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Interactive comment on "Changing nutrient stoichiometry affects phytoplankton production, DOP build up and dinitrogen fixation – a mesocosm experiment in the eastern tropical North Atlantic" *by* J. Meyer et al.

Anonymous Referee #2

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The manuscript presents a large body of interesting data from two multi-treatment mesocosm experiments performed in the Eastern Tropical North Atlantic. The results of the experiments, particularly those relating to the stoichiometry of new organic matter formation as a function of supplied amounts and ratios of inorganic nutrients, have potentially important implications for our understanding of marine biogeochemistry. The manuscript is also reasonably well written and presented in the context of the current literature and consequently I would likely be supportive of eventual publication subject to the authors addressing my remaining concerns outlined below.

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Specific comments

In various places throughout the manuscript (e.g. Page 9993, line 26, Page 994, line 11 etc.) the authors refer to limitation when making inferences on the basis of ratios of available or supplied inorganic nutrients. Actually their own experiments suggest that this link is far from straight forward and I would encourage them to clarify where possible, maybe stating that the dissolved ratios indicate the 'potential for one nutrient to becoming limiting before the other' or sticking to the use of terms like 'deficiency', 'deficit', 'excess' etc. (see e.g. Page 9995, line 1).

Experimental methods and statistical analysis need to be further described in places. In particular, although clear through consulting Table S1, the number of replicate mesocosms for individual treatments should be more clearly indicated to the reader, e.g. through stating in the text on Page 9997. Additionally, on Page 10001 the authors introduce a complex statistical model for the interpretation of the data without providing any justification for why this was required or chosen. Overall I was not sure why the statistical model was required as it appeared to largely be used just for the analysis of the nifH gene/transcript data and it wasn't clear that it added much to the interpretation of this data. Additionally it wasn't clear to me whether the analysis presented in Figure S1 was based on the GLM modelling performed or simple correlation analysis? Additionally, why is Figure S1 in supplementary rather than within main body of manuscript?

Although an entirely feasible explanation, I think any potential causal link between the accumulation/availability of DOP and enhanced N2 fixation needs to be treated with caution on the basis of the data presented and experiment(s) performed. e.g. Page 10005, lines 11-20, an alternative interpretation might be that both the accumulation of DOP and the enhancement of N2 fixation are occurring within the 'varied P' experiments independently simply as a result of the addition of inorganic P. The authors may argue that the time series of DOP, P, POP, N2 fixation might argue against this (e.g. Figure 10), but given only 2 sampling time points for N2 fixation I would argue this

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remains equivocal. I would suggest the authors may simply wish to acknowledge this potential caveat.

Given the extensive measurements of the P pools (see e.g. Figure 10), it would have been useful to see an attempt at mass balance.

Additional minor comments

Page 9995, line 26: '. . .are regarded as key factors. . .'

Page 10004, line 11-15: This text does not appear to be fully consistent with the content of Figure 8? i.e. nifH Fil do not appear to be dominant for either experiment in this figure?

Page 10006, line 12 (and elsewhere): it is worth noting that the POC, PON, POP data reported will not just reflect that of 'primary producers' but actually will represent average values for the whole microbial community.

Page 10008, line 24: The authors could be more specific here. They are specifically discussing excess inorganic P. Related, the authors should use the more specific term DIP to refer to dissolved inorganic P when appropriate throughout (compare Page 9999 line 13 with Page 10008, line 24).

Page 10009, line 9: do the authors mean P* here? i.e. DIP – DIN/16 or some similar definition c.f. Deutsch et al. 2007? If so I don't think the term has been defined to this point in the manuscript.

Page 10009, line 29: '. . .locally prior to offshore transport.'

A number of the figure captions (and associated statistics) require work and/or better description and figures could be clarified in places:

Figure 1: please explain error bars (standard deviations? Standard errors?)

Figure 2: shaded areas were a bit difficult to make out

Figures 3, 4 & 6: error bars for data points need explanation, regression lines also need to be described in caption. Also were the fits model I or model II type regressions?

Figure 7: error bars again need description. Additionally what statistical test was being used here?

Figure 10: error bars.

Figure S1, caption and figure do not appear to match. Caption refers to ‘a’ and ‘b’ parts when there only appears to be one part in figure?

Interactive comment on Biogeosciences Discuss., 12, 9991, 2015.

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