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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.

Anonymous Referee #2

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General Comments:

In order to estimate the phyto-PFT distribution in a wide range of biogeographic conditions, the authors demonstrate a new approach to combine the remote sensing and dynamic model results using an artificial neural network (ANN). They clearly showed the difference in ecological niche of phyto-PFTs, and temporal variations of the PFT composition in the seasonal and the interannual time scales.

Specific Comments:

I am satisfied with their discussion and implications, but I have two major questions related to the method of PhytoANN.

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) 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 form 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 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?

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). The significant difference in interannual variations of phytoplankton composition between PhytoANN and bio-optical 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?

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

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