

Reviewer's comment

*... It is overly cumbersome and difficult to follow. I understand the topic of pre-formed NO<sub>3</sub> is not intuitive but creating new terms such as residual pre-NO<sub>3</sub> and negative and positive residual pre-NO<sub>3</sub> make the topic even more difficult to understand. As such the authors need to be extremely clear as to what each term represents and how each term is used. Perhaps some conceptual diagram is needed.*

Thank you for the comments. We recognize the challenges of following the terminology and concepts and have tried to clarify and streamline the text. We have restructured the introduction and added an appendix with definitions and clarifications. However, we feel the introduction of new terms is necessary to prevent confusion between the various historical methods of defining preformed NO<sub>3</sub>. It is fundamental to understanding what the nitrate:oxygen patterns are telling us about the system's behavior. Residual preNO<sub>3</sub> is a new formulation of the preNO<sub>3</sub> concept and we explicitly note this. The introduction of new terms, negative and positive residual preNO<sub>3</sub> only describes the sign of the signal. It is essential to define this pool clearly as processes not related to traditional concepts of Redfield recycling. Without doing this, the concepts become semantically so convoluted to keep clear that the discussion breaks down. The conceptual diagram sounds good, but is difficult to visualize how that would actually help. We feel that the addition of the appendix of terms should accomplish the goal of clarification.

*1) It is unclear what the motivation of the study is. I thought I understood that the seasonal drawdown of DIC in the absence nutrients would be the motivation, as the intro started with this. However, this was a small part of the paper in the end. Instead the majority of the paper was about explaining negative and positive residual pre-NO<sub>3</sub> signals, but I am unsure why this is important. It is not explained clearly and should be.*

We spend several paragraphs explaining the context of the “missing” nutrient problem to explain DIC drawdown and have clarified their link, particularly in the abstract. The rewritten Introduction is more streamlined and we believe it puts the linkage of preNO<sub>3</sub> and the DIC problem in better context. DIC drawdown requires nutrient inputs, and that makes the nutrient question an overarching problem. The source of these nutrients is unclear and as was stated in the topic sentence of the second paragraph, with the balance of the paragraph providing context for the nutrient sources. The anomalies in the preNO<sub>3</sub> signal provides a means of examining mechanisms that provide nutrient inputs. That is what this paper is about. There are unresolved questions about this feature (specifically the role of non-Redfieldian DOM cycling) that need to be examined (which we do) to clarify if there is indeed a nitrate deficit that is being exported from the nutricline to the surface to support DIC drawdown as suggested by Johnson et al and others. By accounting for variations in the observed nitrate:oxygen distributions, we can use this information to parse the signal into different pools. We have added text to clarify this. Our formulation of preNO<sub>3</sub> introduced in this study shows that the nitrate removed from the nutricline without oxygen increases, approximately balances oxygen production (DIC

drawdown) that has no observable nitrate drawdown. This is where the terms negative and positive preNO<sub>3</sub> become useful. They identify zones of nitrate uptake without O<sub>2</sub> production (negative) and zones of oxygen production without nitrate uptake (positive). Once we examine the suggested mechanisms, we find support for nitrate removed from the nutricline being transported to support the oxygen production 10s meters above it. That is the DIC drawdown. We are not sure how to make this much clearer. It has its own section in the discussion “Contributions to carbon and phosphorus budgets.”

*2) I am also having difficulty in understanding the term residual pre-NO<sub>3</sub>. To me this is a new calculation of pre-NO<sub>3</sub> that specifically accounts for remineralization DOM and POM, as well as non-Redfield stoichiometry. So this is the NO<sub>3</sub> present in the system not accounted for by non-Redfield remineralization of organic matter, whereas standard pre-NO<sub>3</sub> assumes Redfield stoichiometry and does not decompose remineralization into DOM and POM. I do not understand where the term residual comes in. This is just pre-formed NO<sub>3</sub> calculated in a new way isn't it? Residual pre-NO<sub>3</sub> implies there is some component of pre-formed NO<sub>3</sub> that is left over, yet the new calculation is defining all the preformed NO<sub>3</sub> as residual. What am I missing?*

This is a good description so we think the problem seems to be with the terms we introduced. If we retain the original term “preformed nitrate” it creates significant confusion in the literature where the same term will now have 3 definitions. Residual preNO<sub>3</sub> was a term we introduced to identify those components resulting from non-Redfield DOM and POM cycling. “Residual” is one of many words we could have used to describe that the resulting preNO<sub>3</sub> value was the result of non-Redfieldian processes. It is the “residual” value of preNO<sub>3</sub> remaining after accounting for non-Redfield DOM and POM cycling. It can be used to quantify processes other than DOM/POM recycling. It was introduced to minimize the semantic gymnastics required to differentiate the recycling component (POM and DOM) of preformed nitrate from the active biological processes not previously considered as factors in the oxygen:nitrate relationships. The addition of the definitions table in the appendix should help clarify this.

*3. Another major concern I have with this paper is that the contribution of vertically migrating phytoplankton to the positive and negative residual pre-NO<sub>3</sub> signals is determined via difference. Essentially, the residual pre-NO<sub>3</sub> is calculated, the contribution of both TEP and bacteria is accounted for and then the remainder is stated to be from vertically migrating phytoplankton. I am not sure why a calculation has not been made that shows phytoplankton can account for these residual pre-NO<sub>3</sub> signals. Some type of mass balance estimate is required that shows phytoplankton can move this much NO<sub>3</sub> between the upper mesopelagic and the euphotic zone.*

In the introduction, we identified the 3 major processes we wished to consider. By allocating the maximum possible rates to TEP and bacterial nitrate utilization, we minimize the potential contribution by vertical migration. The difference calculation provided the magnitude of the flux required by vertically migrating phytoplankton. The actual field data showing the ranges noted in the open sea is presented starting on line 663 in the text and is explicitly compared to the difference calculation on lines 714-725. Unlike the TEP calculations or bacterial nitrate utilization, the vertical migration estimates are based on extensive field measurements over

multiple years and capture the inherent variability in the system. The rates seen in the open sea (including an explicit calculation for the waters north of ALOHA) are certainly equal to those calculated by difference from the other pools. However, these estimates were not made at ALOHA or BATS (nor was the TEP profile for either station), so are generalized to the broad areas where these species occur (the point of including the distribution diagrams). The vertical migration data is far more constrained by direct examination than either the TEP or bacterial nitrate uptake measurements. Both the TEP and bacterial utilization estimates are based on single profiles.

*4) Shouldn't recycled production be accounted for in the positive residual pre-NO<sub>3</sub> (rPPN) calculation? The rPPN is O<sub>2</sub> production without the expected (non-Redfield) drawdown of NO<sub>3</sub>. In the euphotic zone of both environments analyzed here there is little NO<sub>3</sub> and productivity is primarily recycled (or NH<sub>4</sub> or organic N based). Thus, O<sub>2</sub> can be produced without any drawdown of NO<sub>3</sub>, shouldn't this be included somehow?*

Both ammonium and organic N are recycled, and by definition, consume oxygen during remineralization equivalent to that produced by photosynthesis when used as a nutrient source. We have included a reference by Ascani et al (2013) that points this out. Our analysis of BATS/ALOHA monthly data is of the net  $\Delta\text{O}_2$  and  $\Delta\text{NO}_3$ . Recycled production should have little effect on  $\Delta\text{O}_2$  and  $\Delta\text{NO}_3$  because recycled production is rapidly consumed and produced due to autotrophic and heterotrophic metabolisms. What we are quantifying from the BATS/ALOHA data is the net effect of import or export of O<sub>2</sub> or NO<sub>3</sub> from each layer (i.e. new/export production). This is fundamental ocean biogeochemistry identified over 30 years ago (Goldman 1985 and cited in the text).

*5) Can significant N<sub>2</sub> fixation in these regions affect the residual pre-NO<sub>3</sub> signals? Wouldn't this be similar to cells introducing N to the euphotic zone through vertical migration? N<sub>2</sub> fixation has been used to explain DIC drawdown in oligotrophic regions as well, how does it compare to vertical migration.*

N<sub>2</sub> fixation rates are too low in the nutricline to affect the oxygen:nitrate relationships. It also has high phosphorus requirements whereas the preformed phosphate signature is at or below the base of the euphotic zone (Fig. 4). To account for DIC drawdown near the surface where the DIC problem occurs, there has to be an allochthonous source of P, much like if it is nitrate based, plus elevated Fe to support N<sub>2</sub> fixation. Budgets including N<sub>2</sub> fixation both near station ALOHA (Johnson et al, 2010) and at BATS (Lipshultz et al, 2002) have been unable to support DIC drawdown using measured N<sub>2</sub> fixation rates. The literature also notes that N<sub>2</sub> fixation is highest at the surface and declines rapidly at depths well above the observed residual positive preNO<sub>3</sub> signals.

*6. Lastly, I do not find the pre-PO<sub>4</sub> part of the paper adding much. Likewise, the DIC drawdown discussion is small and I find this to be potentially the most significant part of the paper. I suggest nixing both parts unless they are expanded and made more central to the paper's objectives (which I state above need clarification).*

The prePO<sub>4</sub> section was moved to the main text from the Supplementary section as a response to previous reviewer's comments. Clearly, some readers see the value of having it. The DIC

problem is the one that requires the determination of how nitrate is being transported into the upper water column. We specifically address the role that this nitrate transport plays in DIC drawdown on lines 757-780. As we note above, the DIC context explains the need to examine the preformed nitrate phenomenon. Johnson et al (2010) defined this problem. Our paper is building on the foundation they built.