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Interactive comment on "C : N : P stoichiometry at the Bermuda Atlantic Time-series Study station in the North Atlantic Ocean" *by* A. Singh et al.

Anonymous Referee #2

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Singh et al. use suspended particulate organic matter (POM) and total organic matter (TOM) from the upper 100m, as well as exported POM between 100-500m from the BATS database to investigate ecosystem elemental stoichiometry (C:N:P). They find the C:N ratios in the particulate pools approximate Redfield proportions but that ratios relative to P are much higher than Redfield (i.e. C:P and N:P in both the total and particulate pools). They link these higher than Redfield elemental ratios to plankton abundance, primarily the cyanobacteria Synechococcus and Prochlorococcus and to a lesser extent pico- and nanoplankton. They also suggest elemental ratios differ as a function of growth rates and that elemental stoichiometry is related to the Arctic Oscillation.

Overall I am supportive of this manuscript. It is a good set of data that lends strong support for a non-Redfieldian ocean. While I think this view is becoming widely accepted among oceanographers, showing it in the BATS database is nice in that this data set is used by so many for modeling that part of the ocean. Assuming Redfield proportions in an ecosystem or biogeochemical model based on BATS data is not really an option as shown by this paper. However, the manuscript is not yet ready for publication. I have several comments/questions for the authors that I believe need to be addressed prior to publication.

1. line 63-66, and again at lines 360-364, here the authors claim there is a lot of support for proximate P limitation of productivity in the waters at the BATS site. They then cite several papers of which I would argue none actually support P limitation of productivity. The Lomas et al. 2010 paper actually uses the term P stressed instead of limitation and argues growth of the phytoplankton is Redfieldian when DOP is taken into account. The other papers cited assume P limitation based on Redfield N:P or C:P stoichiometry (i.e. if ratios are greater than 16 or 106 respectively than PO43- is limiting). However, this cannot be the case if the primary producers themselves are not Redfieldian (i.e. if their ratios are naturally greater than Redfield proportions). The Bertilsson et al. and Heldal et al. papers show that even under nutrient replete conditions the cyanobacteria have N:P and C:P ratios higher than Redfield. If this is the case one cannot assume proximal P limitation based on higher than Redfield stoichiometry.

2. Related to the above is that the assumption of P limitation could then be assumed if the particulate ratios were greater than the nutrient replete ratios of the cyanobacteria- which in the BATS data they seem to be (though not by a lot). However, Singh et al. state that phytoplankton account for only 25% of the particulate matter. What is the other 75%? If only 25% of the particulate matter is phytoplankton than it is difficult from the presented data to know their elemental ratios and thus whether or not they are > or < the nutrient replete stoichiometry of the cells.

I would argue there is little direct evidence for P limitation of productivity in these waters and that elemental ratios, in this system where phytoplankton are only 25% of the particulate pool, can not be used to determine limitation status of the primary producers. There is a lot of evidence that shows adding N to the waters of the North Atlantic Subtropical Gyre stimulates primary productivity (see the Moore et al. 2013 review paper- which the authors cite). There is evidence also that shows adding PO43- to the same waters does not stimulate primary productivity. Additionally, the term PO43- limitation (end of paper) should not be used, instead use P limitation as at the start of paper.

3. line 51- add vary between ratios and with

4. 2nd to last sentence of abstract- sentences like this are vague. They do do not say much really and do not add to the manuscript. It is better to state what the climate variability – C:N:P relationship is and means. The authors should examine the manuscript throughout and clean up these types of vague sentences or get rid of them.

5. Line 154- change 2nd as to and

6. Line 190 end of first sentence- cite figure? Fig. 2? Make sure Figures and panels are cited throughout manuscript.

7. Line 205 -206, why not order your figures in the same order as they are presented in the results. So Fig. 5 and 6 would be switched so this sentence cites Fig. 4 & 5. It is easier for the reader to just jump to the next figure as they read than to have to jump ahead 2 figures and then back.

8. Line 206-215- Figure 6 is cited here but is not really presented or compared to figure 4. It makes sense to present them together and the differences or similarities between the pools at each depth range.

9. Line 214-215- seems like POP followed same trend, and TOP increased with mixing and remained high and variable until the next season.

10. Line 224- It would be good to actually compare variability- is the variability really that different? For some things yes– e.g. TOC:TON for others maybe not PON:POP.

Also Fig 5 c legend reads TOC:DON not TOC:TON

11. Line 235- do you really think biological uptake between 100-500 is responsible? What uptake is this- heterotrophic? More detail please.

12. The results end without presenting the flux data, instead it is at line 249 in the discussion. It should be in the results. Also the relationship to the AO is not presented in results- why is that?

13. Line 250 refers to POP flux but cites Fig 8A & B, 8A is PON flux.

14. Line 255- change also almost to more than

15. Line 257- delete however (it is not appropriate in this sentence).

16. Line 263-264- are these differences significant

17. Line 264-268- this again is not a very convincing sentence just a statement of importance that is speculative. I think you need to point out how the data is important. I am not sure how the data you have supports DOM sustaining phytoplankton growth. Something more detailed as to how this data supports this is requested.

18. Line 273- do the changes in POM account for the changes in TOM or do there have to be DOM changes?

19. Line 305- at the BATS site.

20. Line 306-307- why a mixture? N:P of Pro and Syn is same- could be a mix or could be either. Suggests cyano influence on PON:POP.

21. Line 313-314- this sentence refers to ratios, but the figure does not have ratios.

22. Line 320-321- fine hypothesis- but does it make sense? phytoplankton make up ∼25% of the POM (15% of that is SYN) plus some Pro and Picos. So less than 10% can be nanos- if they require low P would the changes you see in their abundances alter the TOP concentrations to the extent you see?

23. Line 331- did you do correlation analysis? If so shouldn't you report r not r2.

24. Lines 335-339- I am not sure how dilution of the inorganic pools affects the ratios of the organic pools? Some more detailed explanation is requested.

25. Line 360-364- I do not see how this paragraph fits in this section relating to microbial export. Seems out of place. Plus see comment 1 in reference to this paragraph.

Interactive comment on Biogeosciences Discuss., 12, 9275, 2015.