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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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Author Response: We thank Referee #1 Marcello Vichi for posting a greatly helpful review of the paper. In the revised paper we have addressed all of the comments brought forward by the reviewer and this has improved the paper. Below are our responses to each of your comments.

1. General comments 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

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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 m3. 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." Thank you for your constructive comments. We unified several methodology subsections, and added the Subsection 2.5 for the definition of blooming region. In introduction, we also add a subsection for the conceptual idea for phytoplankton competition, and made clear the main purpose of this paper as suggested.

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." We used the growth rate instead of the photosynthesis rate throughout the paper as suggested. Thank you for your valuable comment. The reproduced nutrient concentrations are different between models. However, nutrient concentrations are determined by not only physical environment such as vertical mixing and upwelling

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but also biogeochemical processes such as nutrient uptake of phytoplankton and recycling. In the current data set of MAREMIP phase 0, it is difficult to separate physical and biogeochemical effects on the differences in nutrient concentration between models. The difference in physical environment such as mixing and upwelling might affect the magnitude and the peak timing of the spring bloom. But it would not affect to the essential mechanisms of phytoplankton competition which is mainly determined by the internal dynamics of ecosystem.

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." Although we used satellite estimations of percentage of diatoms for the evaluation of the global distribution (Fig. 2), we introduced an observed data, which is the HPLC pigment data (Fig. 1; Hirata et al., 2011) collected from the world ocean, for the evaluation of a relationship between percentage of diatoms and chl-a concentration (Fig 3).

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." One of key findings through this study was an importance of the parameter

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values of maximum growth rate of phytoplankton. We added a discussion in Section 4 (Conclusion) for further improvement of current PFT models based on observational studies as follows: “For further understanding, difference in maximum growth rate between diatoms and nanophytoplankton is one factor that determines whether phytoplankton competition during blooms is controlled by bottom-up or top-down mechanism. In NEMURO and PlankTOM5, diatoms have greater maximum growth rates than nanophytoplankton. On the other hand, there are no differences in maximum growth rate in PISCES and CCSM-BEC. During the blooming season, when nutrient and light limitation are less important, the difference in the maximum growth rate is potentially the main determinant of PFT dominance. Therefore, efforts are required to determine precisely the difference in the maximum growth rate for these function types based on observations. In observational studies, maximum growth rates of phytoplankton including diatoms and nanophytoplankton widely vary from 0.2 to 3.3 /day under conditions of saturating light and nutrient sufficiency (e.g., Williams, 1964; Eppley and Sloan, 1966; Blasco et al., 1982; Schone, 1982; Tang, 1995; Kudo et al., 2000; Milligan and Harrison, 2000). Several studies have found that maximum growth rate tends to decrease with increasing cell size (Sarthou et al., 2005, Finkel et al., 2010). On the other hand, diatoms tend to have larger maximum growth rate than nanophytoplankton of the same cell size (Tang, 1995; Finkel et al., 2010, Ward et al., 2012) although many diatoms are larger than any nanophytoplankton. The observed maximum growth rate and cell size of each phytoplankton type widely varies, and the ranges of variations overlap. For the evaluation of maximum growth rates in current PFT models, it is important to identify the combination of trait values (i.e., parameter values in models) such as cell size and maximum growth rate that are typical of each phytoplankton type for the biogeochemical and ecological functions considered in a given modeling study.“

2 Specific comments

P18086_L21 “CCSM-BEC and PISCES have not been described yet.” P18086_L25 “same as above. Model names are not substantial at this initial stage and only the

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works with models used in this manuscript are cited. I suggest to remove all names from the introduction." We removed all model names as suggested.

P18087_L25 "PFT models are plankton models and not full ecosystem models. (this work is focused on two components of the phytoplankton.)" We modified the word "marine ecosystem" to "a lower trophic level ecosystem".

P18087_L29 "I find a bit strange that modeling 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...." We removed descriptions about the particle aggregations, since it could cause some misunderstandings as suggested. Instead of this, we add a subsection for the conceptual idea for phytoplankton competition

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. As each PFT model is coupled to a different physical model, for the initial conditions and biogeochemical spin-up, we added the reference of the indicated original publications. We also add a subsection 2.5 for the discussion of off-line simulation.

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)." We removed this equation, and added a short discussion about a possibility of underestimation of the percentage of diatoms in this simplified conversion.

P18093_L15-16 “It is not clear if the authors refer to the difference in functional forms or in the parameters.” We mentioned that each model used different dependency terms (i.e., different functional forms) with taxon-specific parameters, and referred the Appendix A for more detail.

P18094_L2-3 “This was already described previously at P18093.” We removed this sentence.

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.” We changed the word “grazing preference” in the equation to “food availability” as suggested.

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.” We changed the color scale (Fig. 1) significantly as suggested. The regional differences can be recognized with new scale.

P18095_L18-19 “It is not clear if “large” is referred to the numerical models or to the satellite models.” We made clear that it referred satellite.

P18095_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.” We calculated the mean bloom month as a function of latitudinal bands and oceans. However, the results couldn’t clearly capture the characteristics of regional difference, because the

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shift from coastal to open oceans is neglected in the longitudinal average. Therefore, instead of the table we added more specific explanations for the regional differences.

P18096_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." We rewrote as December and January.

P18096_L7 "It is hard to tell that the seasonal shifts are reasonably reproduced just by looking at this figure. Please clarify." We described about the maximum timing of the Northern and the Southern Hemispheres separately, because the seasonal shifts in the Southern Ocean were not clear in the observation.

P18096_L10 "I cannot understand the meaning of this sentence." We removed this sentence.

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." We introduced the percentage of diatoms derived from HPLC pigment data for evaluation of model results following the General comment 3#

P18097_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." We changed the order of the sentences as suggested.

P18098-L1 "English: with simulate" We corrected.

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)" We removed the repeated sentence, and added a short discussion about the uncertainty of the conversion from the Alvain's diatom dominance frequency data to the percentage of diatoms at the end of the section 2.2.

P18098-L21 "Why is this considered a discussion? results are still being presented" We combined the section of discussion with the results section as “results and discussion”.

P18098-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.” We changed the title to “Comparison of the limitation factors of phytoplankton growth”.

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.” We removed the part of the corresponding as suggested.

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.” The reference to Table 1 moved as suggested.

P18101_L4-8 “This sentence is not clear although I understand the meaning. I think the authors should explain better what favors diatoms over flagellates, otherwise the sentence may be interpreted such as nutrient limitation is correlated with high chlorophyll concentration!” We rephrased the sentence as follows: “The increasing trend of the relative growth ratio with an increasing magnitude of the bloom is determined by the easing trend of nutrient limitation,. . .”

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.” This is an important point as suggested. To evaluate the importance of differences in grazing equation between models, we need to calculate the absolute values of grazing rate for each zPFT. However, as we don’t have enough data for that calculation at the current stage of MAREMIP, we needed to use the "relative grazing ratio" to cancel out several terms in the equation. Although we capture the qualitative understanding using the relative grazing ratio, for further quantitative understanding we need to store the absolute values of grazing rate in online simulations. As the characteristics of the grazing interactions in the current ecosystem models in MAREMIP Phase 0 are discussed in more detail in Sailley et al., (2013) in a different way, we referred this paper.

P18103_L9 "The sentence is unclear. Does it mean that it is generally independent? It sounds like the authors do not know." It was our careless mistake. We removed the word "clearly".

P18103_L19 "PlankTOM5 behavior is rather strange. Can the authors explain why?" It is difficult to say the behavior of PlankTOM5 is strange compared with other models, as we don't have enough data for validation of zooplankton composition. To explain the zooplankton composition in PlankTOM5, we might need a specific analysis focusing on the zooplankton competition like phytoplankton competition in this study using some quantitative rates. It would be a next theme stemming from this study.

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)." We rephrased the explanation, as suggested.

P18105_L12-29 "PlankTOM5 has 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?" The functional form of microzooplankton is different between PISCES and PlankTOM5 (Appendix B7 and B8), and it has an important role for the different response as shown in Fig 8.

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

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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." As referee suggested, reduction of nutrient concentration associated with strengthened stratification tends to lead decreases in the percentage of diatom and in primary production in all models. However, as we discussed, the main controlling factor of phytoplankton competition is significantly different between models. For example, phytoplankton composition in NEMURO is strongly controlled by bottom-up control (i.e., nutrient limitation), while the PISCES is controlled by top-down control (i.e., grazing by microzooplankton on nanophytoplankton). Therefore, the reduction of nutrient would directly lead decrease in the percentage of diatoms in NEMURO. But it is not so easy to expect what happens as a response to an effect of top-down control in PISCES. For example, under the nanophytoplankton dominant environment, relatively higher grazing pressure by microzooplankton on nanophytoplankton which tends to increase the percentage of diatoms might be expected. This means the difference in sensitivity between models to environmental changes even if the directions of the changes are the same between models.

P18107_L7 "Paper by Le Quere et al is submitted. I suggest to include a brief description of the relevant information and not just the reference." We modified this section following the General comment #4.

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)." We added a discussion for further improvement of current PFT models based on observational studies following the General comment #4.

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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 NO3, NH4 for both that indicates the combined effect of nitrate and ammonium." We used VNO3, NH4 as suggested.

eq_A5 "To avoid ambiguities with the term above, I suggest to use a generic variable for the limiting nutrient, such as Vx PI ; where x = Fe; PO4; Si and equivalently in the Michaelis-Menten form." We used a generic variable as suggested.

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." We added information as "local yearly maximum concentration of silicate", and referred an original publication of Pondaven et al., 1998.

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 may be worthwhile to use a similar notation, like for instance defining a chl:C ratio as θPi ." We used θPi as suggested.

eq_A9 "Same as above but for PAR." We corrected the description α(PAR)[PAR] as α[PAR], as it was a careless mistake.

P18110_L10-13: "The Eppley curve is not an expression of the Q10 factor because it does not scale to a reference temperature T0 that is usually set to 10oC. Make clear if the models are using Q10 or Eppley." The models used the Q10 relationship. We made clear this point as follows; "For the temperature dependency term, the Q10 relationship is employed in all the models,. . ."

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." The treatment of light propagation is different between the models. However, the influence of the difference in light propagation is not important at the surface layer, as suggested.

eq_B1: “here the placeholder Zi is introduced but a completely different notation is used in the next equations, like gZ/M where I would have expected gZi as for phytoplankton. Please make them consistent.” We rewrote all equations with the placeholder Zi as suggested.

eq_B5: “Same as above, (though this time the index I is introduced, but maybe is a typo) and also concerns eq B11-12 B14-15. I would suggest to consider something like. . . 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” Index “i” denotes one resource, and “I” denotes all the resources for grazing in the equation B5. As mentioned above, we rewrote the other equations.

eq_B8: “Here D is used instead of PD. 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.” Dmax is given as a fixed parameter (Table A2) from the original publication of PISCES (Aumont et al., 2004). We rewrote the equation with PD as suggested.

P18113L14_P18114L10: “English: is represents” We corrected.

Table_1: “The italic words are hard to distinguish. Either use bold face or add a * to mark the significant processes” We used * for 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” We changed the color palette and added the labels of latitude and longitude.

Fig_3-4: “The captions partly explain how the average over the regions is computed. This part should go in the methodology. “ We changed the description as suggested.

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” It was careless mistakes. We used the relative photosynthesis ratio and the relative grazing ratio instead of the P/B and G/B ratios, respectively.

Interactive comment on Biogeosciences Discuss., 9, 18083, 2012.