

Dear authors,

I have read this manuscript with a great interest since the topic is highly important for the understanding of the complexity of the primary productivity controlling factors and the role of N₂ fixation in nutrient limited oceans.

General comments:

I find that the manuscript brings interesting combination of field research with modelling. Also, the exploration of nitracline and phosphocline depths variations in oligotrophic and ultra-oligotrophic regions of WTSP, including another analyses, enables a discussion of a high quality. Presented results are clear inspite of the complexity of the conducted research. I find the manuscript excellent, according to my skills to judge an overall scientific contribution. At the same time, I see some possibilities for improvements in Introduction, Methods and Discussion.

Topic – light

Since You mentioned beneficial role of N₂ fixers for the whole plankton community, which is in line with Agawin et al. (2007) and other studies which You cited, it would be interesting to explicitly include a sentence about the interplay between nutrients (dominantly DIN) and light as the common controlling factors in competition between N₂ fixers and non-fixers. It is obvious that Your research went further in more details concerning the link between different types of nutrient limitation and N₂ fixation, but for me as a reader, at least a small part about the light is missing.

I suppose that since the studied area spreads within a short range of geographic latitudes, You did not consider to include the topic of light in this particular manuscript, especially because of small differences in hours of daylight over the year in such an area close to the Equator? Therefore, You focused on the topic of nutrients without mentioning light explicitly, but only indirectly via citations?

Topic – phosphoenzymes

I suggest that You slightly improve the part about the phosphoenzymes that are important in usage of DOP during P-limitation (see below my suggestion). In addition, there is a fine opportunity for the inclusion of findings by Dyhrman et al. (2006) referring to the C-P lyase enzyme in *Trichodesmium*, which can enable this organism to get the competitive advantage over other marine phytoplankton that do not use phosphonates that are considered to contribute even up to 25% in DOP (Dyhrman et al. 2006).

Topic – stoichiometric ratios C:N:P for heterotrophs

I did not understand the reason for implementation of Redfield ratios for the heterotrophs since the literature supports molar C:N:P of e.g. 50:10:1. I see that later in the manuscript the rates of N and P mineralization are adjusted in the model, so You did some compensation, which seems to me rational to do. However, I think that many readers would like to see a sentence with explanation for the Redfield ratios for heterotrophs.

Technical details

My question marks are there only to provoke Your effort to slightly improve the presentation, rather than expecting the explicit answers during the open discussion, as far as I am concerned.

Page 1:

line 8 - only differing by the presence or **absence** of diazotrophs

line 13 – which seasonal changes? It seems that Abstract would be better if you write precisely here about the main results of your research regarding those seasonal changes.

Line 22 – I feel that the logics of the sentence is inadequate. The word „although“ would suggest opposing statement in the second part of the sentence, but in fact the outcome is pretty logical. The area is oligotrophic and since NH_4 and NO_3 are the main inorganic N species taken up by osmotrophs, those nutrients remain low. So, I suggest this sentence: „Since nitrate (NO_3^-) and ammonium (NH_4^+) are the two main N sources taken up by autotrophs, their concentrations remain very low in the oligotrophic ocean being frequently growth-limiting factor in most of the open ocean....

Page 2:

Line 2 - Some species of prokaryotic organisms....

Line 5 – Would it be good to amend this part with a detail about the nature of dissolved N? I mean on this „diazotrophs release dissolved inorganic and organic N...“?

Lines 7-8: I suggest „since it would reduce the N limitation for the phytoplankton and thus enhance primary production in the oligotrophic regions.

Explanation: This is not "characteristic" because some oligotrophic seas are P-limited, therefore "characteristic" seems not to be associated to N exclusively. "in the oligotrophic regions" sounds a bit better at the end of the sentence. Just a suggestion.

Line 10 - bioavailability of dissolved iron (Fe) and phosphate (P).....

Line 13 – It would be nice to have a reference at the end of this sentence since You write about classical paradigma?

After this sentence it would be nice to extend the Introduction by Agawin et al. (2007) and Rabouille et al. (2006), which You already cited, but an explicit note about the role of light for N_2 fixation in Your manuscript is lacking. You might briefly add here that N_2 fixation is highly dependent on the circadian clock (Rabouille et al. 2006) and that the success of non-diazotrophs and diazotrophs depend on the interplay between intensity of light and DIN concentration and the competition for those resources.

Line 22 – DIP availability if the statement is strictly for inorganic P.

Page 3:

Line 3 – Is it ecosystem or You explored ecosystems, one with diazotrophy and one without?

Line 20 – their different biogeochemical characteristics (only a lack of letter „i“ in their)

Page 4:

Lines 3,4 – I do not see in the manuscript where did You mention earlier anything about „ten years“?

Line 4 – How did You define „near-surface layer“?

Line 14 – „consumers“ could be changed to „grazers“? However, this is less important.

Line 25 – after „energy regulator“ it seems appropriate to extend the sentence with „, being itself tightly linked to the daily light cycle (Rabouille et al. 2006). Just an example.

Page 5:

Line 4 – Hereafter You have numerous dots after some units, these dots should be corrected.

Lines 5,6: Why did You change C:N:P from 50:10:1 for heterotrophs, which is supported by literature (Goldman and Dennett (2000), Fagerbakke et al. (1996), Chan et al. (2012), Alekseenko et al. (2014), for which You cite 50:10:1), to the Redfield ones?

Line 9 – Hereafter You write “phosphatase alkaline”. Is it nicer to write alkaline phosphatase, or it has to be phosphatase alkaline?

Line 34 – According to which reference You chose the ratios 1000:100:1? Can You extend the sentence with a comment about grazing pressure from right to the left in these abundances ratios? Then there is no need for reference maybe.

Page 7:

Line 21 – “particulate carbon biomass”, “particulate organic carbon” and “C biomass” are present in the manuscript. Are some of these names associated to the same variable? Can You simplify this?

Line 32 – Why not “over ten years” if You are able to perform model for ten years?

Page 8:

Line 28 – maybe extending the sentence with “compared to winter mixing”.

Page 9:

Line 26 – exclude one of the words, “new” or “fresh” because they carry the same information.

Page 10:

Line 24 – Did you provide an abbreviation explanation for PP before this point in the manuscript?

Page 11:

Line 7 - However, their development also requires **sufficient intensity of light** and other nutrients...

Line 26 –Why “or”? Is it better to write “e.g.” than “or”. There are other phosphoenzymes potentially used to get P from DOP, especially by *Trichodesmium*. I suggest “(alkaline phosphatase, nucleotidase, polyphosphatase, phosphodiesterase)”.

Then You could extend the discussion by “Moreover, C-P lyase found in *Trichodesmium* (Dyhrman et al. 2006) is another enzyme that enables this organism to use previously considered non-bioavailable fraction of DOP, i.e. phosphonates that represent circa 25% of DOP (Dyhrman et al. 2006). Taking into account this possibility, *Trichodesmium* thrives in the oligotrophic oceans with a great success”.

Page 12:

Lines 17-18 – “around the main thermocline between 100 and 500 m depth”. Can You provide a figure for the vertical profiles of temperature?

Page 13:

Line 7 – project, Olsen et al. (2016)). Comment: add comma after “project”.

Page 14:

Line 6 – nutriclines **depths**

Lines 8-9 – Is excess DIP ideal for growth of diazotrophs if the light intensity is not suitable? Maybe “Excess DIP in N-limited surface ocean, if supported by sufficient photosynthetically active radiation, would favor the growth of diazotrophs in comparison to non-diazotrophs”?

Page 24:

Figure 4c – Why do You use “Trichos” if You defined the abbreviation in the Methods as TRI?

References (*already cited)

*Agawin NSR, Rabouille S, Veldhuis MJW, Servatius L, Harriët M, Van Overzee MJ, Huisman J (2007) Competition and facilitation between unicellular nitrogen-fixing cyanobacteria and non-nitrogen fixing phytoplankton species. *Limnology and Oceanography* 52(5): 2233–2248.

Chan et al. (2012) Transcriptional changes underlying elemental stoichiometry shifts in a marine heterotrophic bacterium. *Frontiers in microbiology* doi: 10.3389/fmicb.2012.00159

Dyhrman TS, Chappell PD, Haley ST, Moffett JW, Orchard ED, Waterbury JB, Webb EA (2006) Phosphonate utilization by the globally important marine diazotroph *Trichodesmium*. *Nature* 439(7072): 68–71.

Fagerbakke KM, Heldal M, Norland S (1996) Content of carbon, nitrogen, oxygen, sulphur and phosphorus in native aquatic and cultured bacteria. *Aquatic Microbial Ecology* (10): 15–27.

Goldman JC, Dennett MR (2000) Growth of marine bacteria in batch and continuous culture under carbon and nitrogen limitation. *Limnology and Oceanography* 45(4): 789-800.

*Rabouille S, Staal M, Stal LJ, Soetaert K (2006) Modeling the dynamic regulation of nitrogen fixation in the cyanobacterium *Trichodesmium* sp. *Applied and Environmental Microbiology* 72(5): 3217–3227.