Biogeosciences Discuss., 12, C7190–C7193, 2015
www.biogeosciences-discuss.net/12/C7190/2015/


# *Interactive comment on* "Box-modeling of the impacts of atmospheric nitrogen deposition and benthic remineralization on the nitrogen cycle of the eastern tropical South Pacific" *by* B. Su et al.

**Anonymous Referee #1**

Received and published: 29 October 2015

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.

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.

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?

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 NO3- addition, an interesting finding that could also be discussed.

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.

Minor additional comments.

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

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).

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

Suggested additional references:

Bonnet, S., Dekaezemacker, J., Turk-Kubo, K. A., Moutin, T., Hamersley, R. M., Grosso,



O., ... & Capone, D. G. (2013). Aphotic N 2 fixation in the eastern tropical South Pacific Ocean. DOI: 10.1371/journal.pone.0081265

Guidi, L., L. Legendre, G. Reygondeau, J. Uitz, L. Stemmann, and S. A. Henson (2015), A new look at ocean carbon remineralization for estimating deepwater sequestration, Global Biogeochem. Cycles, 29, 1044–1059, doi:10.1002/2014GB005063.

Kanakidou M. et al., 2012, Atmospheric fluxes of organic N and P to the global ocean, GBC, 26, GB3026, doi:10.1029/2011GB004277

Interactive comment on Biogeosciences Discuss., 12, 14441, 2015.