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Biogeosciences Discussions

Interactive comment on "Diurnal regulation of photosynthetic light absorption, electron transport and carbon fixation in two contrasting oceanic environments" *by* Nina Schuback and Phillipe D. Tortell

Anonymous Referee #1

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general commentsïijŽ The manuscript by Schuback and Tortell examined the variability of several parameters including phytoplankton absorption, FRRF-ETR and primary productivity over 48 hours in the coastal subarctic NE pacific, which I believe should be a very hard work. Moreover, they also compared results of this study with their previous one from an iron limited area, to give the idea that the potential effects of iron limitation on photosynthesis. They showed the first time that NPQ is a good factor for estimating both Φe,C and ΦC, which could contribute to FRRF, numerical models and remote sensing based primary production estimates. It seems that the authors' data

set, results and supplement files are very well prepared and is comprehensive enough to address these aspects of light absorption, electron transport and carbon fixation diurnal regulation. So I do not notice any major concerns with the manuscript, but have some concerns and question want to ask/suggest.

specific commentsïijŽ Page 1, line 4: what is NE Pacific? Page 1 line 19-20: the meaning of this sentence is not very clear for me, so author is saying, comparing to the coastal waters, although under iron-limitation there was a significant reduction of iron-rich photosynthetic units per chlorophyll a (this I can understand), the electron transport per photosystem II is still higher. (Is it right?) If so what cause this higher ETR? Page 1, line 20: put PSII after photosystem II ?, because PSII will soon show up in the caption of Fig.1 Page 2, line 11: I think Fig.1 is a very nice schematic plot, just two suggestions 1) Make difference for a*phy and a*ppc, a*psp, now it seems these three parameters are equal ., 2) I think not very necessary to put 14C here, just C-uptake is OK. Page 2, line 12: maybe add some references here? Page 5ïijŇline 25: not very clear why here a ÌĚ*psp is weighted to FRRF excitation LED, not in situ light? Page 5ïijŇline 26: using assumption that ratio of PSII: PSI =1 whether will affect the accuracy of nPSII calcuation? especially for those samples under iron limitation, which should have decreased PSI abundance. Can authors provide the general range of PSII:PSI for samples with/without iron stress? Page 6ïijŇ line16ïijŽI feel eq.2 is very hard to follow, here are some questions 1) where is the Eis(λ) in Eq.(2) ? 2) Maybe I missed somewhere but I cannot find where you mention that Eis(λ) (i.e. Eis at each wavelength) was measured? or you measured E0+(λ) ?, then using it to estimate Eis(λ). Sorry, just cannot find the related information. 3) not very clear why absolute values of light intensity for 14C P-E curve need to be corrected? And how can you correct light? I think you can only correct 14C-uptake rates, because C uptake rate measured under indoor LED light may differ with that under in situ natural light Page 6ïijŇ line24: a little confused that why Φc-max = α*-14C / ÄĄ*phy? is not =Pmax-14C / ÄĄ*phy ? Page 6, line20-25: I would suggest authors adding equations for how to calculate Φe,C, and ΦC here. For me it is not very easy to get Φe,C because the unit

of ETR per second, but C-uptake is per hour. And it is same to ΦC. I think it will help to understand the meaning of Φe,C ΦC if authors can provide equations and parameters with unit here Page 7, line8-9: I think the datasets Graff (2015) used for developing their bbp-Cphyto algorism mostly came from Open Ocean, where the phytoplankton is the main particle; however, when it is not the case (usually refer to Case II water), I think the algorism may not be suitable here, unless in this study area the backscatter signal mainly come from phytoplankton. And also, the author didn't provide the description of how they correct the backscatter data, so I would suggest authors to remove the phytoplankton carbon part. Page 7, line 25, table 2: previously I thought NPQ should be highly correlated with surface PAR, but actually from the results in table 2 we can found oblivious "decoupling" exists within these two parameters. For example the second 24 hours 20:00, when the PAR is only 24, the NPQ value is actually higher than the NPQ at first 24 hours 12:00, when the PAR is 1054, do authors know the reason? Page 11, line 8: can you explain the reason of what may potentially cause mid-day ETR at OSP14 exceeds the maximum theoretical value Page 11, line 8: as authors mentioned, the weak part of this MS is figure 7c, which is not very easy for primary productivity people to understand. It is telling that at OSP14, even water dominated by smaller phytoplankton and has nutrient limitation; it still has higher PB, which I think against most of the primary production research. Although it might be explained by NPP/GPP reason, I suggest in the future study authors should try to give or adjust the primary productivity rate to same level, for example, also measure respiration rate at same time. Page 11, line 26:-27 adding some references here?

technical correctionsïijŽ Page 2 line 25: "we examined diurnal variability. . .. . ." Page 3, line 29, I cannot find Burt et al. (2018) in references Page 7, line3 and Page 11, line 4ïijŽtyping errors, "NQP" should be "NPQ" Page 11, line 11 and 14, should be Fig 7d Page 11, line 15, Fig 7e is missing here Page 23, Figure 2: typing errors, "OSM14"in figure should be "OSP14" Page 24, Table 1: method column, fourth item, ". . .. . .weighted to spectral distribution of in situ light" Page 32 Figure 7: missing x-axis label Page 28, 32, Figure 4 and 7. The unit for 14C-uptake should be per hour, not per

second

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