

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 #1

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Overview: Meyer et al present observations from mesocosm experiments, which were designed to investigate the impact of varying supply ratios of nitrate and phosphate on phytoplankton growth, nitrogen fixation, diazotroph diversity and POM and DOP. This is a data rich manuscript that, for the most part, is well organised and reports some valuable findings. In particular, the authors find that supplying nutrients at an N:P less than Redfield causes excess DOP production and a shift in community structure to organisms that can grow independently of fixed N and access DOP. They observed an increase in DDAs which has important implications for export and needs to be more

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clearly stated. There are some issues with the manuscript that the authors need to deal with before it should be published. These are listed below. In particular, the motivation for the study needs to be refined and I don't see the need for the model. See below for more detailed comments.

Criticisms:

1. The motivation for this study, ocean deoxygenation due to climate change, and thus reduction of the N:P ratio, is not the only process that will alter then N:P ratio in the ocean. Addition of anthropogenic nitrogen (e.g. see Kim et al 2014), as the potential to perturb the system by adding N in excess of P, thus intensifying or expanding phosphate limited regions and thus force the system the other way. They authors should really alter their motivation to cover both sides of the story here.
2. The ability of nitrogen fixation to modify primary production is likely to be small. If you multiple nitrogen fixation rates by a C:N ratio of ~ 6 , then compare the carbon fixed by diazotrophs to total carbon fixed, it is quite small. Instead, the switch to DDAs will impact carbon export, which is the potentially important here.
3. The author needs to be clear when they refer to P limitation. It is likely phosphate limitation and not phosphorus limitation considering the ability of organisms to access DOP. Please be more explicit about this in the manuscript.
4. You argue that there is low O₂ and N:P ratios (not levels as you have stated) below the MLD in mode water eddies. How typical do you think this is? Is this a wide spread occurrence and if so, how many mode water eddies exist in the Atlantic. It is not clear if this is just a local feature/unique phenomenon or widespread.
5. What measures were taken to prevent contamination of the sample with either nutrients or trace metals? Was the nutrient or iron concentration at the oceanic sample collection site monitored between collection and use in the mesocoms?
6. Nutrients: considering you are reporting the change in nutrient concentrations over time and between treatments, you should really report the precision of analysis as well as limits of detection in section 2.2. Also, the instrument and methods are described for DOP (section 2.4) but not nutrients. This needs to be fixed to be consistent. Again, what was

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the precision and limits of detection for DOP? This should be reported. 7. In section 2.7, what was the atom percent enrichment of the $^{15}\text{N}_2$ water added to the incubations and the final atom percent enrichment at the start of the incubations? This should be reported here. 8. Section 2.8: Model selection. Why did you use a model here? What was the goal? This needs to be stated. Was it necessary? Also, the model description is quite confusing. What is γ and γ_{mm} ? What is Akaike Information Criterion? This section needs to be edited to clarify the goal of the model and perhaps reduce the detail here and refer to other manuscripts where this sort of analysis has been done already. As a follow up on this, on getting to the end of this manuscript, I do not believe the modelling component adds any real value to this manuscript. The study is data rich and there are plenty of interesting and important points to make without including the model. 9. The authors switch between N and P and NO_3^- and PO_4^{3-} . This needs to be fixed and made consistent throughout. 10. The authors report the N:P but not with P relative to one. I think this is confusing. For example, page 10002, lines 14-16. N:P 6.35/1.10, 12.00:1.25. It would be better if this was written as: 5.77 and 9.6 respectively and details of the concentration be inserted into a table, for example. 11. I suggest changing the use of the word 'build up' to 'accumulation'. Also avoid using words like rise and drop, should be increase and decrease respectively. 12. Note that similar observations of the C:N:P ratio for POM were observed by Davis et al 2014 in GRL. 13. I suggest reducing the precision on ratios reported, e.g. change 38.8 to 39 and 21.9 to 22. The decimal places don't add value here.

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