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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.

A. Singh et al.

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Reviewer#2 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. Reply: We thank the reviewer for comments and all the concerns, and their support for the value of the paper. We have addressed them below one by one.

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. Reply: We completely agree with the reviewer; assessing 'limitation' is very difficult. There are also other studies that suggest N is the proximal limiting nutrient in this part of the ocean. At some level it depends what your response variable is, e.g., growth rate vs. chla content, etc. We have now clearly stated throughout that the North Atlantic is potentially P stressed (ll 64-66, 368-370, 372-374).

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.

Reply: We have estimated that Prochlorococcus, Synechococcus and Picoeukaryote contribute up to 75% to the PON. Other phytoplankton (e.g., diatoms, dinoflagellates, Nanoeukaryotes), diazotrophs (e.g., Trichodesmium), bacteria and zooplankton (both micro and macro) might contribute to the other 25%. Trichodesmium, which is abundant during summer at the BATS, has an N:P ratio that varies from 42 to 125 (Karl et al., 1992). But we do not have elemental content of these other (25%) plankton so we cannot state this in the manuscript.

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, cannot 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. Reply: We agree with the reviewer, please see response to comment 1 above. We have now stated that the North Atlantic is P stressed (ll 64-66, 368-370, 372-374).

3. line 51- add vary between ratios and with Reply: added (line 52).

4. 2nd to last sentence of abstract- sentences like this are vague. They 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. Reply: We have revised such vague sentences throughout the manuscript (e.g., ll 53-54).

5. Line 154- change 2nd as to and Reply: Changed (line 154)

6. Line 190 end of first sentence- cite figure? Fig. 2? Make sure Figures and panels are cited throughout manuscript. Reply: Cited throughout the manuscript (line 190).

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. Reply: We have changed the order as suggested by the reviewer. Figs. 4 and 5 compare elemental concentrations at 0-25 m and 25-100 m depth range, while Figs. 6 and 7 compare elemental stoichiometry at 0-25 m and 25-100 m depth range.

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. Reply: We have discussed Fig. 6 (now Fig. 5) in detail now in connection with figure 4 (e.g., ll 215-217). Please see prior comment and response as well.

9. Line 214-215- seems like POP followed same trend, and TOP increased with mixing and remained high and variable until the next season. Reply: We agree. We have revised the sentences accordingly to make this observation more clear (ll 214-215).

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 Reply: Yes, variability in the two depth ranges were significantly different. We have made these clarifications and comments throughout where appropriate. We have corrected the figure caption.

11. Line 235- do you really think biological uptake between 100-500 is responsible? What uptake is this- heterotrophic? More detail please Reply: That was a mistake in interpretation on our part. We have deleted that section of the paper. However, depending upon how you define the euphotic zone it may extent to 150m in the Sargasso Sea. Indeed we can see living phytoplankton that deep or deeper and so while surely from say 200-500m is drive by heterotrophic activity, 100-200m remains part of the transition zone from net particle production to net particle consumption.

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? Reply: We have presented the P flux data in the results section now; this was also a comment of the first reviewer (ll 239-245). The relationship to the AO was not presented in the results simply because the results were the presentation of the stoichiometric data and the link to AO was a derived 'outcome' of the discussion when trying to discuss and interpret patterns. So we feel it is appropriate to leave mention of the AO in the discussion.

13. Line 250 refers to POP flux but cites Fig 8A & B, 8A is PON flux. Reply: We have corrected this (Line 244).

14. Line 255- change also almost to more than Reply: Corrected (Line 268)

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

16. Line 263-264- are these differences significant Reply: This sentence has been modified based on the comments from Reviewer 1 (ll 275-277).

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. Reply: We have revised the sentences and substantiated our claims more soundly (ll 277-282).

We hope the reviewer finds the new text satisfactory.

18. Line 273- do the changes in POM account for the changes in TOM or do there have to be DOM changes? Reply: POM contributes ~10% to TOM (comparing POM and TOM concentration in the Fig. 4) and it is unlikely to account for changes in TOM (looking at the seasonal changes upto 5 μmol kg-1 in the TOM, Fig. 4). So we believe that they have to be predominantly due to DOM changes.

19. Line 305- at the BATS site. Reply: Corrected (Line 319).

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. Reply: Yes, it could also be either theoretically. We have observed Prochlorococcus and Synechococcus both so we state it as mixture. That said we have clarified this sentence.

21. Line 313-314- this sentence refers to ratios, but the figure does not have ratios. Reply: We have moved the figure reference to next sentence, where it was more appropriate and refers to the correct figure (line 332).

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? Reply: As we have stated above, Prochlorococcus, Synechococcus and Picoeukaryote contribute up to 25% to the PON (contribution of all phytoplankton community to POM would be much higher). We do not know the contribution of Nanoeukaryotes to POM (which might be less than 10%) so we would like to keep our hypothesis as such. That said, we recognize the reviewers comment and have tried to expand it such that readers can better understand the context and our point of view on the hypothesis.

23. Line 331- did you do correlation analysis? If so shouldn't you report r not r2. Reply: Yes, we did correlation analysis, and thus now report the value as 'r'.

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. Reply: That was an incorrect formulation. We found that the mixing is too complex a process to explain ratios of the organic pools, hence we have deleted that part of the text.

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. Reply: We

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