

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: 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 N₂ fixation with the other half exported out of their model domain, 2) sedimentary denitrification and phosphate regeneration under suboxic conditions acts to increase N₂ fixation, and 3) this increased N₂ 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 N₂ 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 N₂ fixation was stimulated by Fe addition in the ETSP. Therefore, I would like them to describe the role of Fe limitation on N₂ 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 N₂ fixation was inversely related to ambient Fe concentrations, so that the Fe enrichment raised N₂ 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 N₂ 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 PO₄³⁻”. 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 N₂ 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 Löscher et al., 2014 for N₂ 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 24–Page 21, Line 12 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 ESTP be a NO₃⁻ 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⁻¹ d⁻¹; Kalvelage et al., 2013) from direct measurements are also generally at least 1–2 orders of magnitude higher than N₂ fixation rates in the ETSP (0.01 to 0.9 nmol N l⁻¹ d⁻¹; Dekaezemacker et al., 2013 and Löscher 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 NO₃⁻ source but states explicitly that the **water column** of our model domain could be a NO₃⁻ source, and that the ETSP could be a NO₃⁻ sink when benthic denitrification is considered. Also, our model domain is the whole ETSP, encompassing the OMZ and the adjacent ocean, whose size is ≈ 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 NO₃⁻ 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 NH₄⁺ for anammox, as claimed in Lam et al., 2009, i.e., not all NH₄⁺ 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_4^+ 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⁻¹ 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_3^- 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_2 fixation is modeled. Maybe add a short section giving more detail about this?

Response 12: To provide more details about how we model N_2 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_2 as long as PO_4^{3-} 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_2 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 $\delta^{14}C$ of each box”. Perhaps clarify?

Response 13: To clarify why the average $\delta^{14}C$ 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 $\delta^{14}C$ [Key et al., 2004] of each box and biogeochemical parameters are constrained by literature data. $\delta^{14}C$ is the ^{13}C fractionation-corrected ratio of $^{14}C/^{12}C$, 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}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⁻¹ in the UM and D boxes, accounting for only about 0.14 and 0.0051 % year⁻¹, 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 N₂ 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 17–22 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 NO₃ sink might rather indicate that their model is inaccurate. Again, if the ETSP was a NO₃- 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 NO₃[−] source, but rather that the water column of our model domain including both the upwelling region and the vast open ocean is NO₃[−] 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 NO_3^- sink, which indicates that the whole tropical South Pacific, including the upwelling region, the open ocean, and the sediments, might be a 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_2 -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_2 -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.

References

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