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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 #3

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So called negative preformed nitrate anomalies (rNPN) have been observed in subsurface waters of the North Pacific in earlier studies. This manuscript presents evidence for a more widespread existence of these non-Redfield nitrate distributions. The authors quantify formation rates of rNPN (and a related surface phenomenon ‘residual positive preformed nitrate, rPPN) from data of the time series stations at BATS and HOT. They particularly explore various mechanisms that contribute to the formation of non-Redfield distributions, namely (1) non-Redfield DOM production and degrada-

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tion, (2) TEP formation and degradation, (3) nitrate utilization related to TEP degradation, and finally (4) subsurface uptake and surface (euphotic zone) utilization of nitrate by vertical migrating (micro) phytoplankton. They argue that vertical migrators may be key to understand the generation of rNPN and rPPN at the two time series stations, as well as the well described ‘nitrate-free’ fixation of DIC (carbon overconsumption) in subtropical surface waters, directly by (a) providing a mechanism (4) which is supposed to contribute a large share to the anomaly generation and, potentially, (b) via TEP production by Rhizosolenia that might link together processes (2)-(4).

I have been reading this paper with great interest.

The quantification of mechanisms (1), (2), and (3) is generally sound and based on a consistent dataset, the data from the two time series and related studies at the time series sites. Mechanisms (2) – (3), however, leave a large fraction of rNPN and rPPN unexplained. It is therefore that the authors turn to discuss evidence for vertical migrating phytoplankton from the open ocean (page 23-24), providing convincing evidence that vertical phytoplankton migration exists in the open ocean. However, on page 25 (lines 551f) the go on to say that:

“We estimate the contribution of vertically migrating phytoplankton to the rNPN and rPPN anomaly features at the two stations by subtracting our estimates of the contributions of TEP cycling and bacterial nitrate uptake to rNPN and rPPN formation from the total observed rNPN/rPPN anomaly formation rates”.

That is, the contribution of rNPN and rPPN anomaly formation which is said to be due to vertically migrating phytoplankton are computed by difference of other numbers only – hence there seems to be no direct evidence of the role of migrating phytoplankton to contribute to the subject of this paper (rNPN and rPPN at BAST and HOT, respectively). While being very firm in the title of the paper (“Vertical migrating phytoplankton drive seasonal formation . . .”, the only evidence seems to be similarity of numbers and the possibility that a process observed and quantified elsewhere (eventually under different

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hydrographic and biogeochemical conditions?) may exist and dominate also at BATS and HOT. Actually, the two pages (23-24) that summarises very nicely the work of Villareal and others on vertically migrating phytoplankton appear to avoid to mention the locations and conditions of observations related to migrating phytoplankton explicitly.

I am fine with papers to have a more speculative section. However, this needs to be clearly identified as being speculative, otherwise we leave the scientific ground that facts and clear evidence are the basis of our work.

Given this, I suggest **major revision** of the paper – not that it will be a lot of work for the authors, but the major revision will be related to the core message of the paper. This should be that the authors can not, given the data at hand explain the pattern of rNPN, rPPN at BATS and HOT. They may in addition speculate that migrating phytoplankton may explain the unexplained part of the observed pattern though no clear evidence currently exists.

Specific comments:

Terminology: In the intro you introduce your terms ‘residual negative preNO₃’ (rNPN) and rPPN, respectively. I found the usage of the term residual ‘somewhat’ misleading, in particular since the description in M+M is somewhat unrelated to the usage of this term in the intro and in the rest of the paper. Perhaps ‘apparent NPN’ (etc) is better, particular since rNPN and rPPN rely on a variety of assumptions (fDOM etc.). However, I was particularly surprise not to find these terms in M+M! Instead there you use ‘traditional preNO₃’ (l 171) (of which data are never presented, I think) and ‘updated calculation’ (l 171), initially I wondered whether rNPN is something additional and did not find it well described in M+M. A more clear M+M is required.

Abstract: Given the speculative nature of the quantitative role of migrating phytoplankton to the features discussed in this paper, I suggest to delete the last sentence of the Abstract.

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Introduction: generally very well done.

- p 5, l 95 please give a reference for the O₂:N ratio; generally, I wonder whether such a low ratio is consistent with the observations of variable elemental ratios in POM (e.g. the work of Martiny et al., but also older work, e.g. Anderson and Sarmiento, 1994, give higher values for –O₂:N.)

- p5, 106ff: the quote to the Johnson et al 2010 work: do they really suggest what you cite them for? They rather observe a significant amount of Chla below the euphotic zone and argue that these phytoplankton may take advantage of deep nutrient pulses. Thereafter they cite actually earlier work of Villareal in support of upward transport of nitrate by migrating mats. In your intro it sounds as if Johnson provides independent evidence of the migration, which is not the case, I think.

- it might be helpful to mention the two papers of Fraga, which you discuss later, already in the intro

M+M, this section is partly confusing, and would benefit from careful reworking

- p 8, l 171: 'calculation of the traditional preNO₃ tracer', but this is never being used in the results, rephrase please.

-p7/8, l 173-192: shouldn't this procedure allow to provide error bars for fDOM and rDOM? how large are these? so far, if I understand correctly, the uncertainties in the Tabs mainly derive from literature assumptions about rPOM (10.6 vs. 6.9); how large is the range of rDOM values diagnosed from the data; I see that some of these numbers are provided on p 10 at the beginning of the results section

- p9/10, l193-211: I found this part very confusing

- I missed a data availability statement. The time series raw data are available from well known site, but what about the computed data from this study??

- p 10, l 212: I suggest to add a suppl table with links to the original data used in this

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Results:

- p 10, l 224-226: this is again confusing; do you present results based on the 'traditional preNO₃' formulation in this paper?
- p 11, l 242: I am not sure I really understand how the 'averaged climatology of residual preNO₃' is computed; this goes back to the confusion of the M+M section and should be solved there
- p 11, l 251: from the text it seems that you sum up the /m³ data of two isopycnals, which can't be right, I think, please clarify
- p 12, l 274: really Fig. 1b, not 2b?
- p 13m 276ff: isn't the similarity of values of rPPN and rNPN an artefact of the way you select rDOM and fDOM for the upper layer (just by adopting it from the lower layer)?
- p 13, l 288/9: You compare /m³ rates (rPPN, rNPN) of different layers and argue that they are balanced. But does that make sense for /m³ values of layers of likely varying thickness?

Discussion

I suggest to structure the discussion by subheadings

- p15, l 330: 'Advection mixing' you mean 'lateral mixing', right?
- p15, l 333: reference for CFC data is missing; which kind of age is computed and used here, please provide details
- p16, l 354: plots of preNO₃ vs. . . ., which preNO₃ is given here, the traditional, the improved, the residual?
- p 18, l 403-404 (also p19, l 428): the units of the gradient are strange, shouldn't it be ug XGeq /L /m

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- p 18, 404-413: which physical process mechanism is assumed here, please specify; In general I miss details that feed into the computation in this part

- p 19, 435: the C:-O₂ stoichiometry of TEP should be introduced earlier, p 18 or so; eventually in M+M

- p 20, l 454-459: this should be given earlier, before you do the computation, e.g. p 18 or so, or eventually in M+M*

- p 21, l 467 ff, bacterial N-uptake The underlying assumption is that bacterial biomass is to increase continuously, but aren't the bacteria grazed themselves and the respective N remineralised? I doubt that this process can support NPN anomalies. In particular, the simplistic calculation provided is not convincing and would f.e. require evidence of continuously increasing biomass of bacteria over the growth season that matches the time integrated TEP degradation rates.

- p 23-24: This is a nice summary of evidence for migrating phytoplankton 'somewhere' in the ocean. However, I miss a clear regional and at the same time quantitative link to the topic of this paper: rNPN, rPPN anomalies at BATS and HOT. Statements like "buoyancy reversals, high internal nitrate pools and rapid ascent have been found in multiple taxa from the Atlantic and Pacific Oceans" (l 530f) or "the generalised rates are consistent with the required rNPN and rPPN rates at BATS and ALOHA" (l 544ff) is much too general.

-p 25: l551ff: The authors estimate the contribution of vertically migrating phytoplankton to rNPN and rPPN anomaly features ... by difference, i.e. as the so far (mechanisms 1, 2, 3) unexplained. This is not sufficient to support a text entitled: "Vertically migrating phytoplankton drive seasonal formation of subsurface negative preformed nitrate anomalies ..."

-p 25, l 561ff (summertime DIC drawdown): again, this is highly speculative since you do not provide sound numbers for the role of vertically migrating phytoplankton for the

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two time series stations, based on data from the sites

-p 26, l 575ff: Preformed PO₄ is a very important aspect of the paper; I suggest to present the respective data already in the results section and with the same rigor as the rNPN etc data. You can discuss / provide the interpretation here in the discussion, of course. Questions arise: You assume that fDOM and rDOM are the same for DOP and DON, due to lack of data, as you point out. However, this makes the analysis a very weak one, I think. DOP and DON differ in their composition, shouldn't they also differ in their contribution to AOU, accordingly?

-p 27, 596ff: is there direct evidence for phosphate transport via migrating phytoplankton (at least from other sites); in which form is PO₄ stored in the cells?

-p27, l 600: I think your conclusions start here. Perhaps use a respective section title?

-p 28: l 622: "to confirm the conclusion" "confirm the hypothesis"

-p 28, l 622: "multiple authors": give at least some references

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