models with a clear focus on the controlling factor for the dominance of diatoms and smaller non-silicifying phytoplankton (referred as nanoflagellates). The authors should be praised for undertaking this effort as this kind of activities is hardly to get any attention from funding agencies, while it is very much needed to improve the knowledge about the functioning of PFT models and their usability as a tool for understanding the real system. The paper is well written though some paragraphs need to be better explained; an extension of the discussion and some restructuring of the sections would be beneficial. There are some specific comments in the section below that I would like the authors to address and some slightly major issues that I believe will improve the quality of the original manuscript:

1. the method used to compute the phenology of the bloom is not at all described (e.g. *Racault et al.* (2012)). The explanations of the methodology are scattered throughout the sections and it is not clear whether the choice of the model analysis is consistent with the results of diatom dominance obtained from the satellite models (are the satellite models basing the computation of dominant fraction at the bloom peak?). I would strongly suggest the authors to expand the method section with more details on the computation of the bloom maximum, also indicating that the focus is on diatoms dominance, otherwise it is not clear to me why the authors are excluding values below 0.5 mg Chl m^{-3}. This criteria may not necessarily be the same for all models and it is in contrast with the choice of the regions that in some cases do include subtropical regions where the bloom is known to be dominated by smaller phytoplankton and found below the surface out of reach of satellite detection. Avoiding the subtropical coastal regions could possibly reduce the large standard deviation found in some model results and help to interpret the results.

2. The authors use photosynthetic rates and growth rates as synonyms for gross primary production, but they are not. This is true in the models described here where the growth rate is controlled by a factorial multiple limitation of nutrient and light, but it could be misleading in a more general physiological context since photosynthetic carbon uptake rates may be decoupled from biomass growth. This is a simplification that these models are making and others not (e.g. *Vichi et al.* (2007)) therefore I advise the authors to use the term growth rate throughout the paper. Also, the differences in the availability of resources between the models

for the realized bottom-up control is not mentioned (Sec. 4.1.2). Is it possible that the limitation factors are different because the physical models simulate different upwelling and/or mixing rates? This should be discussed as the readers have no information about the underlying environmental conditions.

3. The manuscript is rather detailed in presenting the differences between the numerical model results. However, there is a large difference between the two satellite model reconstructions, much larger than between the plankton models. This issue is just mentioned at P18097-L15 but not discussed further. For instance, I find strange that the fraction of diatoms decreases with the increasing concentration of diatoms in the Alvain et al. model. I would tend to believe that the satellite models should be taken just as the other models and that an independent measure of plankton composition should be used (as for instance done in *Friedrichs et al.* (2009) with primary production estimates in the equatorial Pacific). I understand that this may be too demanding for this paper but I think some more considerations should be given in the final discussion.

4. It is a model intercomparison paper, therefore I understand the emphasis on the different model behaviors. However, I think some recommendations should be given based on the analysis of the results. The paper is very polite in treating all models equal. However, since all presented models have very similar functional forms a more critical approach on how good the model reproduce the expected behavior and on the future research directions to improve the model skill (including satellite models) would be useful.

2 Specific comments

P18086_L21 CCSM-BEC and PISCES have not been described yet.

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9, C7939–C7949, 2013

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P18086_L25 same as above. Model names are not substantial at this initial stage and only the works with models used in this manuscript are cited. I suggest to remove all names from the introduction.

P18087_L25 PFT models are plankton models and not full ecosystem models. (this work is focused on two components of the phytoplankton.)

P18087_L29 I find a bit strange that modelling papers are cited to refer to the ecology of marine diatoms.

P18088_L03 I guess there is something wrong here! This paper has nothing to do with particle aggregation, at least according to the following abstract: This article reports on a multi-resolution and multi-sensor approach developed for the accurate and detailed 3D modeling of the entire Roman Forum in Pompei, Italy....

P18090_L16 Please describe also the choice of initial and boundary (river) conditions and the spin-up of the models. I suggest the discussion should offer some considerations on the use of reconstructed off-line monthly rates and whether the usage of monthly means of instantaneous rates may change the results.

P18091_L20 The equation is kind of obvious as you multiply and divide by the same quantity. I think it is sufficient to say that the fraction was taken from Alvain et al. and converted to percentage. But it is important to make clear if the method used by Alvain is consistent with the estimation of plankton composition done with the models (see General comment #1).

P18093_L15-16 It is not clear if the authors refer to the difference in functional forms or in the parameters.

P18094_L2-3 This was already described previously at P18093.

BGD

9, C7939–C7949, 2013

P18094_L10 There is a clear distinction between preference factors and availability (see *Gentleman et al.* (2003)). From the equations, I believe these are availability and not preferences.

P18095_L13 It is difficult to judge if it's reasonable given the choice of the color palette. As all OGCMs have a rather coarse resolution, it is unlikely that they will be able to capture the coastal maxima (besides PISCES that uses an additional input of iron from sediments on the shelves). Either you compute some objective measure (e.g. *Stow et al.* (2009)) of reliability or I suggest to exclude the points with depth lower than 200 m and adjust the color scale to improve the visibility of features.

L18-19 It is not clear if "large" is referred to the numerical models or to the satellite models.

L24 I see both a time shift and an overestimation. The results would be easier to follow if you could also provide a table with the mean bloom month for models and satellite as a function of latitudinal bands and oceans.

P18096_L3 It is mostly the central North Pacific and not the western. Please explain better. Also, the use of HNLC regions of the North Pacific at lines 8-9 is too broad. Please specify.

L6 These sentences are a bit confused. Are the authors referring to the SeaWifS data? Then I would presumably say that it is more December and January.

L7 It is hard to tell that the seasonal shifts are reasonably reproduced just by looking at this figure. Please clarify.

L10 I cannot understand the meaning of this sentence.

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9, C7939–C7949, 2013

Interactive Comment

P18097_L1-17 This paragraph is rather difficult to follow. It is not clear what is similar and what is different, especially because the satellite models are in contrast with each other. This should be explained more clearly.

L25-29 This description is confusing. I would suggest to first describe the satellite estimates and then the models, and not to insert some model comparisons in the middle.

P18098-L1 English: with simulate

P18098-L3-9 The fact that there is no trend in Alvain et al is repeated twice (and there is no discussion on why it is the only one that differs)

L21 Why is this considered a discussion? results are still being presented

L23 This title is not correct (see General comments #2). Here you definitely refer to the growth rate and not to photosynthesis, as the light harvesting could continue at very low nutrient concentrations.

P18099_L4-7 This sentence is a little confused. The authors are including some hints of what will be analyzed in the next section. I guess they mean that diatoms cannot be dominant over flagellates just because of bottom-up control on the flagellate population.

P18100_L25-26 I suggest to move the reference to Table 1 after the explanation of Fig. 5, as it is a summary of the findings shown in the picture.

P18101_L4-8 This sentence is not clear although I understand the meaning. I think the authors should explain better what favours diatoms over flagellates, otherwise the sentence may be interpreted such as nutrient limitation is correlated with high chlorophyll concentration!

P18102_L21-27 This is an important point that should be discussed further, while as it stands it is a bit hidden in the description of the results. All functional forms used in current PFT models have an implicit treatment of the closure term, usually treated as a quadratic term (*Edwards and Yool* (2000)) and the models presented here differ quite substantially (NEMURO for instance move the closure term one level further). The authors should discuss whether a conclusion can be derived from these differences.

P18103_L9 The sentence is unclear. Does it mean that it is generally independent? It sounds like the authors do not know.

P18103_L19 PlankTOM5 behavior is rather strange. Can the authors explain why?

P18104_L7-9 This parameterization of the PISCES model should be explained better as it appears that it has an important role in the realized model response (see also the comments on the equations below).

P18105_L12-29 PlankTOM5 has a a rather different response when compared with the other models, though the functional forms are the same of PISCES. Is it only due to the different parameter values?

P18106_L11-15 Please rephrase, the sentence is rather difficult to follow.

P18106_L22-24 I do not agree that these results are sufficient to state that the response to climate change projections may be different. All models have a first-order nutrient limitation control on primary production and therefore I would expect they would give similar results when exposed to substantial reduction in vertical rates of nutrient availability. Moreover, the response may be the same, although for different mechanistic adjustments of the various functional forms. I think this statement should be much more substantiated.

BGD

9, C7939–C7949, 2013

Interactive Comment

P18107_L7 Paper by LeQuere et al is submitted. I suggest to include a brief description of the relevant information and not just the reference.

P18107_L9 This is the only part of the paper where information from the observations are given. And it sounds a bit weird, as by reading the sentence one would think that, by chance, models behave like the observations, while it is by construction the other way around. Models are derived from heuristic observations and should serve to test hypotheses. I think there should be more discussion on what models actually do against the expected (and observed, if possible) system behavior of the plankton ecosystem (see General comment #4) .

3 Comments on the Appendix

There are some inconsistencies and strange notations in the description of the equations. I understand how difficult it is to write different formulations with a unified notation (and this is partly why I suggested the generic notation in Vichi et al 2007), however it is important to use the same notation throughout the description. For instance, sometimes the index i is used for phytoplankton and sometimes l, while different indices are used for the different zooplankton groups.

eq_A3-A4 This parameter is misleading. In A4 it is the algebraic sum of two separate terms, while in A3 is a single combined term. I suggest to use $V_{P_i}^{NO_3,NH_4}$ for both that indicates the combined effect of nitrate and ammonium.

eq_A5 To avoid ambiguities with the term above, I suggest to use a generic variable for the limiting nutrient, such as

$$V_{P_I}^{x}, \text{where } x = Fe, PO_4, Si$$

and equivalently in the Michaelis-Menten form.

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9, C7939–C7949, 2013

P18109_L9-10 The usage of maximum concentrations in PISCES should be explained better. Is it a constant variable for the whole ocean or it changes year by year? This is an important information especially because this maximum could be different for simulations under future climate change conditions.

eq_A8 It is not clear if α is a function of the chlorophyll:carbon ratio or it is a multiplication. I know how it is in Geider et al., and it maybe worthwhile to use a similar notation, like for instance defining a chl:C ratio as θ^{P_i}.

eq_A9 Same as above but for PAR.

P18110_L10-13 The Eppley curve is not an expression of the Q10 factor because it does not scale to a reference temperature T_0 that is usually set to 10^oC. Make clear if the models are using Q10 or Eppley.

L13 Does the models use a different chl-specific attenuation coefficient for light propagation? This may also add to the way light is limited. However, it is likely to be not important here as surface values are considered.

eq_B1 here the placeholder Z_i is introduced but a completely different notation is used in the next equations, like $g_i^{Z/M}$ where I would have expected g^{Z_i} as for phytoplankton. Please make them consistent.

eq_B5 Same as above, (though this time the index l is introduced, but maybe is a typo) and also concerns eq B11-12 B14-15. I would suggest to consider something like

$$g_{P_i}^{Z_j} = G_{max}^{Z_j} \frac{p_i^{Z_j} P_i}{K^j + \sum_i p_i^{Z_j} P_i}$$

where $i = D, F$ (or whatever you call diatoms and flagellates or large and small, etc) and $j = M, Z$. The index can be dropped when describing the grazing by one zooplankton group only, as for CCSM-BEC

eq_B8 Here D is used instead of P_D. Also, it should be explained how the maximum of diatoms concentration is computed in this experiment, as this is a peculiar feature of the PISCES model.

P18113L14_P18114L10 English: is represents

Table_1 The italic words are hard to distinguish. Either use bold face or add a * to mark the significant processes

Fig_1 The color palette is strongly saturated and does not allow to recognize any feature (see also the specific comment above). Latitude and longitude labels should be included

Fig_3-4 The captions partly explain how the average over the regions is computed. This part should go in the methodology. Also, it is not very clear why they are referred as "blooming regions". I thought the regions had fixed boundaries.

Fig_4_8 Y axis label: what are P/B and G/B? I guess it would help the interpretation of the figures if the labels could indicate "D dominance" above the 0 line and "N dominance" below it.

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9, C7939–C7949, 2013

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