

Interactive comment on “Distribution of phytoplankton functional types in high-nitrate low-chlorophyll waters in a new diagnostic ecological indicator model” by A. P. Palacz et al.

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Here we provide a detailed response (in black) to Referee's comments (in blue italics) on our original manuscript. We thank the Referee for the constructive criticism which led us to an improved version of this manuscript.

"1. First, I recommend making clear a definition of the HNLC region in this study. As the authors mentioned in the introduction (P8105, L5-7), the Southern Ocean (AntAtl), the subarctic North Pacific (NEPac) and the equatorial Pacific (EEP) are known as the HNLC regions (e.g., Sarmiento and Gruber, 2006; Fig 4.1.8). However, I am not sure whether the training regions of PhytoANN in the Atlantic Ocean (Black boxes in Fig1)

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are the HNLC regions. This study is mainly focusing on the plankton composition in the HNLC regions. In these regions, the role of iron is known as one of the key controlling factors for the phytoplankton growth (Martin et al., 1994). However, the iron concentration is not used as a component of PhytoANN (P8108, L18-22). Although the authors mentioned that the inclusion of iron didn't improve the result of PhytoANN, I wonder if the training regions of PhytoANN in the Atlantic Ocean are not mainly limited by iron. Several modeling studies showed the global distribution of the limiting nutrient of phytoplankton growth (e.g., Moore et al., 2002, 2004; Aumont et al., 2003; Schneider et al., 2007). The most of the models showed limitations by macronutrient (i.e., nitrate, phosphate or silicate) in the Atlantic Ocean including the training regions of PhytoANN, while the typical HNLC regions (AntAtl, NEPac, EEP) are limited by iron. This result possibly means the mechanism of phytoplankton growth is different between training regions of PhytoANN and the regions of exploratory analysis (typical HNLC regions). Related to the above question, as the source of phyto-PFT for PhytoANN the authors used the simulated PFT biomass by NOBM. NOBM successfully reproduced the phytoplankton composition in the Atlantic Ocean including the training regions (Fig 6). However, significant overestimations of the percentage of diatoms can be seen in the typical HNLC regions (AntAtl, NEPac, EEP). I know difficulties of the representations of features in the HNLC regions by the current PFT models. But this result might mean some problems or shortage in the modeled mechanisms in the HNLC regions, and the significant differences of governing mechanisms between training regions and the typical HNLC regions. Therefore, I would like to request further discussion (1) about the meaning of the exclusion of iron from the PhytoANN algorithms, (2) about the representativeness of the choice of the training regions and (3) about the influence of the overestimations of diatoms in the HNLC regions in NOBM as the phyto-PFT source data of PhytoANN. I understand the exclusion of NO₃ from the PhytoANN algorithms by the implicit inclusion by SST and Chl as authors mentioned (P8108, L23-25). I think this implicit inclusion means that the PhytoANN includes the potential mechanisms related to NO₃. But I am wondering if the variability of iron in the HNLC region is

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difficult to explain by associated changes in other factors such as SST, Wspd, PAR, MLD and Chl. Do the authors believe the PhytoANN potentially includes the effect of iron?"

"(1) about the meaning of the exclusion of iron from the PhytoANN algorithms"

We agree with the reviewer completely. We realize that HNLC regions are primarily iron limited while the northern hemisphere Atlantic is not. Therefore, we would expect physiological mechanisms of some PFTs in the exploratory regions to be different from those in the training regions. Excluding iron from our input space may thus appear as a limitation to this approach, however it could also serve as an advantage in discussing model patterns of global phytoplankton biogeography. In our view, there are two main possible explanations as to why PhytoANN results in HNLC regions are closer to the observed in comparison to the NOBM (which uses Fe as a state variable). On one hand, the PhytoANN could implicitly take the role of iron in HNLC waters into account by assuming that Fe-limiting conditions coincide with some combination of other indicators considered. Implicit consideration of the effects of Fe availability on phytoplankton are known from biogeochemical models too (e.g. Chai et al., 2007). Knowing that Chl is a strong indicator in the PhytoANN, implicit effects of iron could be expected. On the other hand, it is possible that the PhytoANN only weakly, if at all, accounts for Fe-limited conditions but nevertheless projects more reasonable HNLC phytoplankton composition compared to NOBM. In such case, our PhytoANN results suggest that NOBM-derived Fe concentration may not necessarily be a good indicator of phytoplankton community composition in HNLC waters. These results do not suggest that Fe is not an important physiological regulator but indicate that Fe may not be adequately represented in current models. While most biological models predict strong diatom responses to transient Fe inputs in HNLC waters via upwelling due to tropical instability waves, eddies or frontal activities, the magnitude of these projected responses may often be too high as indicated by too dominant role of diatoms in these

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ecosystems (e.g. Le Quéré et al. (2005), Sinha et al. (2010)). Results of SEEDS I and SEEDS II Fe fertilization experiments (Tsuda et al., 2007) clearly demonstrated that strong diatom responses depend on the standing stock of grazers and potentially other factors and not just Fe availability alone. In the equatorial Pacific, it is necessary to better resolve aspects of Si and Fe co-limitation observed in the field (Brzezinski et al., 2011). Moreover, scarce Fe measurements and lack of their time series limit the accuracy of Fe parameterizations which in turn lead to potential errors in global biogeochemical models. We have included a condensed version of this response in the first paragraph of Section 3.5.

"(2) about the representativeness of the choice of the training regions"

It is true that it may appear inadequate to use non-HNLC regions for training and then apply the model to HNLC regions. However, if we disregard the difference in Fe concentrations between training and exploratory regions (on the basis of the arguments listed above), we will find that the range of all indicator values in the training regions is very similar to the range encountered in exploratory regions. This assures that the PhytoANN does not in general extrapolate beyond what it has seen during its training phase. In the revised manuscript, we add a table which lists the ranges of indicators from training and exploratory regions.

Moreover, if the PhytoANN results represent PFT composition in HNLC waters well, then this indicates an adequate choice of training regions. We have experimented with including one or more of the HNLC regions as training regions and this has always resulted in PhytoANN rendering more similar results to NOBM but at the same time further from the observed.

"(3) about the influence of the overestimations of diatoms in the HNLC regions in NOBM as the phyto-PFT source data of PhytoANN"

Please see our comment on overestimating diatoms in NOBM two points above. Additionally, we want to stress that all PhytoANN estimates of PFTs in HNLC regions

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were independent of NOBM PFTs. PhytoANN was trained by NOBM PFT results only from the northern hemisphere Atlantic where NOBM did not overestimate diatoms significantly. This fact was crucial to our choice of the training domain so that we may test whether PhytoANN can generate improved diatom and other PFT estimates in the exploratory regions.

Following the Reviewer's suggestion, we include more of this discussion in Section 2.2 and 2.3 of the revised manuscript.

"2. In Figure 9, the addition of interannual variation of Chl concentration (model and obs.) might be useful for understanding of the difference in plankton composition between estimated results (NOBM, PhytoANN and bio-optical)."

Yes, we agree. Variation of model and observed Chl is added to the Figure in the revised manuscript.

"The significant difference in interannual variations of phytoplankton composition between PhytoANN and biooptical is very interesting. Based on the HPLC data, Hirata et al. (2013) showed a clear single relationship between the percentage of PFT and Chl concentration. This means the existence of single state of PFT composition at each Hcl concentration. And also small variation of Chl concentration tends to show small variations in phytoplankton composition. On the other hand, even in the same Chl concentration, ANN could have different PFT composition by the effect of the other controlling factors such as SST, PAR, MLD, Wspd. Can the author argue about which is more realistic?"

Firstly, Hirata et al. (2011) only showed a "fit" to data. Their data used to derive the fit show scatters, demonstrating that relationships between Chl and PFT or size-structure are also likely to vary regionally and possibly seasonally. Meanwhile, what Hirata et al. (2011) showed is that these fluctuations of the relationships between Chl and PFTs are relatively smaller compared to more dynamic "synoptic relationships" which are approximated by the fit. This was already discussed in Hirata et al. (2011).

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Therefore, Hirata et al. (2011) should not be interpreted, in the first place, as "only a single relationship exists between Chl and PFTs for all ranges of temporal and spatial scales." Thus, it is natural that there would be some relationships unexplained in the fit shown in Hirata et al. (2011), especially if temporal and spatial scales discussed are different from that in Hirata et al. (2011).

While remote sensing methods show reasonable results, realistically Chl can be influenced by other factors beyond the composition of the PFT. For instance, photoacclimation can cause variations in Chl independent of PFTs, as can other factors like growth status, temperature and nutrients. Therefore, the existence of single state of PFT composition at each Chl concentration is likely unrealistic. Including the environmental indicators in the PhytoANN, may further improve the general relationship observed by authors like Hirata et al. (2011), by perhaps accounting for variations in these relationships between PFT and Chl caused by such environmental factors. Results shown in Fig. 3 and 4 (panels i j) suggest that the PhytoANN is capable of accounting for such variability in Chl-PFT relationships. This is also consistent with what a biological model such as NOBM would predict (Fig. A1 and A2).

"Minor Point; Figures 7 to 9 should have index (a) (b) (c). . ."

Corrected as suggested.

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