

We have submitted a revised manuscript (ms), which includes all the modifications described in our two response letters. The main differences from our ms published in BGD (referred to original ms below) are as follows:

1. Abstract is modified in the revised ms to include the estimated fluxes according to the suggestion from the second reviewer.
2. Several sentences are modified in the Introduction section of the revised ms to mainly explain or introduce:
  - Why does benthic denitrification replace anammox as the fixed-N loss process in the model;
  - DON and its bioavailability scenarios are included in our analysis;
  - Why is riverine nitrogen input excluded from of our analysis.
3. One paragraph is added in Section 2.1 Circulation and biogeochemical model (Page 6 of the revised ms) to describe ordinary phytoplankton and nitrogen fixers in detail.
4. A brief description of the nine main model configurations is added in Section 2.2 Model configurations of the revised ms.
5. The explanation to why phosphate deposition is excluded from our analysis is modified in Section 2.3 Atmospheric nitrogen deposition (Pages 7–8 of the revised ms).
6. Three new model sensitivity experiments are performed and added in the revised ms:
  - Model sensitivities to uncertainties underlying the atmospheric nitrogen deposition, atmospheric DON deposition and its bioavailability, and predicted increasing nitrogen deposition;
  - The influence of aphotic nitrogen fixation on the nitrogen budget of our model domain;
  - Model sensitivity to spatially variable Martin Curve exponents  $b$ .
7. The Section 2.6 Synthesis configurations has been integrated into Section 2.2 Model configurations in the revised ms.
8. The ship-collected net primary production from Pennington et al. (2016) is compared to our NPP estimates, which is discussed on Pages 19–20 of the revised ms.
9. Flux comparison with other model or observational estimates are summarized in Table 3 of the revised ms, which are discussed on Pages 20–21 of the revised ms.
10. The Discussion and conclusions section of the revised ms is improved according to the comments from our first reviewer, to have a better understanding of the eastern tropical South Pacific in the light of more recent findings.
11. A new Summary section is added on Page 23 of the revised ms according to the suggestion from the second reviewer.
12. Table 1 of the original ms is modified by explaining the process abbreviations in the caption, which is the new Table 1 of the revised ms.
13. Tables 2 and 3 of the original ms are integrated into one table, which is the new Table 2 of the revised ms. The references of the relative estimates are added in the new table.
14. The sequence of the figures are changed and some of them are modified in the revised ms:
  - Fig. 1 in the original ms → Fig. 1 in the revised ms: a panel is added to show the geographical location of our model domain.
  - Fig. 2 in the original ms → Fig. 2 in the revised ms: the nitrogen deposition and nitrogen fixation are shown separately for the U and S boxes.

- Fig. 3 in the original ms → Fig. 3 in the revised ms.
  - Fig. 4 in the original ms → Fig. 4 in the revised ms: the nitrogen deposition and nitrogen fixation are shown separately for the U and S boxes.
  - Fig. 5 in the original ms → Fig. 6 in the revised ms.
  - Fig. 6 in the original ms → Fig. 7 in the revised ms.
  - Fig. 7 in the original ms → Fig. 8 in the revised ms.
  - Fig. 8 in the original ms → Fig. S1 in the supplementary materials of the revised ms: the nitrogen deposition and nitrogen fixation are shown separately for the U and S boxes .
  - Fig. 5 in the revised ms is new.
15. A supplement is added to the revised ms with Appendices A–D.
  16. Appendix A of the original ms is moved to the supplement of the revised ms.
  17. The term phosphate regeneration in the original ms is changed to phosphorus regeneration in the revised ms, which we think is better.

Please see our response letters for details.

The detailed differences between the original and revised ms are also shown in the marked-up PDF file processed with *latexdiff*, which we have submitted together with the revised ms.

We have submitted two letters respectively in response to the comments from our two referees already. Here we summarise our detailed point-by-point response to referee comments and specify all changes in the revised manuscript (ms) in response to the relevant comments. A list of all relevant changes made in the revised ms is also submitted together with this response. We will refer to the ms published in BGD as the original ms, and all figure and table numbers refer to the original ms unless otherwise noted.

### **Response to to Referee #1:**

**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 N<sub>2</sub> 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<sub>2</sub> 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<sub>4</sub><sup>+</sup> 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  $\text{NO}_3^-$  concentration of our study area by less than  $2 \times 10^{-6} \mu\text{mol N L}^{-1}$ , 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 N<sub>2</sub> 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, N<sub>2</sub> 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 N<sub>2</sub> fixation was never inhibited after NO<sub>3</sub><sup>-</sup> 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<sub>2</sub> fixation has little effect on most tracers except NO<sub>3</sub><sup>-</sup>, which increases by 110 % and 87 % respectively in the UM box and the model domain in AphoticNfix1. While the lower estimate of aphotic N<sub>2</sub> fixation (AphoticNfix2) brings the NO<sub>3</sub><sup>-</sup> concentrations closer to the WOA2009 data, the high estimate (AphoticNfix1) leads to a strong overestimate. We conclude that aphotic N<sub>2</sub> 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 14–24 of the revised ms.

Bonnet et al. [2013] found that N<sub>2</sub> fixation by non-cyanobacteria diazotrophs was never inhibited after NO<sub>3</sub><sup>-</sup> addition in the OMZ where ambient NO<sub>3</sub><sup>-</sup> concentration are in a range of 30–40 μmol L<sup>-1</sup>. 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<sub>3</sub><sup>-</sup> inhibition for N<sub>2</sub> fixations in the aphotic zones. In the surface boxes, PO<sub>4</sub><sup>3-</sup> is the only limiting nutrient for nitrogen fixers and NO<sub>3</sub><sup>-</sup> 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, N<sub>2</sub> 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] is 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.

**Response to to Referee #2:**

**Comment 1:** Su et al. used a prognostic 5-box circulation model from a previous published manuscript (Su et al., Biogeosciences, 2015) to investigate the effects of atmospheric nitrogen deposition and benthic remineralization on the nitrogen cycle of the Eastern Tropical South Pacific (ETSP). Their main findings are that 1) N deposition is offset by half by reduced N<sub>2</sub> fixation with the other half exported out of their model domain, 2) sedimentary denitrification and phosphate regeneration under suboxic conditions acts to increase N<sub>2</sub> fixation, and 3) this increased N<sub>2</sub> fixation is partly removed by stronger water-column denitrification. Overall, they claim that these stabilizing feedbacks keep a

balanced nitrogen inventory in the ETSP. While their results are interesting, I have some major issues with the paper and recommend revisions before publication in *Biogeosciences*.

**Response 1:** The authors thank the reviewer for the generally positive comments and recommendation for publication in *Biogeosciences* after revision.

**Comment 2:** First, I feel that many simplifications have been made in their model. For instance, the authors assume that N<sub>2</sub> fixation is ultimately limited by P supply but omit to consider the important role of Fe. Fe has been shown to control patterns of N fixation, even in regions where Fe depositions are higher than in the ETSP (e.g., see Moore et al., *Nature Geoscience*, 2009). In a recent study published in *Global Biogeochemical Cycles*, Dekaezemacker et al. (2013) reported that N<sub>2</sub> fixation was stimulated by Fe addition in the ETSP. Therefore, I would like them to describe the role of Fe limitation on N<sub>2</sub> fixation in their model.

**Response 2:** While Fe deposition is low in this region, Fe concentrations are not, as also reported by Dekaezemacker et al. [2013]. It is somewhat unclear to us how to interpret the results of the Fe-enrichment experiments of Dekaezemacker et al. [2013] in terms of modeling. Stimulation of N<sub>2</sub> fixation was inversely related to ambient Fe concentrations, so that the Fe enrichment raised N<sub>2</sub> fixation by 200 % with low ambient Fe, but by more than 450 % with 10-times higher ambient Fe, with only minor differences in ambient nitrate and phosphate concentrations. These findings are counter to what would be expected if N<sub>2</sub> fixation was mainly Fe limited. Dekaezemacker et al. apparently came to the same conclusion, writing in their Discussion that “Finally, the high positive Fe\* (Table 1) in 2011 indicates that there is enough iron to support the complete consumption of PO<sub>4</sub><sup>3-</sup>”. In our view, the role of Fe limitation in the region of our model domain thus remains too unclear to warrant inclusion in our model.

**Comment 3:** Furthermore, they also neglected DON, that represents ~ 30% of total dissolved N wet depositions in South America (see Cornell et al., *Atmospheric Environment*, 2003). While I understand that the bioavailability of DON is still unclear, some estimates are available, for instance, Peierls and Paerl (*Limnology and Oceanography*, 1997) suggested that ~ 20 – 30% of atmospheric organic N is readily available to primary producers. Therefore, I believe that they could test different scenarios regarding DON bioavailability in their model. I would also like to see a scenario with increased N depositions that reflects predicted future changes. Second, they separate the coastal and open ocean regions in their model (e.g., U and S boxes) but fail to discuss these separately in their discussion. Coastal regions are highly productive compared to the open ocean, therefore I would expect fluxes to be significantly different, as shown in their sensitivity analysis (Figures 3, 5 and 6). However, only global fluxes for the two regions are shown in Figures 2 and 4. I would like them to separate their model results for these two regions and better discuss these results in their discussion.

**Response 3:** 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 [Paerl and Whitall, 1999], increasing by about 13% the bioavailable nitrogen input into the model domain. Predicted increasing nitrogen deposition is estimated according to the RCP8.5 scenario projections for 2100 [Lamarque et al., 2011], and we have tested this scenario in our model.

We now also show the results separately for the upwelling region and open ocean, and discuss them separately in the revised ms. These results are shown in the new Fig. 5 in the revised ms. The model description about atmospheric DON deposition and increasing nitrogen deposition predicted by Lamarque et al. [2011] is included on Page 11, Lines 13–26 of the revised ms. The results are presented and discussed on Page 16, Line 22–Page 17, Line 12 and Page 19, Lines 18–22, respectively, of the revised ms.

**Comment 4:** Third, I feel a comparison of their fluxes with direct measurements from previous studies is needed in their discussion. For example, do their N<sub>2</sub> fixation and N-loss fluxes match what

can be derived from direct rate measurements from past studies (e.g., Kalvelage et al., 2013 for N-loss and Dekaezemacker et al., 2013 and Lscher et al., 2014 for N<sub>2</sub> fixation) for the considered region?

**Response 4:** We thank the reviewer for this very helpful comment. The flux comparison is included in the new Table 3 of the revised ms. The discussion of this comparison is included on Page 20, Line 25–Page 21, Line 13 of the revised ms.

**Comment 5:** Abstract: Should include actual numbers (ranges) for global fluxes derived from their model.

**Response 5:** We now compare the fluxes in our model with other estimates in Table 3 of the revised ms. We also include some of these numbers in our abstract in the revised ms. We would like to stress, though, that the quantification of these numbers is not the main goal of our analysis.

**Comment 6:** P. 14442, line 20: This statement appears to be incorrect. How can the ETSP be a NO<sub>3</sub><sup>-</sup> source when we observe such high N deficits in the Oxygen Deficient Zone (ODZ) of this region (e.g., Codispoti, Biogeosciences, 2007)? N-loss rates (up to 36 nmol N l<sup>-1</sup> d<sup>-1</sup>; Kalvelage et al., 2013) from direct measurements are also generally at least 1–2 orders of magnitude higher than N<sub>2</sub> fixation rates in the ETSP (0.01 to 0.9 nmol N l<sup>-1</sup> d<sup>-1</sup>; Dekaezemacker et al., 2013 and Lscher et al., 2014).

**Response 6:** We agree that the coastal region could be a sink of fixed-N, particularly when the high rate of nitrogen-loss by benthic denitrification is considered. But we would like to point out here that the criticised sentence does **not** say that the ETSP is a NO<sub>3</sub><sup>-</sup> source but states explicitly that the **water column** of our model domain could be a NO<sub>3</sub><sup>-</sup> source, and that the ETSP could be a NO<sub>3</sub><sup>-</sup> sink when benthic denitrification is considered. Also, our model domain is the whole ETSP, encompassing the OMZ and the adjacent ocean, whose size is  $\approx 20$  times the area of the OMZ.

Specifically, [Codispoti, 2007] reported a significant imbalance of the global nitrogen budget, which was mainly due to high rates of benthic denitrification, but the nitrogen inputs exceed the losses if benthic denitrification is excluded from his analysis. Kalvelage et al. [2013] only estimated the N budget of the actual OMZ. The OMZ is represented in our model by the UM box, which is indeed a strong NO<sub>3</sub><sup>-</sup> sink. The point of our statement is to clarify the different roles of the water-column and the benthic for the fixed-N budget of the model domain. We reformulate this sentence on Page 2, Lines 20–22, slightly to make it easier to understand.

**Comment 7:** Page 14444, Line 1: Noffke et al., Limnol. Oceanograph., 57, 851–867, 2012, who estimated benthic Fe and P fluxes in the ETSP, should be cited here.

**Response 7:** The reference is added on Page 3, Line 28 of the revised ms.

**Comment 8:** Page 14444, Lines 10–14: First, I found this sentence a bit confusing to read. Please rephrase. Second, dissimilative nitrate reduction to ammonium could also be a source of NH<sub>4</sub><sup>+</sup> for anammox, as claimed in Lam et al., 2009, i.e., not all NH<sub>4</sub><sup>+</sup> is necessarily derived from organic matter oxidation in the water-column. How this would affect their water-column estimate of N-loss in their model?

**Response 8:** We agree with the reviewer that NH<sub>4</sub><sup>+</sup> for anammox could be from either organic matter oxidation or dissimilatory nitrate reduction to ammonium (DNRA). Nevertheless, DNRA is also heterotrophic and thus organic matter is also required for DNRA [Lam et al., 2009, Koeve and Kähler, 2010]. We rephrase these sentences to clarify this point in the revised ms on Page 4, Lines 8–13:

Anammox has been observed to be a major fixed-N loss process in the eastern tropical South Pacific (ETSP) in recent studies [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 hence driven by the flux of organic matter into the OMZ.

**Comment 9:** Page 14444, Lines 25–26: Perhaps also cite Kim et al.: Increasing anthropogenic nitrogen in the North Pacific Ocean. *Science*, 346, 1102–1106, 2014.

**Response 9:** We thank the reviewer for pointing out this reference. This reference is added on Page 4, Line 25 of the revised ms.

**Comment 10:** Page 14445, Lines 5–8: I think DON should be considered in their model, with different scenarios regarding bioavailability, since it can represent a significant fraction of total atmospheric N depositions. See my general comments above.

**Response 10:** We have answered this question together with the response to the general question about DON in the atmospheric N deposition. Please refer to **Response 3** above.

**Comment 11:** Page 14445, Lines 16–17: I think they should cite published studies that quantify N riverine inputs in the Pacific Ocean, and if possible, the ETSP. For example, Seitzinger and Kroeze (Global Biogeochemical Cycles, 1998) reported a value of 4 Tg N yr<sup>-1</sup> for the Pacific Ocean.

**Response 11:** No major 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 nitrogen riverine input into the Pacific and increases the NO<sub>3</sub><sup>-</sup> concentration of our study area for less than  $2.0 \times 10^{-6} \mu\text{mol N L}^{-1}$  if we assume that all the riverine DIN is 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 will rephrase the statement on Page 14445, Lines 13–17 of the BGD ms to:

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 from our model analysis 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.

**Comment 12:** Overall: It is a bit unclear to me how N<sub>2</sub> fixation is modeled. Maybe add a short section giving more detail about this?

**Response 12:** To provide more details about how we model N<sub>2</sub> fixation, the following sentences are added on Page 6, Lines 8–17 of the revised ms to replace the content on Page 14446, Lines 9–12 of the BGD ms:

We represent two phytoplankton types in the biogeochemical model, ordinary phytoplankton (Phy) and nitrogen fixers (NF) as defined in Su et al. [2015]. Both Phy and NF concentrations are determined by the steady-state balance between net primary production (NPP) and mortality (M) respectively in the U and S boxes. Phy requires both phosphate and nitrate, and growth of ordinary phytoplankton is described by a Blackman-type dependence on the nitrate and phosphate limitation terms. NF can fix N<sub>2</sub> as long as PO<sub>4</sub><sup>3-</sup> is available. A quadratic mortality term is adopted for both Phy and NF, considering possible viral lysis, phytoplankton aggregation, or a feedback between zooplankton grazing and phytoplankton concentration. N<sub>2</sub> fixers are given a lower maximum growth rate, which is 1/3 of the maximum growth rate of ordinary phytoplankton, in order to account for the high cost of nitrogen fixation [La Roche and Breitbarth, 2005].

**Comment 13:** Page 14445, Line 23: It is unclear to me, and maybe to other non-modelers, why they calibrated their physical parameters to fit “the average  $^{14}\text{C}$  of each box”. Perhaps clarify?

**Response 13:** To clarify why the average  $\delta^{14}\text{C}$  of each box is applied to calibrate the physical parameters of the model, the following sentences are added on Page 5, Lines 20–24 of the revised ms to replace the sentence on Page 14445, Lines 22–24 of the BGD ms:

Briefly, the physical parameters were calibrated to fit the average  $\delta^{14}\text{C}$  [Key et al., 2004] of each box and biogeochemical parameters are constrained by literature data.  $\delta^{14}\text{C}$  is the  $^{13}\text{C}$  fractionation-corrected ratio of  $^{14}\text{C}/^{12}\text{C}$ , which is commonly used in ocean modelling to evaluate and calibrate model physics, because it tends to cancel the effect of the biotic downward transport of  $^{14}\text{C}$  with the rain of organic particles produced by marine organisms.

**Comment 14:** Page 14446: Lines 2–3: They separated their model into coastal upwelling region and open ocean, but their model results are then merged for the two regions in Figures 2 and 4. I think it would be helpful to distinguish between these two different regions in Figures and in the discussion and conclusions section.

**Response 14:** We thank the review for the constructive comments. We have modified all the figures, model results and discussion in the revised ms to distinguish between the two different regions. Please see the Figs. 2 and 4 in the revised ms, and supplementary Fig. S1.

**Comment 15:** Page 14447, Line 6: Are there any uncertainties associated with these estimates of N deposition rates? If so, I think these should also be reflected in their modeled fluxes.

**Response 15:** The data applied in the BGD ms are from a chemistry-climate model [Lamarque et al., 2011], and it is not possible for us to estimate the uncertainties underlying these model results. Instead, we apply an inorganic nitrogen-deposition estimate by Dentener et al. [2006], which is the average of 23 atmospheric chemistry transport models, and investigate the impact of uncertainties on our model results. We include these results in the new Fig. 5 in the revised ms. The model description is included on Page 11, Lines 13–18 of the revised ms. The results are presented and discussed on Page 16, Line 22–Page 17, Line 2 and Page 19, Lines 18–22, respectively, of the revised ms.

**Comment 16:** Page 14447, Line 10–15: It is a bit unclear what they wish to communicate in this paragraph. I suppose that they want to point out that DIP depositions are low, thus justifying neglecting it in their model. I suggest rewriting this paragraph to expose this point more clearly.

**Response 16:** We rephrase this paragraph to read

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].

on Page 7, Line 25–Page 8, Line 2 of the revised ms.

**Comment 17:** Page 14448, equation 2: Katsev and Crowe, *Geology*, 43(7), 2015 (doi: 10.1130/G36626.1) recently suggested a correction to the power law of remineralization under anoxic conditions. How this correction would affect their results?

**Response 17:** Unfortunately, we cannot include the power-law correction of remineralization under suboxic conditions of Katsev and Crowe [2015], because it would require resolving the time course

of particle export and remineralisation. However, our analysis does not resolve this short time scale. Nevertheless, we would like to point out that the variability in the Martin-curve exponent  $b$  implied by Katsev and Crowe [2015] is well within the range of  $b$  values reported by Guidi et al. [2015]. Variations in  $b$  play only a very minor role in our model, which we report on Page 18, Lines 9–12 of the revised ms and in supplementary Figs. S7 and . S8.

**Comment 18:** Page 14449, Line 1: “Martin-curve values” refer to the second part of equation 2 only, as EPU and EPS represents the export production (F). Perhaps clarify?

**Response 18:** We rephrased Page 14449, Lines 1–3 in the BGD ms to

where  $EP_U$  and  $EP_U+EP_S$  represent the export production (F in Eq. 2) in the upwelling region and the whole model domain, respectively;  $AMC_{UM}$  and  $AMC_D$  ( $(\frac{z}{100})^{-b}$  in Eq. 2) are the average Martin-curve values corresponding to the actual water depth ( $z$ ) in the ETOPO2 data;  $SD_{UM}$  and  $SD_D$  represent the percentages in contact with the sediment in the UM and D boxes, respectively (Table 2).

on Page 9, Lines 10–14 in the revised ms.

**Comment 19:** Page 14449, section 2. 4. 2: Their data-based estimate of benthic denitrification is derived from primary production estimates from satellite data. I would like them to also use other more direct ship-based measurements of primary productivity for the area or at least discuss how the two compare. In this respect, see review by Pennington et al., Progress in Oceanography, 69, 285–317, 2006.

**Response 19:** We now discuss these processes also with regard to ship-collected data from Pennington et al. [2006] on Page 19, Line 25–Page 20, Line 13 in the revised ms.

**Comment 20:** Page 14452, Line 11: Again, what is the error associated with this N deposition estimate?

**Response 20:** Please refer to **Response 15** of this response letter.

**Comment 21:** Page 14453, Line 13: Is that local or global  $NO_3^-$  inventories? Please clarify.

**Response 21:** These are the  $NO_3^-$  inventories of the UM and D boxes, respectively. The sentence is modified to

Due to the small sediment-area percentages, the annual nitrogen loss by benthic denitrification is 0.17 and 0.82 Tg N yr<sup>-1</sup> in the UM and D boxes, accounting for only about 0.14 and 0.0051 % year<sup>-1</sup>, respectively, of the  $NO_3^-$  inventories in these boxes (Table 2).

on Page 14, Lines 6–9 of the revised ms.

**Comment 22:** Page 14453, Line 4, Page 14454, Line 2 and Page 14455, lines 1–3: Can these results be included as Supplementary Materials?

**Response 22:** We now provide Figs. S2, S3, and S4 in the supplementary material of the revised ms.

**Comment 23:** Page 14457, Line 2: What about N<sub>2</sub> fixation limitation by Fe? See my general comments above.

**Response 23:** Please see our **Response 2** above in this response letter.

**Comment 24:** Page 14457, lines 7–10: Should also include a model scenario with correspondingly higher future N depositions

**Response 24:** The results for a future N deposition scenario are explained in **Response 3** of this response letter.

**Comment 25:** Page 14459, lines 5–8: This is essentially the same sentence as in the introduction. See my comment for Page 14444, Lines 10–14.

**Response 25:** We remove this sentence from the revised ms and rephrase the sentences on Page 14459, Lines 9–13 to:

Even though water-column denitrification has been considered the major fixed-N loss processes for simplicity, the stimulatory effects between nitrogen fixation and fixed-N loss, and phosphorus regeneration and fixed-N loss still apply even if anammox replaced water-column denitrification as the fixed-N loss pathway. Thus, the nitrogen-balancing mechanism in the ETSP should not depend on whether the fixed-N is lost through denitrification or anammox.

This will be added on Page 22, Lines 18–23 of the revised ms.

**Comment 26:** Page 14459, lines 16–24: I think the fact that many other models and observational results found that the ETSP is a  $\text{NO}_3$  sink might rather indicate that their model is inaccurate. Again, if the ETSP was a  $\text{NO}_3$ - source, we would not observe large N deficits (see my previous comment P. 14442, line 20).

**Response 26:** We do not state that the ETSP is a  $\text{NO}_3^-$  source, but rather that the water column of our model domain including both the upwelling region and the vast open ocean is  $\text{NO}_3^-$  source. Please refer to **Response 6** of this response letter.

**Comment 27:** I find that the discussion/conclusion section ends rather abruptly. I recommend adding a short summary paragraph, including the major implications of their findings.

**Response 27:** A Summary section is added on Pages 23–24 of the revised ms:

The influence of atmospheric nitrogen deposition and benthic remineralization on the nitrogen budget of the ETSP is investigated with a conceptually simple and computationally efficient box model. Additional nitrogen input by atmospheric nitrogen deposition is offset by about 48 % by reduced nitrogen fixation with the remainder being transported horizontally out of the model domain, irrespective of uncertainties underlying the actual rate of nitrogen deposition and bioavailability of deposited DON. All our data- and model-based estimations for benthic remineralization are comparable with those of previous studies based on both models and observations. Modeled responses to these fluxes indicate stabilizing feedbacks, which tend to balance the nitrogen inventory. Variations in these fluxes due to variations in NPP, aphotic nitrogen fixation and organic matter remineralization under different oxygen conditions do not change these feedbacks. In the high-BD configuration, our model domain turns into a  $\text{NO}_3^-$  sink, which indicates that the whole tropical South Pacific, including the upwelling region, the open ocean, and the sediments, might be a  $\text{NO}_3^-$  sink. More research is needed to better constrain the fluxes of nitrogen and phosphorus, including the amount of bioavailable nitrogen deposition, nitrogen fixation and benthic denitrification, to allow a more reliable estimation of the nitrogen budget of the ETSP.

**Comment 28:** Table 1. I found this table rather confusing. The terms are defined both in the legend and in the upper part of the table. I suggest defining all terms in the legend and only including the lower part of the table, explaining the different model configurations (e.g., Syn1, Syn2, Syn3 and Syn4 in different columns and MBD, MPR, DBD, DPR, and N-DEP in rows).

**Response 28:** We agree that the Table 1 in the original BGD ms was confusing. MBD, MPR, DBD, N-DEP, Syn1, Syn2, Syn3 and Syn4 are all model configuration names, so we can not separate them into different rows and columns. However, we modify the caption of the table to improve its clarity. Please see Table 1 in the revised ms.

**Comment 29:** Tables 2 and 3. Another more comprehensive table summarizing all data based estimates (e.g. nitrogen deposition, N<sub>2</sub>-fixation, benthic denitrification and phosphate regeneration) used in their model as well as references for these data would be useful.

**Response 29:** We thank the reviewer for this suggestion. N<sub>2</sub>-fixation is a variable of our model, therefore we do not combine it together with the data-based estimates (nitrogen deposition, benthic denitrification, phosphorus regeneration). We combine Tables 2 and 3 of the BGD ms, and all the data-based estimates as well as their references in one table (Table 2 in the revised ms).

**Comment 30:** Aesthetic detail: Why is the space between N-fix and WC-denif in legend in Figures 2, 4 and 8 so large?

**Response 30:** This is now corrected in all figures in the revised ms.

**Comment 31:** Page 14456, Line 15: I would remove “below the water column” and change the sentence to: “...reaching the sea floor under suboxic conditions...”

**Response 31:** This is changed on Page 18, Line 13 of the revised ms.

**Comment 32:** Page 14459, Line 29: I would change to: “Based on our findings...”

**Response 32:** This is changed on Page 23, Lines 7–8 of the revised ms.

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