We thank anonymous Referee #1 for his/her constructive criticism and valuable comments. In the following we address the points brought up, with referee comments in boldface and author responses in normal typeface.

**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.**
We agree that ocean deoxygenation is not the only process that will likely alter N:P ratios is the ocean. We clarified this in the introduction. However, in the eastern tropical North Atlantic (ETNA) $N_2$ fixation is supposed to be the dominant process for the input of new N over the next decades to centuries (Duce et al. 2008) compared to the North Pacific Ocean (Kim et al. 2014). Moreover, it is debatable how much of the atmospheric anthropogenic nitrogen input is affecting the open ocean of the ETNA. If the input is mostly restricted to the costal upwelling region, biological production would be fueled, export enhanced and degradation of organic matter at depth would increase. The oxygen inventory of the ETNA OMZ would shrink further, thereby enhancing N loss processes, leading to a decrease of N:P ratios in the water column. The fertilization with anthropogenic N would thus be compensated by a negative feedback cycle. In any case, the significance of atmospheric anthropogenic N inputs into the ETNA is unclear. The expansion of the oxygen minimum zone in the ETNA, on the other hand, has been ongoing for the past decades and is expected to continue in the future (Stramma et al. 2009). As the original goal and motivation for this experiment was to study changes in the N:P ratio due to ocean deoxygenation in the ETNA, we focused on the description of our experiment in this context.

**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.**
We agree and thank for this valuable input. The export of carbon might indeed be influenced by a shift of the diazotrophic community towards DDAs and included this point into the discussion. We interpret from our data that primary production might be affected by a shift within the diazotrophic community, especially in the oligotrophic open ocean of the ETNA, where diazotrophs are the dominant primary producers. We clarified this point in the manuscript.

**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.**
We agree with the referee and made the appropriate changes 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.**
We changed “(…) with an accompanied decrease in N:P levels (...)“ to “(…) with an accompanied decrease in N:P ratios (...)“ (p. 9994, line 5-6).
Mode water eddies with very low oxygen concentrations were only recently discovered in the ETNA. These eddies can last several month, transporting shelf water signals offshore (Karstensen et al. 2015). In the ETNA, 5 such events were observed between 2007 and 2012 (Karstensen et al. 2015). We believe that the effect of mode water eddies is not basin-wide but rather of local importance. We clarified this in the manuscript.

**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?**
Containers for water transport were first rinsed with diluted HCL and several times with deionized water. Nutrients in all mesocosms were measured before nutrient manipulation. $NO_3^-$ and $NO_2^-$, $PO_4^{3-}$ and $Si(OH)_4$ were all below the detection limit and far below the manipulation levels (see Fig. 2). We therefore conclude that no contamination with these nutrients occurred during water sampling, transport and mesocosm filling. This information was added to the manuscript.

**6. Nutrients: considering you are reporting the change in nutrient concentrations over time and be- tween 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 the precision and limits of detection for DOP? This should be reported.**
The precision of analysis and detection limits for nutrient and DOP analysis was added. The paragraph describing the nutrient measurements was changed as follows: "Samples (10 mL) for dissolved inorganic nutrients (NO−, NO−, PO3−, Si(OH)4) were taken daily from each mesocosm and measured directly using a QuAAtro Autoanalyzer according to Grasshoff et al. (1999). The detection limit of nutrient analyses were 0.01 µmol $L^{-1}$ for $NO_2^-$ and $PO_4^{3-}$ and 0.03 µmol $L^{-1}$ for $NO_3^-$."

**7. In section 2.7, what was the atom percent enrichment of the 15N2 water added to the incubations and the final atom percent enrichment at the start of the incubations? This should be reported here.**
The preparation of the $^{15}N_2$-enriched seawater was performed as described in Mohr et al. (2010). Degassed seawater was filled into evacuated gas-tight 3L Tedlar® bags without a headspace. Addition of $^{15}N_2$ gas was (depending on the exact water volume in the Tedlar bag) around 10 ml $^{15}N_2$ per 1 L seawater. Dissolution of the $^{15}N_2$ gas was achieved by 'slapping' the bubble with a ruler. After complete dissolution of the added $^{15}N_2$ gas ($^{15}N_2$-enriched seawater), an aliquot of the $^{15}N_2$ enriched water was collected for each preparation of enriched seawater and stored in an Exetainer, the isotopic composition was measured by membrane-inlet mass spectrometry. The $^{15}N_2$ concentration in the prepared batches of enriched water was determined to be 250 µmol $L^{-1}$, which translates in an $^{15}N$-enrichment of about 2 % in the 4.5 L bottle incubations, when adding 100 mL enriched seawater (depending on temperature and salinity). These details were added to the methods section.

**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 gam and gamm? 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.**
As both referees state that they don't see the added value of the model we introduce in the manuscript, we decided to remove it from our manuscript. Instead, we show the original transcript data obtained from the study (new Fig. 8) in order to avoid confusion and make the manuscript more intelligible to the reader (see also response to referee 2).

**9. The authors switch between N and P and NO30 and PO43-. This needs to be fixed and made consistent throughout.**
We made this consistent throughout the manuscript.

**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.**
We prefer to report the N:P ratios as they are. That way, mesocosms with the same initial concentrations of N or P (center points) are easier to distinguish for the reader.

**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.**
We agree and made the appropriate changes in the manuscript.

**12. Note that similar observations of the C:N:P ratio for POM were observed by Davis et al 2014 in GRL.**
Thank you for this valuable input! We will add the suggested reference and the observations of this study to our discussion.

**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.**
We removed the decimal places as suggested.