

# ***Interactive comment on “The distribution, dominance patterns and ecological niches of plankton functional types in Dynamic Green Ocean Models and satellite estimates” by M. Vogt et al.***

## **Anonymous Referee #2**

Received and published: 14 January 2014

### **1 General comment**

This paper is a companion of a previous work by Hashioka et al. (2012) that presents a multi-model analysis done over 4 different Plankton Functional Types (PFT) models. While the focus of the previous paper was clearly on diatom dominance over smaller phytoplankton during the bloom period with the aim of mechanistically understand the differences between the models, this specific manuscript lacks the necessary focus. A major revision is therefore required. I have some major concerns on this manuscript

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



that needs to be addressed before publication may take place:

1. The aims of the work are not clearly expressed and I actually have problems in understanding what the intentions of the authors are. The title tells one story, which everybody would find appealing because ecological niches are still an evanescent theoretical concept in the global ocean. However the content is more a generic overview of the model behaviours (and I include satellite estimates among models here) without any critical assessment or attempt to compare with independent observations of phytoplankton dominance. The reader is left with a sense of discomfort, such as all models are equally inadequate but they are all good. There is indeed an interesting discussion at the end that I found very well done, but it is not sufficient to take away my concerns.
2. There is also a kind of confusion on the target temporal scale of the study and the dominance concept. In the introduction and in Sec. 3.3 one gets the impression that the seasonal cycle is the target, however the remainder of the paper and the more quantitative analysis is performed on the annual means. I believe that the definition of dominance at the seasonal and annual scales must be different and that the same methodology of Sec 2.6 cannot be applied indifferently for both periods. Annual dominance should consider the relative occurrences of monthly dominance, that would probably lead to a much larger presence of coexistence, as I would expect the stratified periods to be more dominated by smaller phytoplankton. If annual biomass is used as indicator of dominance, the seasonal succession is lost because phytoplankton concentrations during stratified periods are small. If this was somehow taken into account in the analysis, it must be better explained.
3. the use of the GAM technique is interesting but kind of germinal in this paper. It is not clear if it was used to identify the environmental factors that determine the dominance of one PFT over another or as an assessment technique to compare

**BGD**

10, C7953–C7962, 2014

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

the different models and the satellite estimates. The a priori choice of a limited set of parameters would exclude the former hypothesis, because in that case a larger set of environmental factors should have been considered and then used to identify the ones that would lead to the maximization of variance. The fact that the explained variance with satellite models is lower is a clear indication that more factors are at play. And the fact that explained variance in DGOM is so high with just the usage of sea surface temperature and nitrate is to me an indication of overdetermination in the implemented parameterizations. This is typical in models driven by Michaelis-Menten dynamics (Flynn, 2010) that do not consider co-limitation. The authors point out the major caveats at the end of the paper but, honestly, I do not think that the central Fig. 4 is sufficient to show that there is any niche distribution that is common to the considered models (see also the specific comment below).

4. Appendices contain figures that are central to the understanding of the manuscript. They look more like separated sections than appendices that expands concepts mentioned in the text. I would suggest the authors to include them in the text.
5. The manuscript lacks a clear conclusion and a final section on recommendations that would suggest the future directions of investigation. It is never investigated whether the ensemble mean performs better than any single model, although this would define an ensemble definition of dominance. For instance, one additional conclusion from this work would be that models should include picophytoplankton to reduce the fuzziness of niches. Picophytoplankton would reduce the dominance of nanophytoplankton and probably let emerge other interesting features. I agree with the authors that there is still too much overlapping in the representation of PFTs in models, but my feeling is that this is more due to the reluctance of modellers to relax their Occam's razor. Given that MAREMIP phase 0 is the state of the art of global PFT models, there is still a long trip in front of us to include

the required level of complexity, and there is no need to be worried that we are running before we can walk (*sensu* Anderson, 2005) because the pace is still rather slow.

## 2 Specific comments

**P17197:L3** Satellite data are indeed promising and necessary tools to monitor global phytoplankton. However, in terms of model validation, they are still as good as DGOMs (e.g. Friedrichs et al., 2009).

**P17197:L20** This sentence is misleading. This evaluation was not done in the manuscript because only models were used.

**P17200:L28** Please report if particulate silicate in NEMURO is biogenic.

**P17202:L21** “Note that...” please explain better the meaning of this sentence.

**P17203:L15-17** Also at lines 26-28. I wonder why these data cannot be considered. If this is the case, it means that the bloom period (the period with the highest concentration) is not representative of the major dominance and this should be considered in the definition of dominance based on annual mean biomass used throughout the paper.

**P17203:L15-17** The definition of dominance and coexistence is crucial in this work. I think the authors should give more attention to this section because I fear their results might change if the definition is changed. I know that there are not many other studies dealing with dominance in DGOM (for instance Vichi et al., 2007) but the choice should be justified. The authors could for instance provide a comparison of PDF for the chosen pPFTS computed over the monthly means for a set of model grid points (or averaged over a region). In addition, as pointed out

C7956

**BGD**

10, C7953–C7962, 2014

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



in my general comments above, I do not think the annual dominance should be computed in the same way as the monthly dominance. The computation of percentage of biomass based on annual means is different from the combination of the percentage of biomass based on monthly means (and so on with weekly and daily means of course). Please use a formula to explain how dominance is defined, as for instance

$$\frac{\sum_i P_i^j}{\sum_{i,j} P_i^j}$$

where  $i$  runs over months and  $j$  over pPFTs. The same for coexistence

**P17205:L5** Please report how many models out of the total 4.

**P17206:L1** MLD is a seasonal feature and annual means are somehow misleading. This correlation between environmental factors is an important issue that makes me think how the a priori factors of the GAM analysis have been chosen.

**P17206:L7-11** If Iron is not considered, why is it mentioned in the data section? This is confusing and probably linked to the fact that some data are used in the appendix for validation but not referenced in the text (see point 4 in general comments)

**P17207:L1-12** This explanation is not completely clear to me. If the GAM is used to model the the dominance of each pPFT at each pixel, it means that it must be a vector of dimension 2 or 3, therefore it should have two indices  $D_i^j$  where  $j$  runs over the number of pPFTs. Is this the case? Also, this means that coexistence is never possible if it is not found in the original definition of dominance and the probability of coexistence cannot be modelled.

**P17207:L17** The procedure used to model the satellite data with GAM should be better explained. In this case I guess you use atlas data.

**Fig\_1** Latitude is given in units of 0-180!

C7957

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



**P17209:L7** I think that saying “tend to overestimate” is kind of an understatement. It largely overestimates the contribution of diatoms found by Hirata et al. (2011).

**P17209:L17** It looks like there is a correlation between coccolithophore abundance and regions with high dissolved inorganic carbon. This is kind of natural, and therefore I would expect that the niche would be not completely identified for this group if based only on nitrate and SST.

**P17209:L22** I do not think that “agree” is the most appropriate way to describe this. I do not see such a clear agreement.

**P17209:L24** This sentence is not clear. Are diazotrophs expected to dominate the annual mean globally?

**P17210:L2-3** Good coverage of what? Is this referred to satellite data?

**P17210:L7-8** Fig. 3 is very difficult to read and the given comments are rather generic. I honestly cannot see the seasonal cycle so clearly. Maybe you could provide annual timeseries from selected regions for all model and data, that would be more explanatory than a collection of stamps.

**P17210:L10-16** I wonder how significant is the dominance of diatoms in December under the Arctic sea ice. Maybe there should be a biomass threshold for the computation of dominance as for instance done by Vichi et al. (2007). This is especially true with models that prescribe a biomass minimum to avoid disappearance of pPFTs. This may also be true for the regions of coccolithophores dominance in L20-26. How larger is their chlorophyll concentration respect to the minimum threshold? (please do not use the term “niches” here but preferably “regions” because you are referring to a spatial feature and not a functional one.

**P17211:L8-10** Please be more specific: explain which model in which region.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

**P17212:L1-6** This specific comment is related to points 3 and 4 in the general comments above. The usage of GAM here is kind of limited. It is apparently used in place of a principal component analysis but limiting the power of the technique, which is to let the driving factors emerge. It was already introduced in sec. 2.7 that MLD was correlated to SST and the analysis in Appendix C confirms this. I think this analysis is interesting because it shows that this is true for models but not for satellite data and therefore I suggest to include it in the section. MLD is a seasonal feature and the annual mean may be of limited explanation power. The authors may consider to include additional statistical momentum such as the standard deviation or use the maximum MLD instead of (or in addition to) the annual mean.

**P17212:L7-10** I do not see much consistency in the patterns of Fig. 4. It is very subjective and an objective measure of similarity should be used. Also, the sentences are vague and kind of contradicting each others. In the case of PISCES and NE-MURO it is not true that diatom dominance is found at high  $\text{NO}_3$  concentration because the scale is logarithmic and the distribution spans 2 orders of magnitude. It would be better to define what is high or low nitrate concentration.

**P17212:L12-13** “statistically modelled probability” is confusing. It is not clear if you refer to models here because also the GAM is a model.

**P17212:L15-17** I do not think it is surprising that diatom dominance from models is more correlated to the simulated nitrate and SST results. This is because there is a direct correlation expressed by eq. (1) while satellite data are much less correlated with the nutrient and SST distributions (see point 3 above). This implies that diatoms can apparently survive and dominate at low nitrate concentrations as well.

**P17212:L21** I seem to see some correspondence between coexistence in Hirata data and diatom niches in PISCES. Can you explain that?

**P17213:L5-11** You could consider other environmental controls here. It is likely that the bottom input of continental nutrients parameterized in PISCES is the reason of diatom dominance in coastal upwelling regions. Also, do you really mean “low-intermediate nutrient concentrations” here? I rather see “high-intermediate”.

**P17213:L27-** The authors may wish to explore further another feature of Fig. 4. I seem to observe that models tends to show dominance over the diagonal of the phase space, that is, there is no dominant group at low temperature and low nitrate concentrations. Satellites shows a much lower control of nutrients, therefore the pattern is more left to right along the temperature axis. This is likely calling for a too strong control of nitrate in the simulated phytoplankton growth and the need to include more stoichiometric variability in functional parameterizations.

**P17214:L9** I thought MLD was excluded from the analysis. Why it is mentioned here?

**P17214:L20** I guess you wish to refer to Fig. 4 here because you cannot justify what is presented in Fig. 5 by the results shown in Fig. 5 itself!

**Fig\_5** I wonder why the probability of dominance from the GAM is presented with a binary color-coding. Is there a specific meaning for the value 0.5?

**P17215:L12-15** You may consider to compare the results with global models that already implements a flexible stoichiometry (e.g. Patara et al., 2012).

**P17216:L10-21** This comment is not clear. Which is wrong? Satellite or models? This definitely calls for an independent evaluation with data like for instance done in Friedrichs et al. (2009) with primary production in the equatorial Pacific or in Vichi et al. (2007) with PFT abundances in selected regions of the world ocean.

**P17217:L12-22** You should make clear when referring to models or satellite estimates. I wonder if this is a case for allowing more coexistence in models (or determining

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)



coexistence in a different way). Probably models are not ready yet to simulate the Hutchinson's paradox.

**P17218:L11** Do you mean mesozooplankton here?

**P17218:L14** What is “different” here? Different respect to the satellites or different between functional types? I would expect them to have different niches otherwise they would be the same group. Please rephrase.

**P17218:L18** Again, MLD was not used in the analysis.

**P17218:L22-23** How can you tell that the seasonal representation is poor if you analysed annual means?

**P17220:L15-17** Why are you not able to characterize these properties with the model data you have? You do resolve the seasonal cycle (Sec. 3.3).

**P17221:L7-11** I would tend to disagree that this paper shows that latitudinal patterns can be explained by few predictor variables. Where was it shown?

**P17223:L17** I think units are in  $\text{mmol m}^{-3}$  and not per litre (would be  $\mu\text{mol l}^{-1}$ ).

**P17223:L17** Please explain how the RMSE was computed. Is this spatial or temporal, or both?

### 3 Comments on the Appendix

#### References

Anderson, T. R. (2005), Plankton functional type modelling: running before we can walk?, *J. Plankt. Res.*, 27, 1073–1081.

**BGD**

10, C7953–C7962, 2014

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



- Flynn, K. J. (2010), Ecological modelling in a sea of variable stoichiometry: Dysfunctionality and the legacy of redfield and monod, *Prog. Oceanogr.*, 84(1–2), 52–65, 10.1016/j.pocean.2009.09.006.
- Friedrichs, M. A. M., M.-E. Carr, R. T. Barber, M. Scardi, D. Antoine, R. A. Armstrong, I. Asanuma, M. J. Behrenfeld, E. T. Buitenhuis, F. Chai, J. R. Christian, A. M. Ciotti, S. C. Doney, M. Dowell, J. Dunne, B. Gentili, W. Gregg, N. Hoepffner, J. Ishizaka, T. Kameda, I. Lima, J. Marra, F. Melin, J. K. Moore, A. Morel, R. T. O'Malley, J. O'Reilly, V. S. Saba, M. Schmeltz, T. J. Smyth, J. Tjiputra, K. Waters, T. K. Westberry, and A. Winguth (2009), Assessing the uncertainties of model estimates of primary productivity in the tropical Pacific Ocean, *J. Mar. Sys.*, 76(1-2), 113–133, 10.1016/j.jmarsys.2008.05.010.
- Patara, L., M. Vichi, and S. Masina (2012), Impacts of natural and anthropogenic climate variations on North Pacific plankton in an Earth System Model, *Ecol. Model.*, 244, 132–147, 10.1016/j.ecolmodel.2012.06.012.
- Vichi, M., S. Masina, and A. Navarra (2007), A generalized model of pelagic biogeochemistry for the global ocean ecosystem. Part II: numerical simulations, *J. Mar. Sys.*, 64, 110–134.

---

Interactive comment on Biogeosciences Discuss., 10, 17193, 2013.

**BGD**

10, C7953–C7962, 2014

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper