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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.

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Reviewer#1 The authors have examined the elemental stoichiometry of total organic material (TOM) and particulate organic material (POM) in the upper 100 m of the water column, as well as the inorganic nutrient pools, over an eight year period at the BATS station. The aim of this study was to quantify C:N:P ratios in all these pools and their relationship to biogeochemical cycling, community structure and the canonical Redfield ratio. The also analyzed the annual and seasonal variability in these parameters. All data were obtained from the publically available BATS web archive. They found that the TOM C:N:P ratios exceeded those of the POM and they present linkages between the

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observed TOM and POM seasonal variability to that of phytoplankton cell abundance and taxonomic group, as well as potential climate drivers for the observed long-term variability in C:N:P stoichiometry. Overall this is a rather straightforward analysis of time-series data from BATS. The C:N:P work appear solid, but I have questions about how the data were used and how that may influence the interpretation of the results. In addition, I believe some restructuring of the manuscript would help to improve its readability. For example there are quite a bit of data that is presented in the discussion section that would fit better into the results section. Reply: We thank the reviewer for going through our manuscript thoroughly. We appreciate the comments and all the concerns. We have addressed them below one by one.

Detailed comments: P9276, ln 16. "C:N:P ratios in the TOM pool were more than twice that in the POM pool". I think this needs to be rephrased. The data in table 1 shows C:N and N:P being $\sim$2$\times$ higher in TOM compared to POM, whereas C:P is $\sim$4-5$\times$ higher in TOM than POM. I suggest breaking this out in its components to make this clearer. Reply: We have broken the sentence into two parts as suggested by the reviewer (ll 44-46), and stated 'at least' rather than 'more than' for C:N and N:P ratios.

P9280, ln 20. At what depth were the sediment traps deployed? (this appears later in the discussion, but should be mentioned in the Materials and Methods). Reply: Sediment traps were deployed at 200 m depth. We have now mentioned in the M&M section (ll 155-157).

P9281, ln 5. How were the 'depth mean ratios' calculated? Was an elemental ratio calculated for each depth and then average over the 7 depths from 5-100 m, or was an average concentration of each element calculated and then the ratio made? How do you weight average the data when the sample spacing is not even (i.e. spacing 5m, 5m, 10m, 20m, 20m, 20m and 20m)? Have you thought about integrating the TOM and POM inventories over your sampling depths instead? This may alter the results but may be more relevant for the comparison of the two depth ranges chosen (0-25 m and 25-100 m). Reply: We first calculated the average concentration of each

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element over the depth segment (e.g., 0-25 m) and then the ratios were calculated from those averages. We have specified this in the manuscript now (ll 171-173). Also, this approach does not require a 'weighting' function to be applied. 5, 10, 20, 40, 60, 80 and 100 m were the target depths but actual depths (sometimes) changed during CTD operation by a few meters (~2-3 m). So all the depth sampled above 25 m were put in 0-25 m, while below it were put in 25-100 m depth. Our concentrations have μmol kg-1 units and changing units to μmol L-1 might propagate uncertainly due to sometimes uncalibrated salinity sensor. Moreover, our analysis is mainly based on 0-25 m depth, where samples were almost equally spaced, and the MLD was hardly shallower than 25 m so concentration of different elements was quite homogeneous. Hence, we have decided not to use integrated values, but the reviewer's comments are duly noted.

Ln 20. Was this trend in TOP based on the depth averaged concentrations over 0-100 m? It is hard to see any 'trends' in the contour plot. My impression of the plot is that 2007 had unusually low TOP whereas during 2008 TOP appeared to be unusually high. Would you get a negative trend instead if using data from early 2008 to early 2009 that would also be significant? Reply: Yes, this trend in TOP was based on the depth averaged concentrations over 0-100 m. It is hard to see in the contour plot. We discovered it from our ratio analysis (Fig. 2) and analyzed TOP separately. TOP values were indeed low in the beginning of 2007 but increased gradually until January 2008. Early 2008 to early 2009 TOP data show negative trend over time but it is much less robust (r2 = 0.39, p-value: 0.03) compared to 2007-2008 trend (r2 = 0.77, p-value: <0.001).

P9282, ln 14. What determined the choice of depth division of the water column at 0-25 m and 25-100 m? Reply: We wanted to analyze annual variation in elemental ratios in different depth segments. Segments were based on MLD, which was normally not shallower than 25 m depth during the summer stratified period. Thus we took this as a 'surface' depth segment. We were also concerned that preferential degradation of TOP should not change annual variation in elemental ratios and hence we decided to

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separate into 0-25 depth segment.

Ln 16. How was the 0-25 m concentrations calculated when sampling depths were 20 and 40 m? Were the data interpolated between 20 and 40 m?

Reply: As stated above, 5, 10, 20, 40, 60, 80 and 100 m were the target depths but actual depths (sometimes) changed during CTD operation by a few meters (∼2-3 m). So all the depth samples above 25 m were put in 0-25 m, while below it were put in 25-100 m depth. Thus, the 20m sampling was always in the shallow segment and 40m always in the deeper segment. Because we didn't integrate the data, but rather averaged data above/below a depth cutoff, there was no need to interpolate the data.

Ln 23-25. Does Trichodesmium not contribute to POM? I do not really see a peak in TOC, but TON and PON peak in month 6. Is that what was meant? This 'peak" also is seen in the 25-100 m portion but that is not mentioned in the text. I would suggest switching the wording around..from " the occurrence of higher Trichodesmium colonies" to " the higher occurrence of Trichodesmium colonies..." or "the higher number of Trichodesmium colonies". Reply: Trichodesmium does contribute to POM but it would hard to see the changes in POM due to the fact that they are particularly patchy in distribution and not very abundant overall so it is actually rare that whole Trichodesmium colonies are captured on the filtered and then measured as POM. However, as they release N (as DON) simultaneously as they fix N2, we see more variation in DON (or TON for the present case) that PON because of the buildup of the former. TOC also peaks in the fifth month but remains saturated afterwards. We have mentioned the similar peak in 25-100 m portion and changed the wording as suggested (ll 214-216).

P9283, ln4-9. Much of this text is an iteration of the first paragraphs of the Results section. I would suggest moving the earlier text and incorporate that under section 3.2.2. instead. Also, see line 7-8 in discussion, which is very similar to what this paragraph is saying, but stated more clearly. Reply: In the first paragraph of results section, we have discussed the entire time series (Fig. 2). Under the section 3.2.2, we

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have discussed the patterns in terms of deep mixing. However, we agree that there was some repetition so we have shortened the text to improve clarity and readability (ll 173-180)

Ln 10. “Minimal variability in concentrations and ratios in the 25-100 m depth horizon..” How was that determined? I find Figs 4 and 6 remarkably similar in terms of the range in mean concentrations, seasonal patterns and variability (error bars) in the N and P pools. The N:P ratios also look quite similar in Fig 5 and 7. Only TOC and POC seem to differ somewhat in concentration range, variability and pattern between the two. I would suggest changing “25-100 m depth horizon” to “25-100 m depth range” Reply: Some of the trends that we have discussed were not as prominent in 25-100 m depth range as they were in 0-25 m depth range. We have discussed this in the manuscript now (e.g., ll 214-216). In addition, TOC and POC values were significantly lower in the 25-100 depth range compared to that in the 0-25 m depth range, as suggested by the reviewer. We have changed ‘horizon’ to ‘range’ (Line 226).

P 9284 – Discussion. The discussion currently contains quite a bit of new data that I believe should be better presented under the result section. E.g. the trap flux data, flow cytometry and chlorophyll. Reply: We have added new data into the results section (added two new sections - 2.3.4 and 2.3.5; ll 238-250). The reason for not including it in the first version is that much of that data was presented as a result in Lomas et al. 2013 (overview of BATS data), but in a different context. We agree that including it here as a result is also appropriate.

P9285, ln 2-4. “On the contrary, our data suggests that TON values increase with depth while TOP values do not change (Figs 4 and 6).” From Figs 4 and 6 it does look like TOP remains fairly constant in the two depth ranges compared, whereas TON goes up a little with depth. However, the TON:TOP ratios in Fig 5 and 6 doesn’t seem to reflect this very clearly, and it even looks like TON:TOP may be slightly lower on average between 25-100 m than above. Am I misinterpreting these data or are there something else I am missing? Reply: We thank the reviewer for this observation. TON

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indeed goes up with depth and TON:TOP is also slightly lower at 25-100 m than above. But our interpretation for TOP was not completely correct. While comparing TOP data at these two segments, we found that it was around 5% higher in the 25-100 m than 0-25 m depth, which is difficult to see in the Figures. We have revised the sentences accordingly to make this more clear and eliminate confusion (ll 274-276).

P9286, ln 5-9. "..the gradual increase in Chlorophyll a during the four months prior to deep mixing is due to a similar increase in MLD before deep mixing". Is this to mean that the increase in chlorophyll is due to increased nutrient influx into the 0-25 m depth range? Could the annual pattern in chlorophyll a concentration be explained by the changes in light flux over the yearly cycle? I.e. phytoplankton containing more chlorophyll during the winter months with lower light flux, but not necessarily more biomass? Reply: Winter mixing, which results in spring blooms thereafter due to nutrient injection into the euphotic zone, is a well recorded phenomenon at BATS. Light could be a limiting factor in the winter and hence the blooms occur during spring. Conceptually, as fall progresses and the MLD increases due to surface cooling, phytoplankton see on average a lower light level which is compounded by the decreasing annual light pattern. So there is likely some photoacclimation going on. This is further supported by the observations of Wallhead et al. 2014, that show that phytoplankton C does not increase, relative to summer, when the MLD is deepening and thus the Chl:C in phytoplankton is arguably increasing. Given that availability of light data is not consistent, and the assumptions involved, we have raised this as a potential explanation but do not state it as a 'conclusion'. Ln 10-14. How were these correlations made? Depth averaged over 0-25 m, or 0-100 m? It is unclear as written. Figure 9 shows only 0-25 m data, but using only such a shallow range may result is a skewed picture. How would data from the full euphotic zone impact the interpretation of the influence of the taxonomic groups on the C:N:P stoichiometry of POM? Reply: Correlations were made over the depth average 0-25 m. We have mentioned in the manuscript now (Lines 309 and 312). We have checked and found Figure 9 (0-25 m depth) does not give a skewed picture. Patterns are the same in the 0-100 data but they are not as prominent as in

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0-25 depth likewise for the elemental concentration parameters. Moreover, our focus is mainly in the 0-25 depth. We thank the reviewer for his/her comment but we believe the presentation and interpretation are accurate.

P9289, ln 6. “Such ratios appear to be largely driven by. . .” This sentence seems to be referring to the average C:N:P ratios of both TOM and POM. Was that the intent? Or was it supposed to refer to the annual or seasonal variability observed, or the out of Redfield ratio that can be inferred from the Synechococcus and Prochlorococcus? I suggest adding some words to make the sentence clearer. Reply: We meant that the seasonal variation in POM stoichiometry appears to be largely driven by the growth of Synechococcus during winter mixing. The Redfield ratio in POM can be explained by Prochlorococcus abundance. We have made both of these statements more clear now (ll 383-387).

Table 1. What is the rational behind the presentation of data collected prior to this study’s window for some parameters? What criteria was used to create the ratios? (The number of observations are much reduced for the ratios relative to each parameter measured by itself). Reply: More data provide better statistics so we wanted to put all the BATS data on the parameters we have analyzed in the Table 1. But for our deep mixing analysis, it was fair to use only concurrent data. Ratios were calculated for each depth, where both (POM and TOM) the parameters were measured. In many cases, both parameters were not measured at the same depth and hence the number of observations are much reduced for the ratios relative to each parameter measured by itself.

Figs 4-7. (see above question for ratios in Table 1). Are the ratios derived from a different subset of samples than what is presented for each parameter measured by itself? There are no "n" number mentioned in the figure legends. Reply: These ratios are derived from a subset of the data listed in the Table 1. However, here we first estimated average concentrations of each element over the depth segment (e.g., 0-25 m) and then the ratios were calculated (please see first comment). We have specified

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this in the manuscript now (ll 171-173). This way, we could include all the data for the time segment January 2005 - December 2011. Now one bar in each figure is obtained from the seven data points (one each year from 2005 – 2011). But this one (of those seven data) datum is estimated from around three points (5, 10, 20 m targeted depth). Hence, mentioning "n" in the figures could be confusing, but we have attempted to make it clearer in the text (ll 171-173).

Minor: "Redfield Ratio" or "Redfield ratio". Both are used throughout. I suggest using only one version. Reply: We have corrected it throughout the manuscript to "Redfield Ratio".

P 9286, ln 7. Spelling Chlorophyll Reply: Corrected (Line 305). Suggestion on Figs 4-9. Box plots would be a very nice way to present these type of data as the data sets are large and the box plot format gives so much more information than the mean and std-deviation. Reply: We welcome this suggestion. We present our data in box plots for the Figures 4-9.

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