General comments

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_2 fixation with the other half exported out of their model domain, 2) sedimentary denitrification and phosphate regeneration under suboxic conditions acts to increase N_2 fixation, and 3) this increased N_2 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*.

First, I feel that many simplifications have been made in their model. For instance, the authors assume that N_2 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_2 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_2 fixation was stimulated by Fe addition in the ETSP. Therefore, I would like them to describe the role of Fe limitation on N_2 fixation in their model. 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.

Third, I feel a comparison of their fluxes with direct measurements from previous studies is needed in their discussion. For example, do their N_2 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 N_2 fixation) for the considered region?

Specific comments:

Abstract:

Should include actual numbers (ranges) for global fluxes derived from their model.

P. 14442, line 20: This statement appears to be incorrect. How can the ESTP be a NO_3^- 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 N_2 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).

Introduction

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.

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_4^+ for anammox, as claimed in Lam et al., 2009, i.e., not all NH_4^+ is necessarily derived from organic matter oxidation in the water-column. How this would affect their <u>water-column</u> estimate of N-loss in their model?

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

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.

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.

Model description

Overall: It is a bit unclear to me how N_2 fixation is modeled. Maybe add a short section giving more detail about this?

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?

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.

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.

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.

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?

Page 14449, Line 1: "Martin-curve values" refer to the second part of equation 2 only, as EP_U and EP_S represents the export production (F). Perhaps clarify?

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.

Results:

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

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

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

Page 14457, Line 2: What about N_2 fixation limitation by Fe? See my general comments above.

Discussion and conclusions

See my general comments above and please revise this section accordingly.

Other comments:

Page 14457, lines 7-10: Should also include a model scenario with correspondingly higher future N depositions.

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

Page 14459, lines 16-24: I think the fact that many other models and observational results found that the ETSP is a NO_3^- sink might rather indicate that their model is inaccurate. Again, if the ETSP was a NO_3^- source, we would not observe large N deficits (see my previous comment P. 14442, line 20).

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

Tables and Figures

Table 1. I found this table rather confusing. The terms are defined both in the legend <u>and</u> 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).

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 <u>references</u> for these data would be useful.

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

Technical corrections

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

Page 14459, Line 29: I would change to: “Based on <u>our</u> findings…”