The authors thank this reviewer for the many very constructive and helpful comments. Below we outline how we improve the manuscript (ms) in response to these comments.

Comment 1: Based on a prognostic 5 box model of the OMZ region in the ETSP published earlier in 2015, the authors focus here on the effects and feedbacks between major sources and sinks in the marine N cycle. They consider in particular the atmospheric deposition and benthic remineralisation. I found this work rather interesting as it tends to show that those processes are important for N cycle in the studied domain. I see however several points concerning the numerous assumptions and/or simplification that have been done in the model that should be addressed before publication in Biogeosciences.

I also find that the short discussion-conclusion section was too much centered on model results. I would thus recommend to take a step back on the results in order to draw a discussion (and conclusions) that may have a stronger and broader impact for the understanding of that complex area in light of recent findings.

Response 1: The authors thank the reviewer for these generally positive comments and the suggestions on the structure of the manuscript. Please see our modifications to the "Discussion and Conclusion" section in the revised ms (Pages 19–23), in which more recent findings as suggested by both reviewers have been included to provide a better understanding of the eastern tropical South Pacific (ETSP).

Comment 2: One can wonder how the uncertainties linked to those numerous assumptions make this version of the model really solid. For example, only heterotrophic denitrification was considered for fixed-N loss process; for atmospheric deposition, DON is not considered (because of a lack of data but it has been shown recently that this fraction could be very important — see below); also riverine inputs are not considered; also phosphorus atmospheric deposition is not considered. And last but not least, aphotic N2 fixation process is not mentioned in the study. I have not too much problems with simplification but at one point, these simplification should be also part of the discussion: how the omission of all those impact/process influence or not the results/conclusions.

Response 2: The Question about atmospheric nitrogen and phosphorus deposition and aphotic N_2 fixation is addressed below in response to **Comments 3** and **4**. Denitrification as the only fixed-N loss processes was already explained on Page 14444 Lines 10–14 of the original BGD ms:

> Even though anammox has been observed to contribute significantly to the fixed-N loss in the ETSP, both denitrification and anammox are ultimately driven by the flux of organic matter into the OMZ. Because the NH_4^+ for anammox from either ammonification or dissimilatory nitrate reduction to ammonium (DNRA) is ultimately determined by the organic matter fluxes into the OMZ [Lam et al., 2009, Koeve and Kähler, 2010].

We rephrase these sentences to clarify this point on Page 4 Lines 8–13 in the revised ms:

> Anammox has been observed to be a major fixed-N loss process in the eastern tropical South Pacific (ETSP) [Lam et al., 2009, Kalvelage et al., 2013]. However, the essential substrates for anammox are ultimately provided by heterotrophic processes [Koeve and Kähler, 2010, Kalvelage et al., 2013], such as organic-matter remineralisation or dissimilatory nitrate reduction to ammonium (DNRA). Thus, both denitrification and anammox are ultimately driven by the flux of organic matter into the OMZ.

No large rivers flow into the ETSP from the coast of Peru. According to Seitzinger and Kroeze [1998], riverine DIN input into the ETSP only accounts for less than 1.5 % of the total riverine nitrogen input into the Pacific and increases the NO_3^- concentration of our study area by less than

2×10^{-6} µmol N L^{-1}, assuming that all the riverine DIN was distributed evenly, which indicates a very minor role in the nitrogen budget of the ETSP. Therefore, riverine nitrogen input is excluded from our model analysis. We rephrase the text on Page 14445, Lines 13–17 of the original BGD ms to read:

> Using a conceptually simple and computationally efficient box model, we here attempt a synthesis considering all essential sources and sinks and their mutual interactions, with the only exception of riverine input, which is excluded because it contributes only negligibly to the nitrogen inventory in the ETSP [Seitzinger and Kroeze, 1998].

on Page 5, Lines 12–15 in the revised ms.

Atmospheric phosphorus deposition is also excluded from our analysis. We clarify the statement from Page 14447, Lines 10–15 of the original BGD ms as follows on Page 7, Line 25–Page 8, Line 2 of the revised ms, also considering the comments from the other reviewer:

> Atmospheric phosphorus deposition is excluded from our analysis because its amount is much smaller than the Redfield equivalent of nitrogen atmospheric deposition [Duce et al., 1991]. This results in N/P (mole/mole) ratios of more than 100, much higher than the average elemental N/P ratio required by phytoplankton [Duce et al., 2008, Mahowald et al., 2008].

Comment 3: I would recommend to give more detail on nitrogen atmospheric deposition used in the model. Inclusion of atmospheric deposition in your model is a hint of the paper: it needs more solid assessments. This is an important addition to the previous model and it is important to provide more information on the data used. The section on atmospheric deposition is very short, and estimates of DIN deposition used need to be more explained. Considering that this area has only been validated by scarce field data, the uncertainty on the flux (from models) are quite high. How these uncertainty impact your model results? Also concerning the fact that atmospheric Organic Nitrogen was not considered in the model although recent work have shown how important this fraction can be for total nitrogen inputs. For ex., Kanakidou et al., 2012 indicate an average of 35 % of Organic Nitrogen of the total soluble N in wet deposition: this deserves to be discussed as atmospheric deposition used in your model is in fact most likely underestimated: how this can impact the results?

Response 3: We thank the reviewer for this suggestion. We modify the "Model description" section about atmospheric deposition on Page 11, Lines 13–26 in the revised ms. Since the atmospheric nitrogen deposition data from Lamarque et al. [2011] only include one chemistry-climate model results, a multi-model perspective could offer additional insights into the influence of uncertainties in nitrogen deposition on our model results. Three recent inter-model comparisons [Dentener et al., 2006, Lamarque et al., 2013, Vet et al., 2014] show very similar performance over our model domain, therefore we choose the results from Dentener et al. [2006], which is also applied in a number of benchmark papers such as Duce et al. [2008]. The results are shown in new Fig. 5 in the revised ms.

Dissolved organic nitrogen (DON) accounts for about 30 % of the total dissolved N deposition in South America [Cornell et al., 2003, Kanakidou et al., 2012], and about 30 % of the deposited DON is available to primary producers, increasing by about 13 % the bioavailable nitrogen input into the model domain. Considering also a suggestion from the other reviewer, we investigate further scenarios for different DON bioavailability and future emission increases. We summarize the results in the new Fig. 5, and on Page 17, Lines 2–12 of the revised ms. The results are discussed on Page 19, Lines 18–22 in the "Discussion and conclusions" section of the revised ms.

Comment 4: There is one process that should be taken into consideration or at least discuss why it is not and how it could change the presented budget: this is the aphotic N2 fixation in that area, a

process that was recently evidenced to be very important in ETSP according to Bonnet et al., 2013. In your study, N2 fixation was only considered in the top 100m layer. Bonnet et al., clearly state in their conclusion: 'These new sources of N could potentially compensate for as much as 78 % of the estimated N loss processes in ETSP, indicating that they need to be taken into account in marine N budgets'. How can this important question be addressed in your work? How this actual process and important source of fixed N will affect your proposed nitrogen-balancing mechanism in that area? Note also that the same authors find that N2 fixation was never inhibited after NO_3^- addition, an interesting finding that could also be discussed.

Response 4: We thank the reviewer for this information. We now include the aphotic nitrogen fixation estimates from the 2010 and 2011 cruises reported by Bonnet et al. [2013] in our model analysis. Supplementary Fig. S5 shows the nitrogen fluxes after including aphotic nitrogen fixation. Photic nitrogen fixation decreases by 39 % and 15 % in the AphoticNfix1 and AphoticNfix2 configurations. Water-column denitrification rate stays steady because more nitrogen input by aphotic nitrogen fixation does not increase export production into the OMZ. The lateral fixed-N effluxes in the AphoticNfix1 and AphoticNfix2 configurations are about 33 and 4 times of that in the control configuration, accounting for 91 % and 78 %, respectively, of extra nitrogen input by aphotic nitrogen fixation. Tracer concentrations at steady state are presented in supplementary Fig. S6. Aphotic N_2 fixation has little effect on most tracers except NO_3^-, which increases by 110 % and 87 % respectively in the UM box and the model domain in AphoticNfix1. While the lower estimate of aphotic N_2 fixation (AphoticNfix2) brings the NO_3^- concentrations closer to the WOA2009 data, the high estimate (AphoticNfix1) leads to a strong overestimate. We conclude that aphotic N_2 fixation is likely closer to the lower (2011) estimate of Bonnet et al. [2013]. We incorporate this process as sensitivity experiment and describe it on Page 12, Lines 1–11 on the revised ms. We also describe the results and discuss this topic on Page 17, Lines 13–26 and Page 20, Lines 14–23 of the revised ms.

Bonnet et al. [2013] found that N_2 fixation by non-cyanobacteria diazotrophs was never inhibited after NO_3^- addition in the OMZ where ambient NO_3^- concentration are in a range of 30–40µmol L^{-1}. In their results, the reasons for this are not resolved because the metabolic potential of diazotrophs is not characterized. In our AphoNfix1 and AphoNfix2 model configurations, we have simply integrated aphotic nitrogen fixation as fixed-N input fluxes into the respective regions, thus avoiding the issue of NO_3^- inhibition for N_2 fixations in the aphotic zones. In the surface boxes, PO_4^{3-} is the only limiting nutrient for nitrogen fixers and NO_3^- does not inhibit the growth of nitrogen fixers.

Comment 5: I found that the model concept and results was often quite decoupled from actual field knowledge and data for the given area. This is the case for my comment regarding atmospheric deposition, N2 fixation; this is also the case for the estimation of the rain rate POC. The 'classical' b=0.82 is taken into consideration although it is well known that b depends on a number of parameter and is not constant over the ocean. In the recent regionalization study from Guidi et al. 2015, it is well demonstrated that 'b' is a non constant number resulting from non uniform remineralisation. We are all aware of that but I believe that it is important to take into account recent findings and at least discuss the limit of your hypothesis in light of those recent findings. See their table 2 for the regions included in ETSP (CHIL, PEQD and SPSG), actually, their 'b' is close to the Berelson value (although lower for the SPSG domain). I think this is an interesting point to better discuss in light of recent data.

Response 5: In the main configuration of our model, a constant Martin Curve exponent $b = 0.82$ is applied for the whole ETSP, because this values is a global average and also very close to the average value estimated for the ETSP by Berelson [2001] (Fig. 2 in Berelson [2001]). A lower b value ($b = 0.4$) under anoxic conditions, as suggested by Van Mooy et al. [2002], is applied in our sensitivity analysis, which can be considered as a lower limit for the Martin curve exponent b. In order to consider the recent findings by Guidi et al. [2015], we also consider spatial variations in b within our model domain. In the UM-box, $b = 0.83$, which corresponds to the Peru-Chile upwelling region (CHIL), is applied. $b = 0.85$ is applied in the D-box, which is the average of the b values in regions named CHIL, PEQD, SPSG, and WARM in Guidi et al. [2015].

The nitrogen fluxes in configurations with different b values (bars marked 'C' in supplementary Fig. S7) are in good agreement with those in our main configuration (bars marked 'A' in supplementary Fig. S7). Nitrogen fixer (NF) concentrations in the surface boxes are also robust after including spatially variable b values (supplementary Fig. S8). Thus, regionally varying b values as implied by Guidi et al. [2015] have only minor effects on our model results. Our corresponding sensitivity experiment is described and its results are presented on Page 12, Lines 22–29 and Page 18, Lines 9–12 of the revised ms, respectively.

Comment 6: Define MBD and DBD also in the text (only in caption Table 1). This will make it easier for the reader.

Response 6: The text below is included on Page 7, Lines 6–17 of the revised manuscript to define the configurations briefly in the text:

> In the NDEP configuration, atmospheric nitrogen input into the surface ocean according to the estimate by Lamarque et al. [2011] in included; MBD and DBD are configurations in which model- and data-based benthic denitrification are included in the control configuration; MPR and DPR represent configurations with model- and data-based benthic phosphorus regeneration respectively. Detailed information of all processes is presented in Sects. 2.3, 2.4 and 2.5.
>
> Nitrogen deposition, benthic denitrification and phosphate regeneration are integrated into the synthesis model configurations to explore the model sensitivity to each process and their mutual interactions in the ETSP. Synthesis configuration Syn1 includes model-based benthic denitrification and phosphorus regeneration; Syn2 includes the data-based benthic denitrification and phosphorus regeneration; Syn3 also includes atmospheric deposition in addition to the processes in Syn1; Syn4 includes atmospheric deposition in addition to the processes in Syn2. The synthesis configurations Syn1 to Syn4 are also summarized in Table 1.

Comment 7: I would rather call the atmospheric source of nitrogen that enters the open ocean available for biota 'reactive' and not 'fixed' (although it is commonly used).

Response 7: We think that "bioavailable" may be a better term, and change it in the revised ms.

Comment 8: I would add a figure of the actual model domain showing the ETSP.

Response 8: We add a panel to Fig. 1 in the revised ms, showing the geographical location of the model domain.

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