Biogeosciences Discuss., 5, S369–S373, 2008
www.biogeosciences-discuss.net/5/S369/2008/
© Author(s) 2008. This work is distributed under
the Creative Commons Attribute 3.0 License.

Biogeosciences Discussions

Interactive comment on "Organic nutrients and excess nitrogen in the North Atlantic subtropical gyre" *by* A. Landolfi et al.

Anonymous Referee #1

Received and published: 17 April 2008

General Over the past decade, there has been a great deal of discussion surrounding estimates of nitrogen fixation based on accumulation of nitrate relative to phosphate, referred to as excess nitrate. Various proxies, N* and DINxs, and knowledge of the ventilation time scale of an isopycnal layer have been used to provides estimate of basin scale nitrogen fixation. Although previous studies have recognised the potential impact of atmospheric inputs and remineralisation of the large DON and DOP pools to create excess nitrogen signals, few studies have attempted to include these processes in their models. Landolfi et al. have used inorganic and organic nutrient data collected on one cruise in the North Atlantic (25.4N) and estimated rates of nitrogen fixation from two new proxies, TNxs and TONxs, which include the DON and DOP pools. I agree with the authors that inclusion of organic nutrients (or some fraction thereof) is required

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

to obtain an accurate estimate of excess nitrogen accumulation and nitrogen fixation in the North Atlantic. However, I do not agree with their approach in including the entire pools of DON and DOP, and also the use of some assumptions they have made in their interpretations. My concerns are listed below.

1. Equations defining TNxs and TONxs assume that the relation between TN:TP and TON:TOP is the same as nitrate:phosphate, i.e. with a ratio of 16:1. There is sufficient data in the literature to show that the ratio of DON:DOP is greater than 16:1. Therefore, assuming this ratio in proxies TNxs and TONxs is favouring accumulation of excess nitrogen. I have include specific page numbers below where I think the authors have made fundamental flaws in their approach to estimating basin scale nitrogen fixation.

2. DON and DOP are complex pools of mostly uncharacterized organic matter. The lability of these pools is probably very different, with a larger proportion of DOP being turned over on fairly short timescales in comparison to the more complex and refractory DON pool. I am not sure we know enough information about the cycling of DON and DOP to insert such large pools into such simple proxies which assume that the pools are quite similar in their reactivity and to the inorganic pools.

3. The premise for the use of TN* is that there is a significant contribution of DON from nitrogen fixation to the TN pool. However, a recent study (Meador et al., 2007) using molecular and stable isotope analysis of DOM found that only a very small fraction of DOM was potentially from nitrogen fixation and indeed this pool was recycled rapidly. The authors have ignored this study (at least in the intro, it is mentioned in the discussion but I believe in the wrong context), assuming that (a) the DON pool represents one pool rather than continuum of various labilities and (b) a large proportion is from nitrogen fixation. These assumptions are flawed.

It would be useful for the authors to summarize their inorganic and organic nutrient data in a table, e.g. split into biogeochemical regions from E-W to show the range in concentrations, especially of DON and DOP, e.g. in the surface, and also the ratios of

BGD
5, S369–S373, 2008

CC BY

DON:DOP and TN:TP etc. The figures are not clear in their present format.

Specific

Pg687, line 11 – 13. Sentences contradict each other, i.e. “recent direct estimates are consistent with geochemical ones:, then “it is unclear whether this discrepancy has been fully resolved”. What discrepancy? The sentence before states direct and geochemical estimates are consistent.

Pg 688: Line 23: Is it appropriate to use an N:P ratio of 16:1 for parameters (TONxs and TNxs) which DON and DOP when the DON:DOP ratio is 16:1. Isn’t this creating an excess DON pool which can be channeled into the excess nitrogen pool, thus elevating estimates of nitrogen fixation?

Pg 688: The authors report that nitrogen fixation adds to the DON pool. However, what about the DOP pool? There is increasing evidence to suggest that the DOP pool is an important source of P to nitrogen fixers. Why is this not mentioned here? Although this would not affect the TN:TP ratio as there is pool of P is fixed unlike N but it will affect the interpretation of TON:TOP. For example, there has been a significant decrease in the DOP pool at station ALOHA in the North Pacific gyre due to increased nitrogen fixation. Could there be a similar mechanism in the North Atlantic?

Pg 688, 689 and 692 (lines 4-5): Over these 4 pages, the authors make a couple of contradictory statements, which need to be clarified.

Pg 688 Why is TONxs and TNxs independent from differential nutrient remineralization/utilization. It is unclear what assumption has been made to make these proxies independent?

Pg 689: “TNxs is insensitive to differential remineralization and accumulation of dissolved refractory material.” But if you assume a ratio of 16:1 for TN and the DON:DOP ratio is $>$ than 16:1, there is potential to include the refractory DON pool that is in excess of DOP at a ratio of 16:1. The authors need to clarify.

BGD

5, S369–S373, 2008

Pg 692: line 4-5. On page 689, authors state the TNxs is insensitive to accumulation of refractory material, but then state that TON is in excess of TOP with respect to Redfield due to accumulation of refractory material. If the authors use a N:P ratio of 16 for TNxs, then surely this will allow there to be an accumulated TON pool in the TNxs parameter? This is a fundamental flaw in this paper.

Pg 691: line 14 and below: Are the gradients in TON from east to west statistically significant, including errors of analysis?

Page 693: the list of “potential parameters creating DINxs or TONxs” signals would be better if it was refined into a discussion rather than listed. The authors also need to explain which of these processes actually occur in the North Atlantic (e.g. denitrification is not a significant process here) rather than just list all potential processes.

Pg 695: the vertical segregation of inorganic (highest conc. in deep) and organic nutrients (highest conc in surface, i.e. source) surely needs some discussion when calculation gradients in these parameters along the same isopycnals but across large regions of the ocean?

Page 698: line 9-10 – are the authors referring to particulate or dissolved organic matter here? Please clarify. Although there is some evidence in the literature about elevated particulate N:P ratios for nitrogen fixation, there are no studies to date that have been able to directly measure the DON:DOP ratio exuded by nitrogen fixers. In fact, to my knowledge, no study has looked at DOP exudation from nitrogen fixers.

Pg 702: I am glad the authors have acknowledged the fact that there have been two direct studies showing the nitrogen exuded by the process of nitrogen fixation does not accumulate in DON pool. However, the authors are overstating the small, rapidly recycled pool of HMW DOM identified by Meador et al. as being a vehicle for TNxs in the North Atlantic. The pool is extremely small compared to the total DON pool. I think the authors have overstated its importance with respect to creating TNxs signals.

BGD

5, S369–S373, 2008

The authors have focused mostly on DON in this manuscript. What about DOP and its dynamics in creating TNxs signals. Perhaps this requires discussion re: the different role of phosphate and DOP with respect to nitrogen fixation and also TNxs and TONxs.

Figures and Tables Figures 2: The numbers are unclear and overlap. Figure 3: Why is there a TOP maxima below 250m? Is this real or analytical? Figure 7: This figure is confusing. It is not described in the figure legend and is not explained in the text. There is no need to have this figure as well as the shopping list of processes that may create excess nitrogen in the text (see comment above). Figure 11. The “sigma theta” text covers some of the data. Please adjust so readers can see data.

1) Does the paper address relevant scientific questions within the scope of BG? Yes 2) Does the paper present novel concepts, ideas, tools, or data? Yes 3) Are substantial conclusions reached? No 4) Are the scientific methods and assumptions valid and clearly outlined? No 5) Are the results sufficient to support the interpretations and conclusions? No 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes 8) Does the title clearly reflect the contents of the paper? Yes 9) Does the abstract provide a concise and complete summary? yes 10) Is the overall presentation well structured and clear? No, some parts of the text need restructured – see specific comments above 11) Is the language fluent and precise? Yes 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? No 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes – see comments 14) Are the number and quality of references appropriate? Yes 15) Is the amount and quality of supplementary material appropriate? NA

Interactive comment on Biogeosciences Discuss., 5, 685, 2008.