

Interactive comment on “Vertically migrating phytoplankton drive seasonal formation of subsurface negative preformed nitrate anomalies in the subtropical North Pacific and North Atlantic” by Robert T. Letscher and Tracy A. Villareal

Anonymous Referee #1

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Authors used pre NO₃⁻ concept to highlight the importance of phytoplankton vertical migration in nutrient supply and DIC drawdown in the surface ocean primarily focusing on the HOTS and BATS data. Preformed NO₃⁻ anomalies are smaller than true concentrations of preformed nutrients, which actually result from transports by mixing and circulation. However, the authors seem to have downplayed contributions of mixing in rNPN and rPPN as they did not consider this physical factor among causative factors in Table 2 (see Comment 9). Vertical migration of phytoplankton assumes significance

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in upward transport of nutrients through their intracellular accumulation in nutricline layers. However, I wonder whether the authors' present option of using preformed nutrients and their anomalies to highlight the significance of vertical migration is justifiable as presented. This approach forced them make several assumptions (see Comment 16) and where calculations were made the uncertainties were not shown/assessed making the exercise not convincing. It may be possible to evaluate the quantitative significance of phytoplankton vertical migration to upward supply of nutrients and disproportional DIC drawdown from nutrient poor surface ocean by measuring intracellular upward nutrient transport and its possible leakage during vertical migration.

The authors are well aware of limitations associated with their assumptions (e.g. lines 453-465; 533-535). The central theme on showing the importance of phytoplankton vertical migration is appreciable several assumptions made with unknown uncertainties leave the reader wondering 'how much to be convinced'.

Specific Comments:

1. Three different sets of data are used in this study (Time-series data from ALOHA and BATS - for specific locations; WOA 2013 - for basin scales in North Pacific and North Atlantic, and also for world ocean; and GLODAP v2 – for preformed nitrates vs CFC ages of waters) – Comments on comparisons/compatibility among these data sets and their products are worthy of inclusion in the manuscript.
2. Line 138: for the benefit of the reader it is important to state details of 'total dissolved nitrogen (TDN)' here though available elsewhere. Whether TDN includes PON component or not is unclear from the details provided. Water samples whether filtered for TDN analyses or not assumes significance here!
3. Line 183: "DON (measured)" is actually not measured but derived (obtained by difference between TDN and nitrate+nitrite, see lines 140-141) (see Comment 2).
4. Lines 186-191: a. Assumption of rDOM constancy over time in each density layer

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may compromise the examination of its temporal variability in the regions. More over near constancy of rDOM values shown in Table 1 for each region might oversimplify vertical variability. If such use is necessary why not consider the top 250 m as just one layer? Please see Comment 16 (a) and (b) below that seem to simplify or play down on actual temporal and spatial variabilities. I am afraid such assumptions may undermine understanding of natural variability and compromise the significance of this study. b. Now that authors evaluated rDOM we know the values of rPOM (since DOM and POM remineralization should account for 100% of AOU, see also lines 190-191) specific for ALOHA and BATS. I believe this rPOM will be more realistic. Why use constants of 10.6 and 6.9 from literature? One can make a comparison with literature data but when one has an opportunity to use realistic values one should do so. In the present case the authors seem to prefer using literature values than their own results. Also I am not clear whether the values 10.6 and 6.9 are specific to PON or TON.

5. Lines 205 – 210: The periods of occurrence of seasonal rNPN and rPPN anomalies and their trends may be shown in x-y plots.

6. Lines 223-225: 'DOM remineralization....that utilizes the preNO₃- tracer' is an appreciable observation and suggestion made in this study.

7. Lines 226-230: It appears that 40 to 67% of estimated AOU is explainable by POM oxidation and thus is equally important as that of DOM. Again authors chose to use climatological averages of fDOM than the actual values observed (Comments 2 and 3 are relevant here).

8. Lines 242-245: Are increases in rPPN in the euphotic zone and rNPN in the sub-surface waters between May-June and Oct-Nov at ALOHA connected? It should be remembered that we have used near constant values of fDOM (50%), fPOM (50%), rDOM (18.1-18.9) and rPOM of 10.6 or 6.9 in the computations for the entire water column of 200 m (see Table 1). Then the results in Figure 1 are mainly reflective of changes and trends in TDN, Nitrate+Nitrite and Oxygen! I guess results in Figure 1a

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and b could be different if the authors used their evaluated values of fDOM, fPOM, rDOM and rPOM!!!

9. Line 369: “Having ruled out lateral mixing. . .” This seems to be a very tentative statement since lines 333-335 for BATS and 353-357 ALOHA clearly indicate lateral mixing influence is considerable in deeper layers. Ignoring mixing effects here is not justifiable.

10. Lines 402 and 427: A TEP gradient of 5-10 $\mu\text{g XG eq l}^{-1}$ was used to assess its contribution to rPPN and rNPN anomalies. I wonder whether such small gradient is sufficient to assessing its role in view of the semi-quantitative nature of TEP measurements and results. The authors should clearly discuss the uncertainties associated with TEP measurements and justify that the gradients used between surface and deep layers are significantly above the analytical errors. Other constraints associated with TEP are identified by the authors in lines 460-465.

11. Lines 413-414: “We assume TEP is comprised of pure carbohydrate with no N content. . .” – This is a simplified statement. The TEP has dominant polysaccharide composition but to assume that no other organic materials (nitrogen containing substances) are attached to TEP is not realistic.

12. Lines 499-502: “. . . it is clear that neither remineralization of N-poor DOM and TEP or heterotrophic bacterial nitrate uptake can quantitatively explain both the. . . .” – How justifiable is this statement given several assumptions involved. The authors have to quantify uncertainties to give confidence to readers at some level!

13. Lines 547-550: The logic in the estimation of the contribution of vertically migrating phytoplankton to the rNPN and rPPN ignoring the contribution of physical N transports is not justified (see Comment 9).

14. Lines 557-558 and 564-565 are confusing! When vertically migrating phytoplankton can help explain the observed summertime DIC drawdown in the absence of mea-

surable nitrate (557-558) why do they state ‘the mixed layer DIC drawdown need not be entirely supported by migrator photosynthesis, instead their nitrate leakage could help explain... (564-565)’. Is not nitrate leaked through excretion used and included in migrator supported photosynthesis?

15. Lines 591-592: “P-limited or P-stressed vertically migrating phytoplankton also take up phosphate at the nutricline...” is an important hypothesis.

16. SEVERAL ASSUMPTIONS: a. Lines 186-187: “our approach assumes rDOM is constant over time within each density horizon investigated at each station” b. Lines 202-205: “For the calculation of the preNO₃ tracer within the euphotic zone, we made the assumption that the values of fDOM and rDOM were equivalent to those empirically derived for the upper mesopelagic density layer present immediately below the euphotic zone at each site” c. Lines 413-414: “We assume TEP is comprised of pure carbohydrate with no N content...” – This is probably highly simplified. We know that TEP has dominant polysaccharide composition but to assume that no other organic materials (say proteins etc.) attached to TEP is not realistic. d. Lines 453-457 related to assumptions on (i) TEP is pure carbohydrate and (ii) TEP sinks rapidly and account for annual carbon export flux. e. Lines 533-535: “Nitrate transport calculations by vertical migration has a number of assumptions and caveats including considerable uncertainty in abundance estimates (Villareal et al., 2014)”

17. Figure 1 & 2: The captions need clarification. In (a) ‘residual pre NO₃- tracer’ and in (b) ‘monthly averaged pre NO₃- climatology’ have been shown as per the present caption. I wonder if (b) actually shows ‘monthly averaged residual pre NO₃- tracer climatology’!! If not I would expect different values of higher magnitude of pre NO₃- in Figure (b) (according to Formula (1, line 178).

18. Figure 3: “the residual pre NO₃- [μ M] tracer (the amount remaining after accounting for DOM contributions to AOU, see lines 233-234)” its maximal value of zero in the world oceans (as shown in this figure) imply that hardly any PON oxidation is ac-

countable for oxygen consumption at 150 m. This sounds unrealistic! (see Comment 7)

19. Figure S1 & S2: 'Residual pre NO₃⁻ is calculated using the values of fDOM and rDOM determined from the BATS station in Table 1 with a value of rPOM = 10.6' – Using fixed BATS values for the entire North Atlantic is not logical as it ignores spatial variability in these values. Figure S1 actually mimics that variability produced by changes in O₂ and Nitrate+Nitrite listed in WOA2013!

20. Figures S3 & S4: See Comment 19 but for North Pacific Ocean.

21. Figure S5 & S6: Why use Redfield ratio of 16 for N:P here? As the DON gets remineralized the associated phosphate is released in dissolved form. Then why not determine $\frac{\Delta \text{DON}}{\Delta \text{DPO}_4}$ with time-series data and use that ratio?

22. Table 2: (a) Lines 133-134 in text: 'Phytoplankton vertical migration. . . .at both stations ALOHA and BATS' is not convincing by the information provided in Table 2, (b) It is not clear whether the FOUR sources listed account for 100% of NPN or PPN features listed, (c) what are the total NPN and PPN values computed? What about contributions from lateral and vertical/diapycnal mixing, however small they are? (d) Consider all factors/sources and show they account for 100% of NPN or PPN evaluated, and (e) vertical migration appears more significant at BATS than at ALOHA? This will not be clear unless one shows the contributions of various sources in terms of percentage totaling to 100.

Authors are well versed with the topic and aware of limitations of this study. Therefore, they should justify their logical statements through evaluations of relevant uncertainties wherever possible. Probably a better approach to convince the readers on the significance of nutrient export by phytoplankton vertical migration is by conducting experiments to quantify intracellular accumulation of N and P and the extent of nutrient leakage through excretion by vertically migrating phytoplankton.

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